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Maurice A. Finocchiaro

Science, Method, and Argument in Galileo

Philosophical, Historical, and Historiographical Essays

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Argumentation Library

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Preface

This book deals with two inter-related topics, Galileo and argumentation. Neither topic by itself needs much introduction, whereas their relationship or connection does.

In fact, Galileo made such significant contributions to physics, astronomy, instrumentation, and methodology that he is widely regarded, by scientists themselves, as the Father of Modern Science. Moreover, he had the misfortune of being the protagonist of one of the most (in)famous trials of all times, which some (Arthur Koestler) have labeled "the greatest scandal in Christendom." And both his scientific activities and his ecclesiastical troubles forced him to constantly engage in concrete methodological and epistemological reflections and discussions to such an extent and in such depth that he may be regarded as the Socrates of methodology.

Similarly, argumentation is a widely practiced and ultimately unavoidable human activity. It is also widely studied in many disciplines, such as philosophy, logical theory, mathematical logic, rhetoric, communication studies, linguistics, and cognitive psychology. At its most fundamental level, argumentation may be defined as a manner of thinking that attempts to justify a conclusion by giving reasons in support of it, or defending it from objections, or both.

Now, part of the connection between Galileo and argumentation may be illustrated with a sketch of how I became interested and involved in both.

During my high school studies, I developed an interest in physics, especially theoretical physics, with an emphasis on the word theoretical. As a result, I went to college with the intention of majoring in physics. However, I soon realized that what I had been calling "theoretical" physics corresponded more properly to what is best called the philosophy or logic of physics. In fact, I found myself fascinated by issues like the following.

Consider the laws of motion of classical mechanics. The law of inertia (the first law) states that every body persists in its state of rest or uniform motion in a straight line unless compelled by an external force to change that state. And the second law of motion states that force is the time rate-of-change of momentum, in other words, mass times acceleration. The questions I started asking myself were the following. The second law sounds like a definition of force; but how can the definition of a word or concept be a law of nature? Moreover, if in the law of inertia we substitute

the meaning of the word force, we get the following: every body persists in its state of rest or uniform motion in a straight line unless compelled to change that state by a change in its velocity, that is, unless it does not persist in that state. But the latter statement sounds like a tautology; and how can a tautology be a law of nature?

Thus, in college I majored in "humanities and science," specifically philosophy and physics. Then I went to graduate school in philosophy, to specialize in logic and philosophy of science. In logic, under the influence of Michael Scriven, I learned and adopted an empirical, practical, informal, naturalistic, and application-oriented approach that emphasizes actual argumentation, reasoning, and critical thinking. In philosophy of science, under the influence of Paul Feyerabend, I adopted an historical approach that aims to learn about the nature of science by studying important episodes in the history of science (e.g., the Copernican Revolution) and the work of great scientists (e.g., Galileo). In an attempt to combine the two, I conceived and wrote a doctoral dissertation on "The Problem of Explanation in Historiography of Science." This was a study of the nature of the explanations which professional historians of science may be expected to give in their discipline; for example, what kind of arguments historians of science use or should use to defend their explanatory claims.

Now, one of the case studies in this work was the explanation advanced by a leading historian of science (Alexandre Koyré) regarding how and why Galileo had succeeded in arriving at the correct law of falling bodies. This was an especially puzzling development in light of the fact that originally Galileo had assumed an incorrect principle, namely that the speed of a falling body increases in direct proportion to the distance traversed from rest. And the puzzle was increased by the fact that René Descartes, who shared the same error, never arrived at the correct law of fall. Such historical argumentation (by Koyré) introduced me to Galileo and his own scientific argumentation about the physics of falling bodies. This kind of inquiry, followed by other steps that can be easily imagined, soon led me to research and write the three essays in Chapters 1, 2, and 3 of this book.

However, there was a related but much more consequential line of development that accounts for several other essays in this book. When I started teaching courses in logic, both introductory and advanced, in light of my empirical approach, I wanted students to learn how to deal with actual arguments, and so I was always looking for examples of real arguments from the sciences, humanities, history, and current controversies. It did not take me long to discover that the seventeenth century Copernican controversy over the motion of the earth provided an extremely rich data base for argumentation, and in particular that Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632) was a uniquely significant sourcebook. Thus, I started focusing on the argumentation in this book both in the teaching of my logic courses and in my research. This research led me to the publication of three books and many articles. Now, most of these articles were incorporated into those books to a very large extent. However, some were not, and some stand out as relatively independent and self-contained essays; such is the case for Chapters 4, 5, 19, and 20 of this book.

Another group of this book's essays originated as follows. In part, this origin relates to the fact that the *Dialogue* is the book that triggered the trial and condemnation of Galileo by the Inquisition in 1633. Moreover, as I elaborate in Chapter 6, in teaching a course on the Introduction to Philosophy, I presented philosophy as characterized in a crucial way by argumentation and as exemplified in an iconic manner by Socrates and Galileo, and other important but less iconic figures. In such a context, Galileo's trial acquired a key focus. This required the study of the religious, theological, and biblical arguments that were widely discussed in the Copernican controversy (but not in the *Dialogue*), as well as the study of arguments about why Galileo was condemned, and about whether or not he deserved to be condemned by the Inquisition. Again, these studies led to the publication of four other books, as well as many articles, most of which could be integrated into those books; on the other hand, some of those articles could not be so incorporated, but have an intrinsic value of their own, and so they are included in the present volume (Chapters 6, 7, 11, 12, and 13).

Next, it ought to come as no surprise that this kind of investigation also led me to study what other scholars had to say about these topics. Now, given that (as just sketched) these topics involved Galileo-related argumentation, and given that (as mentioned earlier) I followed an empirical and informal approach to logic, such Galilean scholarship became itself another rich collection of arguments. This led me to write many essays that were critical examinations of scholarly works on Galileo. The most significant of these essays make up about half of the chapters in this book (Chapters 14–24). Obviously, their significance varies, both in terms of relevance to Galilean topics and relevance to argumentation theory. However, I would argue, each has such a double relevance, which I have tried to reflect in the titles and subtitles of these chapters.

Finally, I should say a word about Chapters 8, 9, and 10, which have not been mentioned yet. The essay in Chapter 8 was occasioned by the publication and reception of Dava Sobel's best-selling book, *Galileo's Daughter*. Both Chapters 9 and 10 were occasioned primarily by my being invited to the symposium on "Music and the Arts in the Thought of Galileo Galilei," at the Egida Sartori and Laura Alvini Seminar on Ancient Music, Fondazione Giorgio Cini, Venice, in May 2010. This led me to research and write about two topics that were new to me: Galileo's intellectual relationship to his musicologist father Vincenzo Galilei, and the problem of whether or not Galileo was the author of a letter addressed to the painter Lodovico Cigoli, dealing with the aesthetic merits of the arts of painting and sculpture. Despite the novelty (for me) and the *prima facie* esoteric character of these two topics, it turned out that the resulting essays provide more substantive and substantial contributions to the argumentation-Galileo theme than most of the other essays in this volume; this will become apparent in a moment, from their summary.

The sketch just given provides a kind of historical account of the origin of these essays. However, I also want to provide a synoptic outline of these essays from a more theoretical point of view.

Again, as already mentioned, this book is a study of two inter-related topics: Galileo and argumentation. Thus, there are arguments by Galileo, about the physics

of falling bodies and about the Copernican hypothesis of the earth's motion. There are arguments about Galileo's relationship to the Catholic Church, by proponents and by opponents of his condemnation. There are arguments by Galilean scholars, about Galileo's life, his scientific contributions, and his trial. And there are arguments by other significantly related people (such as Socrates, Giordano Bruno, and Galileo's musicologist father), in order to compare and contrast them with Galileo's arguments.

From the point of view of argumentation and logical theory, the topics may be described as follows. An ubiquitous, although implicit, concept is *the distinction between the interpretation and the evaluation of an argument*; of course, no one would deny the importance of this distinction, but to properly distinguish and inter-relate these two activities is easier said than done. Another equally ubiquitous concept is that of a *reasoning indicator or argumentative indicator*; this concept is mostly and usually implicit, but occasionally it becomes explicit and crucial (Chapter 10). A third ubiquitous and implicit concept is that of *meta-argumentation*; this is so not only in the sense that I am engaged in arguing about arguments (by Galileo, by his critics or supporters, and by Galilean scholars), but also in the sense that many of the arguments I argue about can themselves be viewed both as object-arguments (vis-à-vis my meta-argumentation) and as meta-arguments (from the viewpoint of what they are about). Another topic is the *structure of argumentation*; that is, how the various claims are inter-related, not only whether a given claim is a conclusion or a premise, but also whether a premise is an intermediate or an ultimate one, and whether two premises are interdependent or independent in their support for the conclusion. Then there is the topic of *fallacy*, that is, a common type of argument that appears to be correct but is actually incorrect; in particular, the fallacies of equivocation, circular reasoning, and begging the question turned out to be especially relevant. Sixth, an important concept that is implicit in many of these essays and explicit in at least one is the concept of *conductive argument*; "that is, an argument which attempts to justify a claim non-conclusively, on the basis of two or more pro reasons that are mutually independent but cumulative, and with the acknowledgment of at least one con reason" (Chapter 10, Section 10.1). A frequent issue that arises in my essays is the *principle of charity*: "that in criticizing an opposing argument or view, one should first formulate a charitable interpretation and then make the latter the target of one's criticism; a charitable interpretation is one that portrays the original argument or view in a reasonably favorable light, as possessing some strength that must be taken seriously, and as free of insignificant or trivial errors which do not affect the main issue" (Chapter 16, Section 16.5). Finally, there are three other normative principles of argumentation, which I derive partly from Galileo and partly from argumentation scholarship; as the citations and references in this book indicate, these principles are widely discussed, although often with different terminology. In my own terminology and framework, we have: the *principle of open-mindedness*, that one should be willing and able to learn, know, and understand the arguments against one's own view; the *principle of fair-mindedness*, that one should be willing and able to appreciate the strength of arguments and reasons against one's own view, even when one is attempting to criticize or refute them; and the *principle of judicious-mindedness*, that one should be willing and able to avoid one-sidedness (by properly taking into

account all distinct aspects of an issue) and to avoid extremism (by properly taking into account the two opposite sides of any one aspect).

With these generalities in the background, let us now go on to discuss some more substantive highlights of the present volume. The book is structured in four parts. Part I: Galilean Arguments; Part II: Comparisons and Contrasts; Part III: For or Against Galileo or the Church; and Part IV: Galilean Scholarship.

The Galilean arguments of Part I pertain primarily to the laws of falling bodies (Chapters 1–3) and to the confirmation of the Copernican idea of the earth's motion (Chapters 4–5). As early as 1604, Galileo had become convinced, based on experimental evidence, of the truth of the law of squares: that the distance traversed by a body falling from rest increases in proportion to the square of the time elapsed. However, he was searching for an explanation of why this was true, by trying to derive it from some simple and intelligible principle. For many years, the hypothesis he worked with was the principle of space proportionality: that the speed of a falling body increases in direct proportion to the distance traversed. However, he was never able to construct a derivation which he found satisfactory. Moreover, at some point he thought he had found a conclusive refutation of this principle. Thus, he began working with the principle of time proportionality instead, according to which the speed of a falling body increases in direct proportion to the time elapsed. From this principle, he was eventually able to construct a derivation of the law of squares which he found satisfactory and which is indeed correct.

For the refutation of space proportionality, Galileo thought he could show that this principle implied instantaneous motion, which would render it absurdly false. However, it turns out that, although space proportionality is indeed false, Galileo's argument to refute it is incorrect. Chapter 1 deals primarily with the interpretation of Galileo's argument against space proportionality. Chapter 2 elaborates my evaluation of this Galilean argument as a fallacy of equivocation. Now, because of the care with which I examine other interpretations of Galileo's argument, and because of my meticulous adherence to the principle of charity, I believe that my attribution of such a fallacy to Galileo is extremely significant and instructive.

Chapter 3 focuses on the principle of time proportionality, although not on Galileo's derivation from it of the law of squares, which is correct and unproblematic. Rather I focus on Galileo's attempt to show that time proportionality is an intelligible notion, sufficiently comprehensible so that by deriving the law of squares from it, we have achieved an explanation of the latter. And I argue that this Galilean argument is a plausible one.

The rest of Part I (Chapters 4 and 5) deals with Galileo's other major contribution to science, namely the confirmation of the Copernican hypothesis that the earth rotates once a day around its own axis and revolves once a year around the sun. This confirmation is found primarily in Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632). Accordingly, Chapter 4 advances the interpretation that this book is essentially a critical examination of all physical, astronomical, and philosophical (but not theological) arguments for and against the motion of the earth. And my evaluation is that Galileo succeeds in showing that the arguments in favor are much stronger than the arguments against, and so it is

probable that the earth moves. And it is also important to note that this does not mean that there was a conclusive proof of the earth's motion, or that the earth's motion could be asserted with certainty. Furthermore, this interpretation and evaluation also correspond to Galileo's own view.

Chapter 5 examines a particular example of such argumentation. That is, one of the many arguments against the motion of the earth was based on the experiment of dropping an object from the top of a ship's mast when the ship is moving forward. The argument alleged that the object falls away from the foot of the mast, toward the back, when the ship is moving; and that only when the ship is standing still, does the object fall to the foot of the mast. Therefore, if the earth rotates (eastward), then a rock dropped from the top of a tower would fall some distance away (westward) from the foot of the tower, thus following a slanted path. However, the experiment from the tower reveals that in fact the rock falls to the foot of the tower. It follows that the earth must not be in rotation.

Galileo criticized this argument in several ways, logical, theoretical, and experimental. Chapter 5 discusses Galileo's critique, as well as several related issues pertaining to the history, the historiography, and the physics of the ship experiment.

In Part II ("Comparisons and Contrasts," Chapters 6–10), I examine similarities and differences between Galileo and other relevant figures. One of these comparisons (Chapter 6) involves thinkers who have their own classic and iconic status: Socrates and Karl Marx; this may appear surprising from the point of view of the subject matter they dealt with (which is very different), but it should become relatively obvious once one takes the point of view of argumentation. On the other hand, the comparison between the Inquisition trials of Galileo and Giordano Bruno (Chapter 7) may at first appear relatively obvious, but a focus on argumentation will help us understand their difference. Other essays focus on Galileo's relationship to his daughter, nun Sister Maria Celeste (Chapter 8), and to his father Vincenzo (Chapter 9); they are included not for the sake of quaintness, but for the significance of the issues involved, especially in the case of the father.

In fact, as I discuss in Chapter 9, Galileo's father, Vincenzo Galilei, was an accomplished musician and musicologist, and a pioneer of the experimental method, in the context of how the kind and harmony of sounds vary when sounds are produced by vibrating strings and we change the tension, or length, or material of such strings. There is no question that Galileo learned the art of experimentation from his father. However, Galileo applied it to the investigation of falling bodies to determine the relationship among speed acquired, distance fallen, and time elapsed. Now, Vincenzo also wrote a book entitled *Dialogue on Ancient and Modern Music* (1591), which in some ways is similar to Galileo's *Dialogue*. Thus, the question arises whether Galileo was also influenced by his father with regard to this book. A comparative analysis of the argumentation in these two books enables us to answer this question (in the negative).

Especially noteworthy is Chapter 10, which is a critical analysis of a letter addressed to an artist friend of Galileo named Lodovico Cigoli, but whose authorship (by Galileo or not) is controversial among scholars. The letter contains a long, complex, and intense piece of reasoning; it advances multiple criticisms of several

arguments for the aesthetic superiority of sculpture over painting, and it formulates several counter-arguments for the superiority of painting. I argue that this letter is not an authentic Galilean document, but was written by someone else. My argument is based on a detailed analysis of the method of argument found in the letter and the one found in well-known Galilean works; although there are some similarities, the differences are much more significant, involving clarity, open-mindedness, and the use of illative terms (i.e., argumentative indicators).

Part III ("For or Against Galileo or the Church," Chapters 11–17) contains several "para-clerical" studies. By "para-clericalism" I mean an approach to the Galileo affair, and to questions about the relationship between science and religion in general, which adopts a perspective that is secular-minded, but appreciative of religion, and yet committed to the belief that such topics are too important to leave to religious believers. Here, an important connection to argumentation is that the para-clerical approach is an illustration of the principles of open-mindedness, fair-mindedness, and judicious-mindedness. Chapter 11 explains the approach by reference to the contributions made by several scholars with connections to the University of California-Berkeley: Paul Feyerabend, John Heilbron, and Ronald Numbers.

Chapter 12 is a refutation of two anti-clerical myths, that during his Inquisition trial Galileo was tortured, and afterwards he was imprisoned as punishment; but I also argue that for about 200 years after the trial the available evidence made these two theses reasonable and well-founded. Chapter 13 examines the vicissitudes of Galileo's career from the viewpoint of both the support and the opposition he received; it stresses the following fact which is known to specialized scholars but seldom elaborated: in the second half of his life (1610–1642), when Galileo held the position of Philosopher and Chief Mathematician to the Grand Duke of Tuscany, despite the persecution he suffered from clerical persons and institutions, his salary was actually paid indirectly by the Catholic Church; it derived from the tax on Church property authorized by a papal bull to finance the University of Pisa.

Chapter 14 is a critical examination of the anthology *The Church and Galileo* (2005) edited by Ernan McMullin, and of McMullin's position on the topic; primarily, it discusses the similarities and differences between the original controversy in 1613–1633 when the Church was persecuting Galileo, and the ongoing subsequent controversy when the Church is usually criticized for its treatment of Galileo; and it also elaborates a critical appreciation of Pope Saint John Paul II's alleged rehabilitation of Galileo in 1979–1992. Chapter 15 is a critical analysis of Thomas Mayer's *The Roman Inquisition: Trying Galileo* (2015); its main relevance here is that Mayer defends what may be regarded as an apologetic thesis, that a crucial document dated February 26, 1616 is authentic and contains a legitimate injunction issued to Galileo by the Inquisition's Commissary; the chapter criticizes Mayer's argument in several ways, chiefly insofar as he is committed to the self-defeatingly anti-clerical claim that the commissary acted legally and properly, whereas almost all the other officials (popes, inquisitors, and Cardinal Bellarmine) did not. Chapter 16 is a constructive elaboration and favorable evaluation of Joseph Agassi's account of Galileo's trial, insofar as Agassi explicitly practices several methodological approaches: the principle of charity; an emphasis on argumentation; and the para-clerical ideal of being

both secular-minded and appreciative of religion. Chapter 17 is a critical appreciation of the scholarly work on Galileo's trial by Mons. Sergio Pagano, Emeritus Prefect of the Vatican Secret Archives; it argues that Pagano has contributed to strengthening conclusively the thesis that the crucial document of February 26, 1616, is authentic and not a forgery; but from this documentary authenticity Pagano infers fallaciously its historical accuracy and its legal validity; this chapter also argues that the document is demonstrably illegitimate and probably inaccurate.

Part IV ("Galilean Scholarship," Chapters 18–24) is a series of critical analyses of most major works of Galilean scholarship of the last half a century; they are listed not in the order in which these works were published, but mostly in the order in which the critiques were written by the present author. Basically, these works are reconstructed as arguments about Galileo and evaluated for their merits and shortcomings. In the process, useful methodological lessons are applied, tested, or derived; some involve conceptual distinctions that need to be made, others involve key substantive points that need to be taken into account.

In Chapter 18, Dudley Shapere's *Galileo: A Philosophical Study* (1974) provides an opportunity to compare and contrast argumentation in the three distinct contexts of philosophy, history, and erudition. In Chapter 19, we see that Alexandre Koyré's *Études galiléennes* (1939) attributes to Galileo critiques of anti-Copernican arguments that are all supposedly invalid because Galileo is presupposing an a priori rationalism which is no more acceptable than that of the anti-Copernicans; although Koyré's argumentation may be appreciated for its intensity and complexity, it is essentially misconceived because he fails to properly distinguish a priori rationalism and critical reasoning. In Chapter 20, we find Paul Feyerabend's *Against Method* (1975) engaged in comparably intense and complex argumentation in order to attribute to Galileo an irrationalist methodology of "anything goes"; but I argue that Feyerabend's analysis can be constructively modified so as to attribute to Galileo a methodology that is soundly rational and judiciously rhetorical. In Chapter 21, William Wallace's *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (1984) is appreciated for its emphasis on Galileo's "suppositional reasoning"; in my view, this is a type of argumentation that enhances the logical character of Galileo's work, although Wallace is more concerned to connect it to the progressive Aristotelianism that prevailed at that time in the Jesuit Roman College. Chapter 22 contains two critical reviews of two books by Stillman Drake, *Galileo at Work: His Scientific Biography* (1978) and *Galileo against the Philosophers* (1976); these works are appreciated for the emphasis on the experimental nature of Galileo's science, but they are criticized insofar as Drake advocates an anti-philosophical interpretation of Galileo that confuses explicit argumentation and methodological analysis with metaphysical speculation.

In Chapter 23, Michele Camerota's *Galileo Galilei e la cultura scientifica nell'età della Controriforma* (2004) is appreciated in several ways, an important one being that he understands and explains very well a key point about Galileo's argumentation in the *Dialogue*; that is, Galileo's criticism of Ptolemaic arguments and elaboration of Copernican ones applies with equal force to Tycho Brahe's world system, which claimed that the planets revolve around the sun, but the whole system revolves daily

and annually around the motionless earth at the center of the universe; the reason is that Galileo's arguments and critiques are formulated around the issue of the earth's motion, and the Ptolemaic and Tychonic systems (despite their differences) both claim that the earth stands still at the center of the universe. In Chapter 24, Alistair Crombie's unpublished typescript *Galileo's Natural Philosophy* is appreciated in several ways, but primarily criticized for its failure to properly understand the concepts of argumentation, demonstration, and disputation; that is, that demonstration is conclusive argumentation, that disputation is argumentation on both sides of an issue, and that disputation can be critical or a-critical (depending on whether or not the arguments on both sides are evaluated for their strength); these concepts were clearly understood and applied by Galileo, whereas Crombie's account is full of confused muddles, inaccurate interpretations, and untenable evaluations.

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I

Chapter 1 was originally published as "Vires Acquirit Eundo: The Passage Where Galileo Renounces Space-Acceleration and Causal Investigation," in *Physis: Rivista Internazionale di Storia della Scienza*, vol. 14 (1972), pp. 125–145. Republished with permission from Casa Editrice Leo S. Olschki, Florence, Italy.

Chapter 2 was originally published as "Galileo's Space-Proportionality Argument: A Role for Logic in Historiography," in *Physis: Rivista Internazionale di Storia della Scienza*, vol. 15 (1973), pp. 65–72. Republished with permission from Casa Editrice Leo S. Olschki, Florence, Italy.

Chapter 3 was originally published as "Cause, Explanation, and Understanding in Science: Galileo's Case," in *The Review of Metaphysics*, vol. 29 (1975–1976), pp. 117–128. Republished with permission from this journal.

Chapter 4 was originally published as "Methodological Judgment and Critical Reasoning in Galileo's *Dialogue*," in *PSA 1994: Proceedings of the 1994 Biennial Meeting of the Philosophy of Science Association*, ed. David Hull, M. Forbes, and R.M. Burian, vol. 2, pp. 248–257 (East Lansing, MI: Philosophy of Science Association, 1995). Republished with permission from the University of Chicago Press for the Philosophy of Science Association.

Chapter 5 was originally published as "Defending Copernicus and Galileo: Critical Reasoning and the Ship-Experiment Argument," in *The Review of Metaphysics*, vol. 64 (2010–2011), pp. 75–103. Republished with permission from this journal.

Chapter 6 has not been previously published. Shorter versions of this essay were presented at several venues: Annual International Conference on Critical Thinking and Educational Reform, Sonoma State University, Rohnert Park, CA, in 1987, 1988, and 1995; Association for Informal Logic and Critical Thinking, meeting in conjunction with the American Philosophical Association, Eastern Division, Atlanta, in 1989; Conference on Critical Thinking and Education, sponsored by the Centre for Research in Critical Thinking and by the University of Cambridge Local Examination Syndicate, at the University of East Anglia (Norwich, England), in 1994; and Second Intermountain Critical Thinking Conference, University of Nevada, Las Vegas, in 1994.

Chapter 7 was originally published as "Philosophy versus Religion and Science versus Religion: The Trials of Bruno and Galileo," in *Giordano Bruno: Philosopher of the Renaissance*, ed. Hilary Gatti, pp. 51–96 (Aldershot, England: Ashgate, 2002). Republished with permission from Taylor & Francis Informa, UK Ltd—Books, through the Copyright Clearance Center, Inc.

Chapter 8 has not been previously published. A shorter version was originally presented on November 6, 2002, as a public lecture sponsored by the Philosophy Department, University of Nevada, Las Vegas.

Chapter 9 was originally published as "Vincenzo Galilei's Musicology and Galileo's Science: Methodological Comparison and Contrast," in *Isis, Journal of the History of Science Society*, vol. 111, no. 4 (December 2020), pp. 740–758. Published by the University of Chicago Press; ©2020 by the History of Science Society.

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Chapter 11 was originally published as "A Para-clerical Approach to the Galileo Affair and to Science vs. Religion," in *Science, Religion and Culture*, vol. 2, no. 4 (December 2015), pp. 125–142; published by the ResearchersLinks on Open Access.

Chapter 12 was originally published as "Myth 8: That Galileo Was Imprisoned and Tortured for Advocating Copernicanism," in *Galileo Goes to Jail and Other Myths about Science and Religion*, ed. Ronald L. Numbers, pp. 68–78, 249–252 (Cambridge, MA: Harvard University Press, 2009). Copyright ©2009 by the President and Fellows of Harvard College; republished with permission.

Chapter 13 was originally published as "Galileo under Fire and under Patronage," in *Ideas under Fire: Historical Studies of Philosophy and Science in Adversity*, ed. Jonathan Lavery, Louis Groarke, and William Sweet, pp. 123–143 (Madison and Teaneck, NJ: Fairleigh Dickinson University Press; and Lanham, MD: Rowman & Littlefield Publishing Group, 2013; all rights reserved). Republished with permission from Rowman & Littlefield.

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Crombie on Galileo and Mersenne, and who is editing an anthology of essays on Crombie's book.

II

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III

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Feyerabend, John Heilbron, and Michael Scriven; I cannot elaborate here, but suffice it to say that the last three were my mentors in graduate school, and the former three were scholars whose background was very different from mine, but whose work was extremely inspiring to me.

Contents

Part I
Galilean Arguments

Chapter 1
Rejection of Space-Proportional Speed

Abstract This essay attempts to provide a critical understanding of the famous passage in Galileo's *Discourse on Two New Sciences* where he rejects the idea that the speed of a falling body increases in proportion to the distance traversed. Accordingly, a new English translation of the passage is made. Several inadequacies in the standard translation of it by Crew and De Salvio are exposed. Stillman Drake's interpretation, based on his discovery of one crucial translation error, is stated and criticized. The traditional interpretation, in accordance with the Merton Rule, is re-examined and shown to be both strengthened by Drake's discovery and yet unacceptable. And a critical explanation is elaborated, avoiding both uncritical elucidation and misunderstanding criticism.

1.1 Introduction

'Vires Acquirit Eundo?' is the title that Antonio Favaro, the great Galileo scholar who edited the definitive edition of his works,[1] once gave to the passage in the *Discourse on Two New Sciences* where Galileo renounces causal investigation and space-uniform acceleration.[2] The Latin phase occurs at about the middle of the passage in the subjunctive mood (*vires acquirat eundo*). It is there used by Simplicio, who is speaking Italian, to describe in unprosaic terms what he believes a falling body to do: "it acquires strength as it keeps going." The poet being quoted by Simplicio is Vergil,[3] who was referring to Fame (or Rumor). But it isn't only Vergil's Fame or Simplicio's falling body that "vires acquirit eundo." The controversy surrounding the passage is also a thing which "acquires strength as it keeps going."

Perhaps no single passage in all of Galileo's writings, at least no passage of comparable briefness, contains so much material for controversy. One certainly need not recall here the philosophical, methodological, and scientific discussions that

[1] Galilei 1890–1909.

[2] See Favaro and Del Lungo 1911, 484–486.

[3] Vergil, *Aeneid*, IV-175.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_1

have surrounded and still surround the ideas contained in the passage. The historical problem, on the other hand, well deserves reformulation: it is simply that of arriving at a *critical explanation* of the passage; that is to say an *evaluation* of its main ideas based on a *genuine understanding* of them. One must try to avoid both uncritical elucidation and misunderstanding criticism.

To these more or less well-known problems (which is not to say *adequately solved ones*), has now been added a scholarly problem with important historical and philosophical consequences. The problem derives from Professor Stillman Drake's discovery that the major modern translations of this passage have been demonstrably inaccurate at one crucial point.[4] This discovery, which Professor Drake tries to exploit both to dismiss older interpretations and to construct a new one, will be discussed in detail below. Here it will suffice to say that the mistranslation involves the rendition of the Italian plural 'le velocità … furon doppie le velocità con le quali …' as the English singular 'the velocity … were double that …' or its French and German equivalent.

The scholarly problem is that there is no available adequate English translation of the crucial passage, and indeed of the whole *Discourse*. It might seem that it would suffice to insert Professor Drake's correction into the standard English translation by Crew and De Salvio.[5]

This will not do, first because, as it will be shown below, Professor Drake's own two-sentence translation involves a prejudicial change of punctuation and a prejudicial inversion of logical connectives. Second, as it will also be shown below, the Crew-De Salvio translation contains many more inaccuracies than the one mentioned by him. In the hope of correcting this situation, of inspiring the undertaking of a new English translation of the whole *Discourse*, and of providing the groundwork for a solution to the historical problem, I have translated the passage from the Italian of the eighth volume of the Favaro edition of Galileo's works.

1.2 The Passage[6]

SALV. It does not seem to me the appropriate time, at present, to enter into the investigation of the cause of the acceleration of natural motion, concerning which various judgments have been produced by various philosophers, some reducing it to the approach to the center, others to there successively remaining less of a portion of medium to cleave, others to a certain action of the ambient medium, which, in reuniting behind the moving body, continuously presses and expels it; one would have to proceed by examining and with little profit resolving these fanciful constructions, as well as others. For now it suffices to our Author that we understand that he wants to investigate and demonstrate for us some properties of an accelerated motion such

[4] Drake (1970a, 229–239; 1970b).

[5] See Galilei 1914.

[6] Galilei 1890–1909, vol. 8, p. 202, line 19 to p. 204, line 35.

that (whatever be the cause of its acceleration) the moments of its velocity go on increasing, after its departure from rest, in that very simple proportion in which the duration of time increases, which is to say that in equal times equal increments of velocity be made; and if it turns out that the accidents subsequently demonstrated be verified in the motion of naturally falling and accelerated bodies, then we may presume that the assumed definition comprehends such motion of falling bodies and that[7] it is true that their acceleration [*sic*; but read: velocity] goes on increasing as the time and duration of motion increases.

SAGR. As far as my intellect can perceive right now, it seems me that, without varying the concept, and with perhaps greater clarity, one could have defined: Uniformly accelerated motion to be that in which the velocity were to go on increasing as the space that is being passed increases; so that, for example, the degree of velocity acquired by a moving body in a fall of four arms were double that which it had at the end of its fall of two arms, and this double that attained in the space of the first arm. For it does not seem to me that it is to be doubted that a falling body which comes from a height of six arms have and strike with an impetus double that which it had at the end of its fall of three arms, triple that which it had at two, and sextuple that gotten in the space of one.

SALV. I am much consoled for having had such a companion in error; and I will also tell you that your discourse has so much verisimilitude and probability that even our Author did not deny, when I presented it to him, having formerly been for some time in the same fallacy. But what afterwards surprised me extremely was to see with four very simple words disclosed as not only false but impossible two propositions which have so much verisimilitude that, having presented them to many, I have not found anyone who would not freely admit them.

SIMP. To tell the truth I would number myself among those who concede: that a falling body [as Vergil might say] "acquire[s] strength as it keeps going," the velocity increasing in proportion to the space, and that the moment of an identical striking body be double as it comes from a double height, both seem to me propositions to be conceded without reluctance or controversy.

SALV. And yet they are as false and impossible as that motion should occur in an instant: and here is for you a very clear demonstration of it. When velocities have the same proportion as the spaces passed or to be passed, such spaces are passed in equal times; if therefore the velocities with which a falling body passed the space of four arms were doubles of the velocities with which it passed the first two arms (as the space is double the space), then the times for such passed spaces are equal: but for the same moving body to pass the four arms and the two in the same time cannot take place except in[8] instantaneous motion: but we see that a falling body makes its motion in time and passes the two arms in less time than the four; consequently it is false that its velocity increases as the space. The other proposition is shown to be false with the same clarity. For, the striking body being the same, the difference and moment of the strikes can only be determined by the difference of the velocity: if then the

[7] Here p. 202 ends and p. 203 begins.

[8] Here p. 203 ends and p. 204 begins.

striking body, coming from a double height, were to strike a blow of double moment, it would be necessary that it strike with double velocity: but a double velocity passes a double space in the same time, and we see the time of fall from the greater height to be longer.

SAGR. Too much obviousness, too much facility is this with which you manifest hidden conclusions: this very great easiness renders them of less value than they were while they remained under contrary appearance. I think that people would little value knowledge acquired with such little labor, as compared to that around which long and inexplicable altercations are made.

SALV. For those who with great brevity and easiness show the fallacies of propositions commonly regarded as true by everybody, to receive for it only neglect instead of approval would be a very tolerable harm; but they find quite disagreeable and annoying a certain other sentiment which is occasionally roused in some, who, pretending to be in the same studies at least equal to anyone at all, become aware of having gone over and accepted as true conclusions which are then by others with brief and easy discourse discovered and declared to be false. I will not call such sentiment envy, likely to be converted afterwards into hatred and anger against the discoverers of such fallacies, but I will call it a stimulus and a longing for wanting sooner to keep the inveterate errors than to permit that the newly discovered truths be received; this longing sometimes induces them to write in contradiction to those truths, unfortunately known internally even by themselves, only in order to keep the reputation of others low in mind of the many and not too intelligent common people. From our Academician I have heard a not small number of similar false conclusions, regarded as true but subject to very easy refutation, and I have also kept record of part of them.

1.3 The Crew-De Salvio Translation

How adequate is the standard English translation of this passage by Crew and De Salvio?[9] Its first problem relates to the second fanciful construction concerning the cause of natural acceleration mentioned by Salviati. This view is described in that translation as "repulsion between the very small parts of the body."[10] This is itself quite a fanciful invention since the Italian is 'restar successivamente manco parti del mezo da fendersi', which I have translated as 'there successively remaining less of a portion of medium to cleave'. What is being referred to is, I suppose, the view that, as the thickness of the air between the falling body and the earth decreases, the body can more easily overcome the resistance, and thus its speed increases.

Second, also in Salviati's first discourse, when he says what kind of accelerated motion he is concerned with, the Crew-De Salvio translation speaks of velocity

[9] Galilei 1914.

[10] Macmillan and Dover editions, p. 166; Northwestern edition, p. 160.

increasing "in simple proportionality to the time."[11] This is supposed to be a translation of 'con quella semplicissima proporzione con la quale cresce la continuazion del tempo', which means 'in that very simple proportion in which the duration of time increases'. What is lost in the Crew-De Salvio translation is the reference Galileo is here making to the elucidations with which he begins the section of the *Discourse* entitled "On Naturally Accelerated Motion."[12] These elucidations contain the famous simplicity argument for time-uniform acceleration based on the "supreme affinity between time and motion,"[13] the importance of which was emphasized by Koyré, who argued that it is the root of Galileo's success in this context.[14]

Third, the last sentence of Sagredo's first discourse means: "For it does seem to me that it is to be doubted that a falling body which comes from a height of six arms have and strike with an impetus double that which it had at the end of its fall of three arms, triple that which it had at two, and sextuple that gotten in the space of one."[15] The original 1914 edition of the Crew-De Salvio translation of this reads: "Because there is no doubt but that a heavy body falling from the height of six cubits has, and strikes with, a momentum double that it had at the end of three cubits, triple that which it had at the end of one."[16] This morass is reprinted verbatim in the Dover Publications edition.[17] The 1950 Northwestern University Press edition corrects the numerical misrepresentation, only to introduce a conceptual one. It reads: "Because there is no doubt but that a heavy body falling from the height of three cubits has, and strikes with, a momentum double that it had at the end of six cubits, triple that which it would have had if it had fallen from two, and sextuple that which it would have had at the end of one."[18] The last two phrases here make it look as if the body dropped from six cubits is being compared to its being dropped from two cubits or to its being dropped from one, whereas the original is comparing one and the same body at different points of its fall.

Fourth, the number of propositions being confuted by Galileo is a crucial issue, as has been stressed by Professor Stillman Drake, who distinguishes four, two representing Sagredo's and two Simplicio's.[19] The Crew-De Salvio translation hopelessly confuses the matter. It starts by translating as 'proposition' (in the singular)[20] the term 'discorso' at the beginning of Salviati's second speech.[21] Then it correctly translates 'due proposizioni' as 'two propositions' at the end of that speech. Then it fabricates

[11] Macmillan and Dover eds., p. 167; Northwestern ed., p. 160.
[12] Galilei 1890–1909, 8: 197–198.
[13] Galilei 1890–1909, 8: 197.
[14] Koyré 1966, 136–155.
[15] Galilei 1890–1909, 8: 203.
[16] Macmillan ed., p. 167.
[17] Dover ed., p. 167.
[18] Northwestern ed., p. 160f.
[19] Drake (1970a, 232f.; 1970b, 32).
[20] Macmillan and Dover eds., p. 167; Northwestern ed., p. 161.
[21] Galilei 1890–1909, 8: 203.

something called 'the proposition', which Simplicio is said to accept, at the beginning of his speech. And finally it reverts back to the plural 'propositions', as in the Italian, at the end of Simplicio's speech.

Fifth, when Salviati argues that it is false that velocity increases as the space, the Crew-De Salvio translation has him use the numerical examples of "eight feet" and "four feet." Now, the units "feet" and the comparison "eight to four" are not those which it makes Sagredo use, which are respectively "cubits" and "four to two." Yet in the original they are the same: "braccia" and "quattro: due." The English thus obscures the fact that Salviati's argument must be directed at Sagredo's problem since the former uses the same numbers and units as the latter. The determination of to whom the argument is directed is important for determining the function of the argument; this has been stressed by Professor S. Drake, yet he is clearly in error when he speaks of "Simplicio's [sic] numerical exemplifications,"[22] and to the extent that it is from the latter notion he concludes that the present argument is a reply to Simplicio and not to Sagredo.[23]

Sixth, throughout the passage, in the Crew-De Salvio translation, the term 'momentum' appears five times in a way offensive to the historical sensibility. It renders the expression 'i momenti della sua velocità' in Salviati's first speech[24] as 'the momentum of its velocity'[25]; and, in Salviati's third speech,[26] it renders the expression 'la differenza e momento delle percosse' as 'the difference of momentum in its blows', and 'percossa di doppio momento' as 'blow of double momentum'.[27] Whereas, clearly, in these three places, 'momento' corresponds to something that Galileo elsewhere, e.g. in Sagredo's first speech in the passage, terms 'grado' (degree) or something we would term (mathematical) 'value'. In Sagredo's first speech it is 'impeto' which is translated as 'momentum' by Crew-De Salvio. The rendition of Simplicio's 'momento' as 'momentum' is least objectionable but still somewhat disturbing.

Finally, the most important error comes in Salviati's argument to show that velocity does not increase as the space. Here the plural 'le velocità con le quali … furon doppie delle velocità con le quali …'[28] is translated as the singular 'the velocity with which … were double that with which …'[29] This was discovered by Professor S. Drake and will be discussed below.

Here it should be mentioned, by way of conclusion, that all of the seven oversights concern relatively straightforward linguistic, philological, and scholarly issues. They are issues of a type that standard scholarly care should resolve or prevent independently of historical and philosophical interpretations; or at least so I should like to

[22] Drake (1970a, 234; 1970b, 34).

[23] Drake (1970a, 233; 1970b, 32).

[24] Galilei 1890–1909, 8: 202.

[25] Macmillan and Dover eds., p. 167; Northwestern ed., 160.

[26] Galilei 1890–1909, 8: 204.

[27] Macmillan and Dover eds., p. 168; Northwestern ed., p. 161.

[28] Galilei 1890–1909, 8: 203.

[29] Macmillan and Dover eds., p. 168; Northwestern ed., p. 161.

believe. Equally should I like to believe that the number of errors in the translation of the passage under consideration is as exceptionally great as the number of issues touched upon by Galileo. One shudders to think otherwise.

1.4 Drake's Interpretation

Lovers of wisdom (philosophers?), of truth (historians?), as well as of Galileo should not fail to be grateful to Professor S. Drake for the new light he has shed on Galileo's famous argument against the space-uniformity of natural acceleration (as well as on Galileo in general).[30] Having discovered that the major modern translations of the relevant passage from the *Discourse* are demonstrably inaccurate, he has thereby argued that Galileo commits no error or fallacy and that there is no trace of his using the Merton Rule. He suggests that it was the mistranslation that helped mislead historians into a Merton Rule interpretation of that argument and contributed to create what he would like to declare the Myth of the Merton Rule (that Galileo used that medieval notion in his discovery of the laws of falling bodies).

Be that as it may, I wish to determine first the validity of Professor Drake's own positive interpretation. This interpretation has three elements: an account of the function of the argument, a reconstruction of the argument itself, and a correction of previous mistranslations of five crucial lines.

The alleged function of the present argument is that of a step in a more general argument designed by Galileo to show the non-equivalence of space-uniform and time-uniform acceleration. This idea he, interestingly, regards as one of Galileo's most important contributions to mechanics. According to Professor Drake the required proof is achieved by Galileo showing that the velocity of actual falling bodies is directly proportional to time *but not* to distance; therefore Galileo gives and has to give *independent* arguments, one showing that the velocity is proportional to time, the other that the velocity is not proportional to distance; the former is Galileo's longer and more well-known demonstration that the law of squares is *both* a consequence of time-uniform definition of acceleration and a fact of nature; the latter argument is the disputed one presently under consideration.

This account of the function of the space-uniformity argument is indeed a creation of incomparable beauty since it finds meaning in many passages in the Third Day of the *Discourse* which had been regarded as digressions. It also ties them together by means of the idea of the non-equivalence of space- and time-uniform accelerations. And it makes possible to specify a previously neglected sense of the originality of Galileo's scientific contributions.

However, if one turns to Professor Drake's rationale, his account loses its plausibility. The rationale is an examination of the context in which the argument is given. He wants to distinguish between Sagredo's and Simplicio's beliefs at this point and to make the disputed argument primarily a reply to Simplicio and only *the beginning*

[30] Drake (1970a, b).

of the answer to Sagredo. However, the passage reveals that even before Simplicio has intervened, Salviati, in referring to Sagredo's propositions, claims that "four very simple words"[31] are sufficient to make one see their falsity and impossibility. And in his final speech in the passage, Salviati stresses that point ad nauseam, in speaking of refutations characterized by "great brevity", "briefness", and "great easiness."[32] That speech, occasioned by Sagredo's comment about the obviousness and facility of Salviati's refutations, would make no sense at this point if it was not felt that Salviati had fully answered Sagredo. Therefore, Salviati's arguments after Simplicio's intervention must be the whole answer to Sagredo as well as to Simplicio on this matter. This is additionally supported and conclusively shown by the fact that the numerical exemplifications used by Salviati are taken from *Sagredo*, and *not* from Simplicio as Professor Drake inaccurately and misleadingly reports.[33]

His reconstruction of the argument, however, is such that it might be acceptable even if the account of its function is not. It is the following:

> If all the infinite instantaneous velocities occurring in actual accelerated fall from rest over any space, say one of four *braccia*, were the respective doubles of all the infinite velocities occurring over the first half, or two *braccia*, of the same fall, then no difference in the times of fall could be accounted for. But we observe a difference in the times; hence proportionality of speed to space traversed from the rest cannot govern the fall of actual bodies.
>
> In this there is no appeal to the correct definition of uniform acceleration, or to any of its consequences; neither is any contradiction asserted to exist within the incorrect law for falling bodies, which is merely shown to be in conflict with experience. Least of all is there any illicit use of a rule restricted to uniform motion.[34]

Now, however sympathetic I may be toward this reconstruction, which frees Galileo's argument from the usual errors projected onto it, I find the reconstructed argument unintelligible. That is, though I do not see any errors, neither do I see any argument. For I read the argument as a *modus tollens* in which a conditional ('if-then') statement is asserted, the 'then' clause denied as a matter of obvious fact, and the denial of the 'if' clause inferred. The argument is incomprehensible because the only problem in the context is the truth of the conditional premise, i.e. the *connection* between the 'if' and the 'then' clauses. I do not see any comprehensible connection between them, and, since Professor Drake does not provide one, his reconstruction provides no understanding of Galileo's argument. Professor Drake's discussion of Tenneur's argument[35] does not really throw any light on that *connection*, since this argument contains a gap at this very point. In other words, in freeing Galileo's argument from errors, Professor Drake has castrated it, has turned it into a non-argument or into an incomprehensible argument. What primarily requires argument, and what Galileo must be taken to be attempting to provide, is why there would be no difference in the times of fall given that the velocities increased in proportion to the spaces

[31] Galilei 1890–1909, 8: 203.

[32] Galilei 1890–1909, 8: 204.

[33] Drake (1970a, 234; 1970b, 34).

[34] Drake 1970b, 34; cf. Drake 1970a, 235f.

[35] Drake (1970a, 235; 1970b, 34–36).

traversed. It is *not* why velocities do not show such increase given that *if* they did *then* the times of fall would be equal.

Moreover, when Professor Drake denies that there is "any illicit use of a rule restricted to uniform motion,"[36] he can be right *at most* as regards the lines he considers. But he ignores completely what immediately follows them, which is another argument designed to show that just as false is the "other proposition" (that the moment of the blow of the same striking body is double when it comes from a double height). This argument speaks of velocity in the singular, not plural, like the previous one, and it appeals to the alleged fact that with a double velocity a double distance is passed in the same time.[37] And this can only refer to uniform motion. Hence in spite of the plurals in the first argument, the passage is infected with confusion and equivocation between uniform and accelerated motion.

Finally, Professor Drake does not mention the fact that Sagredo's immediate response to Salviati's arguments is that they appear *too* easy and *too* obvious,[38] not just *very* easy and obvious, but excessively so. Could this be a hint by Galileo to the "discerning reader" that there is something wrong with them?

The third element of Professor Drake's interpretation is his translation of the five crucial lines, and it too is logically independent of the previous elements. It is thus untouched by the preceding objections. But is it also unaffected by the facts of the matter? Pursuing and extending his own suggestion[39] that one examine the context, one is led to examine the original text, or at least that of the Favaro "National" Edition.[40] From the latter one discovers that the punctuation in Professor Drake's Italian quotation is different from that of the Favaro edition. By inspection one also discovers that Professor Drake's translation reverses the order of two logical connectives.

Let me elaborate on the matter of punctuation first. Professor Drake's quotation reads as follows:

> Quando le velocità hanno la medesima proporzione che gli spazii passati o da passarsi, tali spazii vengon passati in tempi eguali: se dunque le velocità con le quali il cadente passò lo spazio di quattro braccia, furon doppie delle velocità con le quali passò le due prime (si come lo spazio è doppio dello spazio) …[41]

The Favaro edition has a semicolon between 'eguali' and 'se', instead of a colon as Professor Drake quotes. I discount the possibility of a discrepancy between the

[36] Drake (1970a, 236; 1970b, 34).

[37] Galilei 1890–1909, 8: 204.

[38] Galilei 1890–1909, 8: 204.

[39] Drake (1970a, 232; 1970b, 31).

[40] The Leiden 1638 edition and the Favaro "National" Edition differ in minor respects, including punctuation; the differences in punctuation as regards the passage under consideration are such that they strengthen the substance of my criticism.

[41] Drake (1970a, 231; 1970b, 30); presumably quoted from Galilei 1890–1909, 8: 203.

original 1890–1909 Favaro edition, which is the one I have consulted, and the 1929–1939 reprint of that edition, which is presumably the one Professor Drake is using.[42]

This matter involves more than pedantry. The alteration (whether conscious or unconscious) may be shown to have some significance. In fact Professor Drake translates his quotation as follows:

> If the velocities have the same ratio as the spaces passed or to be passed, those spaces come to be passed in equal times: thus if the velocities with which the falling body passed the space of four *braccia* were doubles of the velocities with which it passed the first two (as the space is double the space) …[43]

Since he does not fill in for the dots, we may presume that *for the rest* of the argument he accepts the Crew-De Salvio translation, which is:

> … then the time-intervals required for these passages would be equal. But for one and the same body to fall eight feet and four feet in the same time is possible only in the case of instantaneous [discontinuous] motion; but observation shows us that the motion of a falling body occupies time, and less of it in covering a distance of four feet than of eight feet; therefore it is not true that its velocity increases in proportion to the space.[44]

Thus Professor Drake is not using the colon in the somewhat archaic function of indicating a separation greater than that marked by the semicolon but less than that marked by the period. He must be using it to indicate that what follows is an elucidation of what precedes. But the semicolon in the original can give no such indication. Now, one may want to *interpret* the sentence following that first semicolon as an elucidation of that preceding, but that interpretation should certainly not be presented as a textual report.

On the other hand, a certain amount of interpretation cannot be avoided in translation. Hence the colon in Professor Drake's *translation* is perhaps unobjectionable, in the abstract. But in the context of his argument, where he was criticizing previous mistranslations for having for having rendered plural nouns as singular, the rendition of a semicolon by a colon is somewhat questionable. Equally questionable, though—admittedly—strengthening his interpretation, is in the context his translation of 'se dunque', which literally means 'if therefore', as 'thus if'. When put in this order, these connectives give the impression that the whole hypothetical sentence following the 'thus' is a numerical exemplification of the hypothetical preceding it. And that leads one to attach a meaning to the first hypothetical in the light of the second, and consequently to the relative neglect of the statement with which the argument begins.

It may be said, of course, that Professor Drake was not concerned primarily with giving a new translation of the passage, but with giving a new interpretation; hence he should be granted the liberties he takes with the translation. But I have already criticized the other two elements of his interpretation. What I have now done is to criticize the last element, partly in order to emphasize the existence of the scholarly

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[42] Drake 1970a, 16. I also discount the possibility of Professor Drake's colon being a typographical error, since the colon is present in his quotation in both Drake 1970a, 16, and in Drake 1970b, 30.

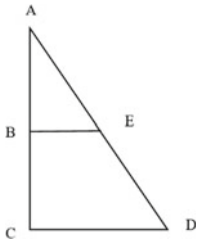
[43] Drake (1970a, 231; 1970b, 30).

[44] Drake (1970a, 230; 1970b, 29); quoted from Galilei 1914, 168.

problem that there is no adequate available English translation of the passage where Galileo renounces space-acceleration and causal investigation, and probably of the whole *Discourse*. The translation given above is aimed at partly remedying the situation and, until someone provides a new full translation, at providing the basis for further historical and philosophical investigations. To these we now turn.

1.5 The Merton-Rule Interpretation

First one should ask, how is the traditional Merton Rule reconstruction of the argument against space-acceleration affected by Professor Drake's a discovery and by the above considerations? One such reconstruction, given by Professor A. R. Hall, is the following:



> Suppose the body to fall from A to C. Using Salviati's figures, AB = 4 feet, AC = 8 feet. Let the velocity at B to that at C be as 1 to 2, in accord to the hypothesis. Then from the "Merton Rule," the uniform velocity equivalent to the "uniform difform" velocity of the fall from A to B is as 1/2 BE and the equivalent velocity for the whole fall from A to C is as 1/2 CD. But these quantities are in the ratio of the distances, that is as 1 to 2; and if twice the distance is traversed at twice the velocity, the time taken is the same, that is, the fall from B to C in instantaneous.[45]

This reconstruction has the merit of having at least the semblance of intelligibility. And in this respect it may be contrasted to Professor Drake's. That is, it provides a comprehensible, though admittedly questionable, connection between the antecedent and the consequent clauses of the first assertion in the argument: "When velocities have the same proportion as the spaces passed or to be passed, such spaces are passed in equal times."[46]

The problem derives from the fact that the connection is established by means of the mean-speed theorem (Merton Rule), which is a consequence of time-uniform acceleration (velocity proportional to time). The problem is transformed into an error by interpreting Galileo to be trying to show that space-acceleration is a logical impossibility. For in this case the aim would not have been achieved; instead all that would have been done is to show that space-acceleration together with at least the mean speed theorem is a logical impossibility.

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[45] Hall 1958, 343.

[46] Galilei 1890–1909, 8: 203.

Now, Professor Drake thinks that his discovery of the mistranslations of the second sentence of Galileo's argument invalidates this traditional Merton Rule interpretation.[47] Yet his retranslation actually strengthens that interpretation. To realize this one must first see that the Merton Rule interpretation is internally incoherent *given the standard Crew-De Salvio translation*. On the basis of this translation it could and should have been questioned on purely logical or philosophical grounds.

The relevant parts of the argument as translated by Crew and De Salvio and as quoted or referred to in such discussions[48] are the following:

> If the velocities are in proportion to the spaces traversed, or to be traversed, then these spaces are traversed in equal intervals of time; if, therefore, the velocity with which the falling body traverses a space of eight feet were double that with which it covered the first four feet (just as the one distance is double the other) then the time-intervals required for these passages would be equal.[49]

Now, the Merton Rule interpretation makes comprehensible the first sentence of this argument by providing the grounds that Galileo might have had in asserting it. But the second sentence, given the way it is translated here, must have been self-evident and needed no explanation. In particular, it does not need to be grounded on the first. For that second sentence speaks of "the velocity with which the falling body traverses a space of eight feet" and "that with which it covered the first four feet" and not of "the velocity which the falling body has after traversing eight feet" and "that which it had after covering four feet." Only if the latter were being referred to would the Merton Rule interpretation of the first sentence have any relevance to the second sentence. Or, to be more exact, since the Merton Rule interpreters concentrate on the first sentence, they actually ignore the mistranslated second. This attitude is the opposite of Professor Drake's who concentrates on the properly translated second sentence and almost ignores the first.

Let us see if a mean-speed theorem interpretation is possible for the correct translation. I have translated the passage as follows:

> When velocities have the same proportion as the spaces passed or to be passed, such spaces are passed in equal times; if therefore the velocities with which a falling body passed the space of four arms were doubles of the velocities with which it passed the first two arms (as the space is double the space), then the times for such passed spaces are equal …[50]

If the first sentence is interpreted as a consequence of space-acceleration *and* the Merton Rule, then the second sentence does become a proper consequence of the first: a situation in which the *velocities* with which a falling body passes the space of four arms are doubles of *those* with which it passes the first two arms is a special case of a situation in which velocities have the same proportion as the spaces passed or to be passed.

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[47] Drake (1970a, 230f; 1970b, 29f).

[48] See, for example, I. B. Cohen 1956, 231.

[49] Macmillan and Dover eds., p. 168; Northwestern ed., p. 161.

[50] Galilei 1890–1909, 8: 203.

It is in this way that Professor Drake's correction of previous translations strengthens, instead of weakening, the Merton Rule interpretation. And yet this reconstruction must be rejected for the following reason. Independently of the logic of the matter, the rhetoric of the situation is such that Salviati could have been using the mean speed theorem only in the most trivial and misleading sense of 'using'. In fact that use must be one such that it is combined with external verbal silence about it. For even if Salviati may be said to have that theorem "in his mind" when he makes the first assertion in the argument, neither Simplicio nor Sagredo could possibly be taking it for granted. They are ignorant of it, since Salviati will not discuss it until later.[51] It is to an argument without even an implicit use of the mean speed theorem that Sagredo, in his last speech in the passage, must be attributing "obviousness and facility." It is *this* argument that must be reconstructed, not a fictional one that Salviati secretly might have had in mind at that point.

1.6 A Critical Explanation

Let us make a fresh start then, though one cognizant of what has been said above. We shall use and refer to our own translation of the passage. It is best to begin by trying to reconstruct Salviati's "very clear demonstration" and worry later about more general issues. There are several essential requirements that the reconstruction must satisfy. One is that it must deal with two different arguments, since that is the number present.

The second argument, which is less problematical, will be examined first. To begin with, there is no problem here about what proposition Salviati is referring to when he say that "the other proposition is shown to be false with the same clarity."[52] In fact the second proposition that seems to Simplicio "to be conceded without reluctance or controversy" is that "the moment of an identical striking body be double as it comes from a double height."[53] And Sagredo's last assertion, which "does not seem to [him] that it is to be doubted" was that "a falling body that comes from a height of six arms have and strike with an impetus double that which it had at the end of its fall of three arms, triple that which it had at two, and sextuple that gotten in the space of one."[54]

The argument may be reconstructed as follows. It is obvious that (1) differences in the impetus for one and the same striking body can only be due to differences in its velocity; assuming then that (2) a striking body coming from a double height strikes with double impetus, it follows that (3) a striking body coming from a double height strikes with double velocity; but obviously (4) a body having a double velocity passes a double distance in the same time that it passes the undoubled distance with the undoubled velocity; hence we may conclude that (5) a striking body falls from

[51] Galilei 1890–1909, 8: 208.
[52] Galilei 1890–1909, 8: 204.
[53] Galilei 1890–1909, 8: 203.
[54] Galilei 1890–1909, 8: 203.

a double height in the same time that it falls from the undoubled height; but it is observed that (6) a striking body falls from a double height in a longer time than it takes it to fall from the undoubled height; therefore (7) the assumption is false, that is, it is false that a striking body coming from a double height strikes with double impetus.

It is clear that if (4) is to be true and obvious, as it is being regarded here by Galileo, it must be a fact about uniform or constant velocity. It is also clear that if (6) is to conflict with (3) and (4), as it is Galileo's intention here, he must be having (5) in mind. But what may be concluded from (3) and (4) is *not* (5), which *does* conflict with (6), but rather the following, which does not: (5') a striking body coming from a double height strikes with a velocity that would enable it to pass a distance equal to that double height in the same time that would be required for it to pass a distance equal to the undoubled height with the velocity that it has after it has fallen through the undoubled height. Why is Galileo inferring the fallacious (5)? *That* is not clear. However, since this reconstruction is obviously accurate and easily comprehensible, the problem is not with the interpretation.

Let us consider the first argument now. Some essential requirements of its reconstruction are the following. First it must assign different functions to each of the first two sentences in the argument, the one preceding and the one following the semicolon; it must not interpret one to be the anticipation or the repetition of the other. Moreover, the first sentence must be construed in such a way that it is obvious and uncontroversial in the context; it should be a statement not requiring justification, least of all an elaborate one using a diagram. Finally a plausible interpretation must be found for the peculiar sequence of logical connectives in the second sentence; the Italian 'se dunque …, adunque', which I have translated as 'if therefore …, then', is best interpreted to mean 'if then …, we may conclude that'.

This argument should then be reconstructed as follows. Assume that (1) the velocity of a falling body is proportional to the space passed; then (2) the velocities with which a falling body passes the double of a given space are doubles of the velocities with which it passes that space; e.g., (3) the velocities with which a falling body passes four arms are double those with which it passes the first two arms. But (4) whenever velocities are as the spaces, such spaces are passed in equal times. Hence it follows from our assumption that (5) the space of four arms and the space of the first two arms are passed in equal times. But we see that (6) a falling body passes the first two arms in less time than the whole four arms. Hence (7) the assumption is false, i.e., it is false that the velocity of a falling body is proportional to the space passed.

(1) is the unstated assumption with whose denial the argument ends. (2) corresponds to the statement that Galileo puts in parenthesis, which is thus given a function. (3) corresponds to the clause following the 'if therefore' and preceding the clause in parenthesis. (4) corresponds to the first statement in the argument; it is essentially a theorem Galileo has already proved,[55] but alas, one about non-accelerated motion (uniform velocities). (5) presumably follows from (3) and (4); it corresponds to the

[55] Galilei 1890–1909, 8: 193.

last clause of the second sentence, the clause following the parenthesis. (6) corresponds to the fourth sentence in the argument, and it is a matter of observation. (7) corresponds to the last sentence, the apparent conclusion of the argument.

There are two problems with this interpretation. The more obvious one is that Galileo is being attributed an error: the misapplication of a fact about uniform motion to a situation involving accelerated motion; that is what is happening when (3) and (4) are combined to infer (5). In other words, the principle being used really means and should be stated more accurately: whenever *uniform* velocities are as the spaces, such spaces are passed in equal times. And neither the velocities with which a falling body passes four arms nor those with which it passes the first two arms are uniform velocities.

Now, it is itself an error and a fallacy to think that Galileo's error, as just described, is that of *confusing* uniform motion and accelerated motion; this would be too enormous to be plausibly attributed to Galileo. The *misapplication* of a fact about uniform motion to accelerated motion presupposes that one is distinguishing the two; it merely involves an attempt to relate them and the actual incorrect relating of the two. Let us not, then, confuse misapplications with confusions.

This distinction between a misapplication and a confusion provides the basis of the answer to Professor Drake's criticism of such a reconstruction of the argument. He says:

> Cazrae, who started the whole controversy in 1642, accused Galileo of assuming the validity for accelerated motion of a law he had previously developed for uniform motion, though the Latin treatise sharply separated the two sections, and no one of his day was more acutely aware than Galileo of the fallacy of any such assumption. Gassendi failed to support Galileo properly against the charge, and seems not to have noticed that Cazrae had replaced Galileo's "velocities" with a "velocity" in the manner of modern translators.[56]

That one can keep the plural 'velocities' is shown by my reconstruction above, which indeed shows how the plural makes the occurrence of the error more intelligible. Moreover, in speaking of "the fallacy of *any* assumption" concerning "the validity for accelerated motion of a law … developed for uniform motion," Professor Drake seems to think that one can never apply facts about uniform motion to situations involving accelerated motion; hence, he concludes, Galileo could not have made the error being alleged here. Though the reason is valid, it is not true.

In fact, the treatise on motion, contained among other things in the Third and Fourth Days of the *Discourse*, has three parts dealing successively with uniform, accelerated, and violent motion. Though the three parts are indeed kept distinct by Galileo (each is called a "book" by him), it is clear that each preceding part deals with a more general topic than that discussed in the following part. That the results concerning both uniform and accelerated motion are applied to violent motion in the third part is obvious. But equally true is the fact that the results about uniform motion are applied to accelerated motion in the second part; for example, the fourth theorem of the first book[57] is used in the proof of the second theorem of the second book (the

[56] Drake 1970b, 32f.

[57] Galilei 1890–1909, 8: 194.

crucial law of squares).[58] Obviously then, the question for Galileo as well as for us is not whether *any* fact about uniform motion can be applied to a situation involving accelerated motion, but which such fact can be applied to which such situation. In the attempted refutation of space-acceleration, Galileo simply made the mistake of applying the wrong fact to the wrong situation.

The second problem with the above reconstruction is that it ignores the third sentence in the argument, the one that speaks of instantaneous motion. In the light of Salviati's prefatory remark that the two propositions "are as false and impossible as that motion should occur in an instant," that sentence should not be ignored. Let us see, therefore, if a function can be assigned to it.

The sentence states: "but for the same moving body to pass the four arms and the two in the same time cannot take place except in instantaneous motion."[59] This seems superfluous at first and has been tentatively ignored because the visible fact that Salviati uses and needs is the one stated in the second clause of the next sentence, namely that the falling body "passes the two arms in less time that the four."[60] That "the falling body makes its motion in time," which is the first clause of that same next sentence, is not something Salviati seems to need, or can see (directly) for that matter.

What is the function of the talk about instantaneous motion? When Salviati prefaces his "very clear demonstration" by saying that the two propositions "are as false and impossible as that motion should occur in an instant," he must mean that each proposition implies instantaneous motion. For the argument we are now considering this means that instantaneous motion is implied by the idea that velocity increases as the space. Now, if all that Galileo wanted to do was to argue for the falsity of the latter idea, then he could have simply shown that it implies a particular falsehood, such as that a falling body passes in the same time the space of four arms and the first two arms of that space. But Galileo is apparently generalizing the implication to instantaneous motion, which he regards as being in direct and obvious conflict with the idea that "the falling body makes its motion in time." By the latter idea he must mean that the position of the falling body changes in a way somehow related to the way that time changes. Whereas in instantaneous motion the position changes without time changing, i.e., in a way not related to time. But if the position were to change without time changing, the velocity could not depend on time either, and if so, then in particular the velocity could not increase as the time. In other words Galileo regards as relatively obvious that instantaneous motion conflicts with time-uniform accelerated motion; therefore the latter conflicts with space-acceleration, which implies instantaneous motion.

In other words, in this first argument, Salviati is trying to show not merely that space-accelerated motion is false, but that it is as false as instantaneous motion; this means: if instantaneous motion is false, as it is, then space-acceleration is false; and he regards it as obvious that *if* time-acceleration is true, then instantaneous motion

[58] Galilei 1890–1909, 8: 120.

[59] Galilei 1890–1909, 8: 201f.

[60] Galilei 1890–1909, 8: 204.

is false. From the demonstrated fact and the obvious one it obviously follows that *if* time-acceleration is true, then space-acceleration is false, which also means that if space-acceleration is true, then time-acceleration is false; that is, space-acceleration and time-acceleration are incompatible, hence not equivalent.

Then what Salviati is doing *directly* is to try to refute Simplicio's first proposition "that a falling body [as Vergil might say] 'acquire[s] strength as it keeps going', the velocity increasing in proportion to the space." This argument, by a few obvious and easy steps becomes the refutation of Sagredo's first proposition, which should be one about the equivalence of space-acceleration and time-acceleration. And so it is: for a careful examination of Sagredo's first speech shows that it consists of two compound sentences separated by a period. The space-uniformity definition of acceleration, included in the first sentence, is an integral part of it and not a distinct thought or proposition because the main verb of that definition is in the infinitive mood. The clause between the semicolon and the period is not a distinct proposition either but rather an exemplification of the last clause of the definition; this is so because the verbs of both clauses are in the same tense and mood: imperfect subjunctive. Thus the thought expressed in the first sentence is: the concept would not be changed if (instead of defining uniform acceleration as being characterized by the velocity being proportional to the time) one defined it as being characterized by the velocity being proportional to the space, that is, for example, by the velocity at the end of a fall of four units being twice that at the end of the first two; that is, the two concepts are equivalent.

The second compound sentence of Sagredo's first speech constitutes the second of the two propositions which are alleged to be refutable "with four very simple words."[61]

The interpretation given indeed reconstructs the two arguments in four very simple words. "E così tutti i veri si rispondono."[62]

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[61] Galilei 1890–1909, 8: 203.

[62] Galilei 1890–1909, 8: 374, meaning "And thus all truths correspond with each other."

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Chapter 2
A Galilean Fallacy of Equivocation

Abstract In an attempt to illustrate and justify the relevance and usefulness of logic in the study of the history of science, Galileo's refutation of space-proportionality as found in *Two New Sciences* is analyzed in the light of some recent historical reinterpretations and with an awareness and appreciation of methodological distinctions. Having distinguished between the structure and the validity of Galileo's argument, the former is shown to be uniquely determined by the recent interpretations, thus reconciling some of their differences. It is then suggested that the argument must be evaluated as logically faulty, either in the sense of being a fallacy of equivocation, or in the sense of being a proof of *ignotum per aeque ignotum*; and some evidence is given supporting the former evaluation. These results are seen as valuable from the logician's point of view, independently of the historian's possible judgments of merit or demerit for Galileo.

2.1 Introduction

The results of the present paper in part vindicate and in part illustrate certain methods which I have been using and am in the process of developing for the study of the history of science. Basically the technique involves the application of logic in historiography of science. There is no question of such methods being *generally* applicable: they obviously are not, if for no other reason than because they are irrelevant to the kind of historical investigation discussed in my *History of Science as Explanation.*[1] However, the study of *some* things in the history of science is made immensely more effective by the use of the basic ideas of logic. I have shown elsewhere[2] that Newton's Third Rule of Philosophizing is one such thing. The present paper confirms my original premonition that Galileo's space-proportionality argument is another such entity.

[1] Finocchiaro 1973.

[2] Finocchiaro 1974c.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_2

Before I expound on this, it should be noted that the just-mentioned applicability of logic in historiography is only one side of the relationship between them; this relationship is, as Benedetto Croce might say, "dialectical" in the sense that there is an equally important dependence of logic on history of science; the nature of this dependence is that history of science provides the logician with some of the most genuine and significant examples of reasoning and arguments available to him; hence it is from the study of the structure and of the validity of such reasoning that the logician should abstract his principles; this is another way of saying that his logical theories must be grounded on logical practice, and that scientific reasoning provides paradigm instances of logical practice. For example, as I hope to show in a future work, and have in part shown in a preliminary way elsewhere,[3] Galileo's *Dialogue* ought to be regarded as a classic of logic (besides a classic of the history of science and of Italian literature).

Since the writing of my first article on the subject of Galileo's refutation of space-proportionality,[4] I have had occasion, from conversation and correspondence with Professor Stillman Drake, to reflect on the question of the similarities and differences of our interpretations.[5] I now believe that these reflections have born some fruit. It is the following.

2.2 Reconstruction

First, one guiding principle throughout these reflections was that these are two distinct questions regarding Galileo's space-proportionality argument: (1) determining exactly what the argument is; and (2) determining how valid the argument is. These two questions are interrelated and ought not to be *separated*, but they do pertain to different features of the argument.

The first problem can be solved more easily than the second in our case. In fact, I believe I can show that Professor Drake's reconstruction is really equivalent to mine. His original reconstruction can be paraphrased as follows:

(D1-2) If the speeds with which a falling body passes a given space are, respectively, doubles of the speeds with which it passes the first half of that space, then spaces of different lengths would be passed in equal times by a falling body.

(D1-1) But spaces of different lengths are passed in different times by a falling body.

(D1-0) Therefore, it is false that the speed of a falling body increases as the space

One problem with this reconstruction so stated is that it seems to ignore the sentence with which Galileo's own statement of the argument begins: "Quando le

[3] Finocchiaro 1974a, b.

[4] Finocchiaro 1972.

[5] Professor Drake's interpretation can be found in Drake (1970a, b).

velocità hanno la medesima proporzione che gli spazii passati o da passarsi, tali spazii vengon passati in tempi eguali."[6] However, in a sense, Professor Drake was not neglecting this sentence because, as I suggested in my article,[7] he was attaching to (D1-2) a meaning in the light of Galileo's first sentence. That is, the sentence that Professor Drake has in his own statement in place of (D1-2) and that *seems* to correspond to Galileo's *second* sentence, is actually a combination of both his first and second. Hence, in this sense, Professor Drake is not neglecting Galileo's first sentence. But what this means is that Professor Drake's reconstruction should be reconstructed as follows:

(D2-21) When speeds are as the spaces passed or to be passed, such spaces are passed in equal times.

(D2-2) Therefore, if the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space, then spaces of different lengths would be passed in equal times by a falling body.

(D2-1) But spaces of different lengths are passed in different times by a falling body.

(D2-0) Therefore, it is false that the speed of a falling body increases as the space.

Now, suppose we ask about this argument (D2), how exactly the conclusion is supposed to follow. First, how does (D2-2) follow from (D2-21)? A plausible answer might be that it is being assumed that

(D2-22) if the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space, then that means that its speeds are as the spaces passed or to be passed.

Then by what logicians call "hypothetical syllogism," from (D2-21) and (D2-22) it would follow that

(D2-2') if the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space, then such spaces are passed in equal times.

Since the relevant "such spaces" mentioned in the consequent clause of this proposition are spaces of different lengths, (D2-2') means (D2-2).

Let us now ask how (D2-0) follows from (D2-2) and (D2-1). What does follow directly from these two propositions, by *modus tollens*, is:

(D2-0') it is false that the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space

Now, if (D2-0) is to follow from this we must assume that

(D2-0") if the speed of a falling body increases as the space, then the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space.

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[6] Galilei 1890–1909, 8: 203.

[7] Finocchiaro 1972, 136–137, 139.

Then (D2-0) follows by *modus tollens* from (D2-0") and (D2-0').

Thus it would seem that Professor Drake's reconstruction, if fully written out, is the following:

(D3-222) When the speeds are as the spaces passed or to be passed, such spaces are passed in equal times.

(D3-221) But, if the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space, that means that its speeds are as the spaces passed or to be passed.

(D3-22) Therefore, if the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space, then such spaces (of different lengths) are passed in equal times.

(D3-21) But spaces of different lengths are passed in different times by a falling body.

(D3-2) Therefore, it is false that the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space.

(D3-1) But, if the speed of a falling body increases as the space, then the speeds with which it passes a given space are doubles of the speeds with which it passes the first half of that space.

(D3-0) Therefore, it is false that the speed of a falling body increases as the space

Now, my own original reconstruction[8]—call it (F1)—can itself be reconstructed as follows—call this (F2):

(F2-22) If the speed of a falling body increases as the space, then the speeds with which it passes a given space are doubles of the speeds with which it passes the first half of that space.

(F2-21) But, when the speeds are as the spaces (passed or to be passed), such spaces are passed in equal times.

(F2-2) Therefore, if the speed of a falling body increases as the space, then spaces of different lengths would be passed in equal times by a falling body.

(F2-1) But spaces of different lengths are passed in different times by a falling body.

(F2-0) Therefore, it is false that the speed of a falling body increases as the space

The step from (F2-2) and (F2-1) to (F2-0) is a pure instance of *modus tollens* and hence presents no problem. The plurals in the consequent clause of proposition (F2-22) make sense since there is no one actual speed with which a falling body passes a given space if its speed were to increase as the space. But how does (F2-2) follow from (F2-22) and (F2-21)? In particular, what exactly is the connection between these two premises? Originally[9] I had thought that (F2-2) did not follow because the speeds mentioned in the consequent of (F2-22) were not uniform, unlike those referred to

[8] Finocchiaro 1972, 141.

[9] Finocchiaro 1972, 142.

in the antecedent clause of (F2-21), which were uniform. It seems to me now that this distinction between uniform and nonuniform *speeds* does not make sense; what does make sense is the distinction between uniform and nonuniform motion, whose relevance is not immediately apparent. At any rate, there is no question that the following proposition would provide a connection:

(F2-23) if the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space, then its speeds are as the spaces passed or to be passed.

Now, when this proposition is inserted between (F2-22) and (F2-21), (F2-2) seems to follow since this part of the argument looks like an extended hypothetical syllogism: If A then B; if B then C; if C then D; therefore, if A then D.

It seems then that my own reconstruction, if fully written out, would be the following:

(F3-23) If the speed of a falling body increases as the space, that means that the speeds with which it passes a given space are doubles of the speeds with which it passes the first half of that space.

(F3-22) Now, if the speeds with which a falling body passes a given space are doubles of the speeds with which it passes the first half of that space, then that means that its speeds are as the spaces passed or to be passed.

(F3-21) But when the speeds are as the spaces passed or to be passed, such spaces are passed in equal times.

(F3-2) Therefore, if the speed of a falling body increases as the space, then the various spaces of different lengths over which it passes are passed in equal times.

(F3-1) But spaces of different lengths are passed in different times by a falling body.

(F3-0) Therefore, it is false that the speed of a falling body increases as the space.

But these two arguments, (D3) and (F3), are equivalent; their only difference is the order in which the various steps in the reasoning are carried out. In both arguments, the conclusion (D3-0) = (F3-0) is based on the same ultimate premises: (F3-21) = (D3-222), (F3-22) = (D3-221), (F3-23) = (D3-1), and (F3-1) = (D3-21).

2.3 Evaluation

Now, if we agree about all this, i.e. about what the argument is, which is the first problem in our methodological stipulation above, and if we also agree that the argument *looks* logically valid, can there be any further disagreement about the appraisal of the argument, i.e. about whether it really is valid or not? The fault, I believe, remains. It lies in the first step of (D3), or equivalently in the mental combining of (F3-22) and (F3-21) in the first inference of (F3). The trouble is that the consequent clause of (F3-22), or (D3-221), does not mean the same as the antecedent clause of

(F3-21), or (D3-222). The consequent clause means something like any one of the following: the speeds of the falling body are as the spaces passed in acquiring those speeds; or, the speeds (of the falling body) are as the total spaces passed from rest; or, the final speeds (of the falling body) are as the total spaces passed to acquire those final speeds. The antecedent clause means: the speeds are as the spaces passed (or to be passed) at those speeds. How do I know that these are the meanings? Because these are the only meanings that render true the two conditional propositions in question, or to be more exact, these are the only meanings which make these propositions better known than the conclusion of the argument and thus make sure that it is not a proof of *ignotum per aeque ignotum.* My evaluation here makes Galileo's argument a classic example of the fallacy of equivocation, which I would not want to use to detract from his merit, but rather to provide a significant and actual example of such a fallacy for application-oriented logicians. They can exploit the situation by actually taking delight in the existence of such a fallacy and by being grateful to Galileo for it.

Perhaps Professor Drake can show[10] by a Eudoxian proof using the Aristotelian-Galilean definition of equal speeds that (D3-222) is true in the sense that when the speeds are as the spaces covered to arrive at those speeds, then such spaces are passed in equal times. In that case Galileo's argument would be logically valid in an abstract sense of logical validity. But then it would run into the problem of proving the unknown by means of the equally unknown, and this is as much of a logical problem as an equivocation would be.

Finally, the question ought to be asked, which of the two meanings did Galileo really have in mind in asserting (D3-222)? One cannot argue, as Professor Drake might be inclined to do, that Galileo must have meant "when the speeds are as the spaces passed to arrive at those speeds, such spaces are passed in equal times" on the grounds that otherwise the argument commits the fallacy of equivocation. One cannot argue this way because if this fallacy is not committed, the fallacy of proving the unknown by means of the equally unknown is committed then. The situation is that the argument commits either one fallacy or the other. And I believe it is more implausible to let Galileo fall into the latter. Moreover, a crucial piece of textual evidence supports my interpretation: the fact that Galileo uses the word "quando" in his first sentence; this indicates that he thinks that the situation mentioned in the antecedent clause of (F3-21) = (D3-222) is realized in nature. And this means that he is talking about speeds being as the spaces passed *at those speeds*. If he had meant speeds being as the spaces passed to acquire them, he would have used the hypothetical "se."

[10] He has stated in correspondence that he has so done in a forthcoming paper.

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Chapter 3
Explanation of the Law of Squares

Abstract In applying the theory of explanation as understanding to the history of science, the problem of the growth of understanding is formulated. To help solve this problem, Galileo's main scientific achievement is interpreted as a reduction of something not understood to something understood, i.e., as an instance of growth of understanding. This view is shown to deepen a number of common interpretations, and to invalidate a number of others, including the one that Galileo did not explain why bodies fall but merely described how they fall. Regarding the law of squares, my interpretation is both justified and elucidated by a detailed examination of the content and form of the literary record of Galileo's work on that law, as found in *Two New Sciences*.

3.1 Explanation as Growth of Understanding

One of the most widely accepted philosophies of the history of science claims that scientific discoveries are explanations. And one of the most plausible theories of explanation asserts that explanation[1] is the reduction of what is not understood to what is understood. Putting the two together we get a very interesting thesis: scientific discoveries are instances of growth of understanding. This idea is valuable independently of the principles which imply it, since it constitutes by itself a distinct theory of scientific progress. Distinct—but perhaps not so clear, for it is as problematic as it is interesting.

For example, from the point of view of pure conceptual analysis, since the reduction of what is not understood to what is understood is *new* understanding, explanation (as defined) seems to involve growth of understanding. But is it the only kind of growth of understanding? It seems that explanation is *quantitative* growth of understanding. Could there be a *qualitative* growth of understanding and if so what would it be? And how would qualitative growth of understanding relate to explanation?

[1] Scriven 1959, 1962.

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Argumentation Library 40,
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The problem can also be seen to arise within the study of the history of science. Here it is almost universally agreed that there is growth of knowledge in the history of science. But as knowledge grows, not only are there more things that become understood, but also more that are *not* understood; for the growth of knowledge makes possible not only the solution of problems but also the formulation of others. Is there then in the history of science such a thing as growth of *understanding* as well as growth of *knowledge*? For that to happen it seems necessary that the difference between the number of solved problems and the number of newly-formulable ones should decrease with time. But this does not seem to be the case.[2]

One aspect of the problem is then the conceptual elucidation of the notion of explanation as growth of understanding. Another is the investigation of the historical reality of growth of understanding in science. These two aspects may be treated separately, but it is most fruitful to synthesize historical description and conceptual analysis by engaging in as much of both as needed. This is what we shall attempt to do, and thus we propose the following synthesized formulation: if scientific achievements are explanations, if explanations are reductions of what is not understood to what is understood, and if the latter are instances of growth of understanding, then scientific achievements are instances of this growth and a proper analysis should both reveal them to be so and elucidate what it is for them to be so. Such an analysis we propose to carry out for Galileo's achievement.

3.2 Galileo's Main Achievement as an Explanation

How is one to understand this achievement? One could say that Galileo is the Founder of Modern Science, or that, in his own words, he created "a very new science dealing with a very ancient subject."[3] But such slogans, though correct, and though they do have a point, do not by themselves create much light. One could improve and elaborate on them and say that Galileo's achievement was (1) the mathematical analysis of the motion of falling bodies,[4] or (2) the systematization of mechanical laws and relations,[5] or (3) the construction, establishment, and testing of a deductive system of hypotheses concerning falling bodies,[6] or (4) the successful criticism of Aristotle's theory of motion,[7] or (5) the mathematization of motion and thus of physics,[8] or (6) that by a consistent application of mathematics to physics and of physics to astronomy, Galileo was the first to bring together mathematics, physics, and

[2] Kuhn 1970, 20–21.
[3] Galilei 1914, 153.
[4] Wartofsky 1968, 419.
[5] Nagel 1961, 154.
[6] Braithwaite 1960, 12–13.
[7] Popper 1959, 442.
[8] Koyré (1966, 156, 227, 288 ff.; 1968, 14, 20, 38).

astronomy—not merely mathematics and astronomy, or mathematics and physics, or physics and astronomy—but all three.[9]

All these interpretations are neither inaccurate nor pointless, but one would like to know why Galileo's achievement has all those features: mathematical analysis, systematization, hypothetico-deductivism, Aristotelian criticism, physico-mathematicism, and unification of domains and disciplines. Is the presence of any of them individually or of all them jointly, accidental? Presumably not. What, then, necessitates their presence? The answer to this question, which is usually not asked, let alone answered, is that Galileo had to do all these things in order to understand the phenomena he set out to explain. In other words, all those features can themselves be understood once we grasp that Galileo's achievement was an explanation, i.e., an instance of growth of understanding.

To be more specific, mathematical analysis was involved because Galileo mathematically described those aspects of the motion of bodies which he did not understand and which he set out to explain, for example, that the distance fallen by bodies should be proportional to the square of the times. The systematization of mechanical laws and relations is present simply because systematization is necessary when there are many things that one does not understand and one wants to understand them all. Hypothetico-deductivism is a feature of Galileo's achievement because he had to create the explanations he needed, instead of looking them up in an encyclopedia. Aristotelian criticism characterizes Galileo's achievement because the Aristotelian theory of motion was an obstacle to the understanding of the phenomena he set out to explain.

The mathematization of physics is a more complicated feature, but it too can be understood in terms of Galileo's explanatory achievement. First of all it should be noted that the mathematization of physics is present in Galileo's work only as an ideal and not as an accomplishment. For though it is obvious that he regarded celestial motions as part of physics, it is equally obvious that he did not treat them mathematically. If Galileo's mathematization of motion were merely that, i.e., a mathematization of a phenomenon, the mathematical physics ideal would be somewhat superfluous as an element of his actual accomplishment. For the mathematization of terrestrial motion as a piece of mathematics stands on its own feet. But Koyré's emphasis on the importance of that element[10] has a deeper significance. The mathematical physics ideal does perform a genuine function: that of making the mathematizations of motion provide an understanding of it. In other words, Galileo's claim that the book of nature is written in mathematical terms is only a mythical expression of the methodological truth that his mathematical demonstrations were not mere mathematical demonstrations but explanations providing understanding of natural phenomena. Similarly for Koyré's thesis that Galileo's mathematization of motion was regarded and presented by him as an experimental proof of Platonism-Archimedeanism and experimental refutation of Aristotelian metaphysics[11]; demythologizing that thesis,

[9] Drake 1970a, 97, 121.

[10] Koyré (1966, 156, 227, 288 ff.; 1968, 14, 20, 38).

[11] Koyré 1968, 39, 43.

we can say that, given Platonistic-Archimedean and Aristotelian metaphysics as the only two alternatives (Galileo's historical situation), his mathematization of motion presupposes the truth of the former and the falsity of the latter in order to constitute an explanation of the phenomenon of motion providing understanding of it.

Finally, if Galileo unified three disciplines and domains, it was because such a unification was needed in order to explain the phenomena that he did not understand. He applied physics to astronomy because he wanted to understand the new and puzzling things that he saw in the skies and that was the best way he had of explaining them. And similarly for his application of mathematics to physics.

Thus, if Galileo's achievement is interpreted as explanatory in the sense of comprehension-providing, many common interpretations of his work are not contradicted but deepened. To be sure, the explanatory analysis does conflict with the still widely held view that Galileo replaced explanation with description in science, that he did not explain why bodies fall but merely described how they fall. However, there is no conflict with the usual evidence alleged to support that view. This evidence is the fact that Galileo said in *Two New Sciences* that "the present does not seem to be the proper time to investigate the cause of the acceleration of natural motion … [but] merely to investigate and to demonstrate some of the properties of accelerated motion (whatever the cause of this acceleration may be)."[12] There are at least three reasons why these words do not support that view and hence do not contradict the explanatory analysis. First, Galileo speaks of renouncing *to investigate the cause* of natural acceleration. This would constitute renunciation of explanation only if one were to equate causal investigation with explanation. But clearly, non-causal explanation and non-explanatory causal investigation are common. Second, even if we could equate the two, what Galileo would be renouncing would be the explanation *of natural acceleration*, and not the explanation of natural phenomena in general. In fact, an examination of the context in which the passage occurs shows that at the same time that Galileo is declining to investigate the cause of acceleration, he is trying to explain various features of the motion of falling bodies, such as the way in which the distance they fall is related to the time of fall. Third, even if the passage could be interpreted as a declaration of the methodological renunciation of explanation, the passage as such would represent a declaration of intention or a statement of Galileo's belief about his own work. But our explanatory interpretation concerns the structure of Galileo's work not his *beliefs about* that work. Those words become relevant to the structure of the work itself only when interpreted as constitutive aspects of that work; when so interpreted, as we shall do below, they support our explanatory analysis.

3.3 Explanatory Analysis of Relevant Passage

In fact, the strongest support for this analysis comes from the structure of the historical record of Galileo's achievement, especially concerning the (times-squared) law of

[12] Galilei 1914, 166–167.

falling bodies, which may be regarded as his single greatest scientific achievement. The exposition of Galileo's results concerning the law of squares is contained in *Two New Sciences*[13] and consists of three main parts: (1) an apparently introductory discussion elucidating the principle that falling bodies acquire equal increments of speed in equal times[14]; (2) a derivation of the law of squares from this principle[15]; and (3) an account of a series of experiments the performance of which would allow one to establish the time-squared dependence of the distance fallen.[16] The last two parts, being relatively well known, require no further elaboration other than their integration into our explanatory analysis. And this can easily be done by interpreting the experimental report as the statement of what was not understood and the derivation as the reduction of the ununderstood to the understood.

However, the first part, the elucidatory discussion, needs more analysis. For the only thing immediately obvious here is that it is a discussion pertaining to something which is understood. It pertains to one of the elements present in explanations, according to the theory of understanding. And this perception as it stands needs both conceptual clarification and textual documentation: clarification as regards the notion of being understood, or comprehensibility; and documentation as regards the full content of the historical entity to which that notion is to be applied. As mentioned above, a synthesized approach will be followed by doing both simultaneously.

Galileo begins by stressing the simplicity of the principle of acceleration he is adopting: it speaks of an increment "which repeats itself always in the same manner," and it is of the same order of simplicity as the idea of uniform speed, which is speed such that "equal distances are traversed in equal time-intervals."[17]

These simplicity considerations would be superfluous, silly, and/or erroneous if they were interpreted as they frequently are by historians, i.e., as the argument that the principle is true because it is simple, or the argument that the principle of time-proportional speed is better than that of space-proportional speed because of the former's simplicity.[18] The theory of understanding suggests an obviously better interpretation: Galileo is saying that because or insofar as it is simple, the principle of time proportionality is comprehensible.

Galileo then proceeds to express the principle he is adopting in five verbally different ways.[19] Clearly, if one is stuck with only one form of words to express a certain idea, it is questionable whether that idea is understood. Hence Galileo's procedure here can be interpreted by saying that, by what he actually does, he is giving a second reason why or respect in which the principle is comprehensible: it is expressible in verbally different ways.

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[13] Galilei 1914, 160–179.
[14] Galilei 1914, 160–172.
[15] Galilei 1914, 173–178.
[16] Galilei 1914, 178–179.
[17] Galilei 1914, 160–161.
[18] Koyré 1966, 136–138.
[19] Galilei 1914, 161–162.

The third thing that Galileo does[20] is to remove the problem that the principle involves "certain difficulties" in that it describes "a phenomenon which baffles the imagination"; and that happens because "while our senses show us that a heavy body suddenly acquires great speed," from the principle "we must infer that, as the instant of starting is more and more nearly approached, the body moves so slowly that, if it kept moving at this rate, it would not traverse a mile in an hour, or in a day, or in a year, or in a thousand years; indeed it would not traverse a span in an even greater time."

Now, one will not understand either this problem, or Galileo's answer to it, or why, as Koyré says, "the best minds of that time"[21] felt the same difficulty unless the difficulty is expressed as follows: the time-proportionality principle seems incomprehensible because the process it describes is unimaginable, in the sense that it is *not visualizable*. For, when so stated, the difficulty is a real one: whereas the principle refers implicitly to arbitrarily slow speeds, such speeds indeed cannot be visualized; and Galileo's answer makes sense, even though it does not in any way show that those phenomena are imaginable or visualizable: it shows instead that there need not be anything problematic about unimaginable and/or nonvisualizable processes.

In fact Galileo's answer is the following. Imagine a heavy block which is allowed to fall from various heights successively onto various stakes; let the block fall first from a great height and subsequently from smaller and smaller heights; obviously the stakes will be driven into the ground by smaller and smaller amounts depending on the smaller and smaller speeds acquired by the block in its fall; obviously also there will be a height (equal for example to the thickness of a leaf) small enough so that the effect on the stake is imperceptible; but since in this case the block fell from a nonzero height, it must have acquired a nonzero speed, however small; hence we know that such slow speeds must exist.

Galileo's clarification of the present difficulty ends here. Thus he does not argue that those speeds are after all visualizable or imaginable, but only that their existence is grounded on visualizable and imaginable processes (the lack of effect on the stake, and the fall of the block from a finite height above). This argument then removes the difficulty in the sense that it denies the necessity of visualizability or imaginability for comprehensibility; what is necessary is presumably that a process should be inferable from phenomena that are themselves visualizable or imaginable.

In summary, the difficulty and its solution are the following: it is undeniable that some processes described by the time-proportionality principle are not visualizable, and that this makes them unimaginable. But this does not in turn mean that the principle is not comprehensible, because those processes are inferable from others which are visualizable and imaginable. It is when an idea involves things not inferable from visualizable or imaginable ones, that the idea is incomprehensible.

The next difficulty that Galileo discusses is the following:[22] if a falling body, in order to achieve a given speed, must acquire every possible value of speed between

[20] Galilei 1914, 162–164.

[21] Koyré 1966, 138.

[22] Galilei 1914, 164–165.

zero and that given speed, then it will never achieve that given speed, since there are infinitely many in-between values. Galileo answers that the falling body does not maintain any of the speeds it acquires for any length of time, it merely passes through each value of speed instantaneously; so that the infinitely many instants in any finite time interval are sufficient to correspond to the infinitely many values of speeds.

The basic structure of this difficulty is: "the principle cannot be true because -----." The answer is: "it is possible for the principle to be true because _____." And if this is to be a step in Galileo's elaboration of the comprehensibility of the principle, we can conclude that another necessary condition or reason for something to be understood is knowing how it could be true.

Fifth, there is a passage[23] containing the words that have misled some to allege a renunciation of explanation. We find here an exchange between a typical intelligent layman—Sagredo—and a typical Aristotelian—Simplicio. Galileo's spokesman—Salviati—intervenes only to close off the discussion. The exchange amounts to the exploration of the possibility that the principle of acceleration Galileo is adopting may be useful in solving the much discussed problem of the cause of the acceleration of falling bodies. The reasoning is the following. If the velocity is proportional to the time elapsed, then it may become easier to understand why it increases in the first place, why there is acceleration at all. For with the passage of time the initial upward impetus that the falling body must have had is lost and thus the natural downward impetus increases; and a falling body must have an initial upward impetus, even if it be that which was just sufficient to hold it still before being released. The suggestion then combines elements of the impetus theory with the principle of time-proportional velocity to explain why there is acceleration at all. And it is perhaps because of the presence of such elements, that Galileo's spokesman does not participate in this exploration except to end it by saying that he is not presently concerned about the cause of acceleration. But he is not completely rejecting it either, otherwise the passage would have been a pure digression, and Galileo probably would not have put it there.

The function of this speculative passage becomes well defined from the point of view of the theory of understanding. Galileo is elaborating on the comprehensibility of the principle of acceleration, and it helps to achieve this aim, to indicate how this principle together with an idea he does not accept *but understands* would explain a given phenomenon. In other words, Galileo need not accept the impetus principle in order to use it in a would-be explanation any more than an Aristotelian needs to accept the acceleration principle in order to use it.

From Galileo's procedure here, it emerges then that an idea is understood only if it is usable without being accepted as true. The importance of this requirement can hardly be over-emphasized: it not only distinguishes the comprehensible from the true; it also distinguishes that which is understood from that which is believed. And though the former distinction tends to psychologize the concept, the latter prevents it from being made subjectivistic.

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[23] Galilei 1914, 165–167.

Finally there is the attempted refutation of the idea that the principles of time-proportional speed (which Galileo is adopting) and of space-proportional speed are equivalent.[24] Galileo argues that if the speed were proportional to the distance traversed, motion would be instantaneous, since different distances are traversed in equal times when the speeds are as the distances; so space-proportional speed would imply instantaneous motion; this in turn would obviously imply the falsity of time-proportionality; hence the two principles are not equivalent because they are incompatible.

It is easy to be misled by the biographical, historical, philological, and logical dimensions of this argument. Biographically speaking, space-proportionality was the principle from which Galileo had earlier attempted to derive the law of squares. Historically speaking, the equivalence of the two principles had always been taken for granted before Galileo and by him in his early investigations. Philologically speaking, as Stillman Drake has discovered,[25] the major modern translations of the relevant Italian passage involve a crucial demonstrable error. From the logical point of view, various allegations have been made: that the argument is wrong because it tries to show the logical impossibility of space-proportionality, which is in fact self-consistent; that the argument begs the issue because it tries to show that space-proportionality is false by assuming one of the consequences of time-proportionality before showing the latter to be true; that the argument involves an equivocation or confusion between average and instantaneous speeds; or, as Stillman Drake claims,[26] that the argument is valid because it is a *modus tollens*: if speeds were proportional to the spaces traversed, motion would be instantaneous, which it is not, hence space-proportionality is false.

All of these logical interpretations involve an inaccurate reconstruction of Galileo's main argument, whose conclusion is that the two principles are not equivalent and whose basic structure is the one outlined above. Moreover, all of them are methodologically objectionable to some extent, depending on how inadequate is the reasoning which is being attributed to Galileo. This applies to the *modus tollens* reconstruction, which may be formally valid, but is in the context a *petitio principii*, i.e., a begging of the conditional premise.[27]

Other things being equal, Galileo's reasoning should be reconstructed as being as adequate as possible. Our reconstruction above attributes to him only the misapplication of a fact about constant speeds to a situation involving continuously increasing speed. But note that this is not to say a conceptual confusion between average speed and instantaneous speed. Moreover, our explanatory analysis makes the argument primarily a step in the elaboration of the comprehensibility of time-proportionality, showing its difference from something with which it can be easily confused. Clearly, if one is to understand time-proportionality, one must be able to distinguish it from space-proportionality.

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[24] Galilei 1914, 167–169.

[25] Drake (1970a, 229–237; 1970b, 28–43).

[26] Drake (1970a, 229–237; 1970b, 28–43).

[27] See Finocchiaro 1972.

3.4 Recapitulation

To conclude, Galileo's results concerning the law of squares, as recorded in *Two New Sciences*, have been structured as an explanation of the time-squared dependence of the fall of bodies. This explanation contains an elucidatory discussion concerning the principle of time-proportional speed. The structure of this discussion has been shown to be that of the following argument: the principle being adopted is comprehensible because and insofar as (1) it is simple, (2) it is expressible in verbally different ways, (3) it is inferable from imaginable things, (4) one can conceive how it could be true, (5) it is usable even if not accepted, and (6) it is logically distinct from apparently equivalent ideas. These six features can be regarded as necessary conditions of and partial grounds for comprehensibility. And in this sense our discussion is an elucidation of the concept of comprehensibility, besides being of course an analysis of Galileo's achievement.

In other words, these conditions for comprehensibility are to be understood in the light of Galileo's work under consideration, and this work is in turn to be understood in their light. When the latter is done, Galileo's main scientific achievement concerning falling bodies becomes the explanation of their time-squared dependence. And when the former is done, those conditions will be felt to be themselves intuitively comprehensible. So our own analysis, the reduction of Galileo's work to them, will be itself an explanation by our own standards, i.e., a reduction of what was not understood to what is understood.

To be sure, it is not clear whether Galileo's achievement is an instance of quantitative or qualitative growth of understanding, in view of the presence of the argument supporting the comprehensibility of that to which the non-understood is being reduced. And moreover, to feel intuitively that the above mentioned conditions are understood and to be open to discuss them further is one thing; to actually make them one's problem and to give a theoretical elucidation of them is another thing. But, for the purpose of our explanation the need of the latter is not as immediate as that of the former; and the non-immediacy of the latter is such that the task can be postponed for tomorrow. What we have explained today is only Galileo's achievement on the law of squares, but this we do claim to have explained.

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Chapter 4
Arguing About the Earth's Motion

Abstract Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632) can be read from the viewpoints of methodological judgment and critical reasoning; methodological judgment means the avoidance of one-sidedness and extremes; and critical reasoning means reasoning aimed at the analysis and evaluation of arguments. Classic sources for these readings are Thomas Salusbury (1661) and the Port-Royal logicians (1662). This focus does not deny the book's scientific, historical, rhetorical, and aesthetic dimensions; it is critical of excessively rhetorical readings; and it suggests solutions to the problems of hermeneutical pluralism, interpretation versus evaluation, and theory versus practice. And the book's methodological judgment and critical reasoning can be shown to correspond to Galileo's own self-reflections.

4.1 Methodological Judgment

The full original title of the book Galileo published in 1632 may be translated as follows: "*Dialogue by Galileo Galilei, Lincean Academician, Extraordinary Mathematician at the University of Pisa, and Philosopher and Chief Mathematician to the Most Serene Grand Duke of Tuscany; where in meetings over the course of four days one discusses the Two Chief World Systems, Ptolemaic and Copernican, proposing indeterminately the philosophical and natural reasons for the one as well as for the other side*."[1] This title could have been abbreviated by focusing on its last clause rather than on its first word, and then this Galilean work would have been known by the title "Philosophical and Natural Reasons for the Ptolemaic and for the Copernican World Systems." Such an abbreviated title would advertise more explicitly both the book's real subject matter (namely, the arguments for and against the earth's motion) and the impartiality of the book's discussion (or at least its intended impartiality). Of course, even as it stands the full title does suggest (intended) impartiality when it asserts that the arguments of both sides are presented and that they

[1] Galilei 1632; cf. Galilei 1967, 1; the translation is mine.

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are presented "indeterminately." However, this word conveys a somewhat oblique suggestion rather than an explicit implication, partly because it is more vague than the term "impartiality," and partly because it is somewhat ambiguous and can also mean "inconclusively."[2]

One reader who must have been struck by the book's impartiality was Thomas Salusbury, the author of the first published English translation of Galileo's *Dialogue* in 1661. In fact, he translated the Italian term which I have transliterated "indeterminately" as "impartially and indefinitely."[3] Here Salusbury seems to be making explicit in his translation what he felt was implicit in the Italian text. This was not a mere slip of the pen on Salusbury's part for in his translator's foreword he explicitly formulates the corresponding interpretation. There, in the context of advancing his interpretation of the Inquisition trial occasioned by Galileo's book, Salusbury says that the book has "been with all veneration valued, read, and applauded by the judicious."[4]

I want to capitalize on Salusbury's talk of impartiality and judiciousness and claim him as the first to have advanced an approach to the critical reading of Galileo's book which I find very fruitful and which in my own way I have been elaborating for at least fifteen years. The key notion here may be labeled judgment, judiciousness, or impartiality; it may be defined as the avoidance of one-sidedness and extremes. The book's title is referring to judiciousness in regard to a substantive issue of physical theory, namely the Ptolemaic versus the Copernican systems, or to be more exact the geostatic versus geokinetic theories.[5] I apply the idea more widely to methodological issues. In methodology the various sides become such ideal types[6] as deductivism, inductivism, hypothetico-deductivism, apriorism, empiricism, positivism, retroductivism, and mathematicism; they can also be such historical traditions as Platonism,

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[2] Drake 1986, 158; the suggestion of inconclusiveness is indeed important because Galileo also wants to convey the sense that the arguments on both sides are inconclusive (though admittedly the pro-Copernican ones are much better).

[3] See the English title page of the *Dialogue* in Salusbury 1661; cf. Santillana 1953, p. liii, n. 1.

[4] Salusbury 1661; cf. Santillana 1953, pp. liv–lv.

[5] This qualification answers the problem of the neglect of the Tychonic system, of which more below.

[6] Although I mean the notion of "ideal type" in the general sense originating from Max Weber, I am adapting the application of this notion to methodology from McMullin 1990, 29–32. I believe McMullin's account of "conceptions of science in the scientific revolution" may be viewed as a comparison and contrast of the conceptions of Descartes, Bacon, Boyle, Kepler, Galileo, Newton, and Locke with the abstractly defined ideal types of deductivism, inductivism, hypothetico-deductivism, and retroductivism. If this interpretation of McMullin 1990 is correct, then I believe it sheds considerable light on his earlier account of "the conception of science in Galileo's work" (McMullin 1978), which may then be seen as a similar analysis focused on Galileo; however, in the latter case I would want to emphasize that we have at least four strands because, as McMullin argues, the Aristotelian ideal of demonstration inherited by Galileo constituted "an ambiguous heritage" (McMullin 1978, 211) in which demonstration meant two distinct activities, namely intelligible explanation as well as conclusive proof; further, the conception of science toward which Galileo was groping can be shown to contain at least two distinct procedures, retroduction from effects to causes and acausal hypothetical reasoning. Parts of McMullin 1967b may be read in the same vein.

Aristotelianism, Archimedeanism, and the tradition of "mixed sciences." However, a number of clarifications are needed here.

I said in the previous paragraph that Salusbury's suggestion is fruitful for the *critical reading* of Galileo's book. I am referring to both the interpretation *and the evaluation* of what he says and does in that text. There is no presumption that Galileo was perfectly or completely judicious and impartial on any one issue (e.g., earth's motion versus rest), let alone all issues, methodological and theoretical. To take the point of view of judgment means simply that one examines whether and if so to what extent the text displays judiciousness on various occasions. To uncritically attribute judiciousness to Galileo would betray on the part of the reader or scholar at the meta-level precisely the kind of injudiciousness we are talking about.

Second, this kind of judiciousness should not be equated with indecision, indecisiveness, indifference, or inability, unwillingness, or failure to make a concrete decision or reach a definite conclusion. The situation here is analogous to the judicial one. In a legal court, a fair judge is not one who does not pass judgment, but one who arrives at it by taking into account all that is relevant and proper. Thus, for example, at the meta-level it becomes possible to join Salusbury in judging Galileo's *Dialogue* largely or essentially judicious, without lapsing into self-referential inconsistency. Or again, at the object level, Galileo's impartiality is not refuted by the fact that his book does after all end up judging that the Copernican arguments are better than the geostatic ones.

Of course, there is no algorithmic or mechanical procedure or rule which guarantees judiciousness, and this represents a third important proviso. That is, one cannot simply split the difference between the two opposite extremes, or uncritically incorporate into one's own conclusion a representative element from all "sides," or formally legislate that each conclusion advanced be counter-balanced by a serious presentation of its opposite.

Finally, it should be noted that my definition refers to "extremes" as well as "sides." The rationale underlying this distinction is easy to see for the case of methodological judgment. Here the sides would be aspects of the methodological situation which are distinct or different but not contradictory or contrary; whereas the "extremes" are the opposite ends of the spectrum along any one aspect. For example, apriorism and empiricism are opposite extremes which constitute a single aspect or side; mathematicism and a purely qualitative approach are another pair of opposite extremes which define a different side; and deductivism and inductivism (under most definitions of these terms) would constitute another distinct spectrum with opposites at each end. Methodological judgment attempts to avoid one-sidedness by taking into account all such spectra, and tries to avoid extremes by reaching a proper balance between each pair of extremes.

However, any further theoretical or conceptual analysis is beyond the scope of this paper. For here we need to go back to Galileo's book and introduce a second theme of my approach.

4.2 Critical Reasoning

There is a second interpretive viewpoint which I find extremely useful and which happens to have classical antecedents. This is the viewpoint of what I label critical reasoning. By critical reasoning I mean reasoning aimed at the analysis, evaluation, and/or self-reflective formulation of arguments.[7] Here analysis means identifying, distinguishing, and interrelating various elements (basically, propositions) for the purpose of understanding the reasoning. Evaluation means determining whether the reasoning is valid or invalid, strong or weak, cogent or fallacious, and so on. By self-reflective formulation of arguments I mean arguments which exhibit an appropriate degree of self-analysis and self-evaluation. For the cases of analysis and evaluation, critical reasoning simply consists of reasoning about reasoning or arguments about arguments, namely arguments which support conclusions about other arguments. For the case of self-reflective reasoning, critical reasoning is simply the presentation of an argument supporting a conclusion about some entity other than an argument (usually some physical phenomenon), as long as the presentation is carried with adequate attention to and awareness of what the structure and strength of the presented argument are.

Like all reasoning, critical reasoning is not always correct; some is correct and some is incorrect. Thus, to say, as I would, that Galileo's book is full of critical reasoning and that critical reasoning is its essential and most fundamental feature is merely an analytical interpretation, not an evaluation of the book. In this regard, critical reasoning may be contrasted with methodological judiciousness, which is primarily an evaluative notion; but, as we saw, judiciousness was given an analytical interpretive import in a derivative manner by means of the notion of *attempting* to avoid one-sidedness and extremes. At any rate, I would also argue that the critical reasoning in Galileo's book is usually correct and cogent, but such an evaluation would have to be supported in a different way than the textual interpretation.

One of the earliest and most significant examples of this kind of reading of Galileo's book is found in the so-called *Port-Royal Logic*, first published in 1662. The *Port-Royal Logic* tried to do in the seventeenth century something analogous to what present-day scholars in the fields of informal logic and critical thinking are aiming at. Thus, it is not surprising that it contains a good illustration of my present point. This occurs in Part III, Chapter 19, entitled "Sophisms: the different ways of reasoning incorrectly." The second of nine sophisms discussed is the fallacy of begging the question. This is defined in a standard manner, namely assuming as true the conclusion you are trying to prove, and a reference is made to Aristotle as the originator of this definition. Then several illustrations are given. The first one of these involves a passage from the First Day of the *Dialogue*. The passage contains Galileo's criticism that Aristotle's empirical argument for geocentrism begs the question. The fallacious argument in question is the following: the natural motion of heavy bodies

[7] I adapt this notion of critical reasoning from the literature on informal logic and critical thinking; for a flavor of the discussions and a glimpse at the issues, see Johnson and Blair 1985, Fisher and Scriven 1997, and Siegel 1988.

is straight toward the center of the universe since their natural motion is opposite to that of light bodies, and the natural motion of light bodies is toward the circumference of the universe; but the natural motion of heavy bodies is also straight toward the center of the earth; therefore, the center of the earth coincides with the center of the universe. The *Port-Royal Logic* approvingly summarizes Galileo's criticism that the first step in this argument assumes that the opposite of a direction "toward the circumference of the universe" is a direction "toward the center of the universe"; that this assumption holds only if the phrase "toward the circumference of the universe" is understood to mean "in a direction which intersects the circumference of the universe at right angles"; and that, when so understood, we can know that light bodies move naturally in that direction (the corresponding premise of the argument) only if we know that the earth is at the center of the universe (the argument's conclusion).[8]

Now, I am inclined to concur with the Port-Royal logicians and with Galileo that the featured argument begs the question. I am not saying that Galileo's interpretation of the relevant Aristotelian text is correct. Nor am I completely sure that the Port-Royal interpretation of Galileo's interpretation and of his evaluation is correct. All I want to claim is that such a reference involves an approach to Galileo's book from the point of view of critical reasoning.

The example just considered involves a reference to Galileo's critical reasoning in the sense of reasoning consisting of the critical analysis of someone else's argument. The next example involves a reference to Galileo's self-reflective presentation of his own argument. This is found in Hume's *Dialogues Concerning Natural Religion.* The occasion comes in a discussion of the issue whether the existence of God can be proved empirically. At the end of Part II, the agnostic Philo claims that in order to prove from experience that an orderly universe must arise from an intelligent divine cause we would need sensory experience of the origin of universes, which is obviously impossible. The theist Cleanthes replies that such a proof of the existence of God is no more impossible than it is to prove the earth's motion from experience, so that if Philo's skeptical objection were effective in the theological controversy it would have been effective in the Copernican dispute. Philo replies that the two situations are not analogous because in the Copernican case it turned out to be possible to acquire the required experience of the motion of other earth-like bodies. To support this last claim he advances the following interpretation of Galileo's book: that Galileo first destroyed the earth-heaven dichotomy, by proving empirically the similarity between the earth and other heavenly bodies; then he proved empirically that the moon, sun, and planets moved (for example, the moon and sun with axial rotation, the planets with heliocentric revolution); and so, by analogy he concluded that the earth also moves.[9]

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[8] Cf. Arnauld 1964, 247–248; Arnauld and Nicole 1965, 243; Galilei 1890–1909, 7: 59–61; Galilei 1967, 34–37; and Hamblin 1970, 151–152.

[9] Cf. Hume 1947, 150–151. Hume is obviously referring to Galileo's book as a whole and does not give any specific reference. I believe the most directly relevant Galilean passage is to be found not in the *Dialogue* but in Galileo's "Reply to Ingoli" (Galilei 1890–1909, 5: 559–561; Finocchiaro 1989, 196–197).

Once again, I am not sure Hume's interpretation is textually accurate in its entirely, though it certainly is in part. Similarly, not all of his evaluations are acceptable, though some are unobjectionable. Moreover, the interpretation is ingenious, and the alleged disanalogy with the theological situation is intriguing. These points could and should be pursued at greater length and in greater depth; but that cannot be done here. Our main point in the present context is to illustrate and substantiate our claim that it is possible to approach Galileo's *Dialogue* from the point of view of critical reasoning, that there is a tradition of such readings, that such an approach is a natural one, and that such an approach makes the book relevant outside the fields of science and scientific methodology, however rich and fruitful the latter fields may be.

4.3 Judgment Calls

The primary way of justifying the critical-reasoning interpretation of the Galilean *Dialogue* would be the direct one of reconstructing the whole book as a series of critical analyses of the geostatic arguments, and self-reflective presentations of geokinetic arguments. And the primary way of evaluating the book's critical reasoning would also be a direct one; we would have to become engaged in the practice of critical reasoning, that is, become concretely involved in reasoning about the Galilean arguments. Similarly, the primary way of justifying a critical interpretation of the book in terms of methodological judgment would be by an appropriate reconstruction of all of the book's methodological discussions.[10]

Here, rather than undertaking this task which would be impossible in the present context, I want to continue in the indirect and allusive style by applying my idea of methodological judgment at a meta-level in such a way as to address a number of issues which the reading of the *Dialogue* presents.

Like many other classics, scientific and otherwise, Galileo's *Dialogue* is a multidimensional work. In the present instance the various relevant dimensions are science, history, philosophy, rhetoric, and literature. These are just the major and general viewpoints from which the book may be approached, and each of them in turn subsumes several more specific points of view. For example, a scientific reading of the book may focus either on physics, astronomy, cosmology, or mathematics. A historical reading may take the book as a document of either the Galileo Affair, the Copernican Revolution, or the Scientific Revolution, not to mention other development such as the history of Aristotelianism or Platonism, or other smaller episodes like the controversy with Scheiner over sunspots. The most common philosophical reading involves methodology and epistemology, which may or may not be distinguished from each other; it may also involve applied logic or informal logic, which corresponds to what

[10] In Finocchiaro 1980, I provided much of the documentation and argumentation for the critical analysis of the *Dialogue* along the lines of critical reasoning and methodological judgment, although the terminology and the conceptual framework there were slightly different; my Finocchiaro 1997 provides a more explicit, simplified, and clarified account, along with some revisions, corrections, and updating.

I call critical reasoning. The rhetorical reading of Galileo's book focuses on such things as persuasion, verbal communication, emotional appeals, linguistic composition, and dramatic structure and interaction. Finally, the book may be studied as literature, namely as a work of art, specifically literary art; this would be an aesthetic reading and focus on such things as the eloquence of the language, the beauty or cleverness of the expression, the vividness of the images, and the like.

All five major dimensions are legitimate, in the sense that each has its place and each is appropriate in some context. Thus, I would object to a one-sided approach which would focus excessively on just one of these approaches or try to reduce all other dimensions to it. In recent years, I believe that most of the abuses in this regard have been committed by the rhetorical reading of Galileo's book. That is, it is typical for scholars who examine it from a rhetorical viewpoint to see rhetoric, and nothing but rhetoric, everywhere in the text, and to be blind or insensitive to its scientific content or its arguments. Frequently, their very understanding of rhetoric is such that rhetoric is conceived as the opposite of science or argument. The code words in such readings are propaganda, polemic, entertainment (as opposed to enlightenment or education), deception, trickery, and sophistry (namely, making the weaker argument appear stronger).[11]

On the other hand, to say that these five readings are individually legitimate is not the same as saying that they are equally legitimate or important. For example, it seems obvious to me that the aesthetic approach to the *Dialogue* is less important than the other four. Here the situation seems to me to be completely analogous to that of Sacred Scripture; the Bible certainly may be read and studied as literature, but its religious content is clearly more important. However, to determine the hierarchy and relative importance of the other four dimensions would be a more difficult undertaking.

Their relative importance would in part depend on their mutual interrelationships. Thus, let me hasten to add that, although it is useful to distinguish these five major dimensions, it would not be proper to claim that they are unrelated and have nothing to do with each other. Instead, I would argue that they are interrelated and that the distinction should not be turned into a separation. On the other hand, the interrelation should not be a conflation and should not lead to a confusion among the various

[11] Feyerabend 1975 may be cited as perhaps the best example of this genre, although I would be the last to deny that his work is valuable from other points of view, as I have argued (besides Chapter 20 of this book) in Finocchiaro 1980, 157–166, 182–200. For example, by taking seriously Feyerabend's distinction between counter-induction and anarchism, what he calls "anarchism" could be reconceptualized in a more constructive and positive fashion than his own rhetoric allows him, and then his "anarchism" would come very close to my notion of methodological judgment. A less scholarly but more emblematic example of rhetorical excess is the view of Reston (1994, 217) that in Galileo's book "his emphasis is more on entertainment than on education"; this interpretation is important, despite the fact that Reston is a journalist and popularizer, because of the method of work followed by him; this author has tried to master the field of Galilean scholarship in three-years time, and so his conclusion seems to have been formed by skimming the surface of current scholarship to see what kind of consensus seems to exist; thus, the interpretation quoted is emblematic of the current state of Galilean scholarship; again, while I criticize Reston's hyper-rhetorical approach, I can also welcome his book as a general cultural appreciation of Galileo.

dimensions. In short, the proper approach is to distinguish without separating and to interrelate without conflating.

Finally, I do not wish to give the impression that there is anything sacred or magic about the number five, as the number of major dimensions. I have already suggested that each dimension subsumes several aspects; thus, depending on how these sub-dimensions are defined and interrelated, we could end with a different number of major dimensions. In other words, another challenge is to examine and define more precisely the difference between a dimension and a sub-dimension. Nevertheless, I believe it is important to retain the notion of a dimension, whether major or minor, because it is important to distinguish this problem of multi-dimensionality (or multi-disciplinarity) from three other problems, the problem of hermeneutical pluralism and variety, the problem of interpretation versus evaluation, and the problem of theory versus practice. To these we now turn.

The notion of judgment also helps us to deal with the fact that, like many other classics, Galileo's *Dialogue* is susceptible of many different interpretations and evaluations. The differences I have in mind are substantive differences within the same disciplinary category or hermeneutical dimension; for example, from the methodological point of view Galileo has been interpreted as an apriorist, empiricist, positivist, mathematicist, deductivist, inductivist, hypothetico-deductivist, and in other ways lacking a handy label. Here I am *not* referring to differences due to focusing on one rather than another of such fundamental dimensions as science, philosophy, history, rhetoric, and literature. The problem here is that stemming from the question of whether "anything goes" in the business of interpreting the classics. In other words, whatever may be the case for scientific inquiry, is epistemological anarchism à la Feyerabend valid in textual interpretation?[12] The problem is, of course, especially acute when the interpretations are incompatible, as apriorism and empiricism are. However, an uneasy feeling remains even when an interpretation comes along which is unpretentious, not easily classifiable, appreciative, and with some basis in the text.[13]

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[12] This problem has been previously formulated explicitly in these terms by McMullin (1978, 211); moreover, he states that "in this essay on Galileo's theory of science, we will try to answer this question" (McMullin 1978, 211); and he takes as his starting point for his solution to the problem the considerations that "it does seem, however, that a divergence of interpretations as deep as this one may derive in part from something in the material itself" (McMullin 1978, 211). Since this problem, this intention, and this staring point are equivalent to my own, it may not be too far-fetched to try to claim McMullin's account as being in the tradition of methodological judgment. In fact, he likes to speak of an unresolved tension within Galileo's work between the Aristotelian theory of science as demonstration and the future hypothetico-deductivist conception which was needed to make sense of his own work in both kinematics and astrophysics; furthermore, as I have pointed out in a previous note, McMullin's own account suggests that each of these two conception of science is itself a somewhat ambiguous mixture of two important and relatively distinct elements; that is, respectively, a mixture of the ideal of conclusive proof and the ideal of explanatory intelligibility and a mixture of retroduction from effect to cause and hypothetical reasoning yielding merely probable claims. I believe it is a small step from an unresolved tension of such important and distinct elements to the avoidance of one-sidedness and extremes.

[13] Here I am referring to Pitt 1992.

I believe the notion of methodological judgment enables us to capitalize on such hermeneutical variety, to make a virtue out of the necessity (so to speak), and to describe the situation positively and constructively. The initial point to stress here is that there are certainly conflicting *tendencies* in Galileo's work, or if you will, *instances* of conflicting methodological *principles*; this applies not just to Galileo's career as a whole, but even if we confine ourselves to the *Dialogue*. Second, he never undertook to articulate a theoretical systematization of methodological and epistemological problems. Therefore, I believe there is no justification for a philosopher or historian of science to want to construct a systematization of Galileo's work in terms of any single methodological theory or principle. This can only be done by being one-sided in regard to the distinct methodological problems Galileo faced and by exaggerating the generality of the solution he gave on some particular occasion. This would happen to the detriment of the other distinct methodological problems and of the opposite way of solving the problem which might be useful on other occasions. In short, the philosopher or historian would be lapsing into injudiciousness in interpreting Galileo.

The problem of interpretation versus evaluation is the problem that these two activities need to be, but often are not, judiciously distinguished and interrelated. By an interpretation of a text I mean an account which aims to provide an understanding of the text, to tell us what the text means. By an evaluation I mean a claim about whether the content of a text is correct or incorrect, valid or invalid, sound or unsound, in accordance with some particular standard or norm.

In some contexts things are clear and the problem does not arise. For example, no one would confuse the question of what principle Galileo holds in regard to natural motion (whether, for example, natural motion is rectilinear or circular) with the question of whether such a principle is correct. Similarly, it is one thing to determine whether Galileo holds that centrifugal extrusion could not take place on a rotating earth, and it is another to determine whether this is physically correct. And again, it is one thing to determine Galileo's view of what kind of daily acceleration and retardation would result from the combination of the earth's diurnal and annual motions, and what kind of tidal motion would result from this acceleration; but it is another thing to determine whether such views are scientifically correct. Now, switching from the scientific to the methodological dimension, Koyré may very well want to claim both that Galileo's procedure was apriorist and that his aprioristic procedure was good physics, but obviously the evidence for these different claims would have to be different; the first is an interpretation of Galileo, the second is an evaluation of his procedure. Similarly, Feyerabend is inclined to say both that the *Dialogue* is full of trickery and of sophistical maneuvers, and that such a procedure was methodologically proper and effective; but clearly he cannot support both claims with the same evidence.[14]

However, in other contexts the situation is not so clear. In particular, I believe that, once again, rhetorical readings of Galileo's book suffer from a damaging equivocation

[14] Cf. Koyré 1978, Feyerabend 1975, and Finocchiaro (1980, 180–223); cf. also Chapters 19 and 20 in this book.

between interpretation and evaluation. To be more specific, it seems that almost everybody expresses a favorable and positive evaluation of Galilean rhetoric, but does so without an explicit justification; what we do find is documentation for some rhetorical interpretation, but then his rhetorical procedures are uncritically declared effective, brilliant, and proper without adequate justification.[15]

My own approach to the book's rhetorical dimension is that at the level of interpretation I find it interesting and important to elaborate this rhetoric, but on the other hand I would judge that more often than not his rhetoric is ineffective; in short, there is a considerable amount of rhetoric in the book, but most of it is bad.

For example, the ineffective rhetoric begins in the title, not in regard to the matter of impartiality discussed above, but rather in contrast to the alternative title which Galileo had originally chosen. As we have seen, the title begins with the word dialogue, then goes on to name the author and to mention two of Galileo's professional titles, then mentions the subject which is described as "the two chief world systems, Ptolemaic and Copernican," and then ends with a more or less explicit suggestion of an impartial treatment. The title Galileo originally wanted was "Dialogue on the Tides," or at least it began with these words. In contrast to that, the actual title is a rhetorically foolish one in the light of the anti-Copernican decree of 1616 and the other ecclesiastic restrictions under which Galileo was operating; for it calls attention directly to the fact that the book is discussing the dangerous and semi-forbidden topic, and so it invites scrutiny from that point of view. On the other hand, a dialogue on the tides would have made it more obvious that the problematic idea of the earth's motion was being introduced indirectly as an hypothesis meant to provide a causal explanation of a phenomenon everyone can observe. It should be added that the rhetorically infelicitous title was imposed on Galileo by the ecclesiastical authorities, and so the blame should go to them and not to him. Nevertheless, the evaluation stands as applied to the book as it came to be published.[16]

Another strikingly bad piece of rhetoric is found in Galileo's preface to the reader. In it he tries to make it look as if the book was a justification of the anti-Copernican decree of 1616. The justification is allegedly that the book contains a discussion of all scientific evidence and philosophical arguments about Copernicanism; now, since its author was present in Rome at the time of that decree, the book is supposed to show to non-Catholics that the Catholic Church did not condemn Copernicanism out of scientific ignorance. He states that it did so for religious reasons. However, he also

[15] An outstanding example would be Moss 1993. My main criticism of her work would be along these lines, although by way of appreciation I can say that I found valuable her accounts of many works by Galileo and others in terms of the three distinct but interrelated notions which she adapts from Aristotle: demonstration, dialectic, and rhetoric.

[16] By contrast, according to the usual interpretation, Galileo originally preferred to entitle his book "Dialogue on the Tides" because he regarded his tidal argument for the earth's motion as conclusive, and the Church imposed dropping mention of the tides because it did not want to put its stamp of approval on that argument; for an elaboration of this view, see, for example, Shea 1972, 173–174. My own interpretation is elaborated in Finocchiaro 1980, 16–18, and I believe it is reinforced by Drake 1986 and MacLachlan 1990.

goes on to summarize the book's content as showing that the scientific and philosophical arguments and evidence in favor of Copernicanism (though inconclusive) are better than those in favor of the geostatic view. Thus, in effect the message is that the Catholic Church is telling Catholics to believe the worse of two scientific theories. Now, although this issue would deserve much more discussion than is possible here, I do not think that this portrays the Church in a good light. In this regard it is important to note that the Decree of the Index of 1616 was based on a judgment by a committee of eleven consultants which asserted in part that the geokinetic thesis was scientific and philosophically untenable[17]; I do not think the Church would have issued a condemnation of a theory that was likely to be true. In short, the book's preface is incoherent, and it fails in its explicit attempt at religious apologetics.

These two examples are by no means the only instances of rhetorical ineffectiveness. They are only the most striking. Other examples, for which there is no time in this paper, involve the self-defeating way Galileo presented the pope's favorite objection at the end of the book; the ineffectual rhetoric of strict demonstration[18] trying to convey the impression that the book provides a strict demonstration of the earth's motion, which is undermined by a much more considerable amount of rhetoric to the contrary as well as by the clearly evident fact that his pro-Copernican arguments, while strong, are inconclusive; the self-contradictory rhetoric of anti-rhetoric, namely a series of remarks to the effect that rhetoric has no place in science, which is belied at almost every turn by his own practice in the text; and the rhetorical excesses in which the book frequently lapses, involving criticism, insults, and sarcasm about his opponents. In regard to the latter, it should be noted that it is possible, interesting, and even valuable to be able to understand and appreciate such alienating excesses; but doing so involves moving to the aesthetic plane and leaving the rhetorical dimension behind, and so my point about the rhetorical situation remains valid.

I should reiterate that here my criticism would be purely rhetorical and not scientific, methodological, or logical. What I am saying is that the book seems to reveal mostly ineptness in the art of persuasion and verbal communication and the handling of human emotions, rather than the brilliant polemicist and master rhetorician we hear so much about. I believe this hermeneutic problem stems from a failure to properly distinguish and interrelate interpretation and evaluation; for example, a failure to realize that rhetoric is neither all good nor all bad, that it has its own standards of

[17] The wording and the punctuation of this document are crucially important, and they suggest that the theological condemnation was based in part on this scientific (mis)judgment. Cf. Galilei 1890–1909, 19: 320–321; and Finocchiaro 1989, 146.

[18] What I am here labeling "rhetoric of strict demonstration" would perhaps be interpreted by other scholars from a historical point of view as important evidence of Galileo's Aristotelian roots, and from a methodological point of view as indicative of the nature of his methodology. I do not mean to deny these Aristotelian influences, which I would now judge as established beyond any reasonable doubt by Wallace (1984, 1992a, b). Nor do I mean to deny the "Aristotelian" component of Galileo's methodology. However, in accordance with the methodological-judgment approach, and as implied (I believe) in McMullin's (1978) account, this Aristotelian component is ambiguous in itself and only one of several. Moreover, once we distinguish the rhetorical from the methodological dimensions and allow the legitimacy of both, I believe that both McMullin and Wallace admit and argue for the rhetorical ineffectiveness of these Galilean demonstrative claims.

evaluation, that rhetorical evaluations (whether positive or negative) need to be justified and not merely stated, and that the justification of a rhetorical evaluation depends partly on the interpretation of the text's content and partly on its own principles.

One last distinction which cries out for judicious exercise is the distinction between words and deeds, theory and practice, or concrete scientific work and methodological or epistemological reflection or awareness. Here the most widespread tendency is to attribute to Galileo a divergence between his scientific practice and his methodological reflection.[19] Thus it may be useful to sketch an account which would not only interpret the *Dialogue* primarily in terms of critical reasoning and methodological judgment, but also show that Galileo was reflectively aware that this is what he was doing and what he should be doing. This sketch would answer the objection that, on the one hand, it may very well be true that the actual accomplishment of the book is that of a methodologically judicious exercise in critical reasoning to the effect that the geokinetic arguments are better than the geostatic arguments, and so Copernicanism is more likely to be true than the geostatic view; but that, on the other hand, Galileo saw himself as providing a conclusive demonstration of Copernicanism in accordance with some specific methodological ideal such as mathematicism, apriorism, deductivism, or the Aristotelian theory of science and demonstration.

4.4 Integrating Theory and Practice

To sketch my own interpretation, I would begin by showing that the *Dialogue* is an argument designed to support the conclusion that the pro-Copernican arguments are stronger than the anti-Copernican ones; or again, that the reasons for believing the earth to be in motion are better than those for believing it to stand still; or finally, that the evidence or support favoring the geokinetic idea outweighs the evidence or support favoring the geostatic one. This means that the Copernican theory is preferable to the geostatic view, or that Copernicanism is more probable or more likely to be true than geocentrism; the conclusion does *not* mean that Copernicanism is either clearly true, or certainly true, or absolutely true, or demonstrably true; nor does it mean that there are no reasons for believing the earth to stand still; nor that the geostatic arguments are worthless. The point is that the book's key thesis is one about the relative merits of the arguments on each side, that this thesis is substantiated and not merely asserted, and that the substantiation proceeds by the reasoned and self-reflective presentation, analysis, and evaluation of the respective arguments. In

[19] The classic example is Duhem (1969), for whom, although Galileo was scientifically right, his epistemological realism was wrong; Duhem claims that "that logic was on the side of Osiander, Bellarmine, and Urban VIII, and not on the side of Kepler and Galileo; that the former had understood the exact import of the experimental method; and that, in this regard, the latter were mistaken" (Duhem 1908, 136, my translation; cf. Duhem 1969, 113). Feyerabend (1985) is another prominent scholar who likes to portray Galileo as saying one thing, but doing another.

short, critical reasoning is a key part of the book's content as well as of the book's approach.[20]

The book's methodological judiciousness is shown by the fact that in the course of his main argument Galileo elaborates a number of methodological ideas which may be described as follows: a critical empiricism which may be equated with critical apriorism, but such that it rejects both naïve empiricism and dogmatic apriorism; a penchant for the mathematical approach, but combined with an awareness of the difficulties in applying mathematical truths to physical reality; epistemological modesty which avoids both skepticism and facile dogmatism; independent-mindedness which advocates neither a total rejection of authority nor a total submission to it; a realistic rational-mindedness which avoids both a simple minded rationalism and a despairing misologism; a judicious attitude toward the principle of simplicity which judges simplicity considerations to be neither worthless nor binding, but rather probable and plausible; a belief that many human concepts (such as the concept of size) are subjective and relative in several ways but not anthropocentric or teleological; a willingness to admit that the stellar parallax objection to Copernicanism cannot be refuted, and that all one can do is to outline a research program designed to test its existence.[21]

After this account of the book's content and structure, the rest of our story would have to be an account of various Galilean reflections corresponding to it, in the sense that these reflections provide the theory of which the book is the practice. These reflections involve Galileo's response to three things: Bellarmine's epistemological objections, the biblical argument against the earth's motion, and the official restrictions by Church authorities.

The heart of Galileo's response to Bellarmine is found in his "Considerations on the Copernican Opinion," written in 1615:

> 6. Not to believe that there is a demonstration of the earth's mobility until it is shown is very *prudent*, nor do we ask that anyone believe such a thing without a demonstration. On the

[20] As mentioned in a previous note, further details and documentation are beyond the scope of this paper, but may be found in Finocchiaro (1980, 1997). I now believe that this critical interpretation also corresponds to the one earlier advanced by McMullin (1967b, 35–42), although the context of his inquiry is different and he emphasizes the notion of a "reduction argument," namely one which "reduces the number of motions in a system" (p. 37). In fact, he is very clear that "most scientists among Galileo's readers were persuaded that the Copernican view had been adequately validated by the arguments of the *Dialogo*, but it is doubtful that they were any more impressed by their strictly *demonstrative* character than the theologians were" (McMullin 1967b, 35); that "a reduction argument … is, in fact, a dynamic argument of a particularly cogent sort" (p. 37); that, for example, Galileo's argument for terrestrial rotation at the beginning of the Second Day "is cogent, and suffices to 'prove' the diurnal motion of the earth in the only sense of 'prove' appropriate to the context" (p. 38); and that with such reduction arguments even "Tycho's own model … could easily be attacked" (p. 40), though they would apply less easily against a semi-Tychonic system in which the earth has diurnal rotation but remains at the center. As suggested below, I now believe our main difference is not about the actual accomplishment of the *Dialogue*, but about Galileo's methodological self-conception; I take the latter to correspond to the accomplishment, whereas McMullin (1967b) takes it to diverge from it and conform to the demonstrative ideal. This difference misled me in my earlier writings and made me neglect the points we share.

[21] For the details, see Finocchiaro (1980, 103–141, 157–164; 1997).

> contrary, we only seek that, for the advantage of the Holy Church, one examine with the utmost severity what the followers of this doctrine know and can advance, and that nothing be granted them unless the strength of their arguments greatly *exceeds* that of the reasons for the opposite side. Now, if they are not *more than 90% right*, they may be dismissed; but if all that is produced by philosophers and astronomers on the opposite side is shown to be mostly false and wholly inconsequential, then the other side *should not be disparaged, nor deemed paradoxical, so as to think that it could never be clearly proved*. It is proper to make such a generous offer since it is clear that *those who hold the false side cannot have in their favor any valid reason or experiment, whereas it is necessary that all things agree and correspond with the true side*.
>
> 7. It is true that it is not the same to show that one can save the appearances with the earth's motion and the sun's stability, and to demonstrate that these hypotheses are really true in nature. But it is equally true, or even more so, that one cannot account for such appearances with the other commonly accepted system. The latter is undoubtedly false, while it is clear that the former, which can account for them, *may be true. Nor can or should one seek any greater truth in a position than that it corresponds with all particular appearances.*[22]

This crucial passage deserves more serious study than it has received and than is possible here.[23] I have italicized the expressions which seem to me to express the more important points. In short, my view is that here Galileo formulates explicitly and reflectively a number of issues which then find their practical and actual execution in the *Dialogue*.

Next, there is Galileo's position in regard to the key religious objection, the biblical argument, elaborated in 1615 in the "Letter to the Grand Duchess." To shorten another long story, here the key claim is a principle attributed by Galileo to one Cardinal Baronio, to the effect that "the intention of the Holy Spirit is to teach us how one goes to heaven and not how heaven goes."[24] This memorable formulation is the one directly applicable in the context of the "Letter," namely in the criticism of the biblical objection; in fact, this objection argued that the earth must be standing still because it is so stated or implied in the Bible; and Baronio's principle directly invalidates the inferential soundness of this argument. The "Letter" also criticizes the truth of argument's premise, but that need not concern us here.[25]

More important for now is a corollary of this principle formulating a norm corresponding more directly to Galileo's procedure in the *Dialogue*. The corollary is that biblical assertions about physical reality ought to be disregarded in natural science, or that scientists are free to investigate physical theories that contradict biblical

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[22] Galilei 1890–1909, 5: 368–369, as translated in Finocchiaro 1989, 85; italics added.

[23] Duhem (1908, 1969) is one who paid some attention to this passage; but it is not generally known that his accounts involves, among other things, interpreting "may be true" as "certainly true" (!!!); Galileo clearly states that, comparing the geokinetic and the geostatic system, the latter is undoubtedly false while the former *may be* true, but Duhem glosses this to mean that "the destruction of one of the two opposing systems assures the certainty of the other one" (Duhem 1908, 131; 1969, 109); for a more general criticism of Duhem, but also some appreciation, see Finocchiaro 1992.

[24] Galilei 1890–1909, 5: 319, as translated in Finocchiaro 1989, 96.

[25] For more details and documentation of the interpretation summarized here, see Finocchiaro 1986; for a different interpretation, see McMullin 1967b, 33–34.

assertions. In the words of the Inquisition Sentence which in 1633 found that procedure to be heretical, the corollary principle is "that one may hold and defend as probable an opinion … contrary to the Holy Scripture."[26] I am here concurring with the Inquisitor's *interpretation*, although I would respectfully dissent from their *evaluation.*

My third point about the methodological relationship between scientific inquiry and biblical interpretation involves Galileo's justification of Baronio's principle. That is, the "Letter" could not have just assumed this unorthodox idea, and so one of its central purposes is to justify it. Now, it is interesting and important that Galileo attempts what might be called an orthodox justification, namely one based on orthodox ideas. These stem primarily from St. Augustine. Galileo accepts Augustine's stress on "prudence"; this is intimately related to judiciousness, and should come as no surprise in the light of our earlier analysis. At the substantive level, the key premise of Galileo's argument is the traditional principle that if a biblical assertion contradicts a physical claim which has been conclusively proved, the latter it to be given priority and the biblical assertion set aside or reinterpreted. Now, the crucial consideration in Galileo's argument is to ask for the rationale for this traditional practice. What is the reason why conclusively proved physical truths are (traditionally and uncontroversially) given precedence over conflicting biblical assertions? Baronio's principle gives the answer and provides the rationale.

Next, once one accepts Baronio's principle, one can apply it to give an answer to another question, yielding another corollary. What should one do when biblical assertions contradict physical claims which have not yet been conclusively proved, but are capable of such a proof? The answer is that the scientist should be free to examine such claims and search for a proof. This corollary must be regarded to be as well grounded as the traditional Augustinian principle; the same reasons that justify the latter, will justify the former. Now, if Galileo were in possession of a conclusive proof of Copernicanism, then he would not have had to write this "Letter," or to answer criticism; he could have simply produced his proof, and the application of the traditional Augustinian principle would have easily and quickly resolved the problem. Thus the mere writing of the "Letter" is an indication that he felt Copernicanism was capable of conclusive proof, though not yet so proved. From this point of view, the *Dialogue* may be described as aiming to establish that the earth's motion is susceptible of conclusive proof (as distinct from establishing that this phenomenon is indeed conclusively proved).

Finally, one may also ask what to do in regard to physical claims which, besides lacking a conclusive proof, are not even capable of being conclusively proved. For this class of propositions, Galileo sees no difficulty in conceding to accept the Bible's word.

A third important element of the methodological background of the *Dialogue* involves the various ecclesiastical restrictions under which he was operating. To make another long story short, the net effect of these restrictions was to push Galileo away from the Aristotelian ideal of demonstration (which is the legacy he inherited)

[26] Galilei 1890–1909, 19: 405, as translated in Finocchiaro 1989, 291.

and toward a fallibilist, retroductive, hypothetico-deductive conception of science.[27] One important restriction involved the proceedings of 1616, especially Cardinal Bellarmine's private warning to Galileo and the anti-Copernican Decree of the Index; these are briefly, clearly, and authoritatively summarized in Bellarmine's Certificate. This document states that Galileo was not supposed to hold, support, or defend the earth's motion.[28] By contrast, in this context we may disregard the special injunction not to discuss the topic in any way whatever, because it is virtually certain that Galileo had no knowledge of it. The other main relevant restriction was Pope Urban VIII's views on Copernicanism, which Galileo was able to directly glimpse at in the audiences of 1624, at which time he started writing the *Dialogue*; there is no simple or clear documentary record here, but we can find hints and references in a number of places, including the book's preface and ending. The restriction may be summarized by saying that only a hypothetical discussion of Copernicanism was legitimate.

Thus, the relevant prescriptions were: no holding, no supporting, no defending, only hypothetical discussion. Beginning with the latter, although the pope had an instrumentalist conception of hypothesis, Galileo seems to have been inclined to construe a hypothesis primarily as a proposition which describes physical reality, which is potentially true or false, whose truth value is not yet known with certainty, and whose status is in the process of being investigated. In regard to the prohibition to hold the proposition that the earth moves, Galileo seems to have interpreted it as meaning that he was not supposed to believe or accept that this proposition is true; however, this was not the same as believing that the proposition is probable. Thus, these two restrictions reinforced each other in suggesting to him that he adopt a probabilist, fallibilist stance. Now, in regard to supporting and defending, these activities may be viewed as correlative of one another is the sense that to support is to argue positively by providing favorable evidence and arguments, while to defend is to argue negatively by answering, refuting, or criticizing objections and counter-evidence. Thus, the question of supporting and defending is intimately related to the question of impartiality. Rather than supporting or defending the earth's motion, Galileo decided to simply discuss the arguments. What kind of discussion of the arguments? He must have felt that there could be nothing wrong with stating, analyzing, and evaluating the arguments on both sides.

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[27] Here I am adopting a thesis first elaborated by Morpurgo-Tagliabue (1981), although I do not agree with other parts of his account.

[28] Galilei 1890–1909, 19: 348; cf. Finocchiaro 1989, 153. The relevant documents usually speak of prohibition which includes the words *tenere* and *defendere*. These Latin words are best taken to mean "to hold, support, and defend" a belief. This interpretation has three elements: accepting or believing an idea as true; justifying the idea positively (with supporting reasons and evidence); and justifying it defensively (by criticizing objections, counter-evidence, and counter-augments). The Latin terms should not be taken to mean simply to support and defend, or simply to hold and defend. As Morpurgo-Tagliabue (1981) has argued, the key point is that the Latin *tenere* can mean simply to hold a belief as well as to hold and support a belief; to this we should add that *defendere* can mean simply to justify defensively or to justify both defensively and positively. In *The Galileo Affair* (Finocchiaro 1989) I usually translated *tenere* and *defendere* as "to hold and defend," but this should be understood in the broad sense which includes all three of the above mentioned elements.

This was a plausible, realistic, and viable program for operating within the restrictions. There were only two ways of violating this interpretation of the restrictions: (1) by presenting the argument(s) for Copernicanism as completely conclusive, which would entitle one to hold, believe, or accept it as true; and (2) by failing to be impartial. Now, it seems to me he could plausibly claim that his book was committing neither of these two violations.

To see this, I would argue that one must first disentangle the key problematic thesis of the earth's motion from other parts of the Copernican system; the ecclesiastic restrictions applied only to this thesis; with respect to other subsidiary theses, there was no difficulty for the book to exhibit belief in their truth and to advance conclusive arguments; such was the case for the propositions that the lunar surface is rough, that the sunspots are part of the solar body, and that the planets (Mercury, Venus, Mars, Jupiter, and Saturn) revolve in heliocentric orbits. Second, once the focus becomes the earth's motion, it is entirely appropriate to omit explicit discussion of the Tychonic system since it shares the geostatic thesis with the Ptolemaic system; as long as the relevant issues were discussed, impartiality could be preserved; and the book did discuss the relevant issues; for example, Galileo is quite clear that the heliocentrism of the five agreed-upon planets is no conclusive evidence for the earth's annual motion, since part of the crucial issue is whether or not the earth is a planet.[29] Third, the book did not hide the fact that there was one important piece of evidence against Copernicanism which remained unrefuted, namely the absence of stellar parallax; the Third Day goes to great lengths in clarifying this objection and admitting its cogency; it also sketches a research program to test its existence, and expresses confidence that the result will favor the geokinetic hypothesis; but Galileo is perfectly aware that the test could yield negative results.[30] Finally, the book is very explicit about the hypothetical, explanatory, and causal form and structure of the two best pro-Copernican arguments, based on the motion of sunspots and on the tides; for the case of the argument from sunspot motion, Galileo himself sketches an alternative geostatic explanation, and so there is no question that he realizes that, while the argument strongly favors the earth's motion, it is nevertheless inconclusive. For the tidal argument, Galileo felt it was even stronger, and was unwilling or unable to seriously consider any alternative explanations; but even here he stated a general objection detracting from its absolute conclusiveness; this was the divine-omnipotence objection favored by Pope Urban VIII, which he was ordered to include in the book; however, in a sense this is a theological version of the epistemological problem of induction and of the logical invalidity of the argument form of affirming the consequent, and so the objection can easily stated in a way which makes it valid; thus, the presumption must be that he realized and understood this;[31] moreover, there is indirect evidence to that effect because Urban's objection also corresponds to an idea advanced by Galileo in the *Assayer* and greatly appreciated by the pope in that context; this is the idea that nature is bountiful and can produce a given effect by

[29] See, for example, Galilei 1890–1909, 7: 354; Galilei 1967, 326–327.

[30] Galilei 1890–1909, 7: 385-416; 1967, 358–389.

[31] For more details on this issue, see Finocchiaro 1980, 8–12.

means of many different effects, a point illustrated by the story of a man searching the cause of sound and ending up killing a cicada in the process.[32]

4.5 Conclusion

I have argued that Galileo's *Dialogue* may be fruitfully interpreted and evaluated from the viewpoints of methodological judgment and critical reasoning. By methodological judgment I mean the avoidance of one-sidedness and extremes with respect to issues in the search for truth; and I define critical reasoning as reasoning aimed at the analysis, evaluation, and/or self-reflective formulation of arguments. This approach has significant historical precedents in the readings of Salusbury, Hume, and the Port-Royal logicians. This focus on the book's philosophical dimension is not meant to exclude its scientific, historical, rhetorical, and aesthetic dimensions. Indeed, such anti-reductionism is suggested by methodological judgment applied at the meta-level. The latter also suggests a criticism of the excessively rhetorical readings prevalent today, as well as viable ways for resolving the problems of hermeneutical pluralism, interpretation versus evaluation, and theory versus practice. Finally, the book's methodological judgment and critical reasoning can be shown to correspond to Galileo's intentions and self-reflections.

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[32] Drake and O'Malley 1960, 236. It should be added that both Galileo's view of Urban's objection and his fable of the search for the cause of sound fit very well with what Biagioli (1993, 301–311) calls the "court nominalism" or "baroque eclecticism"; this was an important part of their social milieu and was accepted by both Galileo and Urban, each in his own way and for his own reasons.

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Chapter 5
Critique of the Ship-Experiment Argument

Abstract This essay is part of a critical comparison between Galileo's defense of Copernicanism and the defense of Galileo from the many attempts to criticize his reasoning or to justify his condemnation by the Inquisition. In such a context, I examine the anti-Copernican argument based on the ship's mast experiment: that the earth cannot rotate because on a moving ship bodies dropped from the top of the mast lag behind, landing astern from its foot. Galileo criticized this argument with (1) a critical counter-argument questioning the analogy between a moving ship and a rotating earth; (2) a theoretical counter-argument concluding that on a moving ship falling bodies do not lag behind; and (3) an actual experiment refuting the anti-Copernican empirical claim. My discussion involves various aspects of the logic, methodology, history, historiography, and physics of such an experiment.

5.1 Introduction

There is no doubt that the most original recent works on the Galileo affair have achieved new heights of erudition, documentation, and sophistication. These achievements have been made possible by a number of circumstances: the maturation of various professional disciplines such as history of science and history of philosophy; the emergence of the new field of the scholarly study of the interactions between science and religion; and the discovery of a few new documents stemming from the Vatican Galileo Commission in 1979–1992 and from the formal opening to scholars of the Inquisition Archives in Rome since 1998.

However, with few exceptions, these scholarly works tend to exhibit several key weaknesses. One is that they usually abound in over-inflated complications, concluding little more than that the Galileo affair is more complicated than previously thought. Another scholarly weakness is historiographical: that is, studies of Galileo's trial tend to be conducted with an inadequate knowledge of previous views, whose four-century old history contains insights that are not, but should be, appropriated and updated, and errors that are often repeated instead of avoided. A third weakness is the unwillingness or inability to learn from Galileo; that is, the practice

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of viewing the events and documents of the Galileo affair as inert material objects that can only be understood or interpreted, rather than as human actions (and thoughts) that can be evaluated and assessed, so as to derive from them useful lessons for us today.

My investigation of this topic is animated by the desire to avoid inflated complications (while, needless to say, avoiding over-simplifications), aiming instead at a genuine and proper simplification of a bewildering mass of material; that is, it aims to articulate a simplifying synthesis, based on primary as well as secondary sources. The investigation is also animated by the desire to take into account the history of the historiography of the Galileo affair, by exploiting the insights and avoiding the errors of previous accounts. Thirdly, I do not shy away from using Galileo (his manner of thinking) as a model to be emulated, based on an accurate and correct understanding and interpretation; the understanding of the model is developed with regard to Galileo's trial (the original affair), whereas the model is then applied to the ongoing controversy about the trial (the subsequent Galileo affair). Finally, because of the features of the Galilean model as I interpret it, I follow an approach that involves defending Galileo from his many critics while mindful of the power and importance of the many objections which they advance, and similarly criticizing the Church while appreciating the power and importance of the many pro-clerical arguments.

In accordance with such a motivation and such an approach, I have elsewhere[1] elaborated the following detailed argument and overarching thesis. The Copernican Revolution required that the geokinetic hypothesis be not only supported with new reasons and evidence, but also defended from many powerful old and new objections (stemming from astronomical observation, Aristotelian physics, biblical texts, and traditional epistemology). This defense in turn required not only the destructive refutation but also the appreciative understanding of those objections in all their strength. One of Galileo's major accomplishments was not only to provide new evidence supporting the earth's motion, but also to show how those objections could be refuted and to elaborate their power before they were answered. In this sense, Galileo's defense of Copernicus was reasoned, critical, open-minded, and fair-minded. Now, an essential thread of the subsequent Galileo affair has been the emergence of many anti-Galilean criticisms (from the viewpoint of astronomy, physics, theology, hermeneutics, logic, epistemology, methodology, law, social sensibility, and morals), but also various defenses of Galileo. It is important to understand both that such criticisms arise naturally and legitimately and that he can be, and has been, defended from them. The overarching thesis justified by this argument is then that today, in the context of the controversies over science versus religion and over individual freedom versus institutional authority, the proper defense of Galileo should have the reasoned, critical, open-minded, and fair-minded character which his own defense of Copernicanism had.

In other words, the defense of Galileo can and should be an exercise in critical reasoning, just as his own defense of Copernicanism was. At the level of a nominal

[1] Finocchiaro 2010.

definition, critical reasoning is simply reasoning aimed at the analysis, evaluation, or self-reflective formulation of arguments; and an argument is a piece of reasoning aiming to justify a conclusion by supporting it with reasons or defending it from objections. At a deeper level, critical reasoning, at least as practiced by Galileo and as aspired to in this investigation, is guided by a number of principles, such as open-mindedness and fair-mindedness.[2]

The most relevant of these principles in this context are some recurrent themes which he both preached and practiced in his Copernican campaign. For example, Galileo regarded open-mindedness as extremely significant and exemplified by the fact that "the followers of the new system produce against themselves observations, experiments, and reasons much stronger than those produced by Aristotle, Ptolemy, and other opponents of the same conclusions."[3] He took fair-mindedness to be equally important and formulated it as the principle that "when one presents arguments for the opposite side with the intention of confuting them, they must be explained in the fairest way and not be made out of straw to the disadvantage of the opponent."[4] And he considered rational-mindedness as essential, that is the requirement that "one examine with the utmost severity what the followers of this doctrine know and can advance, and that nothing be granted them unless the strength of their arguments greatly exceeds that of the reasons for the opposite side."[5]

In this essay, I plan to provide a further illustration of my approach and some further support for the above-mentioned overarching thesis by examining a particular instance of Galileo's defense of Copernicanism that can be usefully compared and contrasted with the defense of Galileo from various criticisms advanced against that defense. The example involves the anti-Copernican argument based on the ship's mast experiment.

5.2 Anti-Copernican Argument and Anti-Galilean Criticism

The ship's mast experiment amounted to dropping a body, such as a rock or cannon ball, from the top of a ship's mast; the experiment is to be done both when the ship is motionless and when it is advancing forward. This experiment provided the basis for an interesting and important objection to the Copernican doctrine of the earth's

[2] Besides extracting these concepts and principles from Galileo, I have, in part, adapted them from the literature on informal logic, argumentation, and critical thinking. See, especially, Ennis 1996; Fisher 1991; Fisher and Scriven 1997, 90–91, 137–143; Govier 1987, 55–80; Govier 1999, 155–180; Johnson 2000, 143–179; Paul 1990, 110, 111, 198; Scriven 1976, 166–167; Woods 1992, 1996, 650–662. In part, these ideas, although not under these labels, can also be found explicitly discussed in Chapter 2 of John Stuart Mill's *On Liberty*; see Finocchiaro 2007.

[3] In Galilei 2008, 216–217; see also Galilei 1890–1909, 7: 153–154.

[4] In Galilei 2008, 283; corresponding to Galilei 1890–1909, 19: 343. See also Querenghi to D'Este, 20 January 1616, in Galilei 1890–1909, 12: 226–227; and Motta 1993a, p. 612, n. 55.

[5] In Galilei 2008, 165; corresponding to Galilei 1890–1909, 5: 368–369.

motion, specifically to the earth's daily axial rotation. The objection can be stated as follows, and here one can hardly improve on the clarity and succinctness of Galileo's own formulation:

> Because when the ship stands still the rock falls at the foot of the mast, and when the ship is in motion it falls away from the foot, therefore, inverting, from the rock falling at the foot one infers the ship to be standing still, and from its falling away one argues to the ship being in motion; but what happens to the ship must likewise happen to the terrestrial globe; hence, from the rock falling at the foot of the tower, one necessarily infers the immobility of the terrestrial globe.[6]

Now, it is well known that Galileo answered and criticized this argument, thus defending Copernicanism from the objection based on the ship's mast experiment. However, the details of Galileo's refutation are less well known, or at least less well understood. Thus, before discussing Galileo's reply, it will be useful to mention some of these misunderstandings, especially since they will illustrate my theme of defending Galileo. In fact, these accounts of this Galilean defense of Copernicus portray Galileo in an unfavorable light, and so my criticism of these accounts will defend him in the sense of freeing him from various questionable practices attributed to him.

One of the most influential accounts of Galileo's critique of the ship's mast experiment has been that of Alexandre Koyré. He used it as an important basis, although of course not the only one, on which to build his apriorist interpretation of Galileo's work and the scientific revolution in general. Let us read Koyré's own words:

> It is thought, pure unadulterated thought, and not experience or sense-perception … that gives the basis for the "new science" of Galileo Galilei. Galileo is perfectly clear about it. Thus discussing the famous example of the ball falling from the top of the mast of a moving ship, Galileo explains at length the principle of the physical relativity of motion, the difference between the motion of the body as relative to the earth, and as relative to the ship, and then, *without making any appeal to experience,* concludes that the motion of the ball, *in relation to the ship,* does not change with the motion of the latter. Moreover, when his empirically minded Aristotelian opponent asks him, "Did you make an experiment?", Galileo proudly declares: "No, and I do not need it, as without any experience I can affirm that it is so, because it cannot be otherwise." Thus *necesse* determines *esse.* Good physics is made a priori. Theory precedes fact. Experience is useless because before any experience we are already in possession of the knowledge we are seeking for. Fundamental laws of motion (and of rest), laws that determine the spatio-temporal behavior of material bodies are laws of a mathematical nature. Of the same nature as those which govern relations and laws of figures and of numbers. We find and discover them not in Nature, but in ourselves, in our mind, in our memory, as Plato long ago has taught us.[7]

Here we have, succinctly stated, Koyré's apriorist, mathematicist, and Platonist account of the ship's mast experiment, of Galileo, and of the scientific revolution. Koyré's account is primarily an interpretation. But obviously, it is also an evaluation, a positive or favorable assessment, because for Koyré Galileo was thus giving birth to modern physics and was doing good physics.

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[6] Galilei 2008, 228; corresponding to Galilei 1890–1909, 7: 169–170.

[7] Koyré 1968, 13. For more nuanced, qualified, and tenable accounts by Koyré, see Koyré (1939, 66–68; 1966, 226–228; 1978, 166–167).

However, with friends like these, one may very well want to consort with one's enemies. That is, with friends like Koyré, Galileo may very well wish the company of his critics. In fact, I think that Koyré's principle of evaluation, that "good physics is made a priori," is flawed, almost the reverse of the truth. Moreover, his interpretation, if accurate, would imply that Galileo had not really succeeded in defending Copernicus from the objection based on the ship's mast experiment, and was rather begging the question.

Although the example of Koyré is instructive and illustrious enough, I also want to give another similar example of a philosopher—Paul Feyerabend. As for Koyré, the sources and reasons for Feyerabend's views are manifold and complex. Similarly, one source is the belief that the ship's mast experiment is a typical Galilean example of the fact that "many of the 'experiences' or 'experiments' used in the arguments about the motion of the earth are entirely fictitious."[8] Feyerabend's account is also a version of apriorism. However, instead of combining apriorism with mathematicism and Platonism, Feyerabend combines it with rhetoricism and anarchism. His words are also worth quoting:

> Galileo replaces one natural interpretation by a very different and as yet (1630) at least partly unnatural interpretation. How does he proceed? How does he manage to introduce absurd and counter-inductive assertions, such as the assertion that the earth moves, and yet get them a just and attentive hearing? One anticipates that arguments will not suffice—an interesting and highly important limitation of rationalism—and that Galileo's utterances are arguments in appearance only. For Galileo uses *propaganda.* He uses *psychological tricks* in addition to whatever intellectual reasons he has to offer. These tricks are very successful: they lead him to victory. But they obscure the new attitude toward experience that is in the making, and postpone for centuries the possibility of a reasonable philosophy. They obscure the fact that the experience on which Galileo wants to base the Copernican view is nothing but the result of his own fertile imagination, that it has been *invented*.[9]

Again, as for Koyré's case, it should be stressed that Feyerabend's rhetoricist and anarchist interpretation of Galileo's defense of Copernicus is meant by him to yield a favorable positive evaluation. That is, for Feyerabend, Galileo succeeded because he proceeded in these rhetoricist and anarchist ways. The connection is that good science is basically an anarchist enterprise, and this principle represents the Feyerabend analogue of Koyré's good physics being made a priori.

However, many of us do not share Feyerabend's principle of evaluation, any more that we share Koyré's.[10] Instead, success achieved by these means would not be lasting or real success. Hence, if Feyerabend's interpretation were accurate, it would justify a negative criticism of Galileo's methodology. In that sense, a critique of Feyerabend's critique becomes a defense of Galileo.

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[8] Feyerabend 1988, p. 77, n. 20.

[9] Feyerabend 1988, 67.

[10] For more criticism of Feyerabend and Koyré, as well as some appreciation, see Finocchiaro 1980, 180–201, 202–223, respectively. See also Chapters 19 and 20 in this book.

5.3 Analysis of the Argument

I have already quoted the passage where Galileo gives his own formulation of the anti-Copernican argument from the ship's mast experiment. To understand it properly, it will be instructive to begin by discussing Stillman Drake's translation of that passage. Doing so will also dispel the idea that my criticism of Koyré's account of Galileo amounts to an endorsement of Drake's account, which is sometimes regarded as its anti-thesis, such that the two of them exhaust all alternatives. To be sure, Drake did not make a big issue out of the ship argument; he did not try to exploit it to build his own account, which he based on other evidence. However, Drake did give a flawed translation of Galileo's statement of the argument as found in the *Dialogue,* and I think this flawed translation is one of the things that has misled some scholars. Drake's translation reads as follows:

> Since when the ship stands still the rock falls to the foot of the mast, and when the ship is in motion it falls apart from there, then conversely, from the falling of the rock at the foot it is inferred that the ship stands still, and from its falling away it may be deduced that the ship is moving. And since what happens on the ship must likewise happen on the land, from the falling of the rock at the foot of the tower one necessarily infers the immobility of the terrestrial globe.[11]

The problem stems from the sentence "what happens on the ship must likewise happen on the land," which instead should read "what happens to the ship must likewise happen to the terrestrial globe." The difference is that Galileo's formulation makes is clear that this anti-Copernican objection is primarily an argument from analogy, the two analogues being the ship (together with its mast) and the earth (together with a tower on its surface). Moreover, Galileo's lengthy discussion treats the objection as an argument from analogy, by advancing two criticisms: first, that the analogy is weak, and second that its key premise is false, namely the premise that on a moving ship the rock falls away from the foot of the mast toward the back of the ship.[12] These criticisms will be elaborated presently.

On the other hand, as translated by Drake the anti-Copernican argument seems to be a generalization or extrapolation from sea to land. This may mislead the reader into confusing the argument from the ship's mast experiment with the tower argument from vertical fall, and into thinking that the former is just a version of the latter. The two arguments are indeed related, but their relationship is the following.

The argument from the ship's mast experiment has two distinct inferential steps, or subarguments, if you will. The first step infers that a body dropped from the top of a tower on the earth's surface falls to its foot if and only if the earth is motionless; and the adduced evidence for this is that a body dropped from the top of the mast on a ship falls to its foot if and only if the ship is not moving forward, and additionally a tower on the earth is like a mast on a ship. The second step uses the intermediate conclusion inferred in this first step to arrive at the further conclusion that the earth

[11] Galilei 1967, 144, corresponding to Galilei 1890–1909, 7: 169–170.

[12] Galilei 1890–1909, 7: 167–169, 169–175, respectively.

is motionless; that is, given that a body dropped from the top of a tower falls at its foot if and only if the earth is motionless; and given that common observation reveals that a body dropped from the top of a tower falls at its foot; it follows that the earth is motionless. In short, in this context the tower argument is one of two main parts of the argument from the ship's mast experiment.

Drake's formulation may also mislead the reader into interpreting the ship's mast experiment as a thought experiment on a rotating earth, and Galileo's counter-argument as an attempted proof of the earth's motion along the following lines: since the rock would fall at the foot of the ship's mast on a rotating earth, it would also fall at the foot of the tower on the same rotating earth; but this is what is actually observed to happen; thus the earth's rotation is thereby confirmed.[13] On the other hand, Galileo's counter-argument in this context is not an inductive argument for the earth's motion, but a refutation of an argument against the earth's motion. Thus, its conclusion is not the proposition that the earth moves, but rather the proposition that this particular anti-Copernican objection is incorrect.

5.4 Galileo's Criticism

Since our focus here is the ship's mast experiment, our primary interest lies in the first part of the fuller argument. That is the part which has the form of an argument from analogy, and whose crucial premise is an observation report from the ship's mast experiment. As already mentioned, Galileo criticizes this subargument in two distinct ways: he undermines its inferential strength by questioning the analogy, and he argues that it is false that on a moving ship the rock falls away from the foot of the mast.[14]

Galileo argues that the analogy is questionable partly because on a moving ship the horizontal motion imparted by the ship to the rock before the drop is violent motion, which might be dissipated after the rock is left to itself; whereas on a rotating earth the horizontal motion imparted to the rock before the drop would be natural motion, at least in the sense that it would be everlastingly shared by the rock with the earth insofar as the rock is a natural part of the whole earth. Another reason for the weakness of the analogy is that on a moving ship air resistance would oppose the horizontal motion acquired by the rock before the drop, since the ambient air would not be moving with the ship, whereas on a rotating earth the air (as a terrestrial body) would probably rotate along; or at least, this would happen for the lower parts of the atmosphere that are trapped between mountains and often contain particles of the elements earth and water. Thus, even if the rock falls behind on a moving ship, it might not do so on a moving earth. However, does the rock fall behind on a moving ship?

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[13] One scholar who comes close to suggesting that Feyerabend attributes such an argument to Galileo is Machamer 1973, 27–28.

[14] Galilei 1890–1909, 7: 167–169 and 169–175, respectively.

Galileo believes that on a moving ship the rock is not left behind but rather falls at the foot of the mast, the same as it does on a motionless ship. He has two reasons for this. One is an experimental report; the other is an indirect theoretical argument. It is only in his "Reply to Ingoli" (1624) that he states that he has done the experiment, and that the results prove his claim about the falling rock on the moving ship. However, in the *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632) he gives only the theoretical argument. The exclusive focus on the theoretical argument in the *Dialogue,* together with the neglect of the claim in the "Reply to Ingoli," is the main factor that misled some scholars to advocate apriorist or rhetoricist interpretations. However, another factor is perhaps the inadequate reconstruction of that theoretical argument. Its careful examination shows that Galileo is basing his conclusion about the moving ship not on innate ideas recollected by Platonic *anamnesis* (as Koyré would have it), nor on experiences invented by his vivid imagination (as Feyerabend would have it), but on more easily observed facts combined or extrapolated by reasoning. Thus, the methodological suggestion is not that good physics is done a priori, or by propagandistic tricks, but that experiments are sometimes unnecessary to ascertain the results of a test, insofar as sometimes it can be shown what these results must be on the basis of known or more easily ascertainable observational facts.

Such a reconstruction of Galileo's theoretical argument is as follows. The more easily ascertainable observational facts are: (1) the undisturbed downward motion of bodies on an inclined plane is accelerated; (2) their undisturbed motion up an inclined plane is decelerated; and (3) the cause of projectile motion is not the motion of the surrounding air. From (1) and (2) one may infer that (4) the motion of bodies on an horizontal plane is conserved if undisturbed, and consequently that (5) the horizontal motion which the rock has before being dropped on the moving ship continues even after being dropped, if undisturbed. Now from (3) one can infer that (6) the cause of the motion of projectiles is the "virtue" impressed on them by the projector, and consequently that (7) the cause of the horizontal motion of the rock, after it has been dropped, is the horizontal "virtue" impressed on it by the hand holding it before dropping. But, (8) there is no way in which this horizontal impressed "virtue" could be disturbed by the vertically downward tendency due to the weight, because (9) the two are at right angles to each other. It follows that (10) the horizontal motion of the dropped falling rock is undisturbed, and hence that (11) that motion will continue, and therefore that (12) the rock will end up at the foot of the mast on the moving ship.

This argument is not gratuitously based on the principle of the mechanical relativity of motion, and so it does not commit the fallacy of begging the question, as implied by Koyré. Nor is it merely a piece of propaganda or rhetorical trick, and so it is not a piece of sophistry, as implied by Feyerabend. On the other hand, the argument is not completely conclusive and compelling. For example, it presupposes the principle of the superposition of motion, which is lurking around in the justification of proposition (8) by (9), and which might need more elaboration. So it is not surprising that Galileo would seek a direct experimental test of the conclusion that

on a moving ship the dropped rock still falls to the foot of the mast, thus refuting the key premise of the anti-Copernican argument based on the ship's mast experiment.

As previously mentioned, Galileo's report is found in his "Reply to Ingoli." Here are his words:

> If you and Tycho [Brahe] wanted sincerely to confess the truth, you should say that you have never tested whether or not any difference is observable in shooting artillery toward the east and toward the west, or toward the north and toward the south (especially in regions near the pole, where according to you the effect should be more noticeable); I am moved to believe this, indeed to be sure of it, by the fact that you put forth as certain and unequivocal other experimental observations which are much easier to make and which I am so sure you did not make as to be able to say that whoever makes them will find the effect to be contrary to what you claimed with excessive confidence. One of these experiments is precisely that of the rock falling from the top of the mast on a ship; the rock always ends up hitting the same spot, whether the ship is standing still or moving forward fast; it does not strike away from the foot toward the stern, as you believed (on account of the ship moving forward while the rock comes down through the air). Here I have been a better philosopher than you in two ways: for, besides asserting something which is the opposite of what actually happens, you have also added a lie by saying that it was an experimental observation; whereas I have made the experiment, and even before that, natural reason had firmly persuaded me that the effect had to happen the way it indeed does.[15]

5.5 Galileo's Performance of the Experiment

So Galileo did not just metaphysically posit, imaginatively invent, or rhetorically concoct his criticism of the anti-Copernican argument based on the ship's mast experiment. Nor did he uncritically or naïvely observe the results of the ship's mast experiment. Rather he defended Copernicanism from this objection by engaging in critical reasoning, which involved some imaginative conceptualization, some experimental observation, some inferential argumentation, and some meta-cognitive awareness of methodological principles. Still, regarding the experimental observation, we can ask whether there is other evidence that he performed the ship's mast experiment, besides his statement in the "Reply to Ingoli." There is.

The evidence is contained in a letter by Francesco Stelluti (1577–1646) to an unknown correspondent dated January 8, 1633. Stelluti was one of the first members of the Lincean Academy and the one who helped its founder (Federico Cesi) with its administration. The unknown correspondent was perhaps Fabio Colonna (1566 or 1567–1640), a Neapolitan aristocrat and botanist and also member of the Academy. Stelluti's letter is a summary of Galileo's *Dialogue.* It begins by relating the difficulties he had in obtaining a copy of the book, which eventually he was able to borrow for a few days. The relevant passage is the following:

> The book is divided into four Days and has three speakers, a Peripatetic, a Ptolemaic, and a Copernican. In the first and second Days, many objections to the earth's motion are advanced by the Peripatetic, and they are all answered and refuted; and it is proved with

[15] In Finocchiaro 1989, 184, corresponding to Galilei 1890–1909, 5: 545.

> many experiments that all phenomena on it, whether it stands still or is in motion, must naturally occur in the same way. Let me make you understand this better. The Peripatetic says that if the earth were in motion, then when we throw a rock upwards it would fall back down a long distance behind; and yet we see it always falls back to us. Similarly, when we drop a rock from a high tower, we see it fall to the foot of the tower; whereas if the earth were in motion, the rock would remain and fall behind by a considerable distance, for the earth moves with a speed of the order of 700 miles per hour. The answer is that even if the earth were in motion, the rock must naturally fall back at the foot of the tower, and not elsewhere, because the rock has two motions, the direct one of falling down and the transverse one given to it by the earth's circular motion; and so in falling down the rock we threw upwards describes almost a semicircle, due to the combination of its two motions.
>
> And I have seen the experiment. I have been with Mr. Galileo on a rowboat with six oars at the Lake of Piediluco; it was moving forward very fast, and we were sitting on opposite sides facing each other. He asked me if I had a heavy object with me. I told him I had the key to my room and handed it over to him. While the boat was moving very fast, he threw the key upward so high that I thought I would lose it into the water. But although the boat had moved forward eight or ten cubits, nevertheless the key fell between him and me; for besides going upwards, it had acquired from the boat another motion, which made it go along and follow the boat as it did.
>
> Moreover, I have been told by Annibale Brancadoro da Fermo, captain of one of the grand duke's ships, that he has made this experiment. That is, while the galley was moving as fast as it could, he fired a small mortar upwards; the ball fell back into the mouth of the mortar although the galley had moved forward very far in the meantime.[16]

The Lake Piediluco experiment probably took place in 1624, during Galileo's visit to Prince Cesi at Acquasparta on April 8–22.[17] Galileo was traveling from Florence to Rome, to pay homage to Pope Urban VIII, who had been elected the year before. This experiment is, of course, not quantitatively precise, but merely qualitative. However, in this particular case quantitative precision was not really needed. Nevertheless, it is a deliberate experiment, as one can see from some other discussions being held that year, which provide additional, if indirect, evidence of Galileo's experimental confirmation of the claim about falling bodies on a moving ship.

This additional indirect evidence is contained in a letter by Mario Guiducci in Rome to Galileo in Florence, dated September 13, 1624.[18] Guiducci was reporting on some discussions he had had with Jesuit Orazio Grassi, professor of mathematics at the Roman College. Guiducci saw Grassi while both were attending a lecture. Before the lecture, they had this conversation. Grassi said that a Greek Jesuit had told him having heard from Galileo in Padua the following claim: that a body dropped from the top of a ship's mast falls along the mast to its foot regardless of whether the ship is moving forward or standing still. Grassi did not believe this to happen and added that if it happens then it could derive from the air moving with the boat. Guiducci replied that one could make the following similar experiment: while running uphill, extend your arm and drop a ball; you will see the ball following you upwards for a

[16] In Conti 1990, 230–231. Here the translation is my own, and the same applies elsewhere, unless otherwise noted.

[17] Conti 1990, 165. My account here also relies on Besomi and Helbing 1998, 2: 431.

[18] Galilei 1890–1909, 13: 205–207.

while. Grassi answered that this is also due to the motion of the air, which was moved by your arm. Guiducci responded that the experiment be done while running against the wind, claiming that the result is the same. Grassi said that he still found it hard to believe. After the lecture, which lasted about an hour, Grassi told Guiducci that he now understood that Galileo's claim was true. Then Guiducci adds the following remark in his letter: "I wanted him to understand this proposition because I think he does not abhor the earth's motion much if there are good reasons for such a motion and the objections advanced to the contrary are removed; among these objections he thought this one was an extremely important difficulty."[19]

Here two things ought to be stressed. First, it is obvious that Galileo thought it was important and desirable to have a direct experimental confirmation of the proposition that on a moving ship bodies fall from the top to the foot of the mast, as they do on a motionless ship; and it is obvious that he made various attempts at such an experimental confirmation. Second, it was during his earlier career at the University of Padua (1592–1610) that Galileo came to accept this proposition; that is, it was much earlier than 1624. However, it probably was after 1597, for in his *Treatise on the Sphere, or Cosmography*, written that year, Galileo still seems to accept the contrary claim.[20] In fact, in that work, in the context of reporting Ptolemy's arguments against the earth's motion, Galileo gives a statement of the tower argument, claiming that if the earth were in motion then bodies dropped from the top of a tower would fall away (westward) from the foot of the tower; and to support this claim he mentions the ship's mast experiment, asserting that on a moving ship bodies dropped from the top of the mast fall away (toward the rear) of the foot of the mast.

5.6 History of Ship Experiment

Now, to better appreciate Galileo's criticism of the ship's mast argument, the logical and methodological analysis elaborated so far should be supplemented by an account of the history of the argument and the experiment. This history reveals a number of confusions and misunderstandings, and it will enable us to grasp both the power and some of the limitations of Galileo's analysis.

The earliest discussion is probably the one in Christoph Clavius's *Commentary of the Sphere of Sacrobosco*. I say probably because this book went through many editions from its first publication in 1570 to its last in 1611.[21] In the last edition we find the following formulation of the geostatic argument based on the ship experiment: on a rotating earth "a stone or arrow projected straight upward with great force would not fall back to the same place, just as we see happen on a swiftly moving ship."[22]

[19] Galilei 1890–1909, 13: 206.

[20] Galilei 1890–1909, 2: 224.

[21] See Lattis 1994, pp. xix, 268.

[22] As translated and quoted in Lattis 1994, 121, from Clavius 1611–1612, 3: 106.

And this particular passage seems to have undergone no revisions since the first edition.[23]

At about the same time, the opposite experimental result was being claimed by Thomas Digges in a work written in 1576 and entitled *A Perfit Description of the Celestiall Orbs according to the Most Ancient Doctrine of the Pythagoreans.* Digges does not mention the ship's mast experiment as part of an objection to the earth's motion, but rather as part of his refutation of the vertical fall objection; in fact, he states the correct result of the experiment, although he does not elaborate. In a section entitled, "What reasons moved Aristotle and others that followed him to think the earth to rest immoveable as a center to the whole world,"[24] he formulates this objection as follows: "things falling should not light on the places perpendicular under them, neither should they fall directly thereto, the same being violently in the mean carried away."[25] Then in a section entitled "The solutions of these reasons with their insufficiency,"[26] he includes the following reply:

> of things ascending and descending in respect of the world, we must confess them to have a mixed motion of right & circular, albeit it seem to us right & straight, not otherwise than if in a ship under sail a man should softly let a plummet down from the top along by the mast even to the deck. This plummet, passing always by the straight mast, seemeth also to fall in a right line, but being by discourse of reason weighed, his motion is [found] mixed of right and circular.[27]

The next discussion occurs in Giordano Bruno's *Ash Wednesday Supper,* first published in 1585.[28] Bruno's discussion is also pro-Copernican but is much more sustained that Digges's; although it contains many important insights, it is unsatisfactory in some respects.

Bruno's initial statement of the argument seems incoherent. Through the character Smitho (for the book is written in dialogue form), he introduces the ship experiment in the context of Aristotle's argument from vertical fall, or vertical projection. It is unclear whether Smitho is giving the ship experiment to provide further support for Aristotle's objection, or as part of its refutation. Moreover, the accompanying diagram Bruno gives is unlabeled and unhelpful.

Then (through the character Teofilo) Bruno claims that the big difference is between bodies that are in the ship and bodies that are off the ship, on the basis of the following experiments. First, consider a boat moving along a river and throw a rock at it from the river bank; the rock will hit behind the ship. Second, on a moving boat, a rock dropped from the top of the mast will fall to its foot, and a rock thrown upwards from the foot of the mast will fall back to the foot; this second pair of experiments seems to be a relatively clear refutation of the Aristotelian argument.

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[23] Regarding the 1581 edition, see Wallace 1984, 258; and regarding the 1585 edition, see Palmieri 2008, p. 449, notes 23 and 26.

[24] Digges 1576, 11.

[25] Digges 1576, 12.

[26] Digges 1576, 12.

[27] Digges 1576, 14.

[28] Bruno 1955, 177–181.

Then (again through Teofilo) Bruno discusses a third experiment: drop two rocks from the top of the mast of a moving boat, one by someone who is attached to the top of the mast, the other by someone who is standing on land (perhaps on a bridge under which the boat is passing); the rock dropped from the boat will fall to the foot of the mast, whereas the other rock will be left behind. Bruno goes on to explain this fact by saying that the rock on the ship "carries with it the virtue of the motor,"[29] but the other rock does not; that it is this "initially impressed virtue"[30] that causes the rock to fall vertically straight; and that the cause is not the place from which the rock's fall starts, nor the place toward which the rock goes, nor the medium through which it moves.

Here Bruno has the correct intuition about the result of the ship's mast experiment. However, he mentions no actual experiments performed either by himself or by others. Nor does he seem to be aware of the controversial character of the experimental result. He does seem to have a theoretical argument, based on the claim that the rock "carries with it the virtue of the motor,"[31] a claim which may be taken as a formulation of the principle of the conservation of motion; but this principle is left unsupported. Nor does Bruno seem to understand that the principle of superposition is also needed, in order to claim that the downward fall does not disturb the forward horizontal motion. Finally, Bruno does not raise at all the question of the strength of the analogy.

Next, we find Tycho Brahe mentioning the ship experiment as evidence against the earth's motion. This happened in his correspondence with Christoph Rothmann, published in 1596 under the title of *Epistolarum astronomicarum liber primus*. Tycho certainly deserves credit for modernizing and strengthening the traditional objections to the earth's motion, and for stressing the mechanical issues.[32] However, he is very brief about the ship experiment. He just claims that a projectile thrown upwards from a ship will land at different positions on the deck depending on whether or not the ship is moving forward.[33]

In this context, deserving of mention is the brief traditional-oriented mention of the ship experiment by Galileo himself, in the work entitled *Treatise on the Sphere, or Cosmography*, written in 1597. This was discussed earlier, but it is worth repeating that Galileo's views on the topic seem to have undergone some evolution.

Next, Johannes Kepler discusses the ship experiment, first in his letter to Fabricius of November 10, 1608, and then in book 1 of his *Epitome of Copernican Astronomy* (1618).[34] I have not examined these discussions first-hand, but from Koyré's account of them,[35] I gather the following interpretation. Kepler discusses the experiment in the context of defending Copernicanism from Tycho's mechanical objections. He

[29] Bruno 1955, 180–181.

[30] Bruno 1955, 181.

[31] Bruno 1955, 180–181.

[32] Koyré 1966, 141–143; Bucciantini 2003, 56–62.

[33] Brahe 1913–1929, 6: 220; Koyré 1966, 141–143; Mosley 2007, 142.

[34] Kepler 1858–1871, 3: 462ff., and 6: 181ff., respectively.

[35] Koyré 1978, 144–153.

seems to question, not the Tychonic claim about the results of the ship experiment, but rather the analogy between the moving earth and the moving ship. His criticism seems to be that the analogy is questionable because bodies like rocks are connected with the moving earth by a force of attraction (which he believes to be magnetic in nature), whereas there is no such connection between them and the ship, and so even if falling bodies are left behind on a moving ship they would not be on a moving earth. This is my reconstruction of Koyré, who judges Kepler's discussion confusing[36] and is primarily bent on showing that for metaphysical reasons Kepler was unable to "put rest and motion on the same ontological level."[37]

The next noteworthy discussion comes in Lodovico delle Colombe's essay "Against the Motion of the Earth," written in 1610 or 1611.[38] Despite his conservative, and almost reactionary, attitude, his discussion is interesting and important because he admits the correct result of the experiment, and then while attempting to explain it away, he advances several subtle considerations of experimental design.

The immediate context of Colombe's analysis is the discussion of mechanical arguments for and against the earth's rotation. On the anti-Copernican side he presents several arguments, although he does not clearly distinguish among them; they involve vertical fall, crossbows aimed vertically upwards, gunshots toward the east and toward the west, and point-blank gunshots. The ship's mast experiment is mentioned as part of an argument in favor of the earth's motion, for he claims correctly that the ball falls at the foot of the mast on a moving ship. However, this is an argument which he wants to refute. His criticism is the following.[39]

First, he says, "it is impossible to make a test that is certain,"[40] because waves are likely to rock the ship so as to prevent the dropped ball from falling along the true vertical. Second, if the ship is moved by oars, then its motion is not uniform but subject to jerking. Third, if the ship is a sailboat, then the mast is being slightly bent forward by the wind, and so when the ball is dropped the point of release is forward of the foot of the mast, and hence the ball's backward lag during free fall just compensates for the forward release, and that's why the ball ends at the foot of the mast. Finally, Colombe makes an obscure point which I interpret as follows: the ball should be dropped from a very great height in order for the backward distance from the foot of the mast to be noticeable.

In summary, Colombe is saying that the performance of the experiment requires great care in order to neutralize the extraneous effects of the rocking of the boat, of the nonuniform motion forward, of the forward bending of the mast due to the wind, and of the mast being insufficiently tall. Implicitly he is insinuating that those experimenters who have obtained the pro-Copernican result may have done so because of insufficient care.

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[36] Koyré 1978, 150.

[37] Koyré 1978, 153.

[38] Galilei 1890–1909, 3: 251–290.

[39] Galilei 1890–1909, 3: 259–260.

[40] In Galilei 1890–1909, 3: 259.

Galileo happened to annotate his own copy of Colombe's essay, and his comment on this passage is especially revealing.[41] He says that as long as the experiment was thought to support Aristotle, it was regarded as proper, but "now that it has been discovered to go against him, it has immediately become improper."[42] It is as if, he adds, one were to call a witness in a lawsuit, and when he testifies against you, you sue him for lying. These comments suggest that Galileo had made or witnessed the experiment, or read reports of it, and that his views on the subject underwent some evolution; thus they reinforce these conclusions which we have drawn earlier.

A few years after Colombe, Francesco Ingoli compiled his own compendium of anti-Copernican arguments, in an essay entitled "Disputation on the Location and Rest of the Earth against the System of Copernicus" (1616). However, it is puzzling that Ingoli does not seem to mention the ship experiment at all. This is especially surprising in light of the fact that Galileo's "Reply to Ingoli" (1624) does discuss the experiment at some length. In fact, as we have seen, the Galilean criticism of the argument from the ship's mast experiment is in some respects clearer there than in the *Dialogue,* that is insofar as the "Reply to Ingoli" leaves no doubt that Galileo is criticizing its factual premise both experimentally and theoretically.

In 1630, Philip van Lansbergen published in the Netherlands a book entitled *Commentationes in motum terrae diurnum et annum,* supporting Copernicanism, praising Galileo, and rejecting biblical literalism.[43] It contains a criticism of the argument from the ship experiment.[44] However, I have not consulted it.

The experiment was also discussed by Libert Froidmont in his *Ant-Aristarchus, sive orbis Terrae immobilis* (1631), and again in his *Vesta, sive Ant-Aristarchi vindex* (1634). Froidmont relates the performance of the experiment by a French engineer named Gallé, who made the experiment in the Adriatic Sea, probably before 1628, and obtained an Aristotelian result. According to Froidmont, "from the top of the main mast of a Venetian galley he dropped a lead ball; the ball did not fall to the foot of the mast, but deviated toward the stern, thus providing for Ptolemy's followers an apparent verification of their doctrine."[45]

After the publication of Galileo's *Dialogue* in 1632, despite the power of its analysis of the ship experiment argument, the controversy continued for a while, but became more focused. One author who was not convinced was Antonio Rocco, who in 1633 wrote a wide ranging rebuttal of the *Dialogue*, in a book entitled *Philosophical Exercises.* The so-called seventh exercise discusses the arguments for the earth's rest and the criticism of those arguments.[46]

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[41] In Galilei 1890–1909, 3: 259, lines 34–38.

[42] In Galilei 1890–1909, 3: 259, lines 34–36.

[43] See Lerner 2004, p. 53, n. 102; McColley 1938, p. 179, n. 84; Montucla 1799–1802, 2: 298; Motta 1993b, 27.

[44] Lansbergen 1630, 10, according to Besomi and Helbing 1998, 2: 426.

[45] Quoted from Koyré 1973, p. 327, n. 2, on which my account in this paragraph relies. Koyré's account, in turn, relies on information found in De Waard and Beaulieu 1932–1988, 2: 74.

[46] Rocco 1633; reprinted in Galilei 1890–1909, 7: 663–691.

Galileo's criticism of the ship experiment argument is quoted and criticized at length.[47] Rocco objects to his criticism of the analogy between the rotating earth and the moving ship, although it is unclear that he wants to defend that analogy, but seems primarily to want to fault Galileo's critical counter-argument. Rocco also objects to his theoretical counter-argument predicting and explaining the result of the experiment, by questioning the principles of conservation and composition of motion and the thesis about the cause of projectile motion.

Furthermore, Rocco objects to Galileo's experimental report with these words: "I do not believe that the rock falling from the top of the mast of the moving ship goes directly to the foot of the mast. If I were to see it, I would strive to find some cause other that the earth's rotation. This could be the immense velocity of the rock, which is not distinctly perceived in such a short distance by our sensitive faculty."[48] He does not say why he does not believe this phenomenon to take place. His hypothetical explanation of the phenomenon (on the assumption it takes place) is somewhat obscure, but perhaps he means something similar to one of the points raised by Colombe, namely that most masts are too short, and the speed of fall is too fast, for us to be able to perceive the backward lag of the fallen body. Rocco's denial of an alternative explanation shows the depth of his misunderstanding of the situation, for Galileo did not try to explain the experimental fact in terms of the earth's rotation.[49] To think so betrays a confusion with respect to the difference between the tower argument and the ship's mast argument, and between the critical undermining of an anti-Copernican argument and the assertion of a pro-Copernican constructive argument; and in this regard Rocco was anticipating the confusions approached by the illustrious twentieth century scholars mentioned earlier.

Finally, Rocco explicitly charges Galileo with the inconsistency of both criticizing the analogy between the moving ship and the moving earth and presupposing it in his counter-argument.[50] Again, here, Rocco fails to understand that Galileo's counter-argument is meant to be a disproof of a geostatic argument rather than a proof of the earth's motion.

In the same year that Rocco published his book, Scipione Chiaramonti published a defense of his views from the criticism in Galileo's *Dialogue.* The discussion is even more comprehensive than Rocco's. In regard to the ship experiment, Chiaramonti claims that it has been made by one Giovanni Cotunio, a professor at the University of Padua, and that it has yielded the Aristotelian result that when the ship is moving the rock falls backwards from the foot of the mast.[51] Moreover, Chiaramonti was not the only author to appeal to Cotunio's experiment and its anti-Copernican result. Cotunio's experiment was also mentioned for the same purpose in a 1638 book by

[47] Rocco 1633, in Galilei 1890–1909, 7: 672–677.

[48] In Galilei 1890–1909, 7: 677, lines 9–14.

[49] See Galilei 1890–1909, 7: 677, n. 43.

[50] In Galilei 1890–1909, 7: 675, lines 14–20.

[51] Chiaramonti 1633, 339.

Giovanni Barenghi critical of Galileo's *Dialogue*.[52] It is unclear what we are to make of such a report, and the topic would deserve more study.

Another relevant discussion is found in Jean-Baptiste Morin's *Responsio pro Telluris quiete* of 1634. According to the account given by Koyré,[53] Morin claims to have made the experiment several times on a boat on the Seine River and to have confirmed the Galilean result, adding the qualification "the first time with surprise, the second time with admiration, the third time with laughter."[54] What Morin means is the following. The man dropping the ball from the mast's top on a moving ship is actually giving his motion to the ball, which is thus projected forward and not merely dropped; that is why the ball does not fall behind. The more proper way of performing the experiment, according to Morin, is to drop the ball from a bridge under which the river boat is passing at the exact time when the top of the mast goes by the person's hand holding the ball; for in this case the ball is left behind and falls toward the stern. Koyré ends with a comment that is easily shared: "Thus, with an argument copied literally from Bruno, but which he obviously does not understand, Morin ends up confirming his geocentric faith."[55]

A few years after Morin, Giovanni Battista Baliani in Genoa performed the experiment, obtaining Galileo's results. His report is important because it contains some quantitative considerations, dispelling some of the qualms that had worried Aristotelians like Colombe and Rocco. His report is contained in a letter to Galileo dated September 16, 1639 and reads as follows:

> I want to share with you an experiment which I was able to do last Sunday, while on board a galley. I had a sailor climb to the top of the mast and from there drop a musket bullet several times while the galley was going fast. This high speed was due to the fact that the crew was rowing with as much force as possible and there was a moderate wind that helped us along considerably. Each time the ball fell to the foot of the mast, without lagging behind at all, with the astonishment of all who were present. The mast was more than 40 cubits tall since the galley was a large one, namely the flagship of our fleet. Consequently, the ball must have been falling through the air for more than three seconds, during which time the galley certainly traveled at least 16 cubits.[56]

The most well-publicized performance of the ship experiment was probably that of Pierre Gassendi. Here is a vivid description of it:

> Monsieur Gassendi having always been very interested to try to confirm the truth of philosophical speculations by experiment, and happening to be in Marseille with the Comte d'Allais in the year [1640], observed, on a galley which put to sea on the special orders of this prince, more illustrious by his love and knowledge of good things than by the elevation of his birth, that a stone dropped from the very top of the mast while the galley was proceeding with the greatest strength and speed possible, fell at a point no different from

[52] Barenghi 1638, 183.

[53] Koyré 1973, pp. 327–328, n. 1. Here my account relies heavily on Koyré, so much so that I am basically paraphrasing his account. Koyré's account, in turn, relies on material found in De Waard and Beaulieu 1932–1988, 3: 359ff.

[54] Quoted in Koyré 1973, p. 327, n. 2, from Morin 1634.

[55] Koyré 1973, p. 328, n. 2.

[56] Baliani to Galileo, 16 September 1639, in Galilei 1890–1909, 18: 102–103.

> where it would have fallen had the galley been stopped and immobile; so that whether the galley was moving or not the stone always fell alongside the mast to its foot and on the same side. This experiment, performed in the presence of the Comte d'Allais and of a large number of other people who were there, seems to many people who have never seen it to be somewhat paradoxical; and for this reason Monsieur Gassendi wrote a treatise *De motu impresso a motore translato,* which we saw that same year in the form of a letter written to Monsieur du Puy.[57]

In fact, in 1640, Gassendi undertook a series of experiments designed to test not only Galileo's claim about the moving ship, but also his laws of falling bodies and of motion in general. Two years later, in *De motu impresso a motore translato,* Gassendi published his experimental results and theoretical interpretations, confirming those of Galileo. These issues were not immediately resolved but engendered a controversy over the Galilean laws of motion that continued for several more years.[58] However, by 1648 the dispute was essentially resolved and Galileo emerged vindicated. In particular, after Gassendi's performance of the experiment and after this general discussion, the ship's mast experiment seems to receive little attention, which is perhaps a sign that its outcome became settled and established.

5.7 Physics of Ship Experiment

It is now time to say a few words about the physics of the ship experiment, to round out the logical, methodological, and historical accounts elaborated so far. It will turn out, however, that this physics relates in part to the history insofar as such physics was discovered or elaborated in the subsequent history of the ship's mast experiment.

The ship's mast experiment is actually very difficult to perform for a reason hinted at by Colombe. That is, with respect to horizontal motion forward, it is easy to create the two different conditions under which the experiment is to be performed, namely a ship moving forward propelled by oars or wind, and a ship not going anywhere but anchored in a harbor. However, even when the ship is anchored in a harbor, and the sea and wind are calm, the ship almost always undergoes some slight rocking motion due to the fact that the surface of the water is never completely still and that objects or people moving within the ship slightly alter its balance or equilibrium. Such slight rocking motion gets magnified at the top of the mast, so that its topmost point is constantly moving back and forth and sideways by a noticeable amount. Thus, even when the ship is standing anchored at a pier, the experiment is likely to yield the result that the rock does not fall at the foot of the mast but at some distance from

[57] Quoted in Koyré 1978, p. 225, n. 149, from the Preface to *Recueil de lettres des sieurs Morin, De la Roche, De Nevre et Gassend, et suite de l'apologie du sieur Gassend, touchant la question* De motu impresso a motore translato (Paris, 1650). See Koyré 1966, p. 225, n. 1; and Koyré 1973, 329. The original passage states that the year of the experiment was 1641, but I have corrected it to 1640, following Koyré 1973, p. 329, n. 1.

[58] See Galluzzi 1993, 2000.

it in various directions. Such a rocking effect will also be present when the ship is moving forward.

This consideration suggests that the experiment must be designed and performed with great quantitative precision, much greater than that suggested even by Baliani, which nevertheless was the most precise of those discussed above. The same suggestion is implied by other considerations, for example if one wants to take into account the slight forward bending of the mast due to the wind (mentioned by Colombe), or the resistance of the air to the forward horizontal motion of the falling rock (mentioned by Galileo).

Now, as one starts to think more precisely about the ship's mast experiment, another aspect of the situation needs to be considered. That is, given the spherical shape of the earth, on a ship moving forward, the top of the mast moves at a speed slightly greater than that of the foot of the mast, by an amount which is a function of the earth's radius and the height of the mast. It follows that the dropped ball, by conserving such a slightly greater speed, will *advance* slightly forward of the foot of the mast. In short, on a moving ship the dropped ball will be deflected *forward* by a small but calculable amount.

This consideration was not explicitly elaborated by Galileo, but it was implicitly hinted at by him. These hints can be found in at least three passages in his *Dialogue.* At the end of the critique of the objection from east-west gunshots and beginning of the vertical gunshots objection, the character Sagredo expresses the point that the top of a ship's mast moves more that its foot.[59] At the end of the critique of the objection from north-south gunshots, Sagredo mentions that in such shots the gun and the target are moving at different speeds, and so there should be some deviation; but he considers it to be so small as to be undetectable.[60] Finally, at the end of the critique of the objection based on the fall of a body from the moon to the earth, it is the character Salviati who states clearly that, far from lagging behind, such a body should advance forward.[61]

Such hints were followed up by Giambattista Guglielmini at the end of the eighteenth century. In 1789–1792 Guglielmini[62] studied the eastward deflection of falling bodies when dropped from a tower on land. He computed the amount of such a deflection, devised some experiments to detect it, and confirmed the prediction. The predicted deviation is of course very small; for a height of about 160 feet used by Guglielmini, the deviation was calculated to be about 2/3 of an inch. Thus all kinds of precautions had to be taken and devised, involving such things as the mechanism of release; stopping microscopic pendular vibrations before release; minimizing disturbances from outdoor traffic, winds, and temperature; averaging out the spread of deflections from one trial to another; and so on.

Guglielmini's idea had considerable repercussions. In 1796, Giovanni Antonio Tadini, using a more precise analysis and more sophisticated mathematical methods

[59] Galilei 1890–1909, 7: 199; Galilei 1967, 173–174.

[60] Galilei 1890–1909, 7: 205; Galilei 1967, 179–180.

[61] Galilei 1890–1909, 7: 259–260; Galilei 1967, 232–234.

[62] See Guglielmini (1789, 1792, 1994); Borgato 1996; Borgato and Fiocca 1994.

than Guglielmini had employed, predicted that the easterly deviation of falling bodies has a value of 2/3 that calculated by Guglielmini, and that the southerly deviation predicted by some should be nought in a vacuum and negligible in air. These theoretical results were later confirmed independently by Pierre S. Laplace's and Carl F. Gauss's own theoretical calculations, using different and more general and powerful principles and methods than Tadini.[63] In 1804, the eastward deviation was experimentally confirmed in Germany by Johann Benzenberg.[64] As late as the first decade of the twentieth century the phenomenon continued to attract some attention, and American physicist E. H. Hall[65] gave an updated sophisticated experimental confirmation of the predicted eastward deviation. However, the challenge in these latter experiments was not to prove or confirm something which was by then unquestioned, but rather to measure with a very high level of precision; there were even mathematical challenges, insofar as physicists now were trying to take into account such things as the fact that the earth's surface is not perfectly spherical, air resistance, air turbulence caused by the falling motion, the nonhomogeneous density of the balls being dropped, and even the effect of lunar gravitational attraction.

One final point is worth mentioning here. Recall that the experiment made by Galileo in the presence of Stelluti in Piediluco Lake was not exactly the ship's mast experiment, but rather that of projecting a body vertically upwards and allowing it to fall back downwards. The vertical projection experiment is actually easier to perform, for several reasons, for example: one does not need a boat with a mast; furthermore, the vertical projection approximately doubles the time elapsed while the body is not attached to the boat, and would presumably double the backward lag, if that should occur.

However, from the point of view of modern physics, the vertical projection experiment involves an additional complication. As it was discovered in the eighteenth century by Jean D'Alembert, the body projected vertically upwards will not return exactly to the place of ejection, but will lag behind (toward the west). In 1771, D'Alembert showed that a body projected vertically upwards deviates westward and calculated the distance from the launching point at which it will fall back, under the simplifying assumption of a location at the earth's equator in a vacuum. He argued that because the projectile is moving in the earth's gravitational field, it will move along an elliptical path relative to the center of the earth; by Kepler's second law, the sector of the ellipse swept over by the line joining the body and the earth's center is equivalent in area to the sector of the circle swept over during the same time by the launching point; thus, by the time the projectile falls back to the earth's surface, the angular distance it has traveled is less than that traveled by the launching point; that is to say, the projectile falls back some distance to the west of that point.[66]

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[63] Tadini (1796a, b, c, 1815); Gauss 1803; Laplace (1803, 1805); Bertoloni Meli 1992; Borgato 1996.

[64] Benzenberg (1804, 1845). See Bertoloni Meli 1992, 433–447; Borgato 1996, 250–256.

[65] Hall (1903, 1904, 1910); cf. Borgato 1996.

[66] Borgato 1996, 222, 243; D'Alembert (1771, 1780). See also Simpson 1743.

These details of physics should not be interpreted as invalidating Galileo's criticism of the ship argument, but rather as suggesting some qualifications and refinements to that criticism. That is, even from the point of view of modern physics, the Galilean claims remain correct as a first approximation: that on a moving ship, the dropped ball falls at the foot of the mast, and the body projected vertically upwards returns to the point of projection on the ship; and similarly, on a rotating earth the ball falls at the foot of the tower and the vertically projected body returns to the point of ejection. However, if one wants to be more precise, then the above mentioned refinements should be added.

5.8 Conclusion

A common argument against the Copernican theory of the earth's motion was the objection based on the ship's mast experiment, claiming that on a moving ship a body dropped from the top of the mast lags behind while falling and lands astern from the foot of the mast. Galileo defended Copernicanism from this objection in three ways: with a critical counter-argument questioning the analogy between the moving ship and the rotating earth; with a theoretical counter-argument concluding that on a moving ship falling bodies do not lag behind; and with an actual experiment refuting the anti-Copernican empirical claim about the moving ship.

This Galilean defense of Copernicus can be, and has been, criticized also in three ways: by claiming that he did not actually make such an experiment; by claiming that his theoretical counter-argument assumes principles (such as the mechanical relativity of motion) which would not have been granted by his opponents, and so begs the question; and by claiming that his criticism of the earth-ship analogy is inconsistent with his other criticism. Such anti-Galilean criticisms can be gleaned from the work of Koyré and Feyerabend.

I have attempted to defend Galileo as follows. I have argued that he actually made the ship experiment. I have also argued that his theoretical counter-argument is neither question-begging nor sophistical, but rather a series of plausible inferences from observational claims. Furthermore, I have argued that his criticism of the analogy is not inconsistent with his other counter-arguments because they are all meant to disprove this anti-Copernican argument and not prove the earth's motion. Finally, from the point of view of modern physics, the phenomenon does not happen exactly the way Galileo describes it; however, the deviations are so small that his description may be deemed correct as a first approximation, and the more exact description can be, and was historically, arrived at in part by following his own hints.

I also claim that my defense of Galileo is analogous to his defense of Copernicus. Part of the analogy is that in both cases there is a factual issue, in one case whether or not on a moving ship falling bodies lag behind, in the other case whether or not Galileo made this experiment. Another similarity is that I have also advanced an interpretive argument claiming that the Koyré and Feyerabend type of criticism misunderstands Galileo in various ways. More generally, I have tried to emphasize

critical reasoning in the same manner that Galileo did, that is with open-mindedness and fair-mindedness toward opposing views.[67]

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Part II
Comparisons and Contrasts

Chapter 6
Socrates, Galileo, and Marx as Critical Thinkers

Abstract One approach to teaching critical thinking and theorizing about it is to study great exemplars like Socrates, Galileo, and Marx, and to undertake a critical comparison and contrast of them. In this essay, I discuss this approach by reporting on a university course I have taught for more than 30 years and by formulating some conclusions and problems suggested by this kind of material. In the process, the critical-thinking practice of these three exemplars will be related to several theories of critical thinking that have been widely discussed by scholars.

6.1 Introduction

The aim of this essay is to describe an Introduction to Philosophy course which I taught for more than thirty years, and to discuss a number of pedagogical and conceptual problems it suggests. Philosophy is there presented not as a subject for its own sake or a discipline in its own right, but as a way of thinking that can be applied, and has been applied throughout the ages, to the most diverse subjects and problems. Three examples of this conception of philosophy are examined: Socrates, Galileo, and Marx. I focus on their thinking, but there is some discussion of their life, of historical conditions, of their relevance today, and of their similarities and differences.

My motivation for devising this course has been the following. Partly I wanted to appeal to a student audience that has little patience for metaphysical speculations and conceptual abstractions, but rather wants ideas that are more or less practically relevant; however, I did not want to pursue practical relevance by merely following the latest trends and fashions. Partly, I wanted to combine the historical and the systematic or problem-oriented approaches to philosophy, but I found a survey of the history of philosophy uninspiring, unenlightening, and pretentious; on the other hand, a purely theoretical and abstract systematic approach struck me as relatively empty and devoid of content. I also wanted to avoid one-sidedness with respect to branches of philosophy, and thus I felt that a good introduction should involve at least two important and importantly different branches. Another motivation was my

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Argumentation Library 40,
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desire to forge a judicious balance between innovation and conservation, between revolution and tradition; from this point of view I feel I am exploring the tradition of revolutionary thought, and I would be prepared to argue that both self-styled revolutionaries and self-styled reactionaries neglect it at their peril.[1] Moreover, I should mention that I was motivated by my interest in reasoning and argument, and so I wanted to be in touch with, and wanted to expose my students to, classic examples of argumentative prose. Finally, I have long been intrigued by the problem of the relationship between theory and practice, or between thought and action, and I believe these three individuals represent instructive syntheses between the two domains.

The course lasts about fifteen weeks, and it is divided into three approximately equal parts. I prefer three classes a week of 50 min each, and this is the schedule I have used in in the discussion below; however, the material can easily be adapted to a schedule with two classes a week of 75 min each. There is a test on Socrates at the end of the first segment, another one on Galileo at the end of the second part, and a comprehensive two-hour final examination at the end of the term. As might be expected, there is no single textbook available, and so I use three source-books, which are supplemented by my own analysis and lectures.

The source-books contain primarily selections from the original, classical writers, but also some commentary. In the case of Socrates the choice is relatively easy, and we read the Platonic dialogues dealing directly with the trial and death of Socrates; there are several editions that contain the *Apology*, the *Crito*, and one or more of the following: *Euthyphro*, *Phaedo*, and *Meno*. The most useful source-book is perhaps the one entitled *The Last Days of Socrates*,[2] which contains *Apology*, *Crito*, *Euthyphro*, and *Phaedo*, and to which I shall be referring in what follows.

In the case of Galileo, for a long time there was no comparable book. At first I used simply Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*, which is available in an inexpensive paperback edition,[3] and which has the advantage of containing detailed and informative discussions of the main philosophical and scientific issues that are pertinent; its main drawbacks are the difficulty of the text, and its lack of significant biographical and historical information about the Galileo affair. Then I switched to a paperback entitled *Discoveries and Opinions of Galileo*,[4] edited, translated, annotated, and with introductions by Stillman Drake, which has the advantage of containing a wealth of biographical and historical information, and selections from Galileo's writings that are relatively more accessible; unfortunately the book focuses on a theme different from what I think is appropriate in this course, and so it does not even contain any material relating to the Inquisition trial of 1633. Thus, I was led to myself create a more appropriate and useful work,

[1] For some arguments along these lines, see Finocchiaro (1988a, b, 1999a).

[2] Plato 1969.

[3] Galilei 1967.

[4] Drake 1957.

namely a collection of the essential writings and documents pertaining to the Galileo affair, translated into English, and properly arranged, annotated, and introduced.[5]

For the third part of the course, most of the time I covered Karl Marx, and so I shall discuss him in this essay. I like to focus on *The Communist Manifesto*, and I have used one of the editions that contain supplementary material like notes, introductions, and short selections from other Marxian writings that relate directly to the content of the *Manifesto*. Eventually, I settled on a superb edition published in the Norton Critical Editions series and edited by a philosopher.[6] However, I should also report during certain periods this third part of the course used other examples instead, such as Simone de Beauvoir's *The Second Sex*, or *The Federalist Papers* by American Founding Fathers Alexander Hamilton, James Madison, and John Jay.[7]

The course begins with a necessarily general and abstract lecture on the conception of philosophy as it will be presented in the course. However, the lecture is then essentially repeated at the end of each of the three parts, and on those occasions I also provide a more concrete discussion by giving illustrations and applications of those general and abstract ideas in terms of the thinker whose study has just been completed. Finally, the same topic is discussed again at the very end, in the context of a comparison and contrast among the three thinkers studied.

6.2 A Conception of Philosophy

At this point it will be useful for me to highlight that initial lecture. Here it should be noted that the terminology I am using has my own semi-technical meaning, which I shall explain, but that my definitions are intended to represent merely one of the standard meanings of these terms. The nature of philosophy is explained in terms of six notions: content-freedom, rationality, judiciousness, practicality, universality, and critical-constructiveness.

I begin by explaining that when I say that philosophy is *content-free* I mean that philosophy is characterized not by the content or subject it studies, but by the approach or procedure it uses; that is, not by what it studies, but by how it studies it. I elaborate this by contrasting philosophy so conceived to other disciplines familiar to students, such as mathematics, biology, history, psychology, political science, and so on. I also take this opportunity to discuss briefly that, in fact, Socrates dealt primarily with questions of good and evil, the meaning of life, the nature of wisdom, and the like; whereas Galileo dealt primarily with topics like the structure of the physical universe, the proper methods to follow in the search for truth, and the nature of knowledge; and Marx treated chiefly social, political, economic, and historical questions like the

[5] Finocchiaro (1989a, 2014b). The difference between these two books is that the later one is a simplified and shorter version of the earlier one.

[6] Marx 1988.

[7] Beauvoir 1972; Rossiter 1961.

nature of capitalism, the origin of wealth and profit, the necessity and desirability of revolution, of socialism, and of communism.

Since philosophy is not defined by its content, one must characterize its approach. And so the other above-mentioned notions are meant to describe it. In saying that the philosophical approach is *rational*, or more simply that philosophy is rational, I mean that it emphasizes reasoning, i.e., that regardless of what it is studying it tries to use reasoning as much as possible. At this point I find the need to make at least one brief remark about what reasoning is, but the main thing I emphasize is that I am talking about *emphasis* on reasoning, and not mere use; in fact, I add that reasoning is used in all disciplines and in everyday life, but that what distinguishes the philosopher is the readiness and willingness always to engage in reasoning. These notions of emphasis and of reasoning need, of course, more discussion, but that cannot be done either here, or there in that initial lecture.

Besides being rational, the philosophical approach is characterized as *judicious*.[8] Here, judiciousness means the avoidance of one-sidedness and of extremes. Therefore, regardless of what one is studying, a philosopher tries or should try to avoid being one-sided or taking an extreme position. I stress that this is distinct from reasoning, the difference being that between giving or assessing reasons for claims on the one hand, and taking into account all sides of an issue and being moderate on the other.

Next, I explain what I mean in saying that philosophy is *practical* in its approach. That is, philosophers do not study a given subject for its own sake, merely as a mental exercise or intellectual game; instead, they are always concerned with its connection with practical life, always willing and ready to try to relate the most abstruse or abstract ideas to the solution of practical problems and the improvement of everyday life. This does not mean that there will always be a connection, or that the connection will always be direct, but rather that, if there is no such connection, the philosopher cannot be spending *all* his life studying that topic; it also means that normally even the study of completely abstract topics has a practical origin, in the sense of being traceable to the attempt to improve practical life. In other words, philosophers are supposed to have a practical attitude or orientation, in the sense that their thinking (regardless of how abstract it may get) has a motivation rooted in their individual experience or in social conditions.[9]

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[8] The importance of judiciousness in philosophy has recently been discussed by Charles Hartshorne (1987). The book contains useful discussions of the balancing of opposite extremes in technical metaphysics and epistemology, as well as in everyday life, and therefore it may be consulted with some profit in this context; it should be added, however, that most of the book (seven out of ten chapters) does not deal directly with the topic, and the part that does (Chapters 1–3) is often disappointing insofar as the element of moderation is difficult to discern in the illustrations that are advanced. A related notion of "judgment" has also been stressed by Matthew Lipman both in the context of philosophical education and of critical thinking, and his views seem very close to those independently elaborated here. See, for example, Lipman (1988a, b); Lipman et al. 1979.

[9] It might be objected that here I am illegitimately equating the practicality of philosophical thinking and philosophers' practical motivation for their thinking. However, all I am saying is that one factor contributing to the practicality of philosophical thinking is philosophers' practical motivation; it is

Fourth, the philosophical approach is *universal*, in the sense that the topics studied are supposed to be relevant to all human beings, and not just to a few, to some, or to a particular group or class or nation. This is a feature of the approach used rather than of the topic treated because such universal relevance is something that may have to be explained or elaborated, rather than something that is immediately apparent. Nevertheless, a philosopher is normally ready and willing to elaborate on this universal relevance.

Finally, there is something I awkwardly call *critical-constructiveness*, to underscore the fact that, though philosophers are critics, they are not merely destructive or negative critics. They are not merely against, but also for, something. The targets of their criticism can be either ideas, beliefs, or doctrines on the one hand, or historical conditions or social institutions or individual actions on the other. Moreover, the constructive aspect of their criticism may be relatively weak or small, but there must be a germ of it, otherwise we would not have the kind of philosophy we are discussing here.

In a single sentence, these points can be summarized by saying that, here, philosophy is being conceived as the study of any subject matter whatsoever, as long as the approach followed is rational, judicious, practical-oriented, universal, and critical-constructive, in the senses specified.

6.3 Socrates

I begin the study of Socrates with a lecture on the political and cultural background to his trial; for the former I sketch such developments as the Persian Wars, the Periclean Age, the Peloponnesian War, the Alcibiades affair, the Spartan victory, the Thirty Tyrants and Critias's involvement, the democratic revolution, and the amnesty; for cultural background I mention the pre-Socratics and the sophists. This lecture is meant to complement the introduction which most editions of Plato's early Socratic dialogues have, for I do not know of any that includes as much political background as I think should be included. I should like to spend more time (for example, two days) on this background, and to include more about social and economic conditions; however, I limit myself to one day due to my ignorance of details, to pressure of time, and to the lack of any readily discernible connection between these conditions and the Socratic ideas.

We then spend three or four days on the *Apology*. Its beginning offers an opportunity to mention a few more biographical details. In order to encourage students to do the reading assignments regularly and to develop good study habits (such as making outlines of the readings), we usually spend the first two or three days summarizing the details of the story and the text, and then we spend one or two days discussing

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not, however, the only factor, another one being that the content of their thinking has consequences that make a difference to practical life.

the ethical, general, and conceptual issues and problems raised. I cover the summarization phase by assigning a number of study questions and by structuring class lectures and discussions around their answers. The questions are as follows: How does Socrates justify himself concerning the "old accusations" popularized by people like Aristophanes? How does he justify himself concerning the formal charges for which he is standing trial? How does he justify himself concerning the way he has lived his life in general? What penalty does he propose for his alleged crimes, and why? How does he justify his behavior at the trial?

After students have had an opportunity to master these relatively factual details, I ask them to review the *Apology*, paying special attention to those passages that discuss such topics as the nature of wisdom and ignorance, the intentional corruption of other persons and the question of the possibility of intentional evil in general, the fear of death and the question of the afterlife, and the nature of political involvement and relationship between politics and morality. In each case, I try to have them think about what the problem is, what the Socratic position is, what are his reasons for taking this position, how adequate his reasons are, and what position they would take and why. Needless to say, such issues are open-ended in many ways; in particular they could be discussed over a much longer period of time. However, one or two days is all the attention they get in this course.

We next go on to the *Crito*, dealing first with the details of the story, and then with the moral issues inherent in it. To help students learn the former, I give them the following study questions: What reasons does Crito advance to convince Socrates to escape from prison? How does Socrates criticize these reasons? Why does he think that it would be morally wrong for him to escape? That is, how many different arguments does he give to show this, and what are the details of each of these lines of argument? I suppose I should mention that my reason for the last question here is that I find three different arguments why Socrates thinks escape would be morally wrong: one involves the principle that it is wrong to break a promise; another is the one that involves the analogy between breaking the law and harming one's parents; and his third one is based on the reason that by escaping Socrates would be losing his integrity and violating the principles he had lived by and expressed at the trial.

After half a period or a period on these easier and more basic details, we spend another half or full period on deeper issues. I ask students to read the dialogue also with the following questions in mind: How adequate is Socrates' criticism of Crito's reasons? How adequate are Socrates' own arguments for not escaping? What real harm would have resulted from his escape? What are the various meanings and the implications of the principle that it is wrong to return evil for evil? Is Socrates correctly applying this principle to his own situation? Again, here I should mention that the point of the last question is that I think Socrates's application is incorrect, in the sense that there is an equivocation: the principle could be taken to mean that any retaliation in response to an evil is itself evil, which is rather implausible; or it could mean that evil retaliation in response to an evil ought to be avoided, which is unobjectionable; now, Socrates seems to begin with the latter, but then switches to the former; that is, the real issue is what type of retaliation is evil and what type is not, and, although Socrates does have the three above-mentioned arguments to try to

show that escape would be an evil response, he does need these other arguments to accomplish that; it follows that the famous principle that two wrongs do not make a right is not providing Socrates with any argument why he should stay in prison.

Finally, notice that, in a sense, the second group of questions about the *Crito* involves questions of evaluation, whereas the former set involves questions of textual interpretation.

The *Euthyphro* comes next in the course, despite the fact that this dialogue is chronologically prior to the *Apology* and *Crito*; moreover, it provides a much better example of both Socratic irony and Socratic method than they do, and so it could be used to provide some of the foundation for understanding them; finally, the position Socrates takes on the topic of the relationship between piety or holiness and divine approval is sufficiently subversive from a religious point of view that it helps us to understand why the formal charges mentioned atheism or heresy among other things. Despite all this, there is a pedagogical reason for placing the *Euthyphro* here in the sequence, namely the apparent negativity and inconclusiveness of the discussion and the considerable difficulty of the text. In regard to the latter, especially the short two-page central passage where Socrates argues that divine approval is not what makes something holy or pious, I explicitly warn students that this is one of the most difficult passages ever written, by any standard.

Unlike the previous two, this dialogue does not lend itself to being discussed in two phases, a first one focusing on summarization or interpretation, and a second one focusing on ethical analysis or logical evaluation. Therefore, in each of the three or four periods devoted to it, I summarize, analyze, evaluate, and derive general lessons from the assigned passage. I structure the main discussion as a series of five distinct definitions or interpretations of piety or holiness, each of which is refuted by Socrates, in such a way that no positive answer to the initial question is given in the text. As always, students are given study questions, and here these ask them to state each of the five definitions, and to summarize each of the five Socratic objections. This main sequence, which takes two or three days, is preceded for part of the period on the first day by a discussion of some preliminary topics, namely Socratic irony, Socratic method, and the story's background leading to the question about the nature of piety or holiness, and involving a character named Euthyphro bringing a legal charge of murder against his own father; furthermore, that main sequence is followed by an extrapolation on my part about the specific answer Socrates would probably give to the question of piety or holiness.

The climax of the main sequence is Socrates's criticism of the interpretation that pious or holy things are those which receive divine approval (*Euthyphro* 10A–11B). The criticism starts with his question whether things are holy because the gods love them, or whether the gods love them because these things are holy; and it ends with the argument that being holy and being loved by the gods cannot be identical because there is something which can be asserted of the first quality which cannot be asserted of the second, namely that the quality of being holy can produce divine love, but the quality of being loved by the gods cannot. All this is quite a mouthful, and I keep on repeating that it is extremely difficult, that I do not expect students on their own to get it out of the text, and that I expect that many will never understand it.

Nevertheless, to help students in tackling it, when this passage is assigned, I give them some study questions that center on the following two abstract principles: (1) if a statement of the form "A because B" is true, then the reverse statement "B because A" is false; and (2) two entities are not identical if there is a property which one of them has but the other lacks. I also tell them to try to understand these principles in general by trying to rephrase them in various ways, and to try to feel the plausibility of each by thinking of instances of situations where it clearly applies. Later, after I have discussed this in class, and after I have explained how these two principles are used, though never explicitly stated, in Socrates' criticism, I ask them to try to think of counterexamples, the existence of which would tend to invalidate these principles; this opens the way for criticism of Socrates's criticism.

The conclusion of my discussion of the *Euthyphro* is taken up by my attempt to try to extract a positive interpretation of piety which can be derived by following Socrates's own line of reasoning, which is never completed in the dialogue, supposedly because Euthyphro is unwilling to carry on the hard thinking required. Here I argue that a pious action is one designed to improve human character, since character is that part of a human being whose improvement can come about only through personal struggle and effort, and hence this is something that even an all-powerful God cannot bring about, and hence it is the most likely candidate for the highest duty human beings have toward God.[10]

The next and last dialogue I usually cover is the *Phaedo*. Despite its being about as long as all three of the above-mentioned works, I spend only about three days on it, explaining to the class that I do so partly for lack of time, partly because of its more Platonic and less Socratic tenor, and partly because I find its arguments more difficult to understand, and less cogent where I understand them. The topics covered center around questions like the following: What is Socrates's conception of death at the beginning of the *Phaedo*? What is his attitude toward death so conceived? What are his reasons, and how adequate are his reasons? What is the main line of reasoning in Socrates's argument from reciprocal processes, and how adequate is this argument? What is meant by the "doctrine of recollection," how is it used to support the immortality of the soul, how is it itself supported, and how adequate is this support?

In connection with this last question I make only a few very brief remarks about this justification of the doctrine of recollection, but spend the greater part of a period on the example in Plato's *Meno* about how an uneducated boy can be easily taught the solution to the geometrical problem of doubling the size of a square. I do this by getting a student volunteer to play the role of Meno, and by my acting the part of Socrates myself. After going through the details of this procedure, I explain and then criticize how it could be used to support the doctrine of recollection; but I also explain how it is best interpreted as providing an illustration of the Socratic method, especially of its maieutic element, which none of the other discussions had yet exemplified.

[10] I owe this interpretation to Vlastos 1971a, especially p. 14.

Other parts of the *Phaedo* upon which I focus are: the problem of "misology" and the need for an "art of logic," which Socrates discusses at about the midway point in the dialogue, and which connects very well with my own theme of the rationality of philosophy; Socrates's sketch of his intellectual autobiography; and the qualification which at the end of the dialogue Socrates makes about the preceding discussion, a qualification which suggests that some things are well worth believing, even when the evidence is inconclusive (114B–115C).

If time allows, I then like to spend one period reviewing from the point of view of ethics or moral philosophy, defined as the branch of philosophy that systematically studies the nature of morality. The review distinguishes general issues, principles, and arguments from each other and from Socrates's own actions and views; it then applies these distinctions to the previous topics and sketches a systematic framework involving the relationship between morality on the one hand and wisdom, intention, the unexamined life, death, politics, and religion on the other.

A second review, just before the test, analyzes Socrates from the point of view of the nature of philosophy introduced on the first day. There is not much to be said about the content-free nature of philosophy other than to remind students to be able to describe the specific subject matter treated by Socrates, and to remind them that it is not this which makes him a philosopher. The emphasis on reasoning demanded by the rationality of philosophy is relatively obvious in the case of Socrates, and I elaborate it by counting and/or naming each of the arguments we have discussed, by comparing and contrasting them from the point of view of the intensity of reasoning, and by mentioning some examples of reasoning where the subject matter is not morality but reasoning or argumentation itself, that is, examples of reasoning about reasoning. The avoidance of one-sidedness and extremes required by the judiciousness of philosophy is illustrated in terms of Socrates's attitude toward three things: toward society, insofar as he shows defiance of unjust laws but respectfully submits to just ones; toward other people, insofar as he is highly critical of their beliefs and actions, but cares deeply about their character or soul; and toward God, insofar as the unexpressed but implicit positive argument about piety in the *Euthyphro* takes the point of view of God by asking what is the highest duty of humans toward God, but answers that this is the improvement of human character, which is something that does not really involve God. The practicality and universality of philosophy are illustrated in more or less obvious ways that need not detain us here. Finally, the critical-constructiveness of Socrates's philosophy is discussed by admitting first that the critical and negative element of his thinking is the more striking and obvious one; then I explain that the maieutic element of the Socratic method is its constructive aspect, though of course the problem remains that it is not always present and does not always get carried out; further, the Socratic discussion of piety turns out to have a constructive aspect, though it was left to the professor to extract the positive lesson from the text, and even he had to have help from scholars specializing on the topic[11]; finally, the general point is made that merely negative criticism is normally injudicious, and therefore constructiveness is required by the judicious nature of philosophy.

[11] I have already stated that I owe the positive interpretation to Vlastos 1971a, 14.

6.4 Galileo

As mentioned above, for the Galilean part of the course there now exists a sourcebook comparable to one of the various editions of Plato's earlier Socratic dialogues.[12]

One pedagogical advantage of this portion of the course is that is has a more readily identifiable and comprehensible substantive issue which serves as the central focus, that is, the controversy about the motion of the earth, climaxing in the trial of Galileo by the Inquisition in 1633. Therefore, students are told that, besides the specific study questions correlated with the various reading assignments, they should always keep in mind, and try to learn something new in regard to, the following three general questions: (1) the main events of Galileo's life; (2) his main scientific discoveries and inventions; and (3) the various arguments for and against the earth's motion.

I begin by spending one period on a preliminary brief sketch of Galileo's life and a brief account of several relevant historical developments at that time. In the former I call special attention to the fact that, after he left his university position as professor of mathematics, Galileo held the title of Philosopher and Chief Mathematician to the Grand Duke of Tuscany, and I connect this to the content-free nature of philosophy. In regard to the historical background, I mention the Protestant Reformation, the Catholic Counter-Reformation, the burning of Giordano Bruno, and the Thirty Years' War.

I then spend about two periods discussing the geostatic view of the universe, which predominated at that time. I explain some of the simpler details and the more basic terminology, and then present the two main traditional arguments in its favor. One was the observational argument, which said simply that the geostatic system corresponds to direct sense-experience, that is, to what we see with our eyes and to what we feel with our kinesthetic sense; this is, of course, essentially true, and therefore this argument was a very powerful one in support of the ancient view. Another one was a theoretical argument, based on the laws of motion widely accepted at the time; the main relevant law stated that all bodies made of the elements earth and water have a tendency to move toward the center of the universe, if they are not already there; now, since the terrestrial globe is simply the collection of all things made of these two elements, it can do nothing but stand still at the center of the universe. At this time I do not criticize this argument, but focus on explaining it and rendering it plausible; moreover, I point out that the only way to attack it is to attack its major premise and come up with a new law of motion, which is something easier said than done.

The next topic is the geokinetic view of the universe, which had been revived and elaborated about 50 years before Galileo by Copernicus. Again, I restrict myself to the simpler details and terminology, and discuss the main pre-Galilean argument in its favor, namely the simplicity argument.

Emphasizing that I am trying to get students to imagine themselves to be living at the time of Galileo, I then spend several days explaining a number of the traditional arguments against the earth's motion, which for centuries had made almost everyone believe the geostatic view. The list of these objections is a long one, and I select

[12] Finocchiaro (1989a, 2014b).

the easier ones. I find it preferable to discuss the Galilean criticism of these various anti-Copernican arguments one at a time, immediately after the statement of each argument.

Although some of the objections were epistemological, and some religious, it is best to begin with the scientific arguments, to impress upon students that the issues in the Galileo affair were partly scientific. Here it is also instructive to discuss at least one astronomical and one mechanical objection. The stellar-parallax argument is one of my favorites: if the earth revolves around the sun once a year, then the apparent position, magnitude, and brightness of a star should undergo periodic variations in the course of the year; but these are not observed; therefore, the earth cannot revolve around the sun. This argument raises many important issues, including the very accessible one about the size of the universe and of interstellar distances, which Galileo clarifies in one text by means a memorable comparison which students find both quaint and unforgettable. That is, Galileo could not answer this objection by claiming that he had been able to detect stellar parallaxes since even the telescope did not reveal any, and in fact they were not observed until the nineteenth century; rather he answered it by suggesting that the distance to the stars is so great that the parallax is very small, too small to be detected with available instruments. However, this involved a tremendous increase in the size of the universe and interstellar distances, and some estimates at the time claimed that the nearest star would have to be about fourteen thousand times further away than the sun (actually it is much farther). Now, a new difficulty with this hyper-distance was that many found it too large, but Galileo pointed out the following: there is nothing implausible with one astronomical distance being fourteen thousand times greater than another astronomical distance because we are all acquainted with natural kinds such that one specimen is even more than fourteen thousand times greater than another, for example an elephant as compared to an ant, or a whale as compared to a gudgeon.[13]

Of the mechanical objections, the most instructive is perhaps the one variously called the tower argument, the vertical-fall argument, or the argument from falling bodies: if the earth rotates on its axis once a day, then bodies would not fall vertically; but they do; therefore the earth cannot rotate. In the discussion of this argument an experiment was often mentioned, to the effect that a rock dropped from the top of the mast of a ship falls at the foot of the mast only when the ship is motionless, but falls away from the foot of the mast when the ship is moving forward. In this connection, Galileo made one of his most famous remarks, again sufficiently memorable to impress even otherwise uninterested beginners. Speaking to his opponents he said: "Here I have been a better philosopher than you in two ways: for, besides asserting something which is the opposite of what actually happens, you have also added a lie by saying that it was an experimental observation; whereas I have made the experiment, and even before that, natural reason had firmly persuaded me that the effect had to

[13] Finocchiaro 1989a, 171. For more details on this argument, see Galilei (1890–1909, 7: 385–416; 1967, 356–397; 1997, 247–281); and Finocchiaro (1980, 41–42; 2014a, 194–209; 2019, 59–60, 142–144, 242–243). See also Graney 2015, whose limitations are serious enough that is should be read in conjunction with the criticism in Finocchiaro (2016, 2017).

happen the way it indeed does."[14] The connections with this course's conception of philosophy, as well as various implications of different sorts, are obvious and need not be elaborated here.

Next, I discuss two epistemological objections that are found more or less explicitly in Cardinal Bellarmine's letter to Father Foscarini.[15] One is the objection from the deception of the senses, holding that the earth cannot move because if it did then our senses would be deceiving us when they report that the earth stands still; but such deception is impossible partly because the senses are God-given, and partly because they are our main instrument for the acquisition of knowledge, and if they cannot be trusted nothing can. The other epistemological objection asserts that Copernicanism is just an unproven theory or hypothesis because its supporting arguments are indirect and hypothetical in the sense that if we assume that the earth moves then we can explain why the observed phenomena are as they are; however, this explanatory power does not make the assumption true; all we can say is that it can account for observations. I discuss Galileo's answers to these objections along lines that may be readily guessed.[16]

The religious objections and Galileo's replies bring us to a reading of his famous *Letter to the Grand Duchess Christina*. Since the full text of this *Letter* is included in the textbook,[17] and since Galileo states those objections with great clarity, and criticizes them with great cogency, at this point in the course we can follow the book relatively closely. Spending two or three days on the topic, I ask students to read the *Letter* with the following questions in mind: Why did Galileo write the *Letter*? How does he state the biblical argument against the earth's motion? How does he criticize this argument? Why does he think that a literal interpretation of the Bible is not always correct? Why does he think that the Bible is not a scientific authority? What does he mean in saying that "theology is not the queen of the sciences," and why does he maintain that? Why does he hold that biblical consensus is not a good reason for interpreting the Bible literally? How does he state the objection based on the authority of the Church Fathers, and how does he answer it? What does he think about the authority of the Catholic Church in scientific matters? What is the Joshua miracle mentioned in the Bible, and what is Galileo's interpretation of how it may have happened? How does this interpretation help him to criticize the biblical objection?

Before going on to a discussion of the trial, I summarize the situation in some such terms as the following: there were two views about the physical universe, the geostatic theory which was accepted by most people and which we now know to be false, and the geokinetic theory which Galileo believed to be more correct and which

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[14] Finocchiaro 1989a, 184; cf. Galilei 1890–1909, 6: 545. For more details on this issue, see Galilei (1890–1909, 7: 164–193; 1967, 138–167; 1997, 155–170); and Finocchiaro (1980, 192–201; 2010a, 2014a, 100–112; 2019, 228–235).

[15] Finocchiaro (1989a, 67–69; 2014b, 78–80).

[16] See, for example, Galilei (1890–1909, 7: 272–281; 1967, 247–256; 1997, 212–220); and Finocchiaro (1980, 124–125; 2014a, 149–153; 2019, 133–134, 237–238).

[17] Finocchiaro (1989a, 87–118; 2014b, 48–77). For more details, see Finocchiaro (2010b, 65–96, 229–252; 2019, 99–108).

we know today is essentially true. But at that time the relevant arguments were either inconclusive or else favored the false geostatic theory; for, at least before Galileo, there were powerful arguments in its favor, and seemingly conclusive objections against the true geokinetic theory; whereas the arguments in favor of the latter were relatively few and weak. Galileo came to be attracted to the then-unpopular view, and his story is that of the struggle to criticize the pro-geostatic and anti-Copernican arguments, and to strengthen and add to the pro-Copernican ones.

The story of Galileo's trial takes two days.[18] I discuss both the first phase which climaxed in the Catholic anti-Copernican Decree of 1616, and the actual trial of 1633 occasioned by the publication of the *Dialogue on the Two Chief World Systems*. Galileo's abjuration offers the opportunity to compare and contrast his behavior and his situation with those of Socrates. The contrast is, of course, that Galileo was apparently willing to give up his beliefs to escape death, whereas Socrates was willing to die for what he believed. However, this is just the tip of the iceberg. One analogy I like to point out is that between Galileo's submission to authority in the 1633 trial and Socrates's deference to the laws leading him to remain in prison, on the one hand, and between Socrates's defiant attitude at his own trial in the *Apology* and Galileo's risk of writing the *Dialogue* (1632) even though the anti-Copernican decree of 1616 had not been rescinded. Of this more below in connection with Galileo's judiciousness.

As in the part of the course on Socrates, if time allows, I like to have a review from the point of view of a branch of philosophy that studies systematically many of the issues Galileo was dealing with, which in this case is epistemology. This can take the form of a discussion of the following problems: the role of authority in the search for truth, the relationship between biblical interpretation and physical investigation, the difference between direct and indirect observation, the nature of hypotheses, the nature of simplicity, and the possibility of limits to the freedom of inquiry. These problems can be formulated in general; illustrations from Galileo can be given; and other examples from the present day can be described. In regard to the latter, it is difficult to resist the temptation of mentioning the evolution-creation controversy.

Finally, I discuss the nature of philosophy in terms of Galileo's thought. The content-free character of philosophy can now be seen clearly from the fact that Galileo and Socrates deal with very different substantive questions. Galileo's rationality, or emphasis on reasoning, can be illustrated, as before, by naming and cataloguing all the various arguments and counter-arguments; interestingly enough, like Socrates, Galileo occasionally engages in reasoning about reasoning, the chief example now being most of the *Letter to the Grand Duchess Christina*, which in a sense is a plea for the desirability of reasoning, rather than using various authorities, in scientific inquiry.

In regard to Galileo's judiciousness, one example is his attitude toward the Catholic Church, which he both respects but also tries to reform, thus avoiding both total submissiveness and total disregard. Another illustration is his attitude toward the Bible, which he rejects as a scientific authority, but accepts as one in matters of faith

[18] For the documents, see Finocchiaro (1989a, 134–153, 256–293; 2014b, 96–106, 119–139). For discussions, see Finocchiaro (2010b, 135–154; 2019, 109–122, 155–170).

and morals, thus showing partial acceptance and partial rejection. A third instance is his attitude toward the two chief world systems, explicitly displayed in a book like the *Dialogue*, where Galileo shows his preference toward the geokinetic view, but does so without disregarding the other side. Fourth, one could mention Galileo's attitude toward Aristotle, whom he admired personally and was willing to follow in spirit, but whom he rejected as far as the letter of his specific conclusions was concerned. And one could also mention Galileo's combination of observation and thinking, in regard to which he avoided both the uncritical empiricism of many followers of Aristotle, as well as the intellectualism of someone no less than Copernicus himself, who had accepted the earth's motion despite unanswerable observational objections.

The practical orientation of Galileo's thinking may be seen from the fact that it was in large measure an attempt to understand his observations and experiments; and also from the fact that his involvement in the Copernican controversy was to some extent motivated by his desire to help the Catholic Church, by preventing her from committing a serious error. Next, we may attach universal relevance to Galileo's thought partly insofar as he deals with epistemological issues such as those mentioned above, and partly because the main scientific issue (especially if phrased in terms of whether or not the earth is at the center of the universe) involves the image that mankind has of itself and of its place in reality. Galileo's criticism is directed at people who hold geostatic beliefs, or who are uncritical followers of Aristotle, or who would want to use biblical assertions to prove scientific conclusions; while his constructiveness is clear from his pursuit of the geokinetic research program, and from his commitment to a number of epistemological principles.

6.5 Marx

The focus of the third part of the course is *The Communist Manifesto*, but the text of this document needs to be supplemented by appropriate introductions, annotations, or additional selections. As mentioned above, there is now available an edition which contains just about all the texts, commentaries, and materials one would want or need in this course.[19]

I begin by telling students that, although the *Manifesto* was jointly authored by Marx and Engels, I shall be using only the name of Marx and the label Marxism both for the sake of brevity and out of a desire to emphasize the more important of the two. I also give students four general study questions that reflect the central issues of this portion of the course: What is the present-day relevance of Marx? What is meant by "socialism" and "communism"? Why did Marx think "communism" to be "inevitable," and why did he think it to be "desirable"? As before, these are topics to be reflected upon in connection with each reading assignment and each lecture, besides the specific study questions that apply to each.

[19] Marx 1988. See also Finocchiaro 1983.

Before going into a detailed textual analysis of the *Manifesto*, about half of this segment of the course is taken by preliminary topics, mostly covered in the book's editorial introduction. I begin with one period on the present-day relevance of Marx from the points of view of the geopolitical situation, the labor movement, and scholarship[20]; by the latter I mean the disciplines of economics, history, sociology, and philosophy. The reference to philosophy here offers the occasion to connect Marx's conception to the so-called "practical" nature of philosophy elaborated in this course; however, by now I am also in the position of being able to say that this conception has also its roots in Socrates, and was not Marx's invention.

On the second day we discuss Marx's life and the historical conditions in nineteenth century Europe. In regard to the former, I like to call attention to the fact that Marx was a philosophy Ph.D. who had planned to go into university teaching. In regard to the latter, I focus on the rise of nationalism, the status of democratic institutions and practices, and the miserable conditions of factory workers generated by the industrial revolution.

A third period is needed to discuss and clarify terminology: not only socialism and communism, but also capitalism, feudalism, Leninism, Marxism-Leninism, social democracy, bourgeoisie, proletariat, and working class.

Next, I discuss the highlights of Marx's so-called materialistic conception of history, and now begins the challenge of condensing and simplifying Marx's complex and controversial ideas without distortion. I must confess that I do not feel I have completely mastered the problem. The problem is compounded by the fact that this and other topics are presented in the textbook's editorial introduction, and thus the instructor's divergence from it may cause student confusion. At any rate I take this opportunity to introduce the notion of a philosophy of history as a branch of philosophy, so that Marx's own doctrine is seen as a particular instance.

Two days are needed for a discussion of Marx's economic theory. This involves the law of capitalist accumulation, the law of capitalist centralization, the law of increasing misery, the labor theory of value, and the theory of surplus value. Not all of these need receive equal attention, and the two that are most important for understanding some of the basic theses of the *Manifesto* are the theory of surplus value and law of increasing misery. The last one also provides excellent material and occasion for a sequence of thesis, criticism, counter-criticism, counter-counter-criticism, and so on.

We then begin a close reading of the first two sections of the *Manifesto*. The first assignment is the first half of section I, and this offers the occasions for explaining and criticizing Marx's theory of classes and class struggle and his theory of the state.

The second half of section I is assigned next, and the central focus is Marx's prediction of the downfall of capitalism. Here I like to distinguish two supporting arguments: what I call the "historical" argument is based on an analogy between the

[20] The end of the Cold War and the dissolution of the Soviet Union have in some ways diminished the relevance of Marx's ideas. However, that applies only from the viewpoint of dogmatic or vulgar Marxism, whereas the discussion of Marx in the context of this course was largely meant as an antidote to it. Moreover, these developments in another way enhance the relevance, because many Marxian ideas can now circulate more easily in the West.

then-current proletarian class struggle against the bourgeoisie, and the past bourgeois class struggle against feudalism; whereas the argument I label "economic" involves the alleged instability of the capitalist system. These arguments are explained and criticized: my criticism of the historical argument raises the difficulty that the rise of the middle class has diluted the importance of the conflict between proletarians and capitalists; and my criticism of the economic argument raises chiefly the issue of the validity of the law of increasing misery, and it thus ties with the previous discussion of this topic.

I next assign the first two and one-half pages of section II, together with the following study questions: Exactly what kind of private property does Marx want to abolish, and what kind does he not want to abolish? What does he mean by the abolition of private property? Why does he want to abolish it? My discussion of the first question focuses on the distinction between personal and profit-making private property, as well as on the problem of mixed and borderline cases; the second question is discussed by clarifying the differences among nationalization, industrial democracy, and profit sharing; and, thirdly, Marx's supporting reasoning is explained by elucidating his textual assertion that "capital is, therefore, not a personal, it is a social, power,"[21] which, of course, involves the previously discussed theory of surplus value. My discussion also presents several criticisms of Marx's argument.

The text of the *Manifesto* itself, in the central part of section II, states and criticizes a number of objections to the abolition of private property. Therefore, this is the next assignment and discussion. Those objections all raise issues regarding possible undesirable consequences of the abolition of private property; these are, respectively, the loss of individual freedom, the loss of individual incentive, the abolition of the family, the communal sharing of women, the abolition of separate countries, and radical changes in religion and morality. If one can get beneath the rhetorical veneer and flourish of Marx's language here, some of his points raise important evaluative issues; moreover, they offer the opportunity to engage in some criticism of his criticism of the then-current criticism of the abolition of private property.

The last two pages of section II of the *Manifesto* require two days of separate discussion. In the first, I reconstruct more explicitly the theory of revolution inherent therein; I do so in terms of the following six phases of revolutionary development: proletarian class organization, dictatorship of the proletariat, transformation of the economy, classless society, withering away of the state, and Kingdom of Freedom. As usual, my discussion includes criticism of Marx's theory, and here the criticism focuses on the comparison and contrast between conditions in capitalist countries and conditions in communist countries.

The last substantive topic is the list of ten specific measures advanced by Marx at this point in the *Manifesto*. Recall that these are proposals such as the abolition of property in land, abolition of inheritance, a progressive income tax, state centralization of financial institutions, state centralization of the means of transportation and communication, state intervention into manufacturing and into agriculture, equal obligation of all to work, and free education for all children in public schools. My

[21] Marx 1988, 68.

study questions ask students the following: for each of these measures, explain what it means, discuss to what extent it has already happened in the United States, indicate Marx's reason(s) for advocating it, and evaluate how desirable it is.

As in previous cases, if time allows, I like to have a review class from the point of view of philosophy of history and social philosophy, considered as branches of philosophy. Here, general problems and conceptual issues are formulated, Marx's position on them is stated, his reasons for his views mentioned, evaluation of his reasons considered, and alternative positions suggested.

My review of Marx from the point of view of the nature of philosophy comes next. It is obvious that the subject matter treated by Marx is as different from that of Galileo as the latter was from that of Socrates; thus, further content is added to the idea that philosophy is content-free.

The question of rationality is more complicated than usual. In fact, we need to distinguish between the material as found in the book, both the text of the *Manifesto* and the editorial introduction, and the material as I have presented it in class; this distinction is needed because it must be admitted that there is no special emphasis on reasoning in the Marxian text or that introduction. The emphasis was rather in my presentation. However, even here, another difference from the previous cases must be pointed out. Whereas in the cases of Socrates and Galileo reasoning was usually emphasized in connection with the question of how and why they arrived at the views they held, in the case of Marx reasoning was emphasized mostly at the level of the evaluation of the correctness or acceptability of the views attributed to him. For example, even class discussions, let alone the book, did not emphasize the reasons why Marx held such doctrines as the materialistic conception of history, the theory of classes, the theory of the state, the law of capitalist accumulation, the law of capitalist centralization, and the labor theory of value; here the emphasis on reasoning came in the evaluation and criticism of these doctrines.

The notion of judiciousness raises a different type of issue. It is that, given our definition of judiciousness as the avoidance of one-sidedness and extremes, the ways in which Marx falls short of this are perhaps more apparent than the ways in which he exemplifies it. For example, Marx's theory of ideology is formulated as the thesis that all ideas are determined by economic conditions; and this is likely to be injudicious because it takes as causal determination what is merely influence, and because it takes to be true of all what is only true of some. Or consider his proposal to abolish profit-making private property, in regard to which it seems clear that he is referring to all profit-making private property; and this is too extreme since it would not allow profit even for a self-employed worker who might own his one-man business. Other examples of injudiciousness could be given, but we must not ourselves go too far and deny that we have at least one good example of judiciousness. This involves Marx's attitude toward the bourgeoisie, elaborated in the first half of section I of the *Manifesto*; there Marx makes it clear both that he is against the bourgeoisie, but that at the same time he is willing and able to appreciate the good it has done, for example, to overthrow feudalism, to encourage equality and freedom, in short, to have been a revolutionary class.

On the other hand, the practicality, universality, and critical-constructiveness of Marx's thought are relatively obvious, and do not need special elaboration here.

On the last day of the course there is a general review, structured as a comparison of how well the three thinkers studied illustrate or fail to illustrate the various features of philosophy. Here I argue that in regard to emphasis on reasoning, the greatest emphasis is found in the Socratic texts; that the texts also show Galileo to be the most judicious; that Marx's doctrines seem to be the most practically-oriented; that there is no significant difference in regard to universality; and that from the point of view of critical-constructiveness, Socrates appears the most negatively critical, Galileo the most constructively critical, and Marx the most radically and deeply critical. These judgments and conclusions are not presented in a dogmatic manner and are not meant to be definitive, and what is more important is the process of comparison and contrast to give a fuller meaning to the various abstract notions that have been taken to define philosophy.

6.6 Conceptions of Critical Thinking

Earlier, in my introduction, I mentioned some of the motivations that led me to develop this course. Each of those motivations corresponds to a feature of the course that could be elaborated at greater length, but here I want to discuss its open-endedness, its connection with critical thinking, and some conceptual-theoretical problems to which it leads.

By open-endedness I mean that the course lends itself to being modified in a number of ways, yielding courses that are longer (full year) or more advanced (up to graduate seminars). One way would be to enhance the theoretical component. That is, one could read Kant's *Groundwork of the Metaphysics of Morals*[22] to pursue more deeply and more systematically the ethical issues and concepts that arise in connection with Socrates's thought and action; I feel the transition between the two could be easily made by engaging, for example, in the type of analysis provided by A. D. Woozley's paper "Socrates on Disobeying the Law."[23] To pursue the methodological and epistemological issues exemplified by Galileo's work and discussed by him, one could read either Bacon's *New Organon*, or Descartes's *Discourse on Method*, or Mill's *System of Logic*.[24] Analogously, as a follow-up to Marx, one could discuss Croce's *Philosophy of the Practical*, or the second volume of Popper's *Open Society and Its Enemies*, or some appropriate parts of Gramsci's *Prison Notebooks*.[25] With such additions, one would have a full-year course, whose first half could be regarded

[22] Kant 1785.

[23] Woozley 1971.

[24] Bacon 1620; Descartes 1637; Mill 1843.

[25] Croce (1981, 1915); Popper 1962; Gramsci (1971, 1975). On the latter, see also Finocchiaro (1988a, b, 1999a).

as the practice of which the second half would provide the theory, the emphases being ethics, epistemology, and social philosophy.

Another way of extending the course would be to add to it an elementary introduction to logic, for which all the examples of arguments would be taken from the Socratic, Galilean, and Marxian texts read earlier. Of course, the kind of logic for which this would be appropriate would be what is variously called informal logic, applied logic, practical logic, argumentation theory, or theory of reasoning, given the intended emphasis on context and on realistic examples.[26]

A way of making the course more advanced would be to concentrate on textual analysis and on the evaluation of the philosophical content of the texts. Here one could read all of Plato's *Phaedo* with as much care as the *Apology*, *Crito*, and *Euthyphro* are examined in the above-mentioned course, and one could also analyze and evaluate with more care the central, difficult argument in the *Euthyphro*; the latter, in particular, is something of a philosophical goldmine, as one may infer from specialized studies on the topic.[27] Further, in regard to Galileo, the focus should be on the *Dialogue on the Two Chief World Systems*, and the type of philosophical issues arising therefrom could be appreciated with the help of some recent works by philosophers.[28] Finally, there would be several ways to add to the text of *The Communist Manifesto*; my inclination would be to read relevant parts of *Capital*, and interpretative and critical works by such authors as Popper, Robert C. Tucker, G. A. Cohen, and Robert Paul Wolff.[29]

In regard to critical thinking, first of all, it should be obvious that I feel the course has sufficient merits from other points of view that its connection with critical thinking is relatively secondary; what I mean is that, even if it could be argued that this course does not teach critical thinking, then I might be inclined to take the attitude expressed by the remark "well, so much the worse for critical thinking." Actually, it is obvious that such a failure is inconceivable, given that Socrates is universally regarded as the patron saint of critical thinking; given that Marx would be widely acknowledged to be another model, despite the more controversial nature of his ideas; and given that the "critical" character of Galileo's thinking is at least a consequence of its revolutionary character, which is paradigmatic.[30] Therefore, the only question in regard to the course is that of what conception of critical thinking it embodies, and exactly how it furthers the educational ideal of critical thinking.

However, a more fundamental point needs to be made. In fact, conceptions of critical thinking are not self-subsisting Platonic entities with a life of their own independently of critical-thinking practices and of critical-thinking courses. The adequacy of those conceptions has to be evaluated in part on the basis of these

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[26] See, for example, Finocchiaro (1983, 1984, 2005, 1–18, 21–33).

[27] For example, Cohen 1971; Friedman 1982.

[28] Feyerabend 1975; Finocchiaro (1980, 2014a).

[29] Popper 1962; Tucker 1961; G. A. Cohen 1978; Wolff 1984. See also Gramsci (1971, 1975); and Finocchiaro (1988a, b; 1999a).

[30] For more details of exactly how Galileo's thought was "critical," see Finocchiaro (1980, 1997, 2010b, 2014a, 2019, 225–248).

practices and courses. I am not saying that the latter are primary, and the former parasitic on them; rather I am denying that the conceptions are primary vis-à-vis the concrete instances. So my position is that there is and ought to be a mutual interaction between the two, whereby conceptions are formulated and tested on the basis of practices, and practices are established and improved with the help of conceptions.

Here it might be objected that conceptions still retain some kind of logical priority over practices because in order to even identify a practice as a *critical-thinking* practice one has to have some idea of what critical thinking is. However, it seems to me that the presupposed idea of critical thinking needs to be merely implicit, rather than explicitly articulated; and my point is that rendering explicit what is implicit presupposes some kind of priority of what is implicit, which is to say of practice.

Thus, let us now go on to comment on the connection between the practice of critical thinking studied in this course and a number of conceptions of critical thinking that have been articulated by some recent scholars of critical thinking.[31] I believe this course comes closest to Eugene Garver's notion of critical thinking as a discipline.[32] He has argued that an attractive model for the teaching of critical thinking would involve three main things: (1) a study of what he calls the formal aspects of thinking, and which I would call theories of thinking, his own examples being Aristotle's *Rhetoric*, Bacon's *New Organon*, and Newman's *Grammar of Assent*; (2) a body of argumentative literature, which he calls a "canon for critical thinking" and which I would call examples of critical-thinking-in-practice, in regard to which he gives several examples including Cicero's *De Natura Deorum*, Machiavelli's *Prince*, Adam Smith's *Wealth of Nations*; and (3) some examples of works displaying the interplay of theory (Garver's "method") and practice (his "canon") of textual interpretation, one of his examples being Hayden White's *Metahistory*. I find this extremely interesting and worthy of more discussion than it has received so far. Obviously, the authors used in my course would fit very well in Garver's "canon"; in fact, I would argue that they should be considered even more fundamental than the examples he gives.

At this point a qualification is perhaps needed to indicate that the correspondence between my course and Garver's conception is by no means exact. I mentioned earlier some of the desiderata for the source-books appropriate in my course, and how they should contain more than the classic texts of the original authors. For example, I stated that, although for the Galilean segment I began by using Galileo's *Dialogue on the Two Chief World Systems*, I later gave it up in favor of Drake's collection, and eventually adopted another one. On the other hand, I can state now that Galileo's *Dialogue* certainly belongs to Garver's "canon," and if there should be any doubt about this, then a study of the details of that book's argumentative

[31] It would be beyond the scope of the present essay to also take into account the work of other scholars who have made contributions to related areas and concepts; among the best and most relevant such scholarship, I would mention D. H. Cohen 2009 and Hitchcock 2017.

[32] Garver 1985. I believe that my course also corresponds in important ways to Matthew Lipman's notion of critical thinking, though his account is sufficiently rich and complex that it deserves separate examination for its own sake, which cannot be done here; but see, for example, the references given in note 7 above.

content and structure should dispel it.[33] However, my main point is that my course is closely related to Garver's conception of critical thinking, but that there is only a partial correspondence. Perhaps he could elaborate further his view in order to give my course a more exact place in his scheme of things, just as I would certainly be interested in trying to put his already-stated conception into practice (given the proper practical conditions).

There is another point I wish to make in regard to Garver's conception, but, in order to make it, two terminological leaps are required. One leap involves my treating as part of his conception of critical thinking some of what he has to say about what he calls "prudence"[34]; the other leap involves equating this prudence with what I have called "judiciousness." I believe the first leap is justified both as a tentative working hypothesis for further exploration, and also by comparison to what other writers on critical thinking have included under the concept. Let me explain this last point.

In fact, if I may be allowed to criticize some of the leading critical-thinking theorists, I would say that there is a general tendency in the field to broaden the category to make it subsume all that is good; and this is a tendency that should be resisted. For example, in a recent sketch of the history of critical thinking, one of the pioneers of the field includes St. Augustine in the list (though he did not say on what grounds), as well as the American Founding Fathers, on account of their commitment to the value of education.[35] Further, on several occasions one of the most intelligent scholars in this field justifies the ideal of critical thinking in part as instrumental to one's being initiated into the tradition of the rational disciplines, and he includes in the latter not only science, mathematics, and history, but also literature and the fine arts[36]; now, he does so without justification, which is to say that he assumes "uncritically" not only that the worth of literature and the fine arts lies in the cognitive realm, but also that the cognitive realm is necessarily ratiocinative.[37] Finally, although I find considerable plausibility in much of what is said in regard to the distinction between

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[33] See Finocchiaro (1980, 1997, 2014a, 2019, 123–154).

[34] Garver (1987a, b).

[35] Glaser 1985. Of course, it is another story if the reason one gives for including the American Founding Fathers is that there exists a book authored by some of them which contains such frequent, intense, and complex augmentation that it may be considered as part of the critical-thinking canon. I am referring to *The Federalist Papers*, which is the collection of essays by Alexander Hamilton, James Madison, and John Jay, written in 1787–1788, and dealing with the issue of whether the United States constitution should be adopted. For some details, see Rossiter 1961; and Finocchiaro (1999b, 2005, 272–276).

[36] Siegel (1980, 16; 1988, 59–60).

[37] In private correspondence, Siegel has addressed this issue in a number of ways. In part he has attempted to shift the burden of proof, to which I would reply that the burden remains his, given that science and mathematics are prima facie as different from literature and the fine arts and any two disciplines can be. He has also mentioned that the importance of critical thinking for literature and the fine arts has been argued in Bailin 1988, to which one might reply that the non-ratiocinative (though still cognitive) nature of the fine arts (including literary art) was argued by Benedetto Croce in such works as Croce (1902, 1909, 1936, 1981). Siegel does not seem to distinguish properly between the creation of an artistic work and its appreciation. Now, following Croce, I would certainly agree that the intelligent appreciation of art involves critical thinking about art in an essential way; however, the making of art involves essentially the mental activity of imagining, which is a mental activity

the weak and the strong sense of critical thinking,[38] a central point of this argument amounts, I believe, to saying that critical thinking is or should be something more than thinking, that it should also involve "critical" acting and behavior, in the sense of both instances and patterns thereof; and one may question the advisability of such an inclusion. I mean, how can one include something other than thinking under the category of thinking? At the very least we have a terminological leap, which brings me back to my original point. This was that there is more justification for including prudence in the notion of critical thinking, than there is for including so much of what is often included, even by the leading authorities in the field.

My other terminological leap was the identification of judiciousness and prudence. This can be easily justified if we examine some of what Garver has in mind. His definition views prudence "as an inferential relation between rules and cases, precepts and examples"[39]; it is, in a sense, the following of rules, but rules which are neither algorithmic nor heuristic. He is very clear that "the problem of prudence … is precisely to make it into something more than cleverness and opportunism,"[40] and that "prudence is not simply the middle ground between extremes, the middle that any reasonable person should adopt just because extremes are bad."[41] To be sure, more work is needed to clarify the concept, though I am inclined to agree with Garver that one should probably not expect a *theory* of prudence, and that only a *history* of prudence is possible. Nevertheless, I believe that my emphasis on the dimension of judiciousness in the thought of Socrates, Galileo, and Marx constitutes, in part, a study of the history of prudence.

I also believe that my course represents at least one type of solution to John E. McPeck's problem, that is, the problem that there is no general skill properly called critical thinking because all thinking is about something in particular.[42] My solution is to expose students to particular cases of critical thinking, and at the same time to try to draw some general lessons, based on what they share in common.

Furthermore, it should be obvious that I share the widespread view that there is more to critical thinking than the possession of some purely cognitive skills. In fact, from my point of view, all such skills would be subsumed under the category of reasoning; now, though what I have called the other features of philosophy may be *related* to reasoning, they are not reducible to it. This is especially true of judiciousness, which I believe also corresponds to those elements different from purely cognitive skills or proficiencies included in critical thinking by such authors as Robert H. Ennis and Richard Paul.

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in which one does precisely something different from critical thinking. For a discussion of other aspects of Siegel's account, see Finocchiaro (1989b, 1990, 2005, 181–192); and Siegel 1990.

[38] See, for example, Paul 1990, especially pp. 87–175, 369–378.

[39] Garver (1987a, 12; 1987b, 67).

[40] Garver 1987b, 65.

[41] Garver 1987b, 73.

[42] McPeck 1981.

Paul speaks of intellectual virtues, and his list includes intellectual humility, courage, empathy, integrity, perseverance, fair-mindedness, and faith in reason.[43] Ennis speaks of tendencies or dispositions,[44] and one of his lists includes, among other things: well-informedness, precision, open-mindedness, orderliness, and flexibility.[45]

Let us now look at some examples to see how their relationship to judiciousness might work. Consider humility, which Paul defines as "a consciousness of the limits of one's knowledge, including a sensitivity to circumstances in which one's native egocentrism is likely to function self-deceptively."[46] Now, a classic instance of humility is Socrates, who discovered its importance while in the process of clarifying the meaning of the Delphic Oracle which had declared that no one was wiser than Socrates. However, if one reads carefully the text of Plato's *Apology*, where the story is told, one finds Socrates coming close to saying that his interpretation of the Delphic Oracle is that human knowledge is worthless, that no one really knows anything, and that all human beings are equal in their ignorance.[47] Such an interpretation would be carrying humility too far; so it is preferable to interpret Socrates as meaning that the message is to be aware of the limitations of one's knowledge. Moreover, also in the *Apology*, in the middle of his speech, after his cross-examination of his accuser Meletus, Socrates displays considerable arrogance when he tells the Athenian people that he thinks he is the greatest good that ever came upon the city of Athens.[48] One could conclude that either Socrates violated the virtue of humility in an important way on a crucial occasion, or that humility is not really a virtue. I prefer to conclude that the virtue of humility cannot be practiced mechanically, but must be handled judiciously; and that humility is not the only virtue, but that there are others which often conflict with it, and whose conflict cannot be resolved by appeal to any other principle, except judiciousness. In the present case, one other virtue in terms of which one could analyze Socrates's behavior along these lines would be that of integrity.

Or consider precision, from Ennis's list. His formulation of the corresponding disposition seems adapted from Aristotle, and displays its judicious character very openly; in fact, Ennis speaks of the tendency or disposition to "demand *as much precision* as the subject matter permits."[49] Actually, it should be mentioned that Ennis mentions explicitly, as a third component of what he calls rational or critical

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[43] Paul 1990, 195–198.

[44] Ennis (1980, 1981).

[45] Ennis 1980, 17.

[46] Paul 1990, 195.

[47] Cf. *Apology* 23A. The Penguin Books translation (by Hugh Tredennick) reads: "The wisest of you men is he who has realized, like Socrates, that in respect of wisdom he is really worthless" (Plato 1969, 52).

[48] Socrates's own words need little comment: "… far from pleading on my own behalf, as might be supposed, I am really pleading on yours, to save you from misusing the gift of God by condemning me. If you put me to death, you will not easily find anyone to take my place. It is literally true (even if it sounds rather comical) that God has specially appointed me to this city …" (*Apology* 30BC; Plato 1969, 62).

[49] Ennis 1980, 17.

thinking, in addition to his list of skills or proficiencies and his list of tendencies or dispositions, "*the exercise of good judgment*"[50]; he does not elaborate, but the intimate connection with what I have called judiciousness is obvious.

One final point needs clarification in regard to the connection between critical thinking and the conception of philosophy advanced in the above-mentioned course.[51] It should be clear (and the students are accordingly warned) that I am presenting there a *particular* conception of philosophy; therefore, I do not deny that there are other ways of conceiving philosophy, and I am not saying that there is a one-to-one correspondence between my definition and every instance of what could properly be called philosophy. Moreover, even for those instances to which my definition becomes interestingly relevant, my claim is not merely descriptive but has a normative element as well; this was apparent in the above discussion when I pointed out that in some ways the Marxian texts did not fully exemplify the features of philosophy in question. Conversely, not every instance of critical thinking will turn out to be an instance of philosophy so conceived; for example, the non-universality of the subject might easily serve as the disqualifying criterion, regardless of how well the other criteria might be exemplified. In short, all I am claiming, and all I need to claim is that there is *some* significant overlap between philosophy and critical thinking in general.

I want to end with the discussion of some problems. One stems from the fact that, in one sense, Socrates was primarily a practitioner of moral criticism, Galileo of methodological criticism, and Marx of social criticism; moreover, I believe that each was relatively uninvolved, uncritical if you will, in regard to the problems and issues in which the other two excelled. The problem is that a critical thinker today, or at least a critical thinker who is historically oriented, should presumably strive to emulate all three; however, this combined ideal is something that none of these three exemplars came even close to embodying, and if they were unable to do it, what are the chances that we ordinary mortals would be successful?

One way out of this might be to try to find other exemplars who embodied a less one-sided type of criticism. My difficulty here would be that I do not know any significantly powerful combinations.

Another possible way out might be the following. It would be to try to find some important feature shared by Socrates's moral criticism, Galileo's methodological criticism, and Marx's social criticism. My suggestion here would be to try to move the discussion to the level of logical criticism, by which I mean the analysis and evaluation of reasons and arguments from the point of view of their inferential relationships. This would be promising for the cases of Socrates and Galileo because the deep structure of their criticism does involve precisely logical criticism[52]; for the case of Marx, this remains to be shown. Moreover, to move the analysis to that level would be to move to the level of cognitive skills, which would have the advantage

[50] Ennis 1980, 17.

[51] The objection behind this clarification is due to Harvey Siegel.

[52] The case of Socrates is, of course, well-known; for the importance of logical criticism in Galileo's work, see Finocchiaro (1980, 1997, 2010b, 2014a, 2019).

of moving in the direction of interrelating them to practical-intellectual dispositions and virtues. It seems to me that as long as no one-sided reductionism was intended, such an exploration would be fruitful.

Next, one could undertake a more extended analysis of the basic notions I am using. For example, in regard to judiciousness, notice that I have defined it in terms of two components, the avoidance of one-sidedness and the avoidance of extremes; now, the first element seems to involve what might be called fair-mindedness, the second one what might be called moderation.[53] Then one might ask what, if anything, these two things have in common that they should be subsumed under a single term. The idea I should like to explore is that of regarding moderation as the special case of fair-mindedness where the two relevant "sides" are two extremities along the same dimension, so that by balancing the two extremes one is avoiding the one-sidedness of each extreme. Of course, this cannot be a mechanical process; one cannot mechanically split the difference that separates the two sides. But, of course, fairness with respect to the distinct sides of a situation is equally problematic. Both processes involve the power of discrimination and judgment, and so it does not seem inappropriate to both distinguish and to interrelate them in the manner my definition suggests.

One might also ask whether moderation is intrinsically desirable since an examination of all the reasons for and against a given position might indicate that an extreme position is called for, in which case it would be "rational" to be "injudicious." My reply here would be to say partly that it is not clear to me how one could tell that a given position was an extreme one, independently of the arguments; that is, if all the arguments favored a given position, why not say that this position is the most judicious as well as the most rational one? Second, suppose there were independent ways of establishing whether or not a position was extreme; then the situation could be described as one where there was a conflict between two applicable norms, the principle of judiciousness and the principle of rationality; now, if one decided to let the latter prevail, that would not mean that judiciousness would not have its own intrinsic value. Third, dealing more directly with the issue, one could say that if a given position was really extreme, and all the arguments favored it, then the thing to do would be to be skeptical of this ratiocinative favoritism, to look for weakness in those favorable arguments, and to try to come up with reasons favoring a more balanced position; to say this would be tantamount to giving priority to the principle of judiciousness over rationality since one would be saying that when the arguments favor an extreme position that is an indication that such arguments are not as correct as they seem. In short, this objection is analogous to questioning the value of fair-mindedness on the grounds that in a particular case all the arguments might point toward an unfair position; therefore, in this regard there is another similarity between the two elements of what I am calling judiciousness.[54]

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[53] The objections in this and the next paragraph are due to Harvey Siegel.

[54] The points discussed in the last two paragraphs also relate to more technical philosophical problems, involving the notions of dialectic, synthesis of opposites, and synthesis of differences; see Hartshorne 1987 and Finocchiaro (1988a, especially Chapters 5, 6, and 7). An additional relevance

Finally, one may raise more explicitly and pursue more systematically the general question of the interrelationship among rationality, judiciousness, practicality, universality, and critical-constructiveness. I have already mentioned that critical-constructiveness is in a sense a special case of judiciousness, that is, judiciousness with respect to the dichotomy of negative and positive evaluation. In the same vein, one could say that practicality involves judiciousness in regard to the dichotomy of theory and practice; however, here I should add that it is certainly possible (indeed very common) to carry practicality too far, in the sense of studying only topics that have a very direct and immediate connection to practical life; it should be clear that I do not mean practicality in that sense, but that I present in my course a corrective to the theoreticist conception of philosophy as the purely abstract study of questions whose answers do not make any difference one way or the other to practical life. The connection between universality and judiciousness is not immediately apparent. In regard to reasoning, on the one hand it is clear that the exercise of judiciousness is a special case of rationality, at least in the sense of being a special case of reasoning, since it does involve the giving and the assessment of reasons; on the other hand, it is not clear in what sense rationality is an instance of judiciousness. One problem would be that, since I have defined rationality as the *emphasis* on reasoning, it might seem to be an instance of the type of one-sided excess that goes against judiciousness; but even for mere reasoning, independently of any emphasis on it, it is not clear what are the two poles with respect to which judiciousness might be operating. In conclusion, though rationality and judiciousness seem to be more fundamental, more work is needed to understand the nature and interrelations among these five notions.[55]

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of Finocchiaro 1999a is that it portrays Antonio Gramsci as a critical thinker, an interpretation also elaborated in Finocchiaro (1988b, 1999a).

[55] I thank Harvey Siegel and Eugene Garver for their comments, and Alec Fisher, Matthew Lipman, Richard Paul, Jonathan Adler, and Perry Weddle for their encouragement.

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Chapter 7
The Trials of Bruno and Galileo

Abstract This essay undertakes a critical comparison and contrast between the Inquisition trials of Giordano Bruno and of Galileo. It focuses not only the facts of the two cases, but also on the procedures followed during the proceedings; on the causes and issues for their respective condemnations; and on their aftermath in modern Western culture. A main conclusion is that whereas Bruno's trial reflected a tension between religion and philosophy, Galileo's trial reflected a tension between religion and science. Although this thesis is an approximation, it is correct as a first approximation, and the qualifications can be usefully elaborated; for example, in Bruno's case, philosophy means primarily metaphysical speculation, and in Galileo's case science means natural philosophy and critical argumentation. A surprising conclusion is that, although the documentation of the two trials is similar only with respect to a single document, this document is the so-called "summary," which did indeed summarize the proceedings and was used by the judges to arrive at their verdict.

7.1 Introduction

The relationship between Bruno and Galileo can and has been studied from many angles. An obvious one is the comparison and contrast of Bruno's *Ash Wednesday Supper* and Galileo's *Dialogue on the Two Chief World Systems*. If such an obvious topic is not the subject of the present paper, that is not because I have any doubts about its fruitfulness and importance. On the contrary, I am convinced by Giovanni Aquilecchia's and Hilary Gatti's recent articles[1] that there is a significant historical and philosophical connection between these two books. Indeed I would go further and claim that Bruno's *Supper* is extremely helpful for understanding Galileo's *Dialogue*, and so deserves careful reading even if one takes a Galilean vantage point and is primarily concerned with understanding Galileo's work. That is, the connection between the *Supper* and the *Dialogue* is important not only for those who are interested in Bruno's influence, and not only for those concerned with Galileo's precursors, but also for

[1] Aquilecchia (1995a, b); Gatti 1997a. See also Aquilecchia 1955 and Massa 1973.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_7

those focused on Galileo per se.[2] However, as stated, this is not the subject of my present investigation.

Much work on the Bruno-Galileo connection could be classified as a case study in the problem of the historical and philosophical relationship between the scientific revolution and that cluster of movements that go by such labels as Hermeticism, occultism, magic, astrology, alchemy, and the cabala. In such an approach, one takes Galileo as a paradigm figure of science and Bruno as emblematic of Hermeticism and the like.[3] I am not saying, of course, that the Hermeticist interpretation of Bruno is correct,[4] but rather that even those works that criticize it may be viewed as contributions to such a case study in the interaction of science and Hermeticism, insofar as they would be reinforcing the thesis that the examples of Bruno and Galileo show that Hermeticism did *not* have a formative influence on modern science. Be that as it may, my point here is that that is not what I plan to explore in this paper.

Another fascinating and important topic is the role that Bruno's trial had in Galileo's troubles with the Inquisition. Here again, although I can appreciate the contributions of those who have written on the topic,[5] and although I am convinced that a full explanation of Galileo's trial should take into account Bruno as a factor, there is nothing I can contribute to this topic at the moment.

The Galileo-Bruno connection could also be studied from the point of view of the similarities and differences in the aftermath of their respective condemnations. We all know, of course, that each trial generated a subsequent *cause célèbre* hinging on the documentation, interpretation, and evaluation of the original episode, and that each trial came to be mythologized and symbolized from various perspectives during the various cultural struggles of the last four centuries, especially in the context of the faith versus reason controversy.[6] But again, that is beyond the scope of this paper.

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[2] An obvious example is that in the *Dialogue* Galileo expresses skepticism about locating the sun at the center of the universe, on the grounds that it is unclear that the universe has a center; but he is noncommittal about claiming that the universe is infinite, presumably because he does not want to be associated with Bruno. A less obvious example is that at the end of Day I of the *Dialogue*, Galileo describes and defends several similarities between the earth and the moon, but goes on to clarify that these do not include the existence of life on the moon because there is no water there; again, this is a way of distancing himself from Bruno by making sure that the geokinetic thesis is not equated with the claim of a plurality of (inhabited) words. Cf. Finocchiaro 1997, 89, 109–111, 222.

[3] See for example, Feingold 1984, Westman 1977, Yates 1964.

[4] For a plausible critique, see Gatti 1999.

[5] See, for example, Garin 1975, 255–281; Gosselin and Lerner (1975, 1977); Lerner and Gosselin 1986.

[6] For some recent partial accounts, see Finocchiaro 1999, Gatti 1997b; for a classic source on Bruno's case, see Gentile 1907; for what may be the first publication on both, see Hinsdale 1829; other useful accounts are Barni 1862, Blind 1889, Brinton and Davidson 1890, Fei 1935, Gallo 1932, Mondolfo 1947, Ricci (1990, 1999), Spampanato 1907, Whitman 1890, and Wilson 1878.

Instead, the aim of this paper is a comparison and contrast of their respective trials.[7] This is a subject that interests me partly because it represents the most striking and obvious point of contact between the two figures, especially for someone like myself who approaches the Bruno-Galileo connection from the side of Galileo. Another reason is that some of the above mentioned subtopics presuppose this one, in the sense that knowledge of the influence of Bruno's trial on Galileo's or of the interaction of the two aftermaths depends on knowing what happened in each trial in a way in which the latter knowledge does not depend on the former. Moreover, knowledge of the trials is even relevant to the science versus Hermeticism issue. For example, the case for Bruno's Hermeticism would be strengthened if it were true, as Yates asserted, that "the legend that Bruno was prosecuted as a philosophical thinker, was burned for his daring views on innumerable worlds or on the movement of the earth, can no longer stand"[8]; and that "it was probably mainly as a magician that Bruno was burned, and as the propagator throughout Europe of some mysterious magico-religious movement [which] may have been in the nature of a secret Hermetic sect, and may be connected with the origins of Rosicrucianism or of Freemasonry."[9] However, Yates's interpretation is not correct, as my account of Bruno's trial will try to show.

But, worthwhile as it may be to criticize a well-known and widely accepted thesis by a great scholar, I want to place my account of the two trials into the context of a larger issue. It is this. If the trial of Galileo epitomizes the conflict between science and religion, then the trial of Bruno may be said to epitomize the clash between philosophy and religion. Similarly, just as many have criticized the science-religion conflictual interpretation of Galileo's trial as an oversimplification, so one could regard as oversimplified the conflictual account of Bruno's trial in terms of philosophy versus religion. However, the time may be ripe for pointing out that calling such conflictual interpretations oversimplified may itself be an oversimplification.

My point would be that on the one hand we must admit that most human actions and historical developments are complex phenomena that have many defining aspects and a multiplicity of contributing causes; on the other hand, it is useful to distinguish an over-simplification from a simplification, and it would be wrong-headed to deny that simplifications can be proper and can contribute considerably to historical, philosophical, and scientific understanding. Applied to our case, this distinction implies that if we are willing to take as viable simplifications rather than flawed over-simplifications the conflictual accounts of the two trials, then their comparison and contrast should enable us to shed some light on the similarities and differences between science and philosophy.

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[7] A look at Salvestrini's (1958) *Bibliografia* reveals that in the relatively short list of works on the Bruno-Galileo connection (30 out of 1750 entries), those dealing with a critical comparison of the two trials are surprisingly few (no more than half a dozen). Of course, in the past half a century since that bibliography was last updated, such critical comparisons have become more common; see, for example, Blumenberg 1987, 353–432.

[8] Yates 1964, 355.

[9] Yates 1973, 542; see also Yates 1967.

One final caveat. Some might feel twinges of anachronism about speaking of science in the case of Galileo and point out that the proper term would be natural philosophy. Now, I could be easily persuaded to concede the linguistic, terminological point here, but I do not think that this concession would affect the substance of the issue. For if one of the proper relata in Galileo's case is natural philosophy, then the corresponding one in Bruno's case would be metaphysical philosophy and the potential contrast would remain. Then the instructive contrast would be between natural philosophy and metaphysical philosophy, namely between natural philosophy of the Galilean kind and metaphysical philosophy of the Brunian kind, or in short between science (in the a restricted sense referring to natural science) and philosophy (in a restricted sense referring to metaphysics or first philosophy).

With these clarifications in mind and these preliminaries out of the way, we can now go on to compare and contrast the details of the two trials to see whether any light can be shed on these issues. I shall begin with an account of Bruno's trial.[10] Then I shall give a briefer account of Galileo's trial.[11] I shall go on to briefly address questions of comparison and contrast. And I shall conclude with a discussion of interpretive issues, primarily whether and how the two trials can be construed in terms of philosophy versus religion and science versus religion.

7.2 Bruno's Trial

In 1591 while in Frankfurt, Bruno received a letter from Venetian aristocrat Giovanni Mocenigo inviting him to go to Venice to teach the nobleman the arts of memory and invention. Bruno accepted, and so in October he moved to Italy. However, he did not immediately enter the service of his new patron; instead he went to Padua for about two months. Eventually, he moved to Mocenigo's house and started tutoring him. But their relationship quickly turned sour.

On May 23, 1592, Mocenigo filed a written complaint against Bruno with the Venetian Inquisition. Bruno was arrested the same day. Two days later Mocenigo filed a second complaint, and four days after that a third one.

Mocenigo's charges may be summarized as follows.[12] The nobleman alleged [1] that Bruno spoke ill of the Catholic Faith, Church, and officials; [2] that he

[10] It should be obvious to any student of Bruno that my account relies heavily on Firpo 1993.

[11] It should be obvious to any student of Galileo that my account in this paper relies heavily on Finocchiaro 1989.

[12] See Firpo 1993, 16 for nos. [1]–[8] and [25]–[27]; and Firpo 1993, 21 for no. [9]. My numbering and formulation of the charges against Bruno follow rather closely the numbering and formulation given by Firpo (1993), so much so that many of my descriptions are simply translations of Firpo's wording. However, he formally identifies only 24 charges, whereas I have found it useful to expand the list to 40 by including a few other incidental charges as well as the censures of Bruno's books, which Firpo discusses but does not treat as additional charges. Thus, what here I label charges nos. [25]–[27] are three of Mocenigo's charges which Firpo (1993, 16) chooses not to denote with numbers because Bruno was able to easily defend himself from them.

held erroneous opinions on the Trinity, the divinity of Christ, and incarnation; [3] that he held erroneous opinions about the facts of Jesus' life and death; [4] that he held erroneous opinions on transubstantiation and on the holy mass; [5] that he maintained the existence of a plurality of worlds and their eternity; [6] that he believed in metempsychosis and the transmigration of human souls into animals; [7] that he approved of and practiced the magical arts; [8] that he denied the virginity of Mary, the mother of Jesus; [9] that he condoned and indulged in sexual sins; [25] that he spoke ill of the doctors of the Church; [26] that he denied that sins deserve punishment; and [27] that he had a previous criminal record with the Inquisition.

As a result of these accusations, the Inquisition immediately started conducting interrogations of the defendant. Within a ten-day period at the end of May and beginning of June, there were six formal examinations, and then a seventh one at the end of July. These depositions are extremely complex, interesting, and informative, and they deserve extended analysis. Here suffice it to say the following. Generally speaking, Bruno denied all charges except the two dealing with the doctrine of the universe and the doctrine of the soul. In regard to these, he explained his views and justified himself by saying that he was speaking and reasoning as a philosopher. His rebuttals ranged from flat denials to more or less qualified ones. For example, he adamantly rejected the charge that he did not believe in the virginity of Mary.[13] And regarding the Trinity, he admitted having had doubts and having felt difficulties in trying to make sense of the mystery of three persons in one God, but he argued that these were internal private questionings which did not amount to disbelief, public rejection, or the holding of erroneous beliefs.

At this stage of the proceedings, the Inquisition also conducted interrogations of four witnesses. Three had been named by Mocenigo: a book dealer named Giambattista Ciotti, a second book dealer named Jacobus Brictanus, and the well-known Venetian aristocrat Andrea Morosini; no one of them supported Mocenigo's charges. By contrast, the witness named by Bruno, Dominican friar Domenico da Nocera, did testify in his favor.

Although Mocenigo's accusations were thus generally denied by Bruno and unsupported by other witnesses, two relatively damaging things did emerge out of Bruno's own depositions, besides his views on the universe and on the soul, namely his life of apostasy (charge no. [10]) and his record of controversial author. From Bruno the Inquisition indirectly learned such details of his biography as these: that he had once been a Dominican friar in Naples and Rome, where he had had problems with the Inquisition, and then he had left the order; that for fifteen years he had wandered throughout Europe, abandoning the practice of Catholicism and consorting with Protestants in such places as Geneva, London, and Wittenberg; and that he regularly read books forbidden by the Index. And the Inquisition also learned that Bruno had authored a considerable number of books; that his books dealt with highly controversial topics; that they contained highly unorthodox views; that they potentially had heretical implications; and that they had often been published with falsified imprints (for example, Venice instead of London).

[13] Cf. Firpo 1993, 175–176.

Bruno must have realized that such admissions and revelations were bound to lead to more serious trouble. Thus, several weeks after the initial depositions, during the interrogation held on July 30, 1592 he confessed some wrongdoing, expressed sorrow, showed a willingness to undergo some punishment, begged forgiveness, and promised to reform himself.[14] The practice of the Inquisition was such that this act of submission ensured that his life would be spared.

However, the trial did not come to an end then because when the central office of the Inquisition in Rome was informed about the case, they thought it was important enough to want to handle it directly themselves. They thus requested that Bruno be extradited to Rome. Because of the independence of the Republic of Venice, such extradition required the approval of the Venetian government and was usually denied in accordance with Venetian law. On the other hand, the law allowed exceptions. And so eventually the Venetian government yielded to the Inquisition argument based on the fact that Bruno was not a Venetian citizen and on the allegation that the Roman Inquisition had started proceedings against Bruno a long time earlier. After about six months of negotiations, Bruno was transferred to Rome in February 1593.

Nothing significant happened until the Fall when a new complaint was filed against Bruno with the Venetian Inquisition. This time the plaintiff was a Capuchin friar named Fra Celestino da Verona, who had been a fellow prisoner of Bruno in Venice. Here it is important to add that Celestino had already undergone a trial, condemnation, and abjuration with the Inquisition in Rome in 1587; that he found himself in the Inquisition prison in Venice in 1592–1593; and that he would later be tried again in Rome 1599 and burned alive at the stake five months before Bruno.[15] We will see later that the Inquisition did take into account the quality and reliability of its sources and informants, but for now we will focus on this new development.

Celestino repeated some of the accusations that had been made by Mocenigo but added several new ones. That is, the Capuchin friar also charged[16] [11] that Bruno maintained that Jesus had sinned; [12] that Bruno held erroneous opinions about hell; [13] that he held erroneous opinions about Cain and Abel; [14] that he had spoken ill of Moses; [15] that he had spoken ill of the prophets; [16] that he had denied that the Church's dogmas are credible; [17] that he disapproved of praying to the saints; [18] that he had spoken ill of the breviary; [19] that he had a habit of uttering blasphemies and displaying blasphemous gestures; and [20] that he planned to burn down the monastery and escape if he should be forced to re-join the Dominican order.

The plaintiff also named three witnesses who had been held in the same prison as himself and Bruno: a Carmelite friar named Giulio da Salò; a Neapolitan carpenter by the name of Francesco Vaia; and someone named Matteo de Silvestris. One of these witnesses in turn named a fourth fellow prisoner, whose name was Francesco Graziano. These four witnesses were immediately interrogated by the Venetian Inquisition, and they confirmed almost all of Celestino's and many of Mocenigo's previous accusations. Two relatively new accusations were also added: De Silvestris charged

[14] Cf. Firpo 1993, 198–199.

[15] Firpo 1993, 43–45.

[16] Firpo 1993, 48.

[21] that Bruno held in contempt holy relics, while Graziano charged [22] that Bruno disapproved of the veneration of sacred images. Having collected these four depositions, the Venetian Inquisition sent them to Rome together with Celestino's complaint.

During the same period (autumn 1593), the Roman Inquisition began its own examination of Bruno, subjecting him to seven distinct interrogations. The first one, or the eighth one since his original arrest in Venice, dealt with Mocenigo's charges. The next five depositions (ninth through thirteenth) involved the new group of charges by Celestino and the new witnesses. The fourteenth interrogation examined once again the plurality of worlds and the infinity of the universe. The fifteenth deposition dealt with the question of the magical arts.[17] Generally speaking, Bruno denied all new charges except for the one about blasphemy, in regard to which he confessed to some occasional and minor transgressions. He continued to reject all the old charges except for those dealing with the philosophical doctrines of the universe and of the soul. And he also added various interesting nuances and qualifications about the plurality of worlds[18] and about the magical arts.[19]

The proceedings so far ended what may be called the accusatory phase of the trial and were followed by what may be called the re-examination phase. In this next phase, first the Inquisition's prosecuting attorney, Marcello Filonardi, compiled a systematic list of charges against Bruno based on the initial examination of all plaintiffs and witnesses.[20] Then Bruno was given a copy of Filonardi's list of charges (edited to delete names), and was required to compile a suggested questionnaire for the re-examination of the witnesses.[21] Next, in the first three months of 1594, the Inquisition in Venice conducted re-examinations of the plaintiffs and of all the significant witnesses who were available: Mocenigo, Celestino, Graziano, de Silvestris, and Ciotti. The aim here was twofold, namely (1) to try to determine whether a particular testifier knew anything about charges which he had not mentioned in the initial phase but had been mentioned by others; and (2) to check the consistency of his new deposition with his own earlier one. Almost all of the charges were confirmed in the sense that they were supported by more than one person. And one new accusation emerged during Graziano's re-examination when he charged [23] that Bruno denied the truth of the story that three kings had paid homage to the baby Jesus.

In the spring and summer of 1594 copies of the proceedings were made. One of these was given to Bruno to use for his defense. He wrote an 80-page memorandum for this purpose, and submitted it in December.

Another development of that year was that Mocenigo filed a fourth complaint to the effect [24] that in his book *Cantus Circaeus*, Bruno had insulted the pope by depicting him as a pig. Thus Bruno duly underwent his sixteenth interrogation

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[17] Firpo 1993, 60–61; Summary [196]–[197]. The latter is a reference to the numbered paragraphs in the Summary of Bruno's trial, as found in Mercati 1942, 55–119 and in Firpo 1993, 247–304.

[18] Summary [93]–[97].

[19] Summary [122]–[123].

[20] Summary [238]; Firpo 1993, 63.

[21] Summary [239]; Firpo 1993, 63–64.

regarding this charge and the latest one by Graziano regarding the adoration of the Magi. Bruno denied both accusations.

The proceedings were now ripe for the next phase, namely the evaluation of the evidence by the Congregation of the Holy Office and its consultants. In January and February 1595, at several meetings of this congregation presided by pope Clement VIII, the proceedings of Bruno's trial were read. It soon became obvious that Bruno's books were an integral part of the trial, and so the Inquisition ordered that a formal censure of them be produced by its consultants, so that the trial could proceed further. However, the censure of Bruno's books proved to be a very difficult task. Part of the problem was their sheer number. In part there was the problem of obtaining copies of the books. The Inquisition never did collect more than about half a dozen of them. A full year and one half after the initial decision to censure the books, in September 1596 the Inquisition decided to appoint three additional consultants to help its regular consultants with the task.

In early 1597, about two years after being commissioned, the book censures were apparently ready. These documents have not survived, but we do know that they amounted to sixteen pages. The plan was to give the defendant a copy of the censures, but only after one more interrogation (the seventeenth) conducted by the inquisitors with the benefit of the censures. This deposition focused on the question of the Trinity and Incarnation, regarding which Mocenigo's charge had not been confirmed by any other witnesses, and on the question of the plurality of worlds and infinity of the universe, regarding which Bruno had admitted holding the doctrine.[22] Bruno's answers were the same as before. After he was given the censures, for the greater part of 1597, Bruno underwent a long series of interrogations in which he replied to the censures of his books. These interrogations may be collectively called his eighteenth deposition,[23] and Bruno's replies amounted to sixty pages of the proceedings. Although the original documents of these replies have also not survived, a Summary of them has survived, and on the basis of this Summary we can reconstruct both the censures and the replies.[24]

The censures focused on eleven theses which can be gleaned from Bruno's books and were deemed objectionable by the Inquisition's consultants. The content and the likely location of these theses may be summarized as follows:[25] [30] The universe is eternal, or temporally infinite (*De triplici minimo et mensura*, Frankfurt, 1591). [31] The universe is spatially infinite (*De l'infinito, universo e mondi*, London, 1584). [32] There exist an infinite plurality of worlds similar to ours in many ways, such as having intelligent life (ibid.). [33] The individual immortality of the human soul is a questionable proposition (*Infinito* and *De la causa, principio et uno*, London, 1584). [34] Substance can neither be created nor destroyed, but only changed in its manifestations, i.e., substance is "conserved" (*Causa*). [35] The earth moves with the

[22] Summary [28], [29], and [92]; Firpo 1993, 78–79.

[23] Following Firpo 1993, 79.

[24] As indicated in a previous note, this Summary is found in Mercati 1942, 55-119 and in Firpo 1993, 247–304.

[25] Cf. Firpo 1993, 80–85; Summary [252]–[261].

several Copernican motions (*La cena de le ceneri*, London, 1584). [36] The stars are animate, i.e., possess rational souls (*Cena*). [37] The earth is animate, i.e., possesses a rational soul (*Cena*). [38] On the question of the relationship of soul and body, the human soul is not the form of the body but is a spiritual substance inhabiting the body in a manner analogous to how a pilot guides a ship (*Causa*). [39] The Holy Spirit may be identified with the soul of the universe (*Causa*). And [40] there is evidence for the pre-Adamite thesis, that some human beings existed prior to Adam and Eve (*De innumerabilibus, immenso et infigurabili, seu de universo et mundis*, Frankfurt, 1591, and *De monade, numero et figura*, Frankfurt, 1591).

By the end of 1597, the examination of Bruno about these theses and the Inquisition's censures of them had been concluded. In March of the following year a summary of the proceedings was compiled, probably by the Inquisition prosecutor; this is a document that has survived and provides for us invaluable information about the trial. However, it was also a useful document for the Inquisition because by then the full proceedings amounted to about 600 pages (or 300 folios) and had thus reached unmanageable proportions for the business of the Congregation of the Holy Office. On the other hand, the summary of this bulk ran to 60 pages (or 30 folios). The rest of 1598 was devoid of proceedings because the pope and the cardinal-inquisitors were away from Rome for about nine months, to celebrate the re-occupation of Ferrara.

Before we go on to the next developments, it should be mentioned that the Inquisition apparently judged that its case against Bruno was legally weak because five of the six hostile witnesses were themselves criminals; because Mocenigo's charges had not been confirmed by any respectable person; and because Bruno himself had denied almost all the charges. In such cases of inconclusive evidence, standard practice offered the option of torture, namely interrogating the defendant while he was subjected to torture; guilt or innocence would then be established depending on whether he confessed or denied the charges under these conditions. This evaluation of the situation is clear and explicit from the minutes of the Inquisition meeting of September 9, 1599.[26]

On the other hand, Bruno had already admitted some of the charges, and the examination of his books (however incomplete and problematic) had revealed some questionable theses. Thus in January 1599, the Inquisition decided to explore a middle course: a list of unquestionably heretical theses definitely held by Bruno would be submitted to him; he would be requested to reflect on them; and he would be asked to declare himself ready to abjure them. The idea was to try to convict him on a short list of charges with respect to which the case against him was strongest; if he cooperated, that might essentially end the trial; if he did not, they could always resort to torture for all the charges. Here it is important to note that this proposal was devised by Robert Bellarmine, who at the time was merely an Inquisition consultant, a position he had held for about two years; he would be appointed cardinal and member of the Congregation of the Holy Office two months thereafter (in March 1599).

Within a few days, Bellarmine and the commissary of the Inquisition had selected eight propositions from Bruno's books and from the trial proceedings. These eight

[26] Firpo 1993, 96–98, 327–329.

theses were not regarded as a complete list, and the plan was to continue examining his books and depositions more carefully to identify additional erroneous opinions.[27] At its next meeting, the full Congregation of cardinal-inquisitors presided by the pope approved the list for Bruno's consideration and abjuration. A few days after that, the defendant was presented with the list of eight theses and was given six days to decide.

This list has not survived, but it probably corresponded to the list of censured theses which had been the subject of the latest proceedings. As we saw, those propositions deal with cosmological, metaphysical, and psychological questions (psychological in the sense of philosophical psychology or doctrine of the soul). Another thesis included among the eight probably dealt with the Trinity, the divinity of Christ, and incarnation; this was the subject of Mocenigo's second charge, and Bruno had confessed doubts and difficulties, but had been careful to deny any positive opinions and to disclaim any relevant writing.[28]

On January 25, 1599, in what may be regarded as Bruno's nineteenth deposition, he appeared before the Inquisition and stated that, if the Holy See and His Holiness declared that the eight propositions were heretical, he would be ready to retract them; at the same time, he presented a memorandum in his defense.[29]

A week later, at a meeting of the Inquisition presided by the pope, it was decided to send three officials to Bruno: the general of the Dominicans, the Inquisition commissary, and Bellarmine. They were to tell Bruno formally that the eight propositions were heretical, their condemnation being not a recent development but going back to the most ancient Fathers of the Church; further, that if he was ready to abjure them as heretical, he would only have to do some penance; but that otherwise, he would be issued an ultimatum of forty days to change his mind or be executed.[30]

Bruno appeared to submit. For on February 15, 1599, in what may be regarded as his twentieth deposition, he declared himself ready to admit that the eight propositions were heretical and to abjure them.[31] Moreover, on April 5, 1599, during the Easter visit to the Inquisition prisoners, the cardinal-inquisitors received a memorandum from Bruno, in which he seemed to have written down his retraction.[32] In fact, at a subsequent meeting of the Congregation of the Holy Office (August 24, 1599), Bellarmine reported that Bruno's April 5 memorandum contained a clear retraction of the eight theses, except for two points regarding which he should explain himself better: an aspect of the first thesis involving the 'Novationist' heresy (which is probably a reference to the issue of the Trinity[33]), and an aspect of the seventh thesis involving the question whether the relation between body and soul is like that of a

[27] On this specific point, as on so many others, I am following Firpo (1993).

[28] This is my interpretation of Bellarmine's talk about the "Novatianist" heresy, in his report to the Inquisition at the meeting of August 24, 1599; cf. Firpo 1993, 94–96, 323–325; also cf. note 33 below.

[29] Firpo 1993, 93, 136 n. 21, 340–341.

[30] Firpo 1993, 93–94, 313–315, 341.

[31] Firpo 1993, 94, 136 n. 23, 316–317, 341.

[32] Firpo 1993, 94, 317–323.

[33] In saying this, I am following a suggestion of Mercati (1942, 37 n. 56) and Mondolfo (1947, 30), as against Firpo 1993, 95, 137 n. 27. Novatian was a third-century Roman priest who led a schismatic

pilot and a ship. The cardinals decided to bring the trial to a conclusion at the first meeting to be presided by the pope; they also granted Bruno's request for eyeglasses, paper, pen, and ink, but not for a knife and compass.[34]

On September 9, 1599, at a meeting of the Inquisition presided by the pope, Bruno's case was discussed at length. There was a consensus among the experts and consultants that, except for Bruno's own admissions, confessions, and statements, the legal argument against him was so far insufficient and that torture was required to determine his guilt or innocence for the unproved charges. After hearing all the opinions, the pope decided that Bruno be ordered to retract the heretical opinions which he had admitted, and that the various depositions be examined more carefully for additional such opinions.[35] The following day, in what may be labeled Bruno's twenty-first deposition, he declared he was ready to admit his errors and do anything which the Church ordered. However, he also presented a memorandum addressed to the pope.[36]

His memorandum reiterated and defended Bruno's opinions and thus belied the declarations he had been making since February 15. It seems that as the actual conclusion and sentencing were approaching, he could not bring himself to abjure philosophical opinions which he felt were not heretical and had never been formally declared to be heresies. He was thus given the 40-day ultimatum to repent or die.[37]

Bruno did not waver from his latest refusal, despite repeated attempts by the Inquisition to convince him to submit. For example, on December 21, during the Christmas visit to the Inquisition prisoners by the cardinal-inquisitors, in what may be labeled Bruno's twenty-second and last deposition, he told them that he had nothing to retract.[38] And when the general of the Dominicans and his deputy tried next, he told them that his views were not heretical and had been misinterpreted by the officials of the Holy Office.[39]

Thus, on January 20, 1600, at a meeting of the Inquisition presided by the pope, after hearing the opinions of the expert consultants and of the cardinals, the pope decided that a sentence of condemnation be issued against Bruno and that he be handed over to the civil authorities for execution. On February 8, Bruno was brought from the Inquisition prison to the house of cardinal Madruzzi, where the sentence

movement. Bellarmine's talk of the "Novatianist heresy" probably refers to the unorthodox view of the relationship between Father and Son advanced in Novatian's *De Trinitate*; although this work had traditionally been attributed to Tertullian, in 1579 Novatian had been shown to be its true author and Bellarmine was aware of this discovery; moreover, although the term Novatianist heresy commonly referred to a different error (namely, the denial of the Church's authority to forgive certain sins) which Novatian also committed, it is implausible to take the term in this sense because Bruno did not question the Church's authority on this point, whereas he did admit having had puzzles about the Trinity.

[34] Firpo 1993, 94–96, 323–325.

[35] Firpo 1993, 96–98, 327–329.

[36] Firpo 1993, 98–101, 138 n. 44.

[37] Firpo 1993, 98–101, 329–333.

[38] Firpo 1993, 101–102, 333–335.

[39] Firpo 1993, 101–102, 333–339.

was read to him in the presence of the cardinal-inquisitors, other Inquisition officials, and the general public.

Although the full text of the official sentence has not survived, from the partial text that is available and other documents, it seems that Bruno was condemned as an unrepentant, obstinate, and treacherous heretic.[40] His heresies apparently included the eight theses of Bellarmine's list, concerning which there was no question that Bruno did hold them, but only whether and when they had been formally declared to be heretical. However, his heresies also included the many charges by Mocenigo, Celestino, and the other hostile witnesses, regarding which the question was whether Bruno did really hold them, although there was no question that they contradicted Catholic doctrine. Here the logic of the Inquisition procedure was that his obstinacy in not retracting the theses to which he had confessed, rendered him guilty of the other opinions of which he had been accused but which had not been otherwise proved.

Finally, the end came on February 17 in Campo dei Fiori: stripped naked and with his tongue tied to prevent him from sputtering offensive utterances, he was tied at the stake and burned alive. Among other reasons, the idea was to execute such heretics without spilling blood and to give them a last opportunity to repent.

7.3 Galileo's Trial[41]

In 1543, Copernicus's book entitled *On the Revolution of the Heavenly Spheres* gave a new argument for an idea first advanced in ancient Greece but generally rejected: that the earth turns daily on its axis and yearly around the sun, and so does not stand still at the center of the universe. Its achievement was to demonstrate mathematically that the known details about the heavenly bodies could be explained more simply and coherently if the sun rather than the earth is placed at the center. This demonstration strengthened the idea, but did not conclusively establish it; for it was a hypothetical argument and the traditional counter-arguments remained unrefuted.

To summarize them, the earth's motion seemed epistemologically absurd because it contradicted direct sense experience. It seemed astronomically false because it had consequences that could not be observed, such as the similarity between terrestrial and heavenly bodies, Venus's phases, and annual stellar parallax. It seemed mechanically impossible because the available laws of motion implied that bodies on a rotating earth would, for example, follow a slanted rather than vertical path in free fall, and would be thrown off by centrifugal force. And it seemed theologically heretical because it contradicted the words and the traditional interpretations of the Bible. Copernicus was aware of these objections and so delayed publication until the end of his life.

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[40] Firpo 1993, 102, 342.

[41] As previously mentioned, this account is adapted from Finocchiaro 1989; to a lesser extent, it also relies on Finocchiaro (1980, 1997).

Galileo was born in 1564 in Pisa. Although his primary interest was physics, as a mathematics professor, he also taught astronomy. But he did not regard Copernicanism as sufficiently well-established to teach it in his courses; instead he covered traditional geostatic astronomy. Nor did he directly pursue Copernicanism in his research, which consisted of investigations into the laws of motion. This work was revolutionary, for he was constructing a new science. He soon realized that his new physics implied that the earth's rotation was mechanically possible and so could be used to strengthen Copernicanism by removing the mechanical objections.

Still, Galileo was dissatisfied with the idea, especially because of its observational astronomical difficulties. Only the invention of the telescope changed that.

In 1609, Galileo managed to make significant improvements to it and constructed an astronomically useful telescope that could not be duplicated by others. With this instrument, he soon made several startling discoveries, such as lunar mountains, Venus's phases, and Jupiter's satellites. Personally, these discoveries enabled him to resign his professorship at Padua and accept the position of "Philosopher and Chief Mathematician to the Grand Duke of Tuscany" in Florence. Scientifically, they led him to reassess Copernicanism, for they removed most of the empirical-astronomical objections and added new favorable evidence. Thus, he felt not only that the geokinetic theory was simpler and more coherent (as Copernicus had demonstrated), not only that it was mechanically better (as his own new physics showed), but also that it was empirically superior (as the telescope now revealed). However, although he had published his telescopic discoveries, he had not yet done so for his new physics.

Moreover, the theological objections had not yet been refuted. Galileo must have also sensed their potentially explosive character. Thus, at first he did not answer them despite the fact that many attacked his telescopic observations on biblical grounds. However, in 1613 the grand duchess Christina confronted one of his followers (named Benedetto Castelli) with the biblical objection: Copernicanism must be wrong because many biblical passages state or imply that the earth stands still. Castelli's answer satisfied the duchess as well as Galileo, when Castelli informed him of the incident. In his letter, Castelli did not explain what his answer had been but stated that it was such that Galileo would have been proud; presumably, the two of them had discussed the topic before, and so Castelli's answer must have been along the lines of Galileo's own answer. The details of Galileo's answer are contained in the letter which he felt the need to write to Castelli. Galileo argued that the objection has three fatal flaws: first, it attempts to prove a conclusion (the earth's rest) on the basis of a premise (the Bible's commitment to the geostatic system) which can only be ascertained with a knowledge of that conclusion in the first place; second, the objection is a nonsequitur, since the Bible is an authority only in matters of faith and morals, not in scientific ones; and third, it is questionable whether the earth's motion really contradicts the Bible, and an analysis of one of the most relevant passages (Joshua 10:12-13) shows that it cannot be easily interpreted in accordance with the geostatic theory, but that it accords better with the geokinetic view.

Although unpublished, Galileo's letter circulated widely. Thus, the traditionalists soon passed to the counterattack. In December 1614 in Florence, a Dominican friar (named Tommaso Caccini) preached a sermon against mathematicians in general and

Galileo in particular. In February 1615, another Dominican (named Niccolò Lorini) filed a written complaint against Galileo with the Inquisition in Rome, enclosing his letter to Castelli as incriminating evidence. Then in March, Caccini made a personal appearance before the Roman Inquisition, charging Galileo with heresy based on hearsay evidence.

The Inquisition conducted an investigation. The consultant who examined Galileo's letter to Castelli reported that it did not deviate from Catholic doctrine. The cross-examination of witnesses exonerated Galileo since the hearsay charges were found to be baseless. However, the Inquisition also consulted its experts for an opinion on the status of Copernicanism.

In February 1616, they reported unanimously that Copernicanism was philosophically and scientifically untenable and theologically heretical. But the Inquisition apparently had some misgivings about this judgment, for it issued no formal condemnation. Instead two milder consequences followed.

First, Galileo was warned to stop defending the truth of the earth's motion. The warning was conveyed privately and orally by Cardinal Robert Bellarmine, with whom Galileo was on good terms, despite their intellectual differences. Galileo was reported to have promised to obey. However, the exact content, form, and circumstances of this warning and this promise are not known, and the documents allow different interpretations.

Second, in March 1616 the Congregation of the Index issued a decree containing three main points. It prohibited completely and condemned a book (by a Carmelite father named Paolo Antonio Foscarini) claiming to show that the earth's motion is compatible with the Bible. It temporarily suspended Copernicus's book, pending correction and revision. (The so-called "corrections" were published in 1620.) And it ordered analogous censures for analogous books. Galileo was not mentioned at all.

This decree was vague, and the warning confusing. So Galileo managed to obtain from Bellarmine a clear statement of what had happened and how he (Galileo) was personally affected. Bellarmine's certificate declared that Galileo had been neither tried nor condemned, but rather personally notified of the decree and told that it meant that the truth of the earth's motion could not be defended.

For the next several years Galileo complied. Then in 1623 Cardinal Maffeo Barberini was elected Pope Urban VIII. He was a well-educated Florentine and a great admirer of Galileo. In 1616, he had been instrumental in preventing the formal condemnation of Copernicanism; and in 1620 he had written a poem praising Galileo. Thus, in 1624 Galileo went to Rome to pay his respects to the new pontiff; he was warmly received by the pope, who granted him weekly audiences for six weeks.

The details of these conversations are unknown. But Urban apparently did not think that Copernicanism was heretical or a forbidden topic of discussion, but that it was a dangerous doctrine whose study required special care. His favorite objection was that it could never be proved absolutely true because the earth's motion is not directly perceivable and all its supporting arguments must be hypothetical; but any observed effects could always be produced by other causes, a possibility that could not be excluded on pain of denying God's omnipotence.

Consequently, Galileo felt freer to re-examine the topic. After many delays, in 1632 he published the *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican.* He had done many things to avoid trouble. In the preface he included suggestions by the authorities, claiming that the work was being published to prove that Catholics knew all the scientific evidence, and thus the anti-Copernican decree of 1616 was motivated by religious reasons, not scientific ignorance. The preface also stated explicitly that, although the scientific arguments favored Copernicanism, they were inconclusive, and thus the earth's motion remained an hypothesis. He also agreed to end the book with a statement of the pope's favorite argument from divine omnipotence. To make sure he would not be seen as defending the geokinetic thesis, he wrote the book as a dialogue among a traditionalist, a Copernican, and a neutral interlocutor; and he filled the discussion with qualifications to the effect that its purpose was to convey information, not to decide the issue. Finally, he obtained imprimaturs from various official censors.

The book was well received in many circles, but complaints arose. The most serious complaint involved a document found in the Inquisition file of proceedings for 1615–1616. It reads like a report of what transpired when Bellarmine warned Galileo to abandon Copernicanism. The cardinal had died in 1621 and so was no longer available. The document states that in February 1616 Galileo had been prohibited not only from defending the geokinetic thesis, but also from discussing it in any way whatsoever. The just-published book clearly violated this special injunction. To be sure, the document does not bear Galileo's signature and so was of questionable legal validity. Under different circumstances this technicality would have been decisive. But at the time the politics of the Thirty Years' War had rendered the pope too vulnerable. (The document's origin is unknown; it is not exactly a forgery, but probably the creation of an over-zealous official.)

Another complaint claimed the book only paid lip service to conducting a hypothetical discussion, but really treated the earth's motion in an unconditional manner. Another charge alleged the work was actually a defense of the earth's motion because it criticized the anti-Copernican arguments and presented favorably the pro-Copernican ones.

The pope did not immediately bring in the Inquisition, but took the unusual step of first appointing a special commission. When they submitted their report, he felt he had no choice but to forward the case to the Inquisition. So Galileo was summoned to Rome.

After many attempts to delay, on January 20, 1633 Galileo left Florence for Rome. When he arrived, he was not imprisoned and was allowed to lodge at the Tuscan embassy, but was ordered not to socialize.

At the first hearing (on April 12), Galileo was asked about the *Dialogue* and the events of 1616. He admitted receiving from Bellarmine the oral warning that the earth's motion could not be defended, but only discussed hypothetically. He denied receiving a special injunction not to discuss the topic in any way whatsoever, and he introduced Bellarmine's certificate as evidence. He also claimed that the book did not defend the earth's motion, but rather showed that the favorable arguments were inconclusive, and so did not violate Bellarmine's certificate.

The special injunction surprised Galileo as much as Bellarmine's certificate surprised the inquisitors. Thus it took three weeks before they decided on the next step. In the meantime Galileo was detained at the Inquisition headquarters, but allowed to lodge in the chief prosecutor's apartment. The inquisitors opted for some out-of-court plea bargaining: they would not press the most serious charge (violation of the special injunction), but Galileo would have to plead guilty to a lesser charge (unintentional transgression of the warning not to defend Copernicanism). He requested a few days to devise a dignified way of pleading guilty to the lesser charge. Thus, at the second hearing (April 30), he stated that the first deposition had prompted him to re-read his book; he was surprised to find that it gave readers the impression that the author was defending the earth's motion, even though this had not been his intention. He attributed his error to wanting to appear clever by making the weaker side look stronger. He was sorry and ready to make amends.

After this deposition, Galileo was allowed to return to the Tuscan embassy for lodging. A report was compiled for the pope, summarizing the events from 1615 onward. Reading it did not resolve Urban's doubts about Galileo's intention, and so he ordered an interrogation under the *verbal* threat of torture.

On June 21, Galileo underwent such a formal interrogation. The result was favorable; that is, even when so threatened, he denied any malicious intention and showed he would rather die than admit malice. The next day, at the convent of Santa Maria sopra Minerva in Rome, he was read the sentence and recited a formal abjuration.

The sentence found him guilty of a category of heresy intermediate between the most and the least serious. The objectionable beliefs were the cosmological thesis that the earth moves and the methodological principle that the Bible is not a scientific authority. The book was banned. And Galileo was condemned to house arrest till his death, which occurred in 1642.

7.4 Similarities and Differences

The facts of the two trials invite many comparisons and contrasts, too many to discuss them in any detail here. However, it is worthwhile to briefly mention some before proceeding to an analysis of our main theme (religion versus philosophy, and religion versus science).

One cannot help but being impressed by the Inquisition's concern for propriety and fair procedure. For example, Bruno was given a copy of the charges and was allowed to suggest cross-examining questions when his trial proceeded from the accusatory to the re-examination phase, and he was given a copy of the full proceedings and allowed to write a lengthy defense before the trial moved to the evaluation phase. In Galileo's case, one of the most striking things is that he was never held in the Inquisition prison during the trial, and that even when he was detained at its palace

between his first and second deposition, he was allowed to lodge in the prosecutor's apartment.[42]

A striking parallel between the two trials is the Inquisition's attempt to reach a compromise by exploring the willingness of the defendants to plead guilty to lesser charges, presumably in exchange for leniency in punishment. In Bruno's case, the compromise is represented by Bellarmine's suggestion to explore his willingness to abjure a short list of theses which Bruno had admitted in his depositions or clearly held in his books. This was a compromise in the sense that Bellarmine was realistic enough to understand that it was hopeless to try to convict Bruno of all charges, but also zealous enough to be determined not to let him go unpunished. In the end this did not work, in the light of what might be called Bruno's own uncompromising attitude toward his own philosophical beliefs; but Bellarmine's compromise could have worked, and for a while it looked like it would. In Galileo's case the compromise is represented by the Inquisition commissary's out-of-court discussion with Galileo after the first deposition, when Galileo's denial and the evidence of Bellarmine's certificate made it clear that the prosecution had no case regarding the alleged violation of the special injunction; the compromise worked in the sense that the defendant did plead guilty to the charge that his *Dialogue* defended the earth's motion.

It should be noted that, whereas Bruno was immediately arrested when Mocenigo filed the first complaint against him and was never released, Galileo was not arrested when Lorini filed his complaint in February 1615; indeed Galileo was not arrested even when a second plaintiff (Caccini) testified against him the following month. Later of course, when additional witnesses and the consultant's report on Galileo's "Letter to Castelli" exonerated Galileo, there was even less reason to arrest him. This difference in the Inquisition's response to complaints may be partly due to the fact that Mocenigo's charges were more serious and more numerous than those of Lorini and Caccini. But it is also probably a reflection of the social position of the parties involved: in Bruno's case, a nobleman was accusing someone who was not only a commoner, but also a non-citizen and kind of a vagrant, a "homeless" person so to speak; in Galileo's case, two common clergymen were accusing someone who was under the protection the Florentine House of Medici, holding the position of Philosopher and Chief Mathematician to the grand duke of Tuscany, and whose astronomical discoveries had dazzled the world of learning and catapulted him into celebrity status.

The issue of torture raises an interesting contrast. To begin with, it is worth repeating that physical torture of defendants was for centuries a standard part of judicial proceedings in both civil and religious courts of almost all countries. It is thus beside the point to bemoan the fact that our two defendants were running such a risk.[43] The more revealing point is to note when, how, and why it was or was not

[42] It would be very instructive to compare and contrast this type of treatment received by Galileo not only with that received by Bruno, but also with much of what went on in the United States during the impeachment of President Clinton in 1998–1999.

[43] To say this is not to condone the Inquisition's use of torture. It is rather to say that it is irrelevant to lament the fact that Bruno and Galileo were at risk of being tortured—irrelevant for the purpose

authorized. We have already seen that torture was prescribed in situations of inconclusive or conflicting evidence. However, in Bruno's case at the Inquisition meeting of September 9, 1599, the pope decided against torture despite the recommendations of the experts, but instead approved Bellarmine's plan. On the other hand, in Galileo's case at the Inquisition meeting of June 16, 1633, pope Urban VIII decided in favor of torture to revolve the issue of his intention. To be sure, I hasten to add that Urban's decision was worded in an ambiguous manner that left it unclear whether actual torture or the verbal threat of torture was meant. This ambiguity and other aspects of the case (such as Galileo's old age) insured that when the formal interrogation was conducted (on June 21, 1633), it was carried out merely with the verbal threat of torture and not under actual torture. A fuller study of this issue for Galileo's case leads us to appreciate and understand more fine distinctions. Having distinguished actual torture from (1) threatened torture, one must go on to distinguish at least four stages of the former: (2) being taken to the torture chamber and being merely shown the instruments; (3) being undressed, as if one was going to be placed on the instrument, without being actually placed there; (3) being placed on the instrument of torture, without torture being applied; and (5) being placed on the instruments and having torture applied. I am sure that the relevant manuals discussed such details in more excruciating and nauseating detail. But even without engaging in such a reading, Bruno's trial suggests that actually applied torture was not a univocal concept. The relevant document is, once again, the Inquisition minutes of September 9, 1599.[44] I have already mentioned that there was a consensus in favor of torture by the various consultants; now, if we examine their various recommendations, we notice that of the six consultants three recommended "ordinary" torture, two "severe" torture, and one "repeated" torture. With three subtypes of actually applied torture, we now seem to have seven procedures that could be subsumed under the notion of torture; in increasing order of strictness, they ranged from the verbal threat of torture to actually applied severe torture repeated more than once. It is unclear why in Bruno's case the pope did not approve torture; whereas in Galileo's case the reason may stem from the personally animosity that Urban had developed in the light of the perceived betrayal by his former protégé.

Aside from the behavior of the Inquisition discussed so far, the conduct of other parties deserves some comment, beginning with the defendants.

The demeanor and attitude of Bruno and of Galileo present a stark contrast. The end results, of course, provide a dramatic difference. But their words and actions are also dramatically different. I would not want to describe Bruno's attitude as

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of historical understanding. In other words, nowadays one can indeed rightly bemoan such use of torture. But this is a moral evaluation: the judicial use of torture in the twenty-first century is morally wrong. Now, if we transpose this judgment to the sixteenth century, we get the proposition that the judicial use of torture was morally wrong in the sixteenth century. I am not so much questioning the truth of this latter proposition, but rather whether it conveys any information content, whether it has any practical import, and whether it enhances historical understanding. Insofar as the answer to the last three questions is negative, then one would also have to ask the question, What is the point of making such a (true) statement?

[44] Firpo 1993, 96–97, 327–329; cf. Mendoza 1995, 262–264.

one of Socratic arrogance since we know that on two occasions during the trial he gave external indications of wanting to retract and submit; instead I would call his attitude courageous, in the Aristotelian sense according to which courage does not involve ignorance of danger or lack of fear but the overcoming of fear in a situation of perceived danger. Regarding Galileo, I think it would be going too far to call him a coward,[45] if for no other reason than because, during his fourth interrogation under the verbal threat of torture, he was firm in denying a malicious intention and was ready to die rather than admit that; nevertheless, his attitude is generally one of meekness. Here some of their words are memorable enough to be worth quoting. Bruno's last words with which he responded to the sentence that was publicly read to him on February 8, 1600 are relatively well known: "You pass your sentence on me with greater fear than I feel in receiving it."[46] But equally revealing is his first utterance at his first interrogation in Venice on 26 May 1592, which is little known: "I will tell the truth: more than once I have been threatened to be made to appear before this Holy Office, and I have always taken it as a joke because I am ready to give an account of myself."[47] The best illustration of Galileo's meekness is his second deposition in which he pleaded guilty of having unintentionally defended the earth's motion, confessing to the base motive of literary vanity, and ending with a promise to undo the damage if allowed to rewrite his *Dialogue*. The deposition ends with the words:

> And for greater confirmation that I neither did hold nor do hold as true the condemned opinion of the earth's motion and sun's stability, if, as I desire, I am granted the possibility and the time to prove it more clearly, I am ready to do so. The occasion for it is readily available since in the book already published the speakers agree that after a certain time they should meet again to discuss various physical problems other than the subject already dealt with. Hence, with this pretext to add one or two other Days, I promise to reconsider the arguments already presented in favor of the said false and condemned opinion, and to confute them in the most effective way that the blessed God will enable me. So I beg this Holy Tribunal to cooperate with me in this good resolution, by granting me the permission to put it into practice.[48]

Also noteworthy is a difference regarding the persons whose charges got the formal apparatus of the Inquisition started. Whereas the initial plaintiff in Galileo's case was a clergyman (Lorini), in Bruno's case it was a layman, indeed someone who was supposed to be his patron. This may mean either that religion was more vigilant in Galileo's case and felt more threatened by his beliefs even though they focused on physical questions, or that Bruno's metaphysical theses and religious comments were so radical and offensive that even a layman could see that they undermined the religious establishment.

Hard to overlook is the figure of Bellarmine and the fact that he had a role in both trials. Exactly what his role was, has been a long standing controversy. Certainly

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[45] As many have done; two of the most eloquent (but ultimately unfair) indictments, see Brewster 1841 and Chasles 1862.

[46] My translation; cf. Schoppe (1600, 351), Mendoza (1995, 66).

[47] Firpo 1993, 154, my translation.

[48] Finocchiaro 1989, 278–279; cf. Galilei 1890–1909, 19: 344.

one cannot deny the emblematic and symbolic connotations of this situation and the inflammatory potential that Bellarmine's involvement has for the cultural wars between reason and faith. The anti-clerical insinuation is that the fact that the man who burned Bruno and silenced Galileo was made a saint (in 1930[49]) speaks for itself. However, the sobering fact is that his involvement in both trials was partial: as we have seen, it came at the end of Bruno's trial; and it occurred only in the first phase of the Galileo affair in 1615–1616, since Bellarmine died in 1621 and had no role in Galileo's actual trial in 1632–1633. Moreover, as I have already argued, his role in the Bruno case is best seen as the moderating one of conceiving and implementing a middle course of action or compromise. On the other hand, his role in the Galileo case was not that of a moderating force because in April 1633 the compromise was worked out by Vincenzo Maculano, the Inquisition commissary (and not by Bellarmine who had died twelve years earlier); and because in 1615–1616 the moderating force who prevented the formal condemnation of Copernicanism as heretical and limited its censure to the indexing of Copernicus's book, was cardinal Maffeo Barberini, the future pope Urban VIII. Instead, in 1615–1616 Bellarmine was the leading exponent of the fundamentalist position that the earth's motion is false and can never me more than a useful fiction because it contradicts the Bible, which is an authority on questions of science and philosophy as well as faith and morals. Bellarmine's biblical fundamentalism is implicit in his famous letter to Foscarini, and has recently been reinforced by newly published evidence that in his early career in his lectures at the University of Louvain he argued in favor of anti-Aristotelian propositions regarding the nature of the heavens; for he did so on the basis of passages from the Bible. My own conclusion is that Bellarmine ought not be demonized, but that neither can his role be idolized or idealized.[50]

One entity other than the Inquisition whose behavior deserves some discussion is the Republic of Venice. We have seen that in Bruno's case, after an initial refusal the Venetian government finally complied with the Inquisition's request to extradite him to Rome. My reading of the relevant documents[51] convinces me that the republic acted properly and could not have legitimately denied the request, after the extradition was justified by appeal to precedent and to the special circumstances of the case. This is a point relevant to Galileo's trial as well, not because Venice was in any way involved in adding to his troubles with the Inquisition, but rather for another reason relating to his decision in 1610 to resign his professorship at the University of Padua and move to Florence. Galileo's decision was criticized by some of his friends as a mistake insofar as he was exchanging the freedom of the Venetian Republic for the financial and social privileges of patronage by the Medici in Florence. This criticism has been

[49] "Bellarmine," *Columbia Encyclopedia* (New York: Columbia University Press, 1975), p. 266.

[50] For some anti-Bellarmine detractors, see Berti 1868, Mondolfo (1930, 1947, 30), Segre 1997, Spampanato (1921, 548ff.); for some admiring accounts, see Duhem (1908, 1969), Feyerabend 1985, Baldini and Coyne 1984; for a balanced account, Firpo 1993, 91–93; see also Blackwell 1991 and Schettino (forthcoming).

[51] Firpo 1993, 199–214.

echoed by many scholars,[52] both admirers and detractors, namely whether or not they include Galileo's decision in the list of items indicating his alleged recklessness. In the light of Bruno's experience, this criticism of Galileo needs to be re-evaluated. I am not suggesting that Galileo could have been arrested by the Venetian Inquisition as easily as Bruno was, or that he would have necessarily been extradited to Rome like Bruno was, or that his protection by Venice would have been as faint-hearted and ineffective as the Tuscan grand duke's was in 1632–1633; nevertheless, his citizenship status in the Venetian Republic was unclear, and so his freedom there may not have been as secure as it is often portrayed when the matter is considered without the benefit of Bruno's case.

The extant documentation for the two trials raises another set of issues. Here the tip of the iceberg is the fact that the proceedings of Galileo's trial have survived, whereas those of Bruno's trial have not (except for the documents of the Venetian stage of the trial and for the trial's summary in Rome). Bruno's proceedings were presumably destroyed along with many other Inquisition archives after Napoleon ordered them transferred to Paris in 1810, as part of his plan to relocate there all Vatican archives. The Galilean proceedings were also transferred, but they received special handling, for they had already been collected into a special file kept in the papal personal archives rather than in general Inquisition archives. Moreover, as befitted the greater notoriety and importance of Galileo's trial, Napoleon planned to have the Galilean proceedings translated into French and published, a plan that never came to fruition.

Regarding specific documents, the sentence of an Inquisition trial was very important because it usually included not only a statement of the penalty imposed on the convicted heretic, but also a description or classification of the heresy in question, the reason for the conviction, and indeed a chronological summary of the proceedings. For Bruno's case, the full text of the sentence has not survived, and we have only a partial copy given to the governor of the city of Rome, who was the civilian official responsible for overseeing Bruno's execution.[53] On the other hand, for Galileo's trial we do have the full text of the sentence. The reason why Galileo's sentence survived is that after his condemnation, the Inquisition made an unprecedented and never duplicated effort to publicize it by sending copies of it to all papal nuncios and all inquisitors outside the papal states. Bruno's sentence did not survive because it must have been destroyed along with the rest of the proceedings during the Napoleonic captivity. In accordance with Inquisition practice, the sentence that concluded a trial was jealously regarded as a secret like the rest of the proceedings. Strangely enough however, for reasons that are unclear, no copy of Galileo's sentence survived within the file of trial proceedings.

Another crucial document is the so-called summary. This is the most significant and the longest of the extant documents for Bruno's trial. As mentioned earlier, it amounts to about 60 pages (30 folios) and is a digest of proceedings running to about ten times that bulk (600 pages, or 300 folios); one of its invaluable features is that it

[52] For example, Gebler 1879, 31–32.

[53] Firpo 1993, 339–344.

contains folio references to the proceedings for all points outlined in the summary. For the Galileo case, the summary runs to 10 pages (5 folios) and refers to about 200 pages (100 folios) of proceedings.[54] Because the Galilean summary is briefer, even relatively speaking (5% as contrasted to 10%), and because in this case we possess the rest of the 200 pages of proceedings, it is easier to overlook its existence and significance. On the other hand, because the summary in the Bruno case constitutes a greater part of the available documentation, the study of Bruno's trial enables us to appreciate the nature of such summaries. In fact, they were summaries of the trial proceedings compiled by Inquisition officials which provided the basis for judgment and sentencing decided upon by the Congregation of cardinal-inquisitors chaired by the pope. It should finally be mentioned that the Brunian summary also attracts attention because of the lateness of its discovery and of its publication: it was first discovered in 1886–1887 in the Vatican Secret Archives, but Pope Leo XIII ordered that the discovery be not divulged, and the document not given to anyone; thus it had to be rediscovered in 1940, and was published in 1942.[55]

And this brings us to the interpretive questions relating to religion versus philosophy and religion versus science. For, when the Vatican official who rediscovered the summary of Bruno's trial published it for the first time, he preceded it by an interpretive introduction in which he was only too happy to declare that the summary reinforced the claim that Bruno was condemned for his religious beliefs, that he deserved to be condemned, and that the blame lay with himself and not his judges.[56] As the next section will argue, I believe that, appearances to the contrary, this apologetic thesis is not correct, and that Bruno was condemned for his philosophical ideas.

7.5 Religion vs. Philosophy and Religion vs. Science

I begin the discussion of interpretive issues with a criticism of Yates's explanation of Bruno's condemnation, namely the thesis that he was condemned because he was a Hemeticist and a magician.[57] The key difficulty with this interpretation is that there is little trace of Hermeticism and magic in the trial proceedings. The many charges against Bruno can be subdivided into three groups involving respectively: (a) religious and theological issues and topics, such as the Trinity, Christ, Moses, Mary, etc.; (b) questions of philosophy, be it natural philosophy or metaphysics, such as the universe and the soul; and (c) sinful practices, such as apostasy, blasphemy, and carnal indulgence.[58] But the question of Hermeticism and magic arises explicitly only in one charge (no. [7]) and indirectly in two others (nos. [3] and [14]). Thus,

[54] Galilei 1890–1909, 19: 293–297; Finocchiaro 1989, 281–286; Pagano 1984, 63–68.

[55] Blumenberg 1987, 371; Mercati 1942.

[56] Mercati 1942, 8, 12.

[57] Yates (1964, 355; 1967; 1973, 542).

[58] Firpo (1993, 88–90) is clear about this tripartite subdivision.

from the point of numbers, taking the whole heap of charges, Hermeticism and magic seem to play an insignificant role at best.

Moreover, if we look at the details of these charges and Bruno's replies, it is difficult to avoid the conclusion that he had effectively refuted them. Mocenigo's explicit charge that Bruno was a magician was substantiated in his second deposition by referring to a book which he had found among Bruno's papers.[59] In his depositions Bruno identified the book in question as being one entitled *De sigillis hermetis, Ptolomei et Aliorum*; and he clarified that this was not a book he had written but rather a manuscript he had had copied by an amanuensis and had not yet had the opportunity to read.[60]

More importantly, Bruno explained his views on the nature of the magical arts. Regarding the art of conjuring, he dismissed it with contempt.[61] As regards the art of divination or judicial astrology, he admitted the intellectual curiosity of wanting to learn about it to see if it has any validity, but be bemoaned the fact that as yet he had not found the time to study it.[62] He also dismissed something which he labeled "mathematical or superstitious magic,"[63] without explaining what he meant by this. Next he commented on magic per se, or natural magic, understood as "knowledge of the secrets of nature together with the ability to imitate nature in her operations and to do things which are popularly seen as wonders."[64] Quoting Saint Thomas Aquinas, Bruno stated that all knowledge can be good or bad depending on whether it is used by good or bad persons, and he went on to argue that therefore there is nothing intrinsically evil in natural magic; that, in his own eloquent words, "it is like a sword, which is bad in the hand of an evildoer but can be good in the hand of someone who feels the fear of God."[65] Finally, he clarified that even for natural magic, his interest in it was theoretical rather than practical or pedagogical: "I have never had the intention of preaching the said science … but only … that I should be informed of the character and theory of the science, because I never liked its practice."[66]

The topic comes up again indirectly in connection with Mocenigo's charge that Bruno held erroneous opinions about the life and death of Jesus. One of these allegedly erroneous opinions was that he thought Jesus was a magician, who performed his miracles by magic. Bruno adamantly denied this charge.[67]

The issue also arises implicitly in the context of Celestino's charge that Bruno had spoken ill of Moses. One of these alleged aspersions on Moses was the claim that Moses was a most astute magician, and it was the art of magic that enabled him

[59] Firpo 1993, 144, 146.

[60] Firpo 1993, 166, 193, 286.

[61] Fifth Deposition, Firpo 1993, 187.

[62] Fifth Deposition, Firpo 1993, 188–189.

[63] Tenth Deposition, Summary [122], Firpo (1993, 275), my translation.

[64] Tenth Deposition, Summary [122], Firpo (1993, 275), my translation.

[65] Fifteenth Deposition, Summary [197], Firpo (1993, 287), my translation.

[66] Summary [197], Firpo (1993, 287), my translation.

[67] Fourth Deposition, Summary [53], Firpo (1993, 174, 262).

to outperform Pharaoh's magicians. Bruno admitted having made such an assertion, but clarified that far from disparaging Moses, the assertion represented high praise for him. And to justify the praise, Bruno elaborated his conception of magic stated above.[68]

In sum, there was not much talk of Hermeticism and magic in the proceedings of Bruno's trial, and whatever talk there was, Bruno's rebuttals to the relevant charges were such that Hermeticism and magic cannot be plausibly said to have been a main reason for his condemnation, let alone the root cause.

Now, if talk of magic is rare, it is equally obvious that discussions of religious and theological topics abound. Only two out of the twenty-eight complaints advanced by the plaintiffs were philosophical rather than religious, namely the fifth and the sixth, dealing with the universe and the soul. Such proportions remain essentially unchanged even if we take into account the book censures (nos. [30]–[40]) because almost all censures were illustrations or specifications of those two charges, and the same proceedings make clear that most of the religious charges as we have numbered them had several parts and so could be subdivided into many more specifications. Are the apologists of the Inquisition right then, when they claim that Bruno was condemned mostly on account of religious and theological issues? Even if this claim were true, the apologetic argument would hinge on the issue of the nature and limits of religious freedom, as distinct from freedom of thought in general. But this issue cannot be pursued here, for I want to focus instead on the truth of the premise, on whether the apologists are right in claiming that Bruno was condemned mostly for religious reasons.

The most authoritative, scholarly, eloquent, and acute of these apologists was Angelo Mercati, the Vatican official who rediscovered and first published the Summary of Bruno's trial. Mercati was the prefect of the Vatican Secret Archives and searched for the Summary ever since being appointed to the post in 1926; he finally found it in 1940 and published it in 1942 under the auspices of the Vatican Library. In Mercati's edition, the text of the Summary is preceded by an introduction in which he provides all kinds of useful information from the point of view of scholarly erudition.

Even on some interpretive issues, his points are well taken. For example, he shows signs of judiciousness and balance when he admits that "there is no doubt that the question of the earth's motion was also a subject of scrutiny by the Roman Inquisition; like the great majority at that time, this [institution] was opposed to it, for reasons that were not so much pseudo-scientific … but rather pseudo-theological."[69] Moreover, I believe he is correct when he says[70] that by and large none of the charges against Bruno involved strictly scientific issues; and that not even the question of the earth's motion is a scientific one in the context of Bruno's trial because his main reason for accepting it is not scientific but philosophical. In fact, Bruno's main argument was that the earth rotates on its own axis in order to partake of the light and heat

[68] Summary [117]–[123], Firpo (1993, 274–275).

[69] Mercati 1942, 8, my translation.

[70] Mercati 1942, 10.

of the sun, and that it revolves around the sun in order to partake of the seasons of the year; and he seemed to regard this as a conclusive and apodictic argument.[71] By stressing this sort of thing, Mercati was undermining the interpretation of Bruno's trial in terms of a science versus religion conflict.

Thus, Mercati felt justified in triumphantly concluding: "the Roman sentence against Bruno can be criticized only if it could be shown to have been issued without legitimate reasons or against reason; this is definitely excluded by the other facts of the trial, as well as especially by the Summary; the latter makes us clearly understand how the Roman Inquisition brought to a conclusion the trial against Bruno only for legitimate reasons of [religious] orthodoxy and for his actual transgressions deserving punishment."[72] In reaching this conclusion Mercati is overlooking two crucially important things. The first might be called the autonomy of philosophy, that is the fact that the charges and censures against Bruno had a philosophical aspect, philosophical in a sense distinct from both science and religion. The second oversight regards the dynamics of the trial, a dynamics which of course is invisible from the Summary (which provides a static time slice) and can only be appreciated from a chronological account and reading of the documents, such as the one I have given above, following in the footsteps of Luigi Firpo.

The dynamical aspects of the trial that need to be stressed are as follows. Bruno was judged guilty of heresy and condemned to death when his memorandum of September 10, 1599 made clear that he was still questioning the heretical status of the short list of theses assembled by Bellarmine and tentatively retracted by himself earlier. It is likely that this list dealt primarily with Bruno's infinitism and animism; that is, with the questions of the temporal, spatial, and plural infinity of the universe; and with issues of the nature, immortality, and individuality of the soul, and its relationship to the body, the world, and material substance. In regard to these propositions, Bruno had a legitimate point in questioning when exactly such theses had been declared heretical, and in suggesting that they had never been formally declared to be heresies. He must have thought that they were not heretical, and that they did not contradict Catholic doctrine.[73] Let us also recall that as late as September 9, 1599, all the

[71] See Bruno 1955, 192–193; Bruno 1977, 185; Fifth Deposition, in Firpo 1993, 188; and Summary [256] and [258].

[72] Mercati 1942, 13, my translation.

[73] Bruno must have been thinking of the strict theological concept according to which a heresy is the denial of a proposition (1a) explicitly (1b) revealed by God and (1c) officially proclaimed by the Church in a declaration addressed to (1d) all who have been (1e) baptized. On the other hand, the Inquisition usually operated with a concept which is looser and broader, according to which a heresy includes the denial of propositions that (2a) can be clearly deduced from divine revelations, and/or (2b) embody common Church teachings, and/or (2c) are clearly contained in Scripture but have not been officially proclaimed by the Church, and/or (2d) are declared articles of faith by lesser Church organs (inquisitors, bishops, popes when not speaking *ex cathedra*, etc.), and/or (2e) are applicable only to a particular person or group, and/or (2f) are held by persons who have not been baptized. As Garzend (1912) has shown, the strict concept is the one prevalent among theologians from the sixteenth century onwards, whereas the broader concept is the one prevalent in Inquisition manuals and practice. Garzend elaborates the distinction to provide an original apology of the condemnation of Galileo, by arguing that although he was not a heretic from the strict theological point of view,

other charges were either not legally proved, or insofar as they were, he had already expressed or was willing to express the appropriate contrition and retraction. When he finally decided that he did not want to retract the philosophical theses which he had spent his whole life elaborating, this defiance made him in the eyes of the inquisitors not only guilty of holding these theses, but also guilty of all the other charges of which he had been accused but which had not been previously judicially established. Thus, in the sentence the inquisitors could indiscriminately list everything, religious or theological charges and practical transgressions, as well as philosophical theses. But the root cause of the condemnation was Bruno's unwillingness to retract his philosophy and the Inquisition's unwillingness to tolerate it.

In other words, the Inquisition believed that there was a contradiction between Catholic doctrine and Bruno's philosophy; that such a contradiction demanded action; and that the appropriate action was the elimination of the dissident. This is the sense in which Bruno's trial is an illustration of the philosophy versus religion conflict, at least as a reasonable simplification and first approximation.

And this brings us to Galileo's trial. In fact, if we replace "philosophy" by "science," there is an uncanny similarity between his trial and that of Bruno. In the Galileo case, the scientific thesis is, of course, the earth's motion. This thesis was considered to contradict the Bible by the inquisitors and by a segment of the Catholic Church that prevailed at the time. Like Bruno, Galileo did not think there was a contradiction, nor did a minority of less powerful segments of the Church. The reason why Galileo did not think that the geokinetic thesis contradicted the Bible was that he did not think the Bible was a scientific authority, but only an authority on questions of faith and morals, and so geostatic assertions in the Bible did not have to be interpreted literally. Correspondingly, the reason why his inquisitors thought there was a contradiction was that they considered the Bible to be a scientific authority, as well as an authority on questions of faith and morals, and so they thought it was Copernicus's geokinetic assertions that should be interpreted nonliterally, namely instrumentalistically. Thus Galileo's trial illustrates the clash between science and religion in the sense that according to the Inquisition there was a conflict between Galileo's scientific theses and the Bible, and it was able to impose its interpretation of the situation on Galileo.

In other words, what I am saying is that a key issue in Galileo's trial was the question whether or not the Bible is a scientific authority. For those (the traditionalists and the majority) who accepted the scientific authority of the Bible, there was no way of also accepting the truth of or the realistic interpretation of Copernicanism; for those (the innovators and the minority) who restricted the authority of the Bible to faith and morals, there was no difficulty, that is no religious impediment, to accepting the earth's motion. The condemnation of Galileo was in large measure the result of a disagreement over this fundamental question, between the principle of separation

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he was one from the point of view of the broader concept of heresy. However, the other side of the coin of Garzend's argument is that Galileo's condemnation was theologically unjustified. I believe the same applies, *mutatis mutandis*, to the case of Bruno.

of Bible and science on one side and the principle of biblical fundamentalism on the other.

To support my interpretation, I would want to emphasize such things as the following. In the first phase of Galileo's trial, there is clear evidence that Bellarmine was a fundamentalist. This occurs in his letter to Foscarini, where he asserts, referring to everything the Bible says, that "if it is not a matter of faith 'as regards the topic', it is a matter of faith 'as regards the speaker'; and so it would be heretical to say that Abraham did not have two children and Jacob twelve, as well as to say that Christ was not born of a virgin, because both are said by the Holy Spirit through the mouth of the prophets and the apostles."[74] Then I would want to emphasize that in both his "Letter to Castelli" and his *Letter to the Grand Duchess Christina*, Galileo's main aim is to justify the methodological principle of separation, expressed in his own words, adapted from cardinal Baronio, with the memorable words: "the intention of the Holy Spirit is to teach us how one goes to heaven and not how heaven goes."[75] And I would also want to emphasize the fact that the final Sentence in the trial specifies that Galileo is being condemned for holding not only that the earth moves, but also "that one may hold and defend as probable an opinion after it has been declared and defined contrary to the Holy Scripture"[76]; for the latter assertion is merely a way of stating the principle of separation.

Besides such textual documentary evidence, I would want to support my interpretation by criticizing a main alternative. To this I now turn. Many scholars argue that Galileo was condemned not for heretical beliefs but for disobedience; not for doctrinal but for disciplinary heresy.[77] His disciplinary infraction was to disobey, by publishing the *Dialogue* in 1632, the personal special injunction issued to him in 1616. This was the prohibition not to hold, defend, or discuss the earth's motion in any way whatsoever, which he had promised to obey. This prohibition has three clauses, involving respectively holding, defending, and discussing, and his *Dialogue* violated at least one and perhaps all three clauses. Such an interpretation also overlooks the dynamics of the trial.

Now, it is indeed true that one of the charges against Galileo in 1632 was violation of the special injunction, and that the text of the sentence makes it sound as if this charge had been proved. But the trial proceedings make clear that the validity of the special injunction came into question after the discussion clause was denied by Galileo in his first interrogation; after he produced Bellarmine's certificate, which conflicts with that same clause; and after the Inquisition officials reflected on the fact that the special injunction document lacked Galileo's signature. On the other hand,

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[74] Finocchiaro 1989, 68; cf. Galilei 1890–1909, 12: 172. Bellarmine's biblical fundamentalism is also obvious from the lectures he gave at Louvain in his early career, in which he supported anti-Aristotelian cosmological conclusions about the heavens on the basis of biblical passages; cf. Baldini and Coyne 1984. And the same position can be attributed to Bellarmine from his major work *De controversiis*, as Baldini (1992, 323) has argued.

[75] Finocchiaro 1989, 96; cf. Galilei 1890–1909, 5: 319.

[76] Finocchiaro 1989, 291; cf. Galilei 1890–1909, 19: 405.

[77] Blackwell 1998, 355; Brandmüller 1992, 144-46; D'Addio 1993, 211-12; Gingerich 1995, 342; Mayaud 1997, 313.

the other two clauses (namely, not to hold and not to defend) had been admitted by Galileo himself, and they were clearly stipulated in Bellarmine's certificate. But proving that in his book Galileo held the earth's motion was not easy, because of its dialogue form, because of the various disclaimers contained in it, and because of the other precautions he had taken in writing and publishing the book. By contrast, proving that the book was a defense of the earth's motion was relatively easier, but even this must have been seen as problematic perhaps because of the point that Galileo could be viewed as presenting and evaluating all the arguments for and against the earth's motion, and it was not his fault if the arguments in favor were stronger than those against. Thus after the first deposition the Inquisition commissary tried to convince Galileo to plead guilty to the charge of having defended the geokinetic thesis. Since he did plead guilty to that, one of the things he may be said to have been convicted of was disobedience in regard to defending the earth's motion.

However, this should not be the end of the analysis. Let us examine more carefully the notion of disobedience or disciplinary heresy in this particular case. Two comments are in order here. First, being forbidden to defend an idea is not like being forbidden to commit some overt practical act, but rather it amounts to being forbidden to think in a certain way, or to defend a doctrine. Second, if one asks why Galileo was forbidden to defend the earth's motion, the answer is that the Inquisition thought that the geokinetic thesis contradicts the Bible and that the Bible is a scientific authority. But these reasons are doctrines; these doctrines were disputed by Galileo; and so his disobedience reduces to a matter of doctrine; and so the sentence is condemning him for both disciplinary and doctrinal heresy.

It thus remains true that, in the trials of both Bruno and Galileo, the persons who did not think there was a conflict between religion and reason were the victims, whereas the winners were those who thought there was a conflict. The relevant aspects of religion were different in the two cases: the authority of the Inquisition in Bruno's case, and the authority of the Bible in Galileo's. And different were also the relevant aspects of reason: philosophical reason in the Bruno case, scientific reason in the Galileo case.

This simplified interpretation of the two trials has to suffice here, although I am aware that it would have to be refined in several ways. For example, I have been using an unanalyzed intuition of the distinction between science and philosophy, and more would have to be said on this topic. One would have to take into account the sense in which Galileo was being a philosopher, specifically an epistemologist and methodologist, when he discussed the relationship between scientific inquiry and Biblical interpretation and articulated the principle of separation. And conversely, one would have to explore the secondary scientific aspects of Bruno's thought, even while reiterating its primarily philosophical or metaphysical character. This would point in the direction of the idea that the things that interacted in Bruno's trial were not just philosophy and religion, and the relata in Galileo's case were not just science and religion. Moreover, the relationship which appears as conflict at a first approximation may have to be complicated and sophisticated in the direction of interaction.

But the conflicts stressed and demonstrated here would probably be part of such interactions.[78]

Appendix I: Outline of the Lost File of Manuscript Proceedings of Bruno's Trial

The Summary of Bruno's trial contains references to the original file of proceedings. That file has been lost, but from the summary it is possible to reconstruct it as having the following content and structure. A reconstruction is also possible from the various relevant comments and information scattered in Firpo (1993). This has been done in the outline given below, and so the references in square brackets are to pages in Firpo's book.

Folios	*Contents*
1–8	Unknown; probably correspondence between the Inquisition offices of Rome and Venice [F41]
9–11	Mocenigo's first three complaints [F41]
12–33	Unknown; probably the book manuscript by Bruno attached by Mocenigo to his first complaint [F41]
34	Ciotti's first deposition (Venice, May 26, 1592) [F42]
35r	Brictanus's deposition (Venice, May 26, 1592) [F41]
35v–57	Bruno's seven depositions by the Venetian Inquisition [F42]
58–83	Unknown; but probably the depositions of Matteo d'Avanzo, Domenico da Nocera, and Andrea Morosini, the second one of Ciotti, and the extradition correspondence [F42]
84–85r	Celestino's initial complaint (Venice, autumn 1593) [F127 n.16]
85v–86v	Giulio da Salò's deposition (Venice, autumn 1593) [F127 n.1]
87r	Unknown [F127 n.1]
87v–88r	Francesco Vaia's deposition (Venice, autumn 1593) [F128 n.7]
88v–89v	Graziano's first deposition (Venice, autumn 1593) [F128 n.14]
90r–91r	de Silvestris's first deposition (Venice, autumn 1593) [F128 n.9]
92-94	Unknown; but probably correspondence between the Venetian and Roman Inquisition relating to sending the new depositions from Venice to Rome [F54]
95v & probably 95r, 96.	Bruno's eighth deposition (Rome, autumn 1593) [F128 n.18]
97–103	Bruno's ninth deposition (Rome, autumn 1593) [F129 n.21].
104-109	Bruno's tenth deposition (Rome, autumn 1593) [F129 n.22]
110–115	Bruno's eleventh deposition (Rome, autumn 1593) [F129 n.23]

(continued)

[78] I gratefully acknowledge that the research for this paper was supported in part by a grant from the National Science Foundation, USA (no. SBR-9729117).

(continued)

Folios	*Contents*
116–120	Bruno's twelfth deposition (Rome, autumn 1593) [F129 n.25]
121	Unknown
122–131	Bruno's thirteenth deposition (Rome, autumn 1593) [F129 n.27]
132–133	Unknow
134–140	Bruno's fourteenth deposition (Rome, autumn 1593) [F129 n.27]
141	Bruno's fifteenth deposition (Rome, autumn 1593) [F129 n.29]
142	Unknown
143–148	List of charges against Bruno compiled from the initial examination of all plaintiffs and witnesses by the Inquisition's prosecuting attorney [F63]
149	Unknown [F129 n.2]
150–157	Bruno's questionnaire for the re-examination of plaintiffs and witnesses, compiled from the prosecutor's list of charges [F63–64]
158–162	Unknown; but probably correspondence between the Venetian and the Roman Inquisition [F129 n.6]
163–168	Mocenigo's deposition, under re-examination (Venice, January–March 1594) [F64]
169–171	Ciotti's third deposition, under re-examination (Venice, January–March 1594) [F64]
172–176	Graziano's second deposition, under re-examination (Venice, January–March 1594) [F66]
177–180	de Silvestris's second deposition, under re-examination (Venice, January–March 1594) [F67]
181–183	Celestino's deposition, under re-examination (Venice, January–March 1594) [F68]
184–185	Unknown
186	Brief memorandum by Bruno [F72]
187–188	Unknown
189	Mocenigo's fourth complaint (Venice, June 1594) [F73]
190–191	Unknown
192–194	Bruno's sixteenth deposition (Rome, Summer 1594) [F73]
195–205	Unknown, but probably more of Bruno's sixteenth deposition and Inquisition instructions to Bruno about his defense [F73]
206–246	Bruno's defense [F73]
247	Unknown [F133 n.11]
248–256	Censure of Bruno's theses found in his depositions as well as in his books [F77–78]
257–261	Bruno's seventeenth deposition (Rome, March–April 1597) [F78–79]
262–265	Unknown [F133 n.16]

(continued)

(continued)

Folios	*Contents*
266–295	Bruno's replies to the censures [F79, 133 n.17]

Appendix II: Master Table of Bruno's Trial

The following table gives a summary and quick overview of the charges against Bruno, their sources, and his responses to them. The first column (A) refers to the 24 charges as identified, numbered, and formulated by Firpo (1993), expanded to explicitly include the book censures and a few other charges, in accordance with what I have done in my essay. The second column (B) refers to the topic with which the charge or book censure deals; this is meant to be just a mnemonic device, and the full description is found in the numbered charge in the body of my essay. The third column (C) refers to the page number in Firpo (1993) where the charge or censure is first formulated. The fourth column (D) refers to the sections (Roman numerals) or paragraphs (Arabic numerals) of the Inquisition Summary of the trial; here I am following Firpo (1993), who added Roman numerals for the sections to the Arabic numerals that Mercati (1942) had given to the paragraphs. The fifth column (E) refers to the charges as identified, described, and listed in Schoppe's letter to Rittershausen of February 17, 1600, as analyzed by Firpo (1993), pp. 103–104. The sixth column (F) indicates the initial plaintiff whose complaint contains the given charge, and in the case of the censures the work involved. The seventh column (G) indicates the plaintiffs, witnesses, or depositions that provide additional confirmations of the given charge. In both columns F and G, I am abbreviating each person's name by using only the first three letters of the last name; the numeral following the hyphen refers to the particular deposition of the several generated by that person; thus, for example, Bru-22 refers to Bruno's twenty-second deposition (December 21, 1599), Moc-3 refers to Mocenigo's third complaint (May 29, 1592), and Moc-d refers to Mocenigo's deposition under re-examination. The last column (H) indicates whether Bruno admitted (+) or denied (~) the given charge and in which one of his many depositions this happened.

A	B	C	D	E	F	G	H
Chrg	Topic	Firpo	Summary	Schop	Plntf	Confirmations	Bruno
[1]	CathChurch	F-16	Sum-i		Moc-1	deS-1, deS-2, Moc-d	~Bru-4&9&13
[2]	Trinity	F-16	Sum-ii		Moc-1	Cel-2, Moc-d	~Bru-3&4&8&17
[3]	Jesus	F-16	Sum-iii	xiii	Moc-1	Cel-1&2, deS-2, Gra-1&2, Moc-d, Vai	~Bru-4&10
[4]	Transubst.	F-16	Sum-v	i	Moc-1	deS-2, Gra-2, Moc-d	~Bru-4&5
[5]	Universe	F-16	Sum-vii&ix	v	Moc-1	Cel-1&2,deS-1&2,Giu,Gra-1&2,Moc-d,Vai	+Bru-3&12&14&17
[6]	Soul	F-16	Sum-xxii	vi	Moc-1	Cel-1&2, deS-1&2, Gra-2, Moc-d	+Bru-3&4&5&11
[7]	Magic	F-16	Sum-xxiii	vii	Moc-1	deS-2, Gra-2, Moc-2, Moc-d	~Bru-5&15
[8]	Virgin Mary	F-16	Sum-xviii	ii	Moc-1	Moc-d	~Bru-4&5
[9]	Sex	F-20	Sum-xxv		Moc-3	deS-2, Gra-2, Moc-d	~Bru-4
[10]	Apostasy	F-21	xix, xxvii, xxviii, xxix	iii	—	Cel-2, Cio-3	+Bru-2&4&5
[25]	Church Drs.	F-16	Sum-xiv		Moc-1	Giu	~Bru-4
[26]	PunishSin	F-16	Sum-xxiv		Moc-1	deS-2	~Bru-4
[27]	CrimRecord	F-16	Sum-xxx				~Bru-5
[11]	SinJesus	F-48	Sum-iv		Cel-1	Cel-2, deS-1&2, Gra-2, Giu	~Bru-9
[12]	Hell	F-48	Sum-vi	xi	Cel-1	Cel-2, deS-1&2, Giu, Gra-1&2, Vai	~Bru-9
[13]	Cain & Abel	F-48	Sum-x		Cel-1	Cel-2, Gra-1, Gra-2	~Bru-12
[14]	Moses	F-48	Sum-xi	ix	Cel-1	Cel-2, Gra-1, Gra-2	~Bru-10
[15]	Prophets	F-48	Sum-xii	xiv	Cel-1	Cel-2, Gra-1, Gra-2	~Bru-10
[16]	Credibility	F-48	Sum-xiii		Cel-1	Gra-1	~Bru-13

(continued)

(continued)

A	B	C	D	E	F	G	H
[17]	Saints	F-48	Sum-xv		Cel-1	Cel-2, deS-1&2, Gra-1&2, Moc-d	~Bru-Sum-143&144
[18]	Breviary	F-48	Sum-xx		Cel-1	Cel-2, deS-2, Gra-1, Gra-2, Vai	~Bru-13
[19]	Blasphemy	F-48	Sum-xxi		Cel-1	Cel-2, deS-1&2, Giu, Gra-2, Moc-d, Vai	+Bru-10
[20]	FuturPlans	F-48	Sum-xxxi		Cel-1	Cel-2, deS-1&2, Giu, Gra-1&2	~Bru-9
[21]	Holy Relics	F-52	Sum-xvi		deS-1	Cel-2, deS-2, Gra-2	~Bru-Sum-149
[22]	Holy Images	F-54	Sum-xvii		Gra-1	Gra-2, Moc-d	~Bru-12
[23]	3 Kings	F-67	Sum-viii		Gra-2	(none)	~Bru-Sum-99
[24]	Pope	F-71	Sum-xxvi	iv	Moc-4	Ver	~Bru-16
[28]	Atheism	F-101	Sum-16		deS-2	Ver	
[29]	Scripture	F-103	—	x			
[30]	Eternity	F-80	Sum-252	v	*De Minimo*		(+Bru-3)
[31]	Infinity	F-80	Sum-253	v	*Infinito*		(+Bru-3)
[32]	Plurality	F-80	Sum-261	v	*Infinito*		(+Bru-3)
[33]	ImmortSoul	F-80	254&255		*Causa, Infinito*		
[34]	ConserSubst	F-82	Sum-255		*Causa*		
[35]	EarthMotion	F-82	Sum-256		*Cena*		
[36]	StarsSouls	F-83	Sum-257		*Cena*		(+Bru-3)
[37]	Earth Soul	F-83	Sum-258		*Cena, Causa, Infinito*		(+Bru-3)

(continued)

(continued)

A	B	C	D	E	F	G	H
[38]	Body/Soul	F-84	Sum-259	vi	*Causa*		(+Bru-11)
[39]	Holy Spirit	F-84	—	viii	*Causa*		(+Bru-3)
[40]	Pre-Adam	F-84	—	xii	*De Monade*		

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Chapter 8
Galileo's Daughter: The Book, the Movie, the Facts, and the Issues

Abstract This essay examines Galileo's relationship to the eldest of his three children, named Virginia, who became a nun with the name of Sister Maria Celeste. It contains a commentary on Dava Sobel's best-selling book *Galileo's Daughter: A Historical Memoir of Science, Faith, and Love* (1999). It also contains a commentary on a movie entitled "Galileo's Battle for the Heavens," first broadcast nationally in the United States by PBS television stations in 2002, produced by David Axelrod and based on Sobel's book. The essay also provides a summary of the main facts of Virginia's life. And it reports on the many internet comments on Sobel's book. All these things provide the occasion for an analysis, from the point of view of the Galileo affair, of the significance of Sobel's book and the light it sheds on some old and new issues.

8.1 Retrying Galileo, 1633–1992, and Beyond

Let me begin by saying a few words about a book which I just finished writing and am in the process of publishing,[1] entitled *Retrying Galileo, 1633–1992*.

In 1633 the Inquisition condemned Galileo for holding that the earth moves and the Bible is not a scientific authority. This ended the original Galileo affair that had started in 1613, involving issues of both physical fact and methodological principle; but a new controversy began, continuing to our own days. The subsequent controversy is about documenting, interpreting, and evaluating the original episode, and so it partly reflects the original issues: whether and how the earth's motion can be proved, experimentally or theoretically; whether the earth's motion contradicts Scripture; whether a literal interpretation of Scripture is always correct; and if not, when Scripture should be interpreted literally and when figuratively. However, the subsequent Galileo affair has also acquired a life of its own, with debates on whether science and religion are compatible; whether individual freedom and institutional authority must always

[1] Please note that these words were part of a lecture delivered in November 2002; cf. Finocchiaro 2005.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_8

clash; whether cultural myths can ever be dispelled with documented facts; whether political expediency must prevail over scientific truth; and whether scientific research must bow to social responsibility.

Besides such controversial issues, the subsequent controversy has two other strands. The historical aftermath of the original episode consists of facts and events stemming from it and involving actions mostly by the Catholic Church, up through the rehabilitation of Galileo by Pope Saint John Paul II (in 1979–1992); but also such actions as emperor Napoleon's seizure of the Vatican file of Galilean trial proceedings and plan to publish its contents (in 1810–1814). The reflective commentary consists of countless interpretations and evaluations of the original episode advanced in the past four centuries by astronomers, physicists, theologians, churchmen, historians, philosophers, cultural critics, playwrights, novelists, and journalists; these accounts are contained sometimes in classic texts such as Milton's *Areopagitica*, Pascal's *Provincial Letters*, and Brecht's *Galileo*, and sometimes in obscure scholarly printed sources.

Although the literature on the affair is enormous, no one had ever conceived or written a book such as mine: the full story of the aftermath had never been told; the reflective commentary had never been systematically examined; and the controversial issues had never been contextualized in the story or anchored in the textual sources. My book aims to do these things by providing an introduction to, and survey of, the textual sources, the chronological facts, and the controversial issues of the Galileo affair from 1633 to 1992; the unifying theme is the idea of series of retrials of Galileo, *after* its original trial (in 1613–1633).

The book interweaves into the narrative many of the most important documents and texts; almost all these texts are from printed *sources*, but they have never been collected and assembled together by anyone in any book in any language or any country; aside from English, the original languages of such texts are (in approximate order of frequency) Italian, French, Latin, and German; most of these texts have never before been translated into English, and the book provides English translations. The book establishes the key *facts*: not only who said what, where, and when; but also who did what, when, and where; and what happened when, how, and in the context of what. The *issues* are interwoven into the narrative as they emerge in the context of the discussion of the various sources and facts. The book's exposition is at an introductory or elementary level, in the sense that it assumes little specialized knowledge and presents what is needed to understand the story, following a basic chronological order (with due exceptions for thematic requirements). But the book is also aimed at experts and specialists, besides at a generally educated audience, because it is dealing in an original way with material they are vitally interested in and establishes the foundations of further scholarly discussions. In the course of the narrative, the book discusses not only the work of many Galilean scholars, but also the views and actions of such figures as Descartes, Leibniz, Voltaire, pope Benedict XIV, Napoleon, John Q. Adams, pope Leo XIII, Bertolt Brecht, Arthur Koestler, and Pope Saint John Paul II.

The manuscript is 420 pages single spaced, or about 290,000 words. About one-third of this bulk is made up of long quotations from sources. The bibliography lists about 2300 entries.

Now, I would love to chat about the book, asking the audience to look at the table of contents. I could then spend a few minutes talking about each of the 18 chapters and about the 65 sections into which they are subdivided. At a minute per section, that would take up another full long lecture. If the audience consisted primarily of my philosophical colleagues, I could just focus on the reactions and activities of Descartes and Leibniz, discussed in chapters 3 and 5; how Cartesian philosophy derives from the Church's condemnation of Galileo; and how Leibniz spent more time in (unsuccessful) efforts to convince Church authorities to repeal its condemnation of Copernicanism and of Galileo, than in inventing the calculus or writing on the metaphysics of monads. If I had a lay audience of educated and intelligent persons, I could focus on chapters 15 and 17, where I discuss the historical fiction in Bertolt Brecht's play *Galileo*, the fictional history in Arthur Koestler's *Sleepwalkers*, and the alleged rehabilitation of Galileo by Pope Saint John Paul II. If the audience consisted of scholars specializing on Galileo, I could focus the origin, development, and demise of the myth that was instrumental in generating the Galileo affair as the heated controversy it has been in the past two centuries; this was the myth that Galileo was condemned not for being a good astronomer but for being a bad theologian; it started around 1784 and is discussed in chapters 8 and 11.

However, since I obviously have no time to do any one of these things, let alone all, and since the title of this essay mentions Galileo's daughter, let me switch gears and discuss that topic. Actually there is a very important connection between the two topics, retrying Galileo and Galileo's daughter. The connection is as follows. I chose 1992 and the closing date of my investigation because that was the year when John Paul II explicitly and formally concluded the re-examination of the Galileo affair which he had started in 1979. And these dates are significant in light of the fact that the controversy was being re-examined by the same institution—the Church—that condemned the astronomer, as well in light of the geopolitical and geocultural importance of this Church and this pope. However, I do not claim or imply that the year 1992 represents the end of the process of retrying Galileo, that is the end of the controversy about the facts, the causes, the issues, and the lessons of his trial. The Galileo affair has become one of those episodes with which every educated person and every age wants to come to grips, and so it will go on for the foreseeable future.

In fact, the question of Galileo's daughter has already received enough discussion to qualify for becoming the subject of at least a section of an additional chapter to the story, which could be added by some future scholar or by myself in some re-edition of my book, or which could be written up simply as a separate essay. To this topic I now turn.

8.2 *Galileo's Daughter*, the Book by Dava Sobel

In the fall of 1999, a book was published entitled *Galileo's Daughter* and subtitled *A Historical Memoir of Science, Faith, and Love*. The author was a woman named Dava Sobel who had a background as a science journalist. Several years earlier she had published a book entitled *Longitude*, which told the story of the solution of the problem of finding longitude at sea and which had become an international best-seller. Although in the earlier book Sobel was telling a relatively unknown story, whereas the new book focused on the relatively well known story of Galileo's life, there was every reason to expect that Sobel's talent for story-telling and popularization would come through in the newer book as it had in the earlier one. And indeed the book *Galileo's Daughter* has also become an international best-seller.

The book is not a biography of Galileo's daughter, but rather a biography of Galileo, stressing his scientific work, his trial by the Inquisition, and his relationship to his elder daughter. It is a popular rather than scholarly work, but it is based on research and study undertaken by the author herself, and it takes into account and shows acquaintance with some primary sources and with relevant and recent scholarly work. It is thus surprisingly accurate and free of error as compared with the typical popularization.

The book does not provide a novel view of Galileo's scientific work or trial, but the focus on Galileo's relationship to the daughter does provide an engrossing human-interest story and a refreshing portrayal of the "father of modern science." Moreover, because Galileo's daughter became a nun at age 16 and lived in a convent for the rest of her life, the focus on their relationship and on the daughter's devotion and love does have implications for how one views Galileo's attitude toward religion.

The book has become something of a sensation and a phenomenon. For example, I have two acquaintances in different parts of the country who belong to book discussion clubs, and both clubs have discussed it. If one checks the Amazon.com website, one can read (November 2002) no fewer than 170 customer reviews. To get some perspective on this, I checked the number of customer reviews for Thomas Kuhn's *Structure of Scientific Revolutions*, which is probably the best-selling and most cited scholarly book of all time; Amazon.com lists only 60 customer reviews for this modern classic. Similarly, the Amazon.com sales rank for Sobel's *Galileo's Daughter* is (November 2002) 179, as compared to 1829 for Kuhn's book. Thus, I take Sobel's book as a document whose reception is as revealing of contemporary culture as the book's content is revealing of the life and times of Galileo and his daughter. Accordingly, later I will examine and comment on some of the Amazon.com comments.

8.3 "Galileo's Daughter," the Movie

Another indication of the emblematic status reached by Sobel's book is the fact that it has now been made into a movie. The same had in fact happened to her earlier work on longitude. The movie on Galileo was first broadcast nationally in the United States by PBS television stations on October 29, 2002. It was billed as a NOVA production for WGBH/Boston; written and produced by David Axelrod, of Green Umbrella Ltd, in Los Angeles. The movie takes the form of a docu-drama; it is two hours long and is entitled "Galileo's Battle for the Heavens." The program states that it is based on and adapted from her book. And generally speaking this is correct, although it is obvious that NOVA and the producer consulted many other scientists, scholars, and institutions.

By and large, the docu-drama does follow the book in regard to balance of topics, such as Galileo's life, his scientific work, the Inquisition trial, his daughter, and her letters to him. However, whereas the book is unusually accurate and judicious in comparison to the genre of popular books, the movie falls short of the book since the movie contains several factual errors and misleading interpretations; but I would judge their number and frequency to be about average for such genre of docu-drama. Moreover, I feel the movie has a significant dramatic flaw insofar as the actor who portrays Galileo displays diction and physical movements that are jittery and uneven, and a physical appearance that seems chronologically inconsistent.

A few of the movie's erroneous or questionable interpretations are worth noting. The movie indicates (around the 45th minute) that Galileo believed that his observation of the phases of the planet Venus was a conclusive proof of the Copernican system, and that Church authorities correctly denied such conclusiveness. However, Galileo was well aware that the phases of Venus proved only one element of the Copernican system, namely that Venus circles the sun at a distance smaller that the sun-earth distance; he was also aware that regarding the location and motion of the earth, other evidence was needed, and that all evidence available to him was only suggestive and probable, and that a conclusive proof of the earth's motion and location eluded him.

If this is an error that introduces an anti-Galilean and pro-clerical slant in the movie, the production is Solomonic in its errors, and there are others that suggest an anti-clerical slant. For example, near the end, after the condemnation, there is a scene where Galileo is locked in his own house, from the outside, to serve his sentence of house arrest. Although I grant some dramatic value to this, it is a sensationalist fabrication that should not be tolerated. It is true that he was formally under house arrest in his own home in Arcetri; he was not free to go to Florence or otherwise leave town; and there were other restrictions. But he was living in a villa, a house with a small garden or farm, and he could come and go as he pleased in and out of his house, including walking a few blocks to his daughter's convent.

Having introduced the book and the movie, I now want to discuss the facts of the life of Galileo's daughter, focusing on her letters, which are not only the main source of information, but also a noteworthy literary document.

8.4 Facts About Galileo's Daughter

Galileo's daughter was born in 1600 when Galileo was a professor at the University of Padua and living out of wedlock with a Venetian woman named Marina Gamba. The child was given the name of Virginia. She was followed in 1601 by a sister named Livia and in 1606 by a brother named Vincenzo. When in 1610 Galileo resigned his professorship at Padua and moved to Florence in the position of Philosopher and Chief Mathematician the grand duke of Tuscany, he and Marina decided to separate and to let the children remain with the father. At age 16, Virginia became a cloistered nun in the monastery of San Matteo in Arcetri, on the outskirts of Florence, and she took the name of Sister Maria Celeste. The following year, her sister did the same, taking the name of Sister Arcangela.

Maria Celeste's greatest legacy consists of the letters she wrote to her father. One cannot help being impressed by her warmth, love, intelligence, sensitivity, and unassuming eloquence. Although no letters by her father to her have survived, one hundred and twenty-four of hers to him have. The first is dated May 10, 1623, in which she expressed condolences for the death of Galileo's sister Virginia, after whom she had herself been named at birth.[2] The last is dated December 10, 1633, in which she expressed her joy at the fact that Galileo had finally been given permission to return to his villa in Arcetri and would soon move there from Siena.[3] Once he was in Arcetri (on December 17), they were living within walking distance of each other, and there was no longer any need for letters. Unfortunately, Maria Celeste died on April 2, 1634 after a brief illness, and the old man felt devastated.[4]

The most touching of her letters is perhaps the one in which, a few months after Galileo's trial and condemnation, she reported that she had finally been able to read the text of the Inquisition's sentence. Recall that the sentence states that Galileo had been found guilty of vehement suspicion of heresy and condemned to undergo several penalties: recitation of an abjuration; prohibition of his 1632 book (*Dialogue on the Two Chief World Systems*); perpetual house arrest; and the salutary penance of reciting the seven penitential psalms once a week for three years. Maria Celeste had decided to assume onto herself the burden of her father's salutary penance. Written on October 3, 1633, the main part of the letter is the following:

> It seems to me a thousand years till I see you back again safe and well! … I would not have you doubt that all this time I have never ceased from commending you to God with my whole heart, for indeed I feel too anxious for your spiritual and bodily health ever to have neglected praying for you. To give you a proof, I will tell you that as a great favour I managed to get a copy of your sentence shown me, and though on the one hand it grieved me to read it, yet on the other hand I was glad to have done so, because I found out a way of being of some slight use to you; namely, by taking upon myself that part of the sentence which orders you to recite the seven Penitential Psalms once a week. I began to do this a while ago, and it gives me much pleasure: first, because I am persuaded that prayer in obedience to Holy Church must be efficacious; secondly, in order to save you the trouble of remembering it. If I had

[2] Galilei 1890–1909, 13: 116–117.

[3] Galilei 1890–1909, 15: 352–353.

[4] Galileo to Bocchineri, 27 April 1634, in Galilei 1890–1909, 16: 84–85.

> been able to do more, most willingly would I have entered a [stricter] prison than the one I live in now, if by so doing I could have set you at liberty.[5]

Of course, it was not legally or theologically possible to substitute one person's penance with that of another, but that was irrelevant. The point was to share her father's burden by taking on an equal burden herself, so that the old man would have a companion in his misery. Human nature being as it is, the effect of such company in such misery is proverbial.

Another revealing letter is the one Sister Maria Celeste sent her father on August 10, 1623. She was replying to Galileo having sent her copies of admiring letters he had over the years received from cardinal Maffeo Barberini. What had motivated the father to show them to her nun-daughter was the fact that Barberini had just been elected pope Urban VIII. This development is what led Galileo to resume his "battle for the heavens" by writing and publishing the *Dialogue* and then being indicted, tried, and condemned as a suspected heretic by the Inquisition. However, here now we want to focus on the human element on this development. That is, here we have a father showing off to his nun-daughter that the head of the Church is a great admirer of her astronomer-father. In fact, in 1620 cardinal Barberini had gone so far as to write a Latin poem in praise of Galileo, and had sent it to him together with an accompanying letter; the poem was perceptively and prophetically entitled "Dangerous Adulation."[6] And Galileo had received another supportive letter from cardinal Barberini as late as June 24, 1623,[7] when he himself had no idea that he would be elected pope six weeks later. That happened on August 6. Galileo being as well connected as he was, and communication between Rome and Florence being surprisingly efficient, he learned the news a few days later, sent Barberini's letters to Maria Celeste without delay, and apparently she replied immediately, on the 10th. Here is the main point of what she wrote in the second one of the 124 of her letters that have survived:

> I cannot describe the pleasure with which I perused the letters of the illustrious Cardinal who is now our high priest, knowing as I do how greatly he loves and esteems you. I have read the letters several times, and now send them back as requested, having shown them to no one except Sister Arcangela, who, as well as myself, is much delighted to see how greatly you are favoured by such an important personage. May the Lord give you health to fulfil your desire of visiting his Holiness, so that you may enjoy a still greater measure of his favour. Seeing how many promises he makes in his letters, we may hope that you will easily get something to help our brother. Meanwhile we will not fail to entreat the Lord, from whom all grace proceeds, that your desire may be granted you, if indeed it be for the best. I imagine that by this time you will have written a most beautiful letter to his Holiness, to congratulate him on his having obtained the tiara. As I feel rather curious about it, I should like extremely, if you do not object, to see a copy of what you may have written. I thank you infinitely for what you have sent, and also for the melons, which we were very glad to get.

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[5] Allan-Olney 1870, 268–269. Cf. Galilei 1890–1909, 15: 292–293; Sobel 2001, 323–327. In the last sentence of this passage, I have corrected Allan-Olney's *straiter* into *stricter*, as a translation of the Italian *stretta*.

[6] Cf. Gattei 2019, 281–308.

[7] Galilei 1890–1909, 13: 118–119.

> As I have written in very great haste I must beg you to excuse the bad handwriting. All join me in hearty greetings.[8]

The last letter from which I will have time to quote here is the one Maria Celeste wrote her father on March 19, 1633. Since February, Galileo was in Rome, where he had been summoned by the Inquisition to stand trial, although the first deposition did not occur until April 12. Lodging at the Tuscan embassy under conditions of house arrest, Galileo was in relatively good health and spirits, feeling that he would be able to explain himself to the satisfaction of the authorities. Although Maria Celeste knew that her father had gone to Rome on some kind of business, he had spared her the pain of telling her the real reason or the whole truth. So, on March 19, she wrote her father primarily to ask him to bring her a gift from Rome, namely a diptych with the images of Jesus and the Virgin Mary. Here is the relevant passage:

> I want you to bring me a present on your return, which I trust is not far off. I am sure that at Rome copies of good pictures are easily obtained, and I should like you to bring me a little picture the size of the enclosed piece of paper. I want the kind they make to shut up like a little book, with two portraits, one an *Ecce Homo*, and the other a Madonna, and I wish them to have as tender and heavenly an expression as possible. I do not care for ornaments. A plain frame will be quite good enough, for I only want it on purpose to keep it always by me.[9]

From these letters Maria Celeste Galilei emerges as a person with a great soul and a loving heart. But such love is also revealing about Galileo: the conclusion I would want to draw is that if he could raise such a daughter, and if he could inspire such love, his own personality must not have been as rotten as his many enemies, old and new, like to portray; such anti-Galileanism is best exemplified by Bertolt Brecht's play and by Arthur Koestler's *Sleepwalkers*. However, focusing on the daughter, Galileo's own humanity gains in stature.

8.5 Readers' Comments on *Galileo's Daughter*

Finally, just as I am interested in Galileo's daughter primarily insofar as she enables us to gain a better understanding of her father, I am interested in Sobel's book primarily insofar as its reception enables us to understand better the significance of Galileo and the Galileo affair in Western culture. Now, one way of glimpsing at the reception of Sobel's book is to examine the reader's comments on it found at the website Amazon.com. The great majority of these comments are appreciative and positive, but it is more instructive to examine the negative ones.

The most common criticism of the book has been that the title is misleading because it suggests that the main topic is Galileo's daughter, whereas in fact the book is primarily a biography of Galileo. The most sophisticated statement of this

[8] Allan-Olney 1870, 110–111. Cf. Galilei 1890–1909, 13: 120; Sobel 2001, 3–5.

[9] Allan-Olney 1870, 240–241. Cf. Galilei 1890–1909, 15: 70–71; Sobel 2001, 209–211.

criticism was made on October 13, 2002, by someone named Melissa Shogren from Redmond, Washington. It says:

> This book saddened me. I wrote a doctoral dissertation many years ago about Scandinavian women writers in the late 1800's. One of the points I discussed was how hard it was for these women to be heard and accepted as authors. Too often they were overshadowed by the male authors of the day, sometimes justifiably so, sometimes not. Sobel's book perpetuates the stereotype of the woman who can only be viewed through the lens of the great men around her. Granted, this was a VERY great man who dominated and directed her life. But, there is so much the author could have done to bring Galileo's daughter into clearer focus. How about a deeper discussion of the role women played in the society of the day? More on convent life and why women chose and were forced to choose this type of life? Quote more from the letters? All that came through for me was a portrait of an almost servile, fawning daughter. Was this typical of the day? Are there other examples of women's letters from the period? As I found in *Longitude*, Sobel is better at presenting the science than making the characters human and immediate. I look forward to another book that would truly explore Galileo's forgotten daughter.

What are we to make of this criticism? It seems to amount to objecting that the critic would have liked Sobel to write a different book. This criticism is unfair. Admittedly, the title does not accurately represent the book's content, but one can hardly blame Sobel for wanting to first catch our attention with a catchy title; it is readily apparent that the title should not be taken too literally, but rather it is meant to convey the idea that the book focuses much more on Galileo's relationship to his daughter than the typical biography of a great scientist does. Since one of several aspects of the book is indeed Galileo's daughter, when contextually understood the title is not too misleading; it misleads only those who want to be misled.

Another reader (Lise Alper, from Englewood, CO, on February 25, 2001) asks "What's all the fuss about?" and then elaborates as follows:

> 'Galileo's Daughter' has been a best-seller now for at least 6 months. I have no idea why. This book is well written. The research is workmanlike. The illustrations are good. The only fault I can find with it, is: why was it written in the first place? Sister Maria Celeste was a loving, supportive daughter to Galileo. Wow! That's a revelation! I guess no other great minds in history had loving supportive families. It's not even that Sister Maria Celeste was a forgotten figure until Dava Sobel dug her up. Judging from the source material cited at the end, there are at least 4 other books (mostly Italian language) that deal primarily with Sister Maria Celeste. So, again I ask: why was this book written? Admittedly, it's always good to get a refresher course in the importance of a figure such as Galileo. His breakthroughs, insights & originality of mind were truly awe-inspiring. The history of the Catholic Church is also always good to remind us of the necessity of the Protestant revolution. There just doesn't seem to be a current need for another book on the subject.

One could take issue with this reader's remark about "the necessity of the Protestant revolution." However, rather than get involved in such a controversy, I want to point out that such a remark suggests that for some people Sobel's book has been the occasion for re-fighting the struggle between Catholicism and Protestantism. Moreover, if we reflect on and take seriously this reader's why-question, sooner or later one possible explanation to be considered would be the one which another reader explicitly advanced.

That is, soon after the book was reprinted as a Penguin paperback, a reader named M. Spencer (from New York, on February 5, 2001) posted some comments that carry the heading: "Penguin Targets Female Buyers (Was it Title VII?)." Such a heading suggests a kind of reverse-sexist explanation of the book's phenomenal success. Be that as it may, this reader goes on to add the following cynical remarks: "There is an interesting untold story here … of fornication and illegitimacy without 'Inquisition' and corrupt and grueling convent life during the same period Jamestown and the Plymouth Colony were founded. While a good biography of Galileo, this is no story of the father and daughter relationship nor a story of the daughter's life or calling." I take these remarks as an indication of cynicism rather than Puritanism, for it is hard to believe that this reader is really objecting to Galileo's fathering a daughter out of wedlock and to the Inquisition's failure to persecute him for such a sin.

8.6 Conclusion

In conclusion, what I call the Galileo affair is alive and well. It did not end in 1992 when Pope Saint John Paul II closed his re-examination and concluded his rehabilitation of Galileo. Sobel's book and the NOVA television program are the latest evidence that the topic is an inexhaustible source of heat and light about such issues as the relationship between science and religion, between individual freedom and institutional authority, between Catholicism and Protestantism, and even about the relationship between men and women.

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Chapter 9
Galileo's Father: Method and Argument in Musicology, Physics, and Astronomy

Abstract This is a critical analysis of the relationship between Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632) and his father Vincenzo's *Dialogue on Ancient and Modern Music* (1581). The analysis is carried out with the awareness that Vincenzo practiced systematically the experimental method in the science of music, and thus influenced Galileo, who applied it in the physics of falling bodies. Moreover, there are some conspicuous similarities between the two books, e.g. their titles and their nuanced critique of authority. However, there is no comparison between Galileo's and Vincenzo's books with regard to dramatic power, unified coherence, critical reasoning, and methodological self-reflection. Furthermore, whereas the son favors the moderns, the father favors the ancients; and whereas the son advocates an anti-clerical position, the father advocates a pro-clerical position. It follows that, unlike the case of experimentation, the son in writing his *Dialogue* did not learn, and could not have learned, much from his father's *Dialogue*.

9.1 Introduction

Vincenzo Galilei (c. 1520–1591) was a noteworthy and controversial musician, composer, musicologist, and historian of music, and so his life and work can be studied and have been studied from the point of view of the history of music.[1] Vincenzo was also the father of Galileo Galilei, and so could be studied but has *not* been studied from the point of view of the son, given that Galileo's scientific contributions destined him to become the "Father of Modern Science,"[2] and that his Inquisition trial made him the protagonist of "the greatest scandal in Christendom"[3]; such an approach might lead us to view Vincenzo primarily as "Galileo's father" in a manner analogous, but appropriately different, to how "Galileo's daughter" (Virginia

[1] See, for example, Cohen and Prins 2019; Gozza 1989; and Konoval 2018.

[2] Einstein 1954, 271; and Hawking 1988, 179.

[3] Koestler 1964.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_9

Galilei) was viewed in the international best-seller *Galileo's Daughter*.[4] It should also be possible to focus on the *relationship* between the respective lives and works of father and son, although, of course, such a relationship has many distinct aspects; this approach will be followed here with regard to their *intellectual* relationship, in particular an aspect that may be called *methodological*.

In fact, it is well known that Vincenzo Galilei practiced systematically the method of experimentation in the field of the science of music, and that in this regard he influenced his son Galileo, who applied it in the physics of falling bodies. Moreover, there is a very striking similarity between their most famous works, Vincenzo's *Dialogue on Ancient and Modern Music* (1581) and Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632); this pertains to their attitude toward authority. These two facts suggest the potential fruitfulness of, and provide the motivation for undertaking, a relatively novel project: a more detailed critical comparison between the father and son's two dialogues, in part to determine the father's influence on the son in this regard. Thus, I shall proceed by reviewing the two relatively well-known facts, and then elaborating the critical comparison.

9.2 Vincenzo's Influence on Galileo's Experimental Methodology

To prevent misunderstanding and focus our attention, we must be careful in formulating and describing the historical importance and topical relevance of Vincenzo's methodological influence on Galileo. It should be formulated by saying that Vincenzo's experimentation in musicology had a formative influence on the experimental methodology which Galileo used and elaborated in the course of his epoch-making contributions to physics and astronomy. Let me explain this initial point by discussing some of the relevant literature.[5]

In 1975, Stillman Drake published a study of the role of music in Galileo's experiments. The experiments in question were probably performed in 1604 and involved balls rolling down inclined planes. Galileo was trying to measure such variables as distance traversed, time elapsed, and speed, in order to determine whether the relationship among these quantities could be described in terms of some relatively simple mathematical formula. At the time he was feeling his way toward the so-called law of odd numbers; that is, that the distances traversed by a falling body in successive equal times are to each other as the odd numbers from unity. Eventually, of course, he established this law, as well as others related to it; for example, the so-called law of squares, according to which the distance traversed by a falling body increases as the square of the time elapsed; and the principle of uniform acceleration, that the

[4] That is, Sobel 1999.

[5] Coelho 1992; Drake (1975, 1992, 1999); Palisca (1992, 2003). See also: Fabbri 2019; Heller-Roazen 2011, 65–69; and Peterson 2011, 149–173, 255–258.

instantaneous velocity acquired by a falling body increases in direct proportion to time.

In this kind of experiment one obviously needs a precise way of measuring time intervals. Music enabled Galileo to do this. That is, the musical training which Galileo received mostly from his father enabled him to measure equal times very accurately by means of listening to the beats: at various points the inclined plane was fitted with bands so that the rolling ball would cause sounds when going over them; the distances between one band and another would then be adjusted until the time-intervals could be judged equal as detected by the musically trained ear. Such bands were analogous to so-called "frets" in musical instruments. The whole apparatus was, to use Drake's incisive words, "a kind of metronome."[6]

In a later paper, in 1992, Drake elaborated the connection by studying some of the experimental investigations that had been carried out earlier by Vincenzo himself and witnessed by Galileo. This had happened in 1588, when Vincenzo conducted a series of experiments to determine the sounds produced by vibrating strings if their tension, rather than their length, is changed. In the process, Vincenzo discovered what may be called a law of physics, specifically in the field of acoustics: that is, that if different sounds are produced by different tensions to which a given string is subject, "the ratio 3:2 does not hold for the perfect fifth."[7] According to Drake, these experiments probably led Galileo specifically to his study of pendulum motion, besides teaching him the value of experimentation in general and enabling him to learn various aspects of experimental design.

Now, Drake is not the only scholar who has studied this connection between Galilei father and son.[8] Another one who has done so is a scholar who is more well known in musical circles, namely Claude Palisca. In a paper published in 1992, and stemming from the same conference that yielded Drake's 1992 paper, Palisca argued that Vincenzo was an experimental scientist in the full sense of the term. Palisca's thesis is a general one regarding Vincenzo, although he left the resulting connection to Galileo implicit. Palisca summarizes his account of Vincenzo's multi-faceted scientific-experimental activities as follows:

> He experimented with strings of various materials to determine whether an unison could be achieved between them and whether at a fixed tension and length two unison strings of different material produced unisons when stopped at equal segments of the length of the open string. He hung weights to strings of different lengths, diameters, and quality to determine what factors determined the consonance of the octave and what numerical ratios resulted from measuring each dimension when the octave was produced. He experimented with pipes of various lengths and diameters and resultant volumes to determine the ratios of these dimensions necessary to produce the octave, and inversely to discover what consonances were produced when any of the dimensions were in duple proportion.[9]

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[6] Drake 1975, 310.

[7] Drake 1992, 215.

[8] Needless to say, my acceptance of Drake's evidence and thesis on this point does not amount to a wholesale acceptance of everything he said about Galileo; note, for example, that I criticize Drake in Sect. 9.5 below.

[9] Palisca 1992, 151.

Palisca is keen to point out that such experimental investigations involved much more than mere observation, since they involved tinkering of the type needed when tuning a lute or a keyboard instrument. He also points out that Vincenzo's motivation was not, however, merely practical, but had a theoretical aspect, namely the search for truth or acquisition of knowledge. That is why he can be regarded as an experimental scientist.

To give my own twist to these relatively well known conclusions advanced by Drake and Palisca, I would say the following. Vincenzo Galilei was a pioneer and skillful practitioner of the experimental method in the field of acoustics, or the science of music, as he himself might have labeled or conceived it. His son Galileo learned the experimental method largely from him, and then of course went on to refine and improve it in general, as well as to practice it in the field of kinematics, or the science of motion; in this field Galileo managed to discover some of the fundamental laws of motion. Galileo's combination of relatively novel substantive physical laws and relatively novel procedural methodological principles and practices, together with their application to the Copernican hypothesis in the astronomical and cosmological field, was instrumental in producing the scientific revolution and, as already mentioned, made him become, in the intuitive judgment of working scientists, "the father of modern science." Now, given that the crucial methodological innovation was the art of experiment, and given Vincenzo's role in Galileo's development of the experimental method, it follows that the musician Vincenzo Galilei might be called the grandfather of modern science.

In the context of such a grandiose claim, it should be stressed that here we are talking about active experimentation as distinct from mere observation. Passive observation had been advocated and practiced by empiricists, including Aristotle and his followers, from times immemorial. However, active experimentation is as much aprioristic as empirical. For it involves the active intervention of the investigator into the natural order and the manipulation of various natural conditions in order to create artificially produced and reproducible conditions. Observation or sense-perception has a role, of course, but it is only a part of a complex process. The process also involves the following elements: thinking or theorizing in order to design the apparatus and plan the experimental conditions; manipulation and intervention in order to actually build the apparatus and create the conditions; numerical measurements to fully exploit the information the experiment can provide; and mathematical analysis to render the numerical measurements intelligible. This difference with mere observation is enormous.

It is perhaps impossible to overestimate the importance in human history of the rise of experimentation as distinct from mere observation in the search for truth and the acquisition of knowledge. And so it may be appropriate here to quote the eloquent words of Antonio Gramsci, the modern classic who intuited this deep truth when the tragedy of his imprisonment enabled him to think about things from the point of view of eternity.[10] I quote Gramsci for the eloquence of his words, and not to further justify my claim by appealing to his authority, since in this field of epistemology

[10] Gramsci (1965, 58; 1994, 1: 83). Cf. Finocchiaro 1999, 14; Joll 1977, 100–101.

or scientific methodology I for one would not regard him as a legitimate authority. Here, then, are Gramsci's eloquent words:

> There is no doubt that the establishment of the experimental method separates two worlds of history, two epochs, and it begins the process of the dissolution of theology and of metaphysics, and the process of the development of modern thought, whose crowning is the philosophy of praxis. Scientific experience is the first cell of the new method of production, of the new form of active union between man and nature. The scientist-experimenter is also a worker, not a pure thinker, and his thinking is continually controlled by practice, and vice versa, until a perfect union between theory and practice is formed.[11]

This influence of Vincenzo Galilei on his son's practice of experimentation involves only a particular, although crucial, element of Galileo's scientific methodology. We shall soon go on to explore whether a similar influence is present in Galileo's practice of argumentation. However, before we leave this element of experimentation, it is useful to mention a directly related issue which cannot be elaborated here and would require much more extensive investigation. That is, Galileo's works are filled not only with reports of experiments he actually performed, but also with descriptions of imaginary experiments which he had not performed; here the question arises whether such "thought experiments" could really be performed and what results they would yield. Moreover, even for actual experiments, Galileo often reports numerical values that are simplifications and approximations at best; here the question would be whether or in what sense such numerical reports are correct. Obviously, a complete account of Galileo's scientific methodology would have to include not only the relationship between active experimentation and passive observation, but also the role of thought experiments and of simplifications and approximations.[12]

9.3 A Striking Parallel Between the Two *Dialogues*: Nuanced Critique of Authorities

As mentioned earlier, this experimental-methodological connection between father and son provides one general reason for wanting to explore their relationship further; but there is another more specific reason. This involves a possible connection between their two respective major works. For both father and son, about ten years before their respective deaths, published books written in dialogue form that may be considered their respective masterpieces. Both books deal with their respective specialties, and do so by undertaking a critical comparison and contrast between the ancient and the modern systems. Of course, the substance or content of each book is different, music for Vincenzo and cosmology (astronomy and physics) for Galileo, and so in this case a study of their relationship can be expected to make sense only from the point of

[11] Gramsci 1975, 1449. Cf. Gramsci 1971, 446; Finocchiaro (1988a, 89–90; 1988b, 71–72).

[12] For details on such long-standing and controversial issues, which have generated a monumental literature, see for example: Drake 1973; Finocchiaro 2010a; Koyré (1966, 226–228; 1968, 13; 1978, 166–167); MacLachlan 1976; Matthews 2004; Naylor 1977; Palmieri 2008; Settle 1961.

view of the form of these works; this would involve such things as methodological or epistemological orientation, philosophical outlook, argumentative structure, literary style, and so on.

Such an expectation can be based on something more concrete than merely the initial impressions just summarized, which can be gathered from the titles of the two books and from very general facts about their authors. This more concrete piece of evidence is a striking passage that occurs at the very beginning of Vincenzo's *Dialogue on Ancient and Modern Music*. In this passage, one of the book's two speakers makes several methodological suggestions that are immediately and completely accepted by the other speaker:

> Before Your Lordship begins to untie the knot of the proposed questions, I wish in those things which sensation can reach that authority always be set aside (as Aristotle says in the Eighth Book of the *Physics*), and with it the tainted reason that contradicts any [sense] perception at all of truth. For it seems to me that those who for the sake of proving some conclusion of theirs want us to believe them purely on the basis of authority without adducing any further arguments are doing something ridiculous, not to say (with the Philosopher) acting like silly fools. This privilege was not conferred on anyone but the most wise Pythagoras—whom you mentioned just now—by his followers. I wish further that you agree to let me freely ask questions and to answer you without any sort of adulation, as suits those who search for the truth of things.[13]

To say that this is a striking passage means especially that it will so impress someone acquainted with Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*. In fact, this passage is reminiscent of many Galilean passages that elaborate the same themes. In particular, there is a crucial passage at the beginning of the Second Day where Galileo includes all four of the points mentioned by Vincenzo in this passage, and does so with language that has the same tenor, although it is much more polished and elaborate.[14]

The first one of Vincenzo's suggestions appears to be an endorsement of Aristotle's principle that sense perception should have priority over speculative reason. Galileo's *Dialogue* expresses a similar reference and a similar endorsement, primarily in connection with the conclusions implied by the new telescopic observations.[15] A good example occurs in the context of his argument that the heavenly region and bodies are changeable like the terrestrial region and bodies, thus refuting the substantive Aristotelian thesis that the heavens are immutable, but the earth is mutable. Galileo's words are worth quoting:

> Does he [Aristotle] not affirm that what is given to us by experience and the senses must be put before any theory, however well-founded it may seem? And does he not say this decisively and without any hesitation? … Consider now these two propositions, which are both parts of Aristotle's doctrine: the last one, claiming that one must put the senses before theorizing; and

[13] For the first two (long) sentences of this passages, I am quoting the translation in Palisca (1992, 143), which I think is slightly better than the translation in Palisca (2003, 12); for the last two sentences, I am quoting from Palisca (2003, 12), since they were not included in Palisca 1992. Cf. V. Galilei 1581, 2. I should add that I first read these remarks by Vincenzo in Palisca 1992.

[14] Galilei 1890–1909, 7: 132–139; and Galilei 1997, 117–128.

[15] Galilei 1890–1909, 7: 57, 75, 80, 136–137, 348; and Galilei 1997, 83, 96, 104, 123, 223.

> the earlier one, which regards the heavens as unchangeable. The last one is much more solid and serious than the earlier one, and so it is more in accordance with Aristotle to philosophize by saying 'the heavens are changeable because so the senses show me' than if you say 'the heavens are unchangeable because theorizing so persuaded Aristotle'.[16]

And this brings us to the second part of Vincenzo's suggestions. It appears to be a rejection of the excessive or exclusive reliance on authorities, which might be called a rejection of authoritarianism, or a plea for independent-mindedness. Of course, the presence of these traits in Galileo's work is well known. However, there are several places in the *Dialogue* that express such ideas. The following passage, in particular, is very similar in content and tenor:

> anyone who has eyes in his head and in his mind should use them as a guide. Not that I am thereby saying that one should not listen to Aristotle; on the contrary, I applaud his being examined and diligently studied and only blame submitting to him in such a way that one blindly subscribes to all his assertions and accepts them as unquestionable dictates, without searching for other reasons for them. This abuse carries with it another extreme impropriety, namely that no one makes an effort any longer to try to understand the strength of his demonstrations. Is there anything more shameful in a public discussion dealing with demonstrable conclusions than to see someone slyly appear with a textual passage (often written for some different purpose) and use it to shut the mouth of an opponent? If you want to persist in this manner of studying, lay down the name of philosophers and call yourselves either historians or memory experts, for it is not right that those who never philosophize should usurp the honorable title of philosopher.[17]

A third part of Vincenzo's suggestions is the point that the blame for authoritarianism does not lie in the authority or great thinker being appealed to, but in his followers. Vincenzo's example here is Pythagoras and his numerical ratios for musical consonances. In the *Dialogue*, Galileo uses the example of Aristotle and his doctrines in natural philosophy regarding the world system and the location and behavior of the earth in it. With this change however, Galileo's general point is identical, as the following words reveal: "I believe and to some extent know that the world is full of very extravagant brains, whose follies should not redound to the discredit of Aristotle … It is some of his excessively cowardly followers who are responsible for making us think less of him."[18]

Vincenzo's fourth and final request is a plea for the freedom of discussion, argument, and criticism, without having to worry about offending others for holding different opinions. This point too, is echoed by Galileo in the comprehensive methodological passage at the beginning of the Second Day of his *Dialogue*. In fact, that preliminary discussion ends with these words:

> SALVIATI. … So, Simplicio, come freely with reasons and demonstrations (yours or Aristotle's) and not with textual passages or mere authorities because our discussions are about

[16] Galilei 1997, 104; cf. Galilei 1890–1909, 7: 80. There is also a reference to this point in the comprehensive methodological passage at the beginning of the Second Day (Galilei 1890–1909, 7: 136–137; 1997, 123), but that reference does not provide an equally quotable and self-contained passage.

[17] Galilei 1997, 126–127; cf. Galilei 1890–1909, 7: 138–139.

[18] Galilei 1997, 123; cf. Galilei 1890–1909, 7: 136.

> the sensible world and not about a world on paper … Simplicio together with Aristotle is firmly on the side of [the earth's] immobility; because of this, he will present step-by-step the motives for their opinion, I will present the answers and arguments for the contrary side, and Sagredo will say what goes on in his mind and to which side he feels drawn. SAGREDO. I am happy with this arrangement, but on the condition that I am free to introduce whatever simple common sense may suggest to me. SALVIATI. Indeed, I beg you to do exactly that.[19]

9.4 Content, Structure, and Character of Galileo's *Dialogue*

Thus, there are at least two initial reasons that suggest the potential fruitfulness of examining the relationship between Vincenzo's *Dialogue on Music* and Galileo's *Dialogue on the World Systems*. The first is that it is already relatively well established that the father influenced the son with regard to the development of the method of experiment. The second is that there is a philosophical reflection at the beginning of the *Dialogue on Music* which is hard to miss for even a casual reader and which contains several themes that can be easily seen to have found an echo in Galileo's *Dialogue*.

Assuming now that we have decided to undertake a more detailed critical comparison of the two dialogues, one way of proceeding next would be to study the Galilean *Dialogue* to arrive at some critical interpretation of that book. Then on the basis of such an understanding one would go on to examine the *Dialogue on Music* for possible sources, influences, similarities, and differences. Such a procedure is natural for someone who approaches this topic from the side of the son. However, such a reverse chronological order also makes logical and historiographical sense because, with all due respect to the world of music and the history of music, the significance of this investigation stems from the greater significance of the later achievement by the son, and so if one were to study the father's work first one would not really know what to focus on. Moreover, such meta-cognitive awareness can help one avoid the pitfalls of being Whiggish or wise after the event.[20] My critical interpretation of Galileo's book may be sketched as follows.

The *Dialogue on the Two Chief World Systems* is the book in which Galileo elaborated his mature synthesis of the new astronomy and the new physics; that is, the astronomy made possible by Nicolaus Copernicus's work[21] and by Galileo's own telescopic discoveries, and the physics which he and others were building by rejecting the doctrines of Aristotelian natural philosophy and adapting the work of Archimedes. For example, the book contains discussions of the mountains on the moon, the phases of Venus, sunspots, the principle of conservation of motion,

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[19] Galilei 1997, 127–128; cf. Galilei 1890–1909, 7: 139.

[20] For details, see such works as the following: Beltrán Marí (1994, pp. xi–lxxiv; 2006, 429–482); Besomi and Helbing 1998, 2: 1–906; Camerota 2004, 399–469; Clavelin 1974, 224–267; Finocchiaro (1980; 1997; 2010a; 2014; 2019, 123–154); and Mayer 2015, which book ought to be read in conjunction with Finocchiaro 2015.

[21] Here I am talking about astronomy as Galileo elaborated it in his *Dialogue*, where unfortunately he did not take into account Johannes Kepler's contributions.

and the laws of falling bodies and of pendulum motion. Thus, the *Dialogue* is a key document of that epoch-making development which has come to be called the Copernican Revolution, or more generally the Scientific Revolution.

However, the book was also the immediate cause of Galileo's trial and condemnation as a suspected heretic by the Inquisition in 1633. Thus, the text is also a crucial document of the controversy which came to be known as the Galileo Affair, and which has become the source of inexhaustible and continuing reflections and disputes about the nature and relationship of science and religion. Now, since the religious institution which was involved in the affair was a Catholic Church which in that historical context happened to wield political power, its relevance extends even further and includes the question of the interaction between science and politics.

The book is also a masterpiece from other points of view, the most important and far-reaching of which is perhaps critical reasoning. That is, the book is a critical examination of all scientific and philosophical arguments on both sides of the Copernican controversy, the key issue being whether the earth moves by rotating daily around its own axis and revolving annually around the sun, or whether it stands still at the center of the universe. Such critical reasoning is crucially connected with the other two already-mentioned features of the book. This may be explained as follows.

Some of the traditional arguments against the Copernican idea of the earth's motion were based on astronomical (naked-eye) observation, for example: the apparent dichotomy between the properties and behavior of heavenly bodies and of terrestrial bodies; no phases exhibited by the planet Venus; and no drastic variation in the apparent size of the planet Mars. Galileo was able to answer these objections by elaborating the discoveries he made with the telescope, which revealed similarities between heavenly and terrestrial bodies, the phases of Venus, and appropriate variations in the appearance of Mars. Next, some of the anti-Copernican arguments were based on indisputable observations of the motion of terrestrial bodies, for example: that freely-falling bodies move vertically; that gunshots toward the east and toward the west range equally; and that bodies unattached to the earth's surface do not fly off toward the sky. The problem with these phenomena was that (according to Aristotelian physics) they could not occur on a moving earth. Galileo was able to answer such mechanical objections against the earth's motion by arguing that, according to his own more adequate new physics, on a moving earth such phenomena would occur as observation de facto reveals. In this manner, Galileo's criticism of the astronomical and of the mechanical arguments against Copernicanism was also an elaboration of the new astronomy and physics that made the Copernican Revolution possible.

Now, let us look at the connection with the Inquisition trial and condemnation. To begin with, it should be noted that Galileo's *Dialogue* contains *no* critical examination of a group of arguments that might be labeled biblical, hermeneutical, theological, and religious; here the main argument was that the earth's motion was contrary to Scripture, based on various scriptural passages. He had criticized this argument at an earlier stage of his career (1613–1616),[22] and as a result he had been warned

[22] On this particular topic, see Galileo's "Letter to Castelli," in Galilei 1890–1909, 5: 281–288, translated in Finocchiaro, 1989, 49–54; Galileo's *Letter to the Grand Duchess Christina*, in Galilei

by Church authorities that he was not supposed to defend the earth's motion from such religious objections. He had also been warned that the earth's motion should not be defended as true, but only as an hypothesis for explaining and predicting observed phenomena. However, in the *Dialogue*, besides presenting and explaining the scientific and philosophical arguments for and against the earth's motion, Galileo took the liberty of evaluating or assessing their validity and strength. It turned out that the arguments in favor of the earth's motion were much stronger than those for the earth's rest, and this yielded the conclusion that the geokinetic hypothesis was more likely to be true than the geostatic one. The Inquisition could not tolerate such critical reasoning, and eventually Galileo agreed to plead guilty to such a transgression.

The *Dialogue* is also a model of methodological reflection. That is, it is full of discussions meant to clarify concepts, rules, and principles needed in the search for truth and acquisition of knowledge. This was the result partly of Galileo's psychological inclinations, but also of the fact the specific scientific issues with which he was dealing were so fundamental that they raised issues about how to go about resolving them. For example, the telescopic discoveries raised questions about the propriety of using artificial instruments in the search for truth. And the mechanical objections to the earth's motion and the new physics required to answer them raised issues about the quantitative value of the difference between mechanical phenomena on a moving and on a motionless earth; and this in turn raised questions about the nature of mathematics and its application to natural science. Moreover, many of the anti-Copernican arguments were explicitly methodological, epistemological, and philosophical, and so Galileo's criticism of them involved methodological issues. A good example of this is the argument from the deception of the senses: if the earth moves, then the human senses would be deceived, since even Copernicus admits that we do not sense or perceive any such motion; but if our senses are deceived, then the main human instrument of knowledge is unreliable and knowledge is impossible. This methodological aspect of the *Dialogue* and of Galileo's work in general is so significant that one may characterize it by calling Galileo "the Socrates of methodology."[23]

Furthermore, the book is pregnant with rhetorical meaning because many of the questions under discussion were not susceptible of rigorous proof or disproof, or of empirical observational confirmation or disconfirmation, but involved considerations of plausibility and persuasiveness. Moreover, because of his tensions with ecclesiastical authorities, Galileo could not always say what he meant and did not always mean what he said.

It should also be mentioned that, because of Galileo's gifts as a writer, and because he poured his heart and soul into the project, the *Dialogue* is a literary masterpiece.

1890–1909, 5: 309–348, translated in Finocchiaro 1989, 87–118; and the critical analysis of these letters in Finocchiaro (2010b, 65–96; 2019, 99–108). Most strikingly, see also the recent discovery by Salvatore Ricciardo of the original manuscript copy of Galileo's "Letter to Castelli," which has received wide discussion, for example in: Abbott 2018; Anonymous 2018; Camerota et al. (2018, 2019); and Camerota and Ruffo 2019, 303–322.

[23] For this particular judgment, see Finocchiaro 2019, 4.

In fact, some have placed its literary value among the top handful of masterpieces of the Italian language, along with such classics as Dante's *Divine Comedy*.[24]

Finally,[25] Galileo wrote the book in the vernacular Italian, rather than the scholarly Latin, which would have been normal for its subject matter of astronomy, physics, cosmology, and philosophy. The fundamental reason for this is that he wanted to appeal also to a broader audience of educated nonexperts, and he was acutely aware of the fact that the ideas he was presenting were of general cultural significance, and not merely the concern of professional scientists and philosophers. Accordingly, he also wrote the book in the form of a dialogue among three speakers: one expert taking the Copernican side, another expert taking the geostatic point of view, and a third character who is an intelligent and educated layperson. What all this means is that the book was originally conceived and largely written as an attempt to combine a democratic and an elitist ideal, an egalitarian and an aristocratic commitment.

9.5 Toward a Comparative Analysis of Vincenzo's *Dialogue*

The next and final task in this project is to examine Vincenzo's *Dialogue on Music* to see whether or not there are any aspects of its content or form that may be regarded as having been adopted or contradicted by his son, at least in his own *Dialogue*. In the present context, this is perhaps the main task, and everything up to now may be regarded as preliminary or propaedeutic prolegomena. This task may also be expected to be indefinitely long, and having already motivated it, all one can do here now is to get it started. Note also that I speak of adoption or contradiction on Galileo's part, for while cases of adoption are obviously important from the point of view of positive influence, cases of contradiction or rejection may be regarded as instances of negative influence. Furthermore, we should be open to the possibility of similarities or shared features which, instead of suggesting influence from father to son, may be important for other reasons. Finally, one should also be on the lookout for cases of differences or disparate features, for they too can be significant insofar as they suggest that in these cases the son must have used other sources, or must have used his own creativity and fashioned the results out of his own genius.

Despite the intellectual motivations mentioned above, this task is much more daunting than one might think. The main reason is that Vincenzo's book is full of highly technical and archaic terminology about music.[26] My plan here is to bypass

[24] For this particular judgment, see Calvino 1968, 106–107; and cf. Bellini 2006.

[25] Perhaps it should be added that Galileo's *Dialogue* was conceived, written, and published in a historical context in which the dialogue form was a popular and significant literary genre; such was the situation in sixteenth and seventeenth Italy; for more details on this, see Cox 1992. I do not elaborate on this aspect because both Galileo and Vincenzo were obviously part of the same cultural milieu and so the point applies to both their books.

[26] For example: (1) "It is necessary as a base for this tall edifice to examine diligently every interval of the diatonic species concerning which the following question arises: What diatonic species best conforms with the intervals with which we compose and sing today? … The first species we will

these hyper-technicalities and hyper-archaicisms without any fatal loss by focusing on relatively formal matters.

Let us begin with the full titles of the two books. If one starts reading Vincenzo's *Dialogue*, in a facsimile of the original edition of 1581, one can't help being struck by the book's full title. That is, there is a parallelism with the full title of Galileo's *Dialogue*, which parallelism is relevant to an important interpretive problem in Galilean studies. The key feature is that in both titles the author's first and last names immediately follow the word *Dialogue*.

The full title of Vincenzo's book reads as follows: *Dialogue by Vincenzo Galilei, Florentine Nobleman, on Ancient and on Modern Music*.[27] The full title of Galileo's book is much longer, but begins similarly and has the same structure: *Dialogue by Galileo Galilei, Lincean Academician, Extraordinary Mathematician at the University of Pisa, and Philosopher and Chief Mathematician to the Most Serene Grand Duke of Tuscany; where in meetings over the course of four days one discusses the Two Chief World Systems, Ptolemaic and Copernican, proposing indeterminately the philosophical and natural reasons for the one as well as for the other side*.[28]

Now, it is well known that this title in Galileo's printed book was not his own favorite one, insofar as it lacks any mention of the ebb and flow of the sea, that is, of the tides. In fact, he referred to this book as his *Dialogue on the Tides*, during the period when he actively wrote the book in 1624–1630, as well as during the initial stages of the imprimatur process starting in 1630, when he traveled to Rome to have the manuscript approved for publication. The connection is that Galileo was convinced that the puzzling phenomenon of the tides could be explained satisfactorily only as the effect of the earth's motion, and so such an explanation provided the strongest argument (although not the only one) in favor of the Copernican hypothesis; indeed, the fourth and last Day of the book deals with this topic.

However, Pope Urban VIII did not want the tides mentioned in the title, for various complicated reasons. For example, he thought that such a geokinetic explanation of

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examine … is … the tense syntonic of Ptolemy. After we have examined this, we will see … the very ancient ditonic diatonic, the ratios of which were investigated in the sixtieth Olympiad with subtle arguments by the rigorous Pythagoras" (V. Galilei 2003, 11–12; cf. V. Galilei 1581, 2). (2) "The followers of Aristoxenus added to his thirteen modes two more on the high end, so that the extreme notes of their diapasons fell outside the ordinary system at that end … Their project was very much favored by their discovery that the extremes of the thirteen that he proposed corresponded at the octave and not at the seventh, like those of Ptolemy. His highest was the Hypermixolydian, which was nothing but a replica of the Hypodorian. They did the same with the Hypoiastian and the Hypophrigian, raising them an octave to form the Hyperaeolian and the highest, the Hyperlydian" (V. Galilei 2003, 135–136; cf. V. Galilei 1581, 56). (3) "The ancient musicians, then, divided their tonoi into plagals and authentics, leaving between one and another … the space of a diatessaron" (V. Galilei 2003, 183; cf. V. Galilei 1581, 75).

[27] "Dialogo / di Vincentio / Galilei Nobile / Fiorentino / della Musica Antica / et della Moderna."

[28] "Dialogo / di / Galileo Galilei Linceo / Matematico Sopraordinario / dello Studio di Pisa. / E Filosopfo, e Matematico primario del / Serenissimo / Gr.duca di Toscana. / Dove ne i congressi di quattro giornate si discorre / sopra i due / Massimi Sistemi del Mondo / Tolemaico, e Copernicano; / Proponendo indeterminatamente le ragioni Filosofiche, e Naturali / tanto per l'una, quanto per l'altra parte." Cf. Finocchiaro (1980, 12–18; 1997, 359–360; and 2005, 133).

the tides would lend the geokinetic hypothesis a realistic status which he found unacceptable, because he wanted to limit it to an instrumentalist function. Another reason involved the pope's perception of the book's relationship to the Index Decree of 1616, which had declared Copernicanism to be contrary to Scripture, but which the pope interpreted as not equivalent to a declaration of heresy; so, up to a certain point, he wanted to convey the message that discussion of the Copernican idea of the earth's motion, although problematic, was allowed. Thus, Galileo was obliged to drop from the book's title any reference to the tides.

As just described, these facts are relatively uncontroversial, but they generate many other issues that are controversial. One of these is the question whether Galileo had originally structured his book in such a way that the tides were the primary topic of discussion as a long-standing problem to be solved, or puzzling fact to be explained, and then the world systems were a secondary topic introduced insofar as the best explanation of the tides made use of the hypothesis of the earth's motion, which was not only questionable from an astronomical, physical, and epistemological point of view, but also from a religious or theological point of view. Such an order and structure in the book's composition would be the reverse of the one in the actual book, where the two chief world systems are the primary topic, and then among the very long list of arguments and evidence there is one dealing with the tides. To resolve this question, Galileo scholars have examined various features of the content and structure of the book, various references to it in the correspondence of the period, and various aspects of the logic and methodology of the argument based on the tides.[29]

Besides using such evidence, one of these scholars, Stillman Drake, used the wording in the book's title to support his thesis that the published book reversed the topical order and explanatory structure of the original manuscript.[30] Drake claimed that as its stands, the full title in the published book is odd, insofar as it seems to mention no main topic, but rather mentions a topic only in the sentence following Galileo's name, titles, and affiliation. That is a sentence which Drake claimed should be regarded as the book's subtitle, and of course what is mentioned there is the chief world systems. Drake also claimed that what happened is that Galileo, in order to comply with the instructions of Church authorities, besides restructuring the book's content, made the following change in the original title. The change was simply to drop the words "on the Tides" immediately after the words "dialogue." That is, the original title supposedly read: *Dialogue on the Tides, by Galileo Galilei, Lincean Academician, Extraordinary Mathematician at the University of Pisa, and Philosopher and Chief Mathematician to the Most Serene Grand Duke of Tuscany; where in meetings over the course of four days one discusses the Two Chief World*

[29] See Drake (1980, 1983, 1986); Finocchiaro (1980, 12–18; 2014, 215–219; 2019, 144–149); Pitt 1992; and Shea 1972, 172–189.

[30] Drake (1980, 5–6, 17–19; 1983, 23–24, 28–29; 1986, 40–41). It should be noted that Drake's 1983 essay stems from an address delivered by Drake in 1982 to the Accademia dei Lincei, at a celebration of the 350th anniversary of the publications of Galileo's *Dialogue*.

Systems, Ptolemaic and Copernican, proposing indeterminately the philosophical and natural reasons for the one as well as for the other side.

Now, we can go back to the title of Vincenzo's own book. Given that its title was not burdened with vicissitudes and problems like those affecting the titling of Galileo's book, the title of Vincenzo's book suggests that it was normal to have the author's name immediately after the word *Dialogue*, and before a description of the book's content. Thus, there was nothing odd, peculiar, or unusual in the title of Galileo's book. It was much longer than the title of Vincenzo's book, given the more numerous and longer academic and professional titles which Galileo held, and given the longer description of the book's approach as well as content which Galileo wanted to convey. However, aside from the length and the detailed description, the structure of the two titles is the same, namely: 'dialogue', author's name, author's affiliations, and topic. These considerations invalidate, I believe, this particular argument by Drake, although, of course, they do not affect his other arguments supporting his interpretation.

Next, let us examine the two books from the point of view of their dramatic power. An easily noticeable difference between Vincenzo's and Galileo's books is that the former has only two interlocutors in the dialogue, but the latter has three. This is important for the following reasons.

The three speakers in Galileo's *Dialogue* are: Salviati, named after a Florentine friend of Galileo's who was dead by the time he wrote the book; Sagredo, named after a Venetian friend who was also deceased; and Simplicio, named after the sixth century philosopher Simplicius, who is one of the greatest commentators on Aristotle. As a first approximation, these three speakers have the following roles. Salviati is the spokesman for the Copernican world system, whose key thesis is that the earth is a planet revolving annually around the sun and rotating daily around its own axis; thus he defends the Copernican arguments and criticizes the geocentric ones. Simplicio is a spokesman for the Ptolemaic or Aristotelian world system, whose chief claim is that the earth stands still at the center of the universe, with all the heavenly bodies revolving around it; correspondingly, he defends the Ptolemaic and Aristotelian arguments and criticizes the Copernican ones. And Sagredo is an intelligent layman, who does not know much about the topic but is interested and curious to learn more, by listening to both sides and making up his mind as a result of the critical scrutiny of their arguments.

Although these are the predominant roles of the three Galilean speakers, these roles are only a first approximation because Galileo also frequently takes the liberty of portraying them in more nuanced ways.[31] For example, sometimes the anti-Copernican arguments are stated by Salviati, before they are subjected to criticism; this is especially true for the more-modern arguments, because Simplicio confesses that Salviati knows them better than he does. Sometimes Simplicio makes distinctions or raises objections which are acknowledged by both Salviati and Sagredo as being important and essentially valid. And Sagredo often objects to Salviati, and not

[31] Besides the nuances described in what follows, others are discussed in Hall 2017.

just to Simplicio. The upshot of such communicative, dramatic, and logical interactions is that Galileo's own position can only be equated to the synthesis of all three speakers; it would be an over-simplification and a mistake to equate Galileo with Salviati.

By contrast, Vincenzo's *Dialogue* has only two speakers.[32] They are both named after Florentine aristocrats, amateur musicians and composers, acquaintances of Vincenzo's, who were still alive at the time of the book's publication. One is Giovanni Bardi, who was actually a patron of Vincenzo and the host of the academy known as the Florentine Camerata. The other is Piero Strozzi, a member of one of the leading aristocratic families of Florence, who later founded his own musical academy. Bardi is by far the main speaker, and Strozzi has the relatively minor role of asking for information, explanations, and justifications. These are provided by Bardi in what are long uninterrupted monologues. And Strozzi seldom, if ever, criticizes, counter-argues, objects, or rebuts. Regarding Vincenzo's own position, the same thing applies as for the case of his son, namely that it should be equated to the synthesis of what the two speakers say.

As a general impression, there can be no doubt that Vincenzo's dialogue lacks the dramatic, dialectical, or argumentative power of Galileo's. I believe this is due partly to the asymmetric or unbalanced relationship of Bardi and Strozzi; partly to the lack of a third interlocutor; partly to their less well-defined role; and partly to the less well-focused topic. For example, there does not seem to be a single instance of the maieutic or Socratic method in Vincenzo's book, which would have been quite feasible even without a third speaker; nor are there any instances of arguments and counter-arguments between equals or peers. On the other hand, Galileo's *Dialogue* is filled with examples of both.

To call Galileo's *Dialogue* an argumentative or dialectical drama may be taken to be relabeling a feature which also struck his contemporaries. Two of their descriptions are particularly relevant here: in 1632, immediately after reading the book, Tommaso Campanella wrote to Galileo saying that he regarded the book as a "philosophical comedy," reaching such heights "that we need not envy Plato"; and in 1634 and 1635, the French intellectual and diplomat Nicolas Claude Fabri de Peiresc, first in a letter to Cardinal Francesco Barberini and then in a letter to Galileo, both written in Italian, called the book a "problem-oriented play."[33]

Needless to say, this comparison is not meant to be an invidious one made in order to detract from Vincenzo's work. Instead the proper conclusion to draw is that it was not from his father that Galileo could have learned how to write a work with

[32] This contrast is also mentioned in Hall 2017, 140–141; although she is dealing primarily with Galileo's works, the contrast is relevant and important as an indication of the contextual historical novelty of his literary style.

[33] "Comedia filosofica" and "che non avemo a invidiar Platone": Campanella to Galileo, 5 August 1632, in Galilei 1890–1909, 14: 366–367. "Scherzo problematico": Peiresc to Barberini, 5 December 1634, in Galilei 1890–1909, 16: 169–171, at line 43; Peiresc to Galileo, 1 April 1635, in Galilei 1890–1909, 16: 245–248, at line 68. Cf. Finocchiaro 2005, 52–56, 376 n. 53; Westman 1984, 334.

the dramatic and argumentative power of his *Dialogue*. Galileo was rather imitating, improving upon, and surpassing Plato, not his own father.

Let us now go on to a very general feature of the content of the two books, so general that it may be regarded almost as a formal feature, and may thus provide a meaningful comparison or contrast between them. Vincenzo's most general thesis is that ancient music is superior to modern music. On the other hand, Galileo's most general thesis is that modern or Copernican astronomy is superior to ancient or Ptolemaic astronomy. This suggests that in their respective fields, the father was some kind of reactionary, whereas the son was some kind of revolutionary. But various clarifications and qualifications are in order.[34]

First, the meaning of ancient and modern may be somewhat vague or ambiguous, especially for Vincenzo, who explicitly uses these terms. However, I believe it is relatively clear that by ancient he refers to the period of classical Greek, Roman, and Hellenistic civilization; and by modern he means occurring roughly in the 100 years previous to the time of his writing. Galileo does not stress the blanket terms ancient and modern, but uses the more specific labels Ptolemaic and Copernican. Nevertheless, under the label Ptolemaic, Galileo includes the Aristotelian world view, and so he is talking about the same chronological period in the history of astronomy which his father was referring to for the history of music. Similarly, if we allow for the fact that Galileo wrote his book about 40 years after his father wrote his *Dialogue*, then the Copernican world view was the modern one that started being elaborated by Copernicus about a century before. Thus, chronologically speaking, the meaning of ancient and modern in the two Galileis can be made clear and is essentially the same.

Second, in their respective criticisms, both father and son make an important distinction. That is, although Vincenzo is critical of modern music, and of the claims of such musicians and musicologists as Franchino Gaffurio (1451–1522), Heinrich Glarean (1488–1563), and Gioseffo Zarlino (1517–1590), he has great respect for their individual accomplishments and aims to improve upon them.[35] Similarly, although Galileo is highly critical of Aristotelian and Ptolemaic astronomy and cosmology, he has great respect for Aristotle and Ptolemy individually. The real problem lies, as we saw earlier in reporting Galileo's attitude, with their modern followers living in his time, who have an excessively uncritical attitude toward their masters and do not realize that if these masters were living at that time they would agree with Galileo instead; for example, they would join him in holding that the heavenly bodies are changeable because many changes have been observed in recent times.

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[34] It may also be added that this aspect of the two works makes them important documents in the long-term controversy about the cultural significance of the ancients vs. the moderns. This is the controversy that later became enshrined into the *querelle des anciens et des modernes* (e.g., by Fontenelle 1687), and which eventually some labeled "The Eternal Feud" (Gilbert 2014). However, already at the time of the two Galileis, it was being explicitly and intelligently discussed by Tassoni (1620, Chapter 10, "Paragone degli ingegni antichi e moderni").

[35] "These writers deserve the highest praise, and the world owes them perpetual obligation": V. Galilei 2003, 7; cf. V. Galilei 1581, 1.

Third, let us ask what they mean by "superior," which in part relates to the justification of their main theses. What Galileo means is relatively clear, but what Vincenzo means is hard to fathom.

In holding that Copernican astronomy is superior to Ptolemaic astronomy, Galileo means primarily that the key Copernican claims are more likely to be true than the key Ptolemaic claims; and of course the former reduce to the thesis that the earth moves, the latter to the thesis that the earth stands still. And the general reason for this greater likelihood is that the Copernican theory is better than the Ptolemaic one with respect to a number of cognitive values such as the following: observational accuracy, systemic simplicity, explanatory power, and research fruitfulness.

For example, almost all telescopic observation corresponded to the Copernican and not to the Ptolemaic theory. Moreover, as Copernicus himself had shown, the known facts about the motions of the heavenly bodies could be systematized on the basis of a few basic hypotheses like the earth's motion, without having to add extra assumptions in an ad hoc manner, which was required in the Ptolemaic system. Furthermore, presumably, the Copernican theory could, but the Ptolemaic theory could not, explain the existence of the tides. And Copernicanism had produced more new discoveries and results in 100 years than Aristotelian cosmology had done in 2000 years.

Finally, it is important to understand why Galileo displayed a comparative point of view and focused on the relative merits of the two world views, claiming that the Copernican view was more likely, more accurate, more systematic, more explanatory, and more fruitful, rather than claiming it to be absolutely true, accurate, and so on. One reason was that direct sense experience still favored the Ptolemaic view, and this criterion continued to have some value, and indeed is an element of observational accuracy; but after telescopic observation became possible, direct sense experience was not the only such element. Another reason was that although most telescopic observation did favor Copernicanism, even the telescope failed to reveal an annual parallax of the fixed stars, that is an annual shift in their apparent position which should be observable if the earth revolved around the sun.

Now, *mutatis mutandis*, let us see whether something analogous can be said with regard to Vincenzo's claim that ancient music is better than modern music. To move in that direction, more needs to be said about what he refers to by modern and by ancient music. By modern music, the key phenomenon Vincenzo has in mind is polyphonic or contrapuntal music, and by ancient music he means homophonic or monodic music. This understanding presupposes the historical claims that ancient music was monodic and modern music was contrapuntal, and the historical accuracy of these claims has been questioned.[36] However, Vincenzo's key point is that monody is superior to counterpoint, and this is primarily a theoretical or aesthetic claim, largely independent of the historical question.

Now, once the focus becomes polyphony and monody as musical practices, rather than ancient and modern music as historical periods, two important consequences should be mentioned. First, Vincenzo was not the only critic of polyphonic music. The

[36] Palisca 2003, p. lx.

Catholic Church, especially in the wake of the Council of Trent, was also critical.[37] Thus, in a sense, Vincenzo was taking a pro-clerical position. However, since there is no question that in many other ways Vincenzo was secularist-minded, here we have at least an ironical situation, or perhaps an additional example of the many unresolved contradictions in his *Dialogue*, which Claude Palisca has described.[38] Second, biographically speaking, Vincenzo's critique of contrapuntal music seems to have been a temporary phase of his career. In fact, in his earlier career he had been a skillful performer and composer of polyphonic music, and later he wrote (although he did not publish) a treatise on the art of counterpoint, arguing that it is centrally important.[39] Such shifts of opinion may, of course, be explicable and/or justified, but they certainly raise a warning flag. The second shift is especially troublesome since it seems to involve reverting back to the pre-*Dialogue* position. These facts suggest that perhaps this major argument in the *Dialogue on Music* need not be taken seriously. However, I have not yet identified or discussed this major argument.

In the attempt to find this argument, it is helpful to realize that although Vincenzo's argument for monody and against polyphony is a major theme of his *Dialogue*, it is not the only one. There are several other themes that do indeed relate to the topic of ancient and modern music, but cannot be really integrated into the critical-evaluative argument of the superiority of one over the other. For example, there are parts of the book that are purely historical, elaborating accounts of ancient musical instruments and notation. And there are parts that are purely theoretical, elaborating analyses of the problem of tuning and of various tonal and modal systems.

This feature is also worth mentioning in this context because it generates an important difference from Galileo's *Dialogue*. The latter book also discusses many different topics, from Aristotelian natural philosophy and the observational discoveries made possible by the telescope to the physics of falling bodies, the theoretical interpretation of planetary motion and of sunspots and of new fixed stars (novas), and the phenomenon of the tides and fluid motion. However, all these topics can be made part of arguments for or against the earth's motion, whose analysis and evaluation generates the Galilean justification of the greater probability of Copernican over Ptolemaic astronomy.[40] On the other hand, in the case of Vincenzo's book, one must learn to disregard many parts for the purpose of finding the thread of his major argument against counterpoint and for monody. This difference between the two works may be described by saying that Galileo's book has a high degree of unity and coherence which Vincenzo's book does not possess.

Let us now focus on this particular aspect of Vincenzo's book, namely on his critical-evaluative claim that ancient or monodic music is superior to modern or polyphonic music. Bibliographically speaking, this discussion is found mostly and roughly in the middle of the book,[41] after the theoretical analysis of the tuning

[37] Palisca 2003, p. liii.

[38] Palisca 2003, p. xxi.

[39] Palisca 2003, pp. xxi, lvii.

[40] See the sketch given above.

[41] V. Galilei 1581, 79–90; and V. Galilei 2003, 197–227.

controversy and of tonal systems in the first half, and before the historical account of ancient musical instruments and notation in the second half. If one examines this discussion, there is no question that Vincenzo does attempt to justify his critical claim with some supporting reasons and does defend it from some objections.

For example, there is a strand of argument claiming that ancient monodic music had powerful effects on the emotions and feelings of the audience which modern polyphonic music cannot even approach.[42] There is another strand arguing that modern music is too hedonistic, in the sense that "its goal is only to delight the hearing, while that of the ancient was to lead others by its means into the same affection as one felt oneself."[43] Another strand argues that modern contrapuntal music is too formalistic, treating as inviolable, rules that are at best approximate guidelines; here Vincenzo has in mind the rule "that prohibited two perfect consonances of the same species from succeeding one another. Another rule decreed that from an imperfect consonance, one had to proceed to the nearest perfect consonance."[44] Another argument is the following, and here I quote verbatim: "the nature of low pitch is different from the high, and the intermediate is different from either of these. Further, … the property of fast movement is different from the slow, and the intermediate is unlike either of these. Since these two principles are very true, we may easily conclude … that singing in consonance in the manner modern practitioners use is an impertinence. For consonance is a mixture of low and high pitch that … strikes the ear without offense, with delight, or most smoothly."[45]

There is no doubt that these arguments raise interesting and important issues about the nature and purpose of music. One issue in particular is the question of the role of expression of feelings in music and more generally in art. In this regard, Vincenzo seems to hold a conception of art which is reminiscent of the aesthetics of Benedetto Croce[46]; that is, the Crocean view that art is the expression, contemplation, or intuition of feeling, and so music would be such a "lyrical intuition" by means of sounds or songs. However, this Crocean conception is perhaps merely implicit in the *Dialogue on Music*, in the sense that Vincenzo is assuming or presupposing it in some of his arguments. On the other hand, at the level of explicit reflection, one can't help being struck by the following general conception which Vincenzo states at the beginning of this part of the book: "Note that when I now speak of the usefulness of music, I mean the true music … music, I say, was introduced by humanity … principally to express with the greatest effect their ideas and states of their souls in celebrating the praises of the gods, master-minds, and heroes, … and secondarily to impress these ideas with equal force in the minds of mortals for their utility and

[42] V. Galilei 1581, 81; and V. Galilei 2003, 200–201.

[43] V. Galilei 2003, 224; cf. V. Galilei 1581, 89.

[44] Palisca 2003, p. lv. Cf. V. Galilei (1581, 80, 81; and 2003, 197, 201).

[45] V. Galilei 2003, 202; cf. V. Galilei 1581, 81.

[46] Croce 1965, Part I, Chapter II, pp. 15–25; and "Aesthetica in nuce," in Croce 1951, 195–197. Fano (1947, 31–32) mentions that Croce, in some of his historical works, expressed a general appreciation for non-philosophical treatises on art of the seventeenth century, and that this appreciation can be taken to implicitly and indirectly apply to Vincenzo Galilei's *Dialogue on Music*; cf. Croce (1919, Part I, Chapters II and III; and 1967, 88–89).

solace."[47] Regarding this assertion, Croce would certainly object to the theological as well as to the utilitarian connotations which Vincenzo seems to be attributing to music.

Thus, Vincenzo's book does contain arguments for his major thesis of the superiority of ancient or monodic music, and these arguments do raise important aesthetic or philosophical issues about the nature and function of music in particular, and of art in general. Now, two analogous claims could be made about Galileo's book: that it contains arguments for his major thesis of the probable truth of modern or Copernican astronomy or cosmology, and these arguments raise important epistemological, methodological, or philosophical issues about the nature of science, the search for truth, or the acquisition of knowledge. However, there is no comparison with regard to such things as the intensity or frequency of substantive argumentation and the subtlety and concreteness of the philosophical reflections. This judgment is not referring to the correctness or incorrectness of their respective major theses; for it would be prejudicial and an oversimplication to make much of the fact that Galileo's main conclusion was essentially right, and Vincenzo's essentially wrong. Rather the judgment is referring to the character of their substantive reasoning and of their philosophical reflections. That is, Vincenzo's reasoning is much harder to find, to understand, to reconstruct, and to assess; and his philosophical reflections are much less well-grounded in the substantive discussions, much less subtle, and much less well-balanced or judicious. By contrast, as mentioned earlier, Galileo's *Dialogue* is a model in the art of critical reasoning and a classic in the art of methodological reflection.

9.6 Conclusion

In summary, Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* and Vincenzo Galilei's *Dialogue on Ancient and Modern Music* represent more of a case of contrast, than of parallelism. There is no comparison between these two works with regard to dramatic power, unified coherence, depth of critical reasoning, and incisiveness of methodological self-reflection. Moreover, whereas the father is siding with the ancients, the son is siding with the moderns; and while the father is advocating a pro-clerical position, the son is advocating an anti-clerical position. This last pair of claims is a simplification, but not an over-simplification, because the proper nuances and qualifications can be added if need be. It follows that, historically speaking, the son, in writing his *Dialogue on the World Systems* did not learn, and could not have learned, much from his father's *Dialogue on Music*.

This relationship between these two works contrasts with the father-son relationship regarding the method of experimentation, for which case it is undeniable and significant that the son did learn it largely from his father. The latter case, it will be recalled, provided us with the main motivation to undertake the comparison of the

[47] V. Galilei 2003, 201; cf. V. Galilei 1581, 81.

two dialogues. It should also be recalled that another motivation for this undertaking was the apparent similarity of the anti-authoritarian themes expressed in a passage at the beginning of Vincenzo's *Dialogue on Music*, with many central themes in Galileo's *Dialogue*. Now, in light of the general critical comparison of the two books undertaken here, one should perhaps re-examine that passage and those themes to determine whether or not the correspondence is more apparent than real. But that is a task for another occasion.

Acknowledgements I was first exposed to the possibility of a critical comparison between Vincenzo Galilei's musicology and Galileo's science at the Tenth Aston Magna Academy on Music, the Arts, and Society, on "Foundations of the Italian Baroque, 1560–1620," at Rutgers State University in 1991; I thank Raymond Erickson for that opportunity. The possibility re-emerged at an event dubbed "The Re-Trial of Galileo," at the University of New South Wales, in 2009, on the occasion of the International Year of Astronomy; there, I happened to be encouraged to pursue the present project by Andrew Byrne, an Australian musician who, playing the chitarrone and the lute, performed works by Vincenzo and by Michelangelo Galilei (Galileo's brother). Next, a short version of this paper was first presented at the conference "Galileo musico: Music and the Arts in the Thought of Galileo Galilei," at the Egida Sartori and Laura Alvini Early Music Seminars, sponsored by the Giorgio Cini Foundation, Venice, 2010; for this, I thank the organizer Pedro Memelsdorff.

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Chapter 10
Painting vs. Sculpture in the Cigoli Letter

Abstract This essay is partly a case study of the role of logic in historiography. It is also partly a test case for the thesis of a Galilean correspondence between aesthetic attitude and scientific thought, advanced by Erwin Panofsky, Alexandre Koyré, and John Heilbron. Intrinsically, it is a discussion of the authenticity of the letter to Cigoli dated June 26, 1612, widely attributed to Galileo, containing argumentation about the relative aesthetic merits of painting and sculpture. I undertake a systematic analysis of the letter's method of argument, comparing and contrasting it with Galileo's. I argue that the letter does have some Galilean characteristics: critical reasoning; *ad hominem* argumentation, in the seventeenth-century sense; and appeal to experimentation. However, the letter falls short of the typical Galilean open-mindedness, fair-mindedness, and clarity; crucially, it uses several illative terms which Galileo never uses, and does not use the one he uses most often. The latter features outweigh the former. Moreover, I discuss some aspects of the letter's substantive content, primarily a theory of vision that disregards the dynamics of perspective and the faculty of binocularity, which Galileo understood and exploited very well.

10.1 Introduction

This paper has philosophical, historical, and erudite motivations and aims. I begin with the matter of erudition.

The National Edition of Galileo's collected works includes a letter dated June 26, 1612 and addressed to Lodovico Cardi da Cigoli (1559–1613), a friend of Galileo's and a painter then living in Rome (Galilei 1890–1909, 11: 340–43).[1] This letter contains a long, complex, intense, and brilliant piece of argumentation. It advances multiple criticisms of several arguments for the aesthetic superiority of sculpture over painting, and it formulates several arguments for the opposite thesis that painting is superior to sculpture as an art form. In the course of one of these arguments, there is also a reference to the alleged superiority of instrumental music over vocal music.

[1] For an English translation, see Panofsky 1954, 34–37.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_10

The chief editor of Galileo's works, Antonio Favaro, transcribed the letter from a manuscript copy made in the second half of the seventeenth century, found in a file labeled "Galileo's Letters" in the Masetti Archives in Florence. He printed the letter in italics and prefaced it with the following warning: "We seriously doubt the authenticity of this letter: partly because there is no trace of the topic it discusses in the many letters by Cigoli to Galileo; partly because, conversely, only one of the many topics discussed in those letters is mentioned here; and partly because its style does not always have a Galilean flavor" (Galilei 1890, 1909, 11: 340).[2] That is, Favaro was judging that this letter was probably not authored by Galileo, based on reasons of its substantive content and its style, although he was not saying whether he meant literary or logical style, or both.

Presumably Favaro saw no way of resolving the question of the letter's authenticity by means of considerations involving physical evidence or archival research. In this essay, I am making this assumption for the sake of the argument, although I do not exclude the possibility of conducting deeper and more updated archival inquiries. However, these would have to be carried out by others and are outside the scope of my investigation here. Until and unless such inquiries are carried out, and until and unless they yield conclusive evidence, all we can do is to engage in indirect argument and probable reasoning.

Favaro's judgment and reasons have not convinced all scholars. In particular, about a century ago, Margherita Màrgani (1922) attempted to give a point by point refutation of his reasons and a justification of the letter's authenticity. I do not find her criticism and constructive argument convincing, but I want to focus on one aspect of her approach which I find valuable and want to pursue further, more deeply, and more systematically. It involves Favaro's third reason, about the letter's un-Galilean style.

On this issue, Màrgani discusses not only Galileo's literary or linguistic style, but also his logical style, scientific methodology, manner of reasoning, or method of argument. She claims (Màrgani 1922, 564, 568) to find in the letter a manner of reasoning that corresponds to the one discernible as characteristic of the corpus of Galileo's works. This finding provides her with her main reason for claiming that the letter is authentic. I think such a correspondence would indeed constitute very strong evidence in favor of the letter's authenticity. However, I am dissatisfied with her analysis of the correspondence, which strikes me as too generic and superficial in regard to the Cigoli letter, and too partial and limited with regard to the Galilean corpus. Thus, I plan to undertake my own analysis.

Before doing that, however, another critic of Favaro and defender of the letter's authenticity is worth mentioning. I am referring to Erwin Panofsky's monograph on *Galileo as a Critic of the Arts* (1954).[3] This is a wide-ranging and multi-faceted work, and the Cigoli letter is only one of several themes. But it fits very well with the others in the sense that Panofsky attributes to Galileo an aesthetic attitude which

[2] All translations are my own, unless otherwise noted.

[3] See also Panofsky (1956a, 1956b). Cf. Koyré 1955; Rosen 1956; Shea 1985; Bredekamp 2001, 184–186; Tongiorgi Tomasi 2007.

he labels variously classicism, anti-mannerism, and critical purism, and which he finds not only in the Cigoli letter, but also in Galileo's literary criticism on Torquato Tasso, and even in Galileo's scientific work relating to the circular vs. elliptical orbits of the planets. Moreover, even on the Cigoli letter, Panofsky's account is manifold, stressing mostly various elements of its substantive content.

However, one aspect of Panofsky's discussion is that he too thinks that the manner of reasoning exhibited in the letter has important consequences for the question of its authenticity. His own words are worth quoting: "The strongest argument in favor of Galileo's authorship, however, is the … letter itself. The claims which had to be refuted are, needless to say, entirely conventional; but the manner in which they are refuted constitutes the only original contribution to the subject since Leonardo da Vinci" (Panofsky 1954, 7). Again, I accept the validity and fruitfulness of using the letter's manner of reasoning to help us decide the authenticity of Galileo's authorship, but I find Panofsky's logical and methodological analysis too partial, one-sided, and haphazard. This will become evident below in the course of my analysis.

We are now almost ready to proceed to a systematic analysis of the content and structure of the Cigoli letter, with the aim of determining how Galilean or un-Galilean is its manner of reasoning or method of argument, and thus helping us decide the authenticity of the letter.

Before undertaking this investigation, however, it is worth noting that this issue is of more than merely antiquarian interest. There are at least two reasons for this, one methodological or philosophical, the other substantive or historical.

The methodological reason is that this investigation turns out to be an illustration of a relationship between the history and the philosophy of science. Now, the history-philosophy relationship can be, and has been, explored in several ways, one of the most common and fruitful being the study of how epistemological or methodological claims about the nature of science depend on evidence from the practice and development of science.[4] Here my focus is different and relatively novel. On the present occasion the branch of philosophy involved is what has been variously called applied logic, informal logic, practical logic, logic of real arguments, argument analysis, argumentation theory, philosophy of argument, immanent dialectical approach, and historical-textual approach to logical theory.[5] The relevant relationship is that of using philosophical ideas or techniques in order to address historical problems.[6] As mentioned, the historical problem here is the question of the authenticity of the Cigoli letter. And I will be exploiting primarily two logical notions, which deserve a brief but explicit elaboration.

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[4] As readers and authors of this journal can well testify, and as may be seen from Cobb 2011 and Chalmers 2011.

[5] Cf., *respectively*, Toulmin 1958, 255; Johnson and Blair 2002, and Walton 1989; Woods et al. 2002; Fisher 1988; Scriven 1976; Eemeren et al. 1996; Govier 1999; Krabbe 1999, and Houtlosser and van Laar 2007; Finocchiaro 2005, 14, 34–45.

[6] For some older examples of this kind of logical exercise, see Finocchiaro 1973, 1974, 2005, 340–360.

One notion reflects applied logic's key concern with the careful reconstruction of real arguments actually occurring in argumentative practice. Thus, in my reconstruction of the various arguments in the letter, I will be using a methodical numbering system for keeping track of various claims and indicating their place in the network that makes up the propositional macrostructure of arguments. The numbers are given in square brackets at the beginning of the sentences or clauses expressing the various claims. The key rules are these: if a given claim is labeled [n], then the premises or reasons that directly support it are labeled [n1], [n2], [n3], etc.; if claim [nm] is part of some subargument, then the premises or reasons directly supporting it are labeled [nm1], [nm2], [nm3], etc.; and when a given claim [n] is supported independently by two or more sets of premises, then letters are used to distinguish one set from another, e.g., [na1], [na2], [na3], …, [nb1], [nb2], [nb3], …, [nc1], [nc2], [nc3], etc.[7]

The other main notion is the concept of conductive argument; that is, an argument which attempts to justify a claim *non-conclusively*, on the basis of two or more *pro* reasons that are mutually *independent but cumulative*, and with the acknowledgment of at least one *con* reason. The term *conductive* is not important, and such arguments are also commonly called pro-and-con arguments and balance-of-considerations arguments. Some applied logicians have claimed that this type of argument provides the main alternative to deductive and inductive arguments, while other have maintained that all argumentation ultimately or normally consists of conductive arguments.[8] However, even without endorsing such sweeping claims, there is no question that conductive arguments so defined constitute an important class of arguments; for they are ubiquitous whenever one is trying to justify evaluations, interpretations, classifications, recommendations, policies, etc. Now, the connection with the present topic is that, as will become apparent, the main strand of argumentation in the Cigoli letter is an interesting and instructive example of a conductive argument; similarly, *my own* argument on the question of this letter's authenticity is itself a conductive argument, and while I would defend it as a *cogent and strong* argument, I do not pretend it to be conclusive and apodictic.

These two notions (systematic reconstruction and conductive argument) are technical elaborations of a very simple idea, namely the distinction between conclusions (or claims) and premises (or reasons). However, this distinction is merely the tip of a theoretical iceberg, and in fact those two notions amount to adding the following elaborations and refinements to that simple idea. To begin with, there are the two traditional distinctions between minor and major premises and between implicit and explicit premises. Then there is the distinction between intermediate and final premises, namely between premises which in a given argument are supported by further reasons, and premises that are not; this reflects the important fact that argumentation normally consists of more than a single inferential step. Next, there is

[7] I have adapted this framework from Angell 1964, 369–393. Cf. Eemeren et al. 1996, 16–19; Finocchiaro (1980, 311–331; 2005, 39–41); Fisher 1988, 19–24; Krabbe 1999, 470; Scriven 1976, 41–43.

[8] For discussions of the general concept, as well as of specific claims, see Govier 1999, 155–182; Johnson 2000, 84–88, 92–95; Johnson and Blair 2002, 349; Scriven 1976, 78–81; Wellman 1971, 51–83.

the distinction between linked and independent premises, namely between premises that support the same conclusion only if joined together, and premises that support it separately but cumulatively; this reflects the very important point that argumentation often consists of multiple arguments for the same conclusion. There is also the distinction between pro and con reasons, namely between premises that support a given conclusion and premises that support the denial of that conclusion; this reflects the important fact that reasoning normally involves reaching some conclusion in the light of both evidence and counter-evidence, and this is the important cognitive phenomenon which is stressed by the concept of conductive argument. And to mention just one last point, there is the challenge of devising visual representations or diagrams of such complex argumentative structures, usually in the general shape of tree branches or tree roots. In short, the crucial point (stressed by proponents of the applied-logic or informal-logic approach to logical theory) is that such technicalities are needed for a serious understanding and evaluation of argumentation and reasoning.[9]

Finally, the substantive or historical reason for the significance of the present issue is that the content of the Cigoli letter has been used (together with other evidence) to support a thesis about the correspondence between aesthetic attitude and scientific thought in Galileo, and by implication, in science in general. The argument was provided by Koyré in his enthusiastic essay review of Panofsky's book, in which he spoke of the "capital interest and importance" (Koyré 1955, 835) of such a correspondence. Panofsky welcomed such a generalization and elaboration of his original thesis, and so in an abridged and revised version of his work (published as an article in the official journal of the History of Science Society), he spoke of "an aesthetic attitude no less consistent than—and possibly interrelated with—his [Galileo's] scientific convictions" (Panofsky 1956a, p. 3). Echoes of such a thesis can be heard even from the inimitable Heilbron in his recent original account of Galileo's life and works; Heilbron claims that "perhaps the best single-word descriptor for Galileo is 'critic'. He was a true connoisseur of the arts and sciences, able, says Viviani, to talk intelligently and with apt quotations, on virtually every respectable subject with all sorts of people. As a connoisseur, he argued the excellence of painting over sculpture, monody over counterpoint, one version of Dante's hell over another, and, in mathematics, Archimedes over everybody. Galileo was the embodiment of baroque *buon gusto* in matters of art and science" (Heilbron 2010, pp. v–vi; cf. pp. 16–23).

Needless to say, this question of the (historical or substantive) pertinence of the present investigation should not be turned into a question of (professional) impertinence. That is, my aim is *not* to insinuate the general untenability of the views of Panofsky, Koyré, and Heilbron on the Galilean correspondence between aesthetic experience and scientific inquiry. For although they do assume the letter's authenticity, for them this is a relatively minor assumption; their views depend mostly on other evidence and arguments, and would thus be largely unaffected even if it turns out that the Cigoli letter is inauthentic. Rather, my aim is to suggest that the question

[9] The points mentioned in this paragraph are discussed not only in the sources cited in the two previous notes, but also in Freeman 1991; Govier 1999, 155–180; and Snoeck Henkemans 1992.

of the authenticity of the Cigoli letter is indeed pertinent to other larger issues, above and beyond the matter of pure erudition and the question of the history-philosophy connection.

10.2 Reconstruction of the Letter's Argumentation

The letter consists of eight paragraphs, the first six of which are full of argumentation, and the last two of which consist of informal remarks about what occasioned the letter and about the context and the personal conditions of the writer and the recipient. The argumentation in those first six paragraphs can be reconstructed as follows.

The first paragraph seems to have three main parts, each dealing with a slightly different issue. I shall first examine them separately, and then attempt to combine them.

The first part of the paragraph[10] criticizes the following argument: [S1] sculpture is more admirable than painting because [S11] sculptures have relief (along the third dimension) but paintings do not.[11]

The criticism claims that [P1] if and insofar as sculptures have relief but paintings do not, that would imply that *painting* is more wonderful than sculpture. The critical argument is this. [P11] The relief perceived in a sculpture is visible insofar as it is a painting, not insofar as it is a sculpture: for [P111] painting is the art that represents by means of shades of light and dark; and [P112] sculptures show relief only insofar as some parts are shaded light and some dark, as proved by the fact that [P1121] if we take a statue and darken its light surfaces so as to produce a single uniform shade, it will appear devoid of relief.

In other words, this criticism tries to show that the stated premise of the first pro-sculpture argument, even if that premise were acceptable, would imply the opposite of its conclusion.

The second part of that first paragraph[12] advances a critical argument that may be regarded as a different criticism of the same target argument.

The criticism is now that [P2] it is not exactly true that sculptures have relief but paintings do not. For [P211] without having actual relief, a painting can show depth much better than a sculpture; for example, [P2111] a painting can represent on a plane not only the depth of a human figure amounting to one or two cubits, but also the distance to a town or the expanse of a sea amounting to many miles. And [P212] to say that the sense of touch can reveal the visual deception of such a painting is true but irrelevant, because [P2121] sculptures and paintings are not

[10] Galilei 1890–1909, 11: 340, lines 4–14.

[11] Galilei 1890–1909, 11: 340, lines 4–5. I am labeling this and other pro-sculpture arguments by the letter 'S', and I use the label 'P' for pro-painting arguments or critical arguments against pro-sculpture arguments. Note also that I am beginning to label the various constituent propositions by the methodical numbering system introduced above (Sect. 1).

[12] Galilei 1890–1909, 11: 340, lines 14–22.

made to be touched but to be seen. Now, adding the assumption that [P213] the relief of sculptures is meant to represent three-dimensional depth, it follows that [P21] paintings have better means of accomplishing what sculptures accomplish by means of relief.

In other words, this second criticism aims to show that the stated premise of the first pro-sculpture argument is not exactly true.

In the third part of the first paragraph,[13] the argument under criticism seems to be a different one, namely that [S2] the relief possessed by statues is an admirable quality because [S21] it enables them to deceive us and to appear natural more easily.[14]

The criticism of this second pro-sculpture argument claims that [P3] it is not true that relief enables statues to deceive us more easily. The critical argument is that [P3a] paintings possess a kind of relief that produces optical illusion more than sculptures, since [P3a1] both paintings and sculptures possess the relief deriving from shades of light and dark, and [P3a2] paintings have colors but sculptures do not. Moreover, [P3b] regarding the sense of touch, it is not true that sculpture can deceive it more than painting: for [P3b1] to deceive means to operate in such a way that the sense to be deceived regards something not as it really is, but as it is intended by the representation; but [P3b2] no one who touches a statue will be deceived into thinking it is a live person; and [P3b3] if a statue is to deceive the sense of touch, it has to deceive not only the sensation of relief, but also the sensation of soft vs. hard, warm vs. cold, smooth vs. rough, and heavy vs. light.

Proceeding now to the second paragraph,[15] it seems to contain a critical argument designed to justify a thesis that is the opposite of the one advanced by the advocates of sculpture. The reasoning is the following. [P411] The visible depth of a statue does not derive from its three-dimensional relief. For [P41111] of the three dimensions, only width and height are subtended by the eye, since [P411111] we see only the surface of visible objects; but [P41112] the depth is not perceived, because [P411121] our vision does not penetrate inside opaque bodies; thus, [P4111] the eye sees only width and height, but not depth or thickness. But we said earlier that [P412] such visible depth derives from shades of light and dark. Therefore, [P41] we know depth not directly as an intrinsic object of vision, but indirectly as a result of shades of light and darkness. Now, [P42] sculptures get their shades of light and dark from nature, and paintings get them from human art. It follows, that [P4] a good painting is more admirable than a good sculpture.

Of course, this conclusion may be simplified as the thesis that the art of painting is more admirable than sculpture, which is just the opposite of the conclusion of the pro-sculpture argument criticized above.

Before proceeding, it is useful, and it turns out to be possible, to integrate all the points discussed so far. Let us note that the two pro-sculpture arguments explicitly mentioned and criticized so far may be integrated by combining them into a longer and more complex one. That is: [S3] sculpture is more admirable than painting because

[13] Galilei 1890–1909, 11: 340–341, lines 22–39.

[14] Galilei 1890–1909, 11: 340, lines 22–24.

[15] Galilei 1890–1909, 11: 341, lines 40–57.

[S31] sculptures have relief but paintings do not; and [S32] the relief possessed by statues is an admirable quality because [S321] it enables them to deceive us and to appear natural more easily. Now, taking this longer and more complex pro-sculpture argument as the target of criticism, then all four critical arguments presented above can be connected to it and structured as follows.

As mentioned last, the fourth criticism (P4, advanced in the letter's second paragraph) may be interpreted as a counter-argument, namely an argument that tries to justify the opposite of the main conclusion of the target argument. The first criticism (P1) had prepared the ground for this, without actually doing so; for that criticism amounts to arguing that the first premise stated in the pro-sculpture argument (independently of whether it is actually true or acceptable) would imply that painting is superior, namely the opposite of what the sculpture advocates think. In other words, even if we agree with them on that first premise about the relief of sculpture, what would follow is the superiority of painting, not the superiority of sculpture. However, the second criticism (P2) in the sequence undermines the truth or acceptability of that first premise, by casting doubt on whether we can really assert that sculptures have relief and paintings do not. Finally, the third criticism (P3) attempts to refute a second key premise in the fuller pro-sculpture argument, a premise that had been presupposed in the first target argument (S1) above, but which was explicitly stated in the second target argument (S2); namely the claim that relief enables sculptures to deceive more easily.

Let us now go on to the letter's third paragraph.[16] It begins with a statement of a fourth pro-sculpture argument, that is: [S4] sculpture is superior to painting, because [S411] nature makes men like sculptures and not like paintings, and so [S41] sculpture imitates nature more closely than painting.[17]

The criticism of this argument has three strands. First,[18] [P5] it is not true that nature makes men like sculptures and not like paintings, because [P51] nature makes men colored as well as sculpted, and the evidence for this is that [P511] she sculpts them and gives them color.

Second,[19] [P6] if it were true that nature makes men like sculptures and not like paintings, this would be a sign of imperfection and demerit for sculpture, for the correct aesthetic principle is that [P61] the more the means of representation are different from the things represented, the more the representation is challenging and potentially wonderful. To see this, consider the fact that [P61a1] in antiquity silent theater was esteemed more than spoken theater, and [P61a2] the explanation of this fact is that the former used a means of representation completely different from the actions represented. Consider also the fact that [P61b1] we admire a musician who by singing moves us to empathize with a suffering lover more than if he tried to achieve the same effect by just weeping; and [P61b2] the explanation of this fact is that singing is very different from, and indeed contrary to, pain and tears, whereas weeping is very

[16] Galilei 1890–1909, 11: 341–342, lines 58–79.

[17] Galilei 1890–1909, 11: 341, lines 58–59.

[18] Galilei 1890–1909, 11: 341, lines 59–60.

[19] Galilei 1890–1909, 11: 341–342, lines 60–75.

similar. Finally, consider that [P61c1] we admire an instrumental musician even more (than a vocal musician) if he achieves the same effect (of making us empathize with, for example, a suffering lover) without singing but by merely playing an instrument; [P61c2] the explanation of this fact is that the inanimate chords of an instrument are less suited than a live voice to mimic the feelings in our soul.

The third strand[20] of this criticism is that, given the aesthetic principle of differential representation just elaborated, [P71] there is little or no value in imitating sculpting nature by means of sculpture and representing three-dimensional objects by means of three dimensions; whereas, [P72] it is very challenging to represent solid objects of three dimensions by means of two dimensions in a plane. Thus, [P7] in this regard, painting is more wonderful than sculpture.

That is, we may summarize the letter's third paragraph by saying that it advances a three-fold criticism of the pro-sculpture argument (S4) from the imitation of nature. First, the key premise is questionable, namely the claim that nature makes men like sculptures and not like paintings. Second, the argument is inferentially unsound insofar as this key premise, if true, would imply the inferiority rather than the superiority of sculpture. And thirdly, it is possible to counter-argue that painting is indeed superior precisely because its imitation of nature is more indirect and its representation more different. This three-fold criticism is elegantly similar to the four-fold criticism of the previous pro-sculpture argument (S3).

Proceeding now to the fourth paragraph,[21] the target argument is now obviously this: [S5] the art of sculpture is superior to painting because [S51] sculptures are eternal, or at least more durable, as compared with paintings.

The criticism is brief. That is, [P8] this argument is worthless because [P81] it is not the art of sculpture that is responsible for the durability of sculptures, but the medium used, namely marble; but [P82] sculptures do not possess this value any more than naturally occurring rocks. Moreover, [P83] both sculptures and paintings are, strictly speaking, perishable.

In other words, the argument from eternity is worthless in three ways. First, insofar as durability is aesthetically relevant, the credit goes to the medium and not to the human art; and so the conclusion does not follow from the premise. However, durability is largely irrelevant because otherwise naturally occurring rocks would possess aesthetic value; and so the stated premise is largely irrelevant. Moreover, the stated premise is not exactly true. Again, note the pattern in the comprehensiveness of the criticism.

The fifth paragraph[22] in the letter seems to go back to the subject of the third one, namely the imitation of nature. It repeats a point already made there in the first criticism, namely that painting uses colors but sculpture does not. The presence and role of this brief paragraph is puzzling.

[20] Galilei 1890–1909, 11: 342, lines 75–79.

[21] Galilei 1890–1909, 11: 342, lines 80–83.

[22] Galilei 1890–1909, 11: 342, lines 84–86.

The sixth paragraph[23] also goes back to the topic of imitation, but advances a new counter-argument, namely the following: [P911] sculptors imitate things as they really are, painters things as they appear; but [P912] in reality things are in only one way, whereas they appear in countless ways; so, [P91] the task in the art of painting is much more difficult than in the art of sculpture; therefore, [P9] to excel in painting is much more admirable than to excel in sculpture.

In summary, the Cigoli letter is a long, complex, and intense piece of reasoning. It advances multiple criticisms of several arguments for the superiority of sculpture over painting, and it formulates several counter-arguments for the superiority of painting.

10.3 Galilean Aspects of the Letter's Method of Argument

How Galilean is this reasoning? Can we use what we know about the character and manner of Galileo's reasoning and argumentation to help us resolve the question of the authenticity of the Cigoli letter? Let us go on to explore this question.

Critical Reasoning In one respect, the letter looks like a typical example of what may be called critical reasoning, which Galileo practiced widely.[24] Here, by critical reasoning is meant reasoning aimed at the interpretation, analysis, evaluation, or self-reflective construction of arguments. By argument is meant a piece of reasoning aiming to justify a claim by supporting it with reasons or defending it from objections. And by reasoning is meant a form of thinking consisting of interrelating our thoughts in such a way as to make some thoughts dependent on others, by way of some thoughts being based on others or some thoughts following from others.

The best example of such critical reasoning in the Galilean corpus, and a paradigm example for all times and places, is Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican.* This is the fateful work published in 1632 that triggered the Inquisition trial and condemnation of Galileo the following year. The book is also Galileo's mature synthesis of physics, astronomy, and methodology. The whole book takes the form of a critical examination of the arguments for and against the Copernican hypothesis that the earth is a planet moving annually around the sun and daily around its own axis. For the many arguments against the earth's motion, Galileo gives clear statements, incisive analyses, and unfavorable assessments of them; and he also gives several arguments in favor of the earth's motion, formulated self-reflectively with an awareness of their relative merits and potential weaknesses. Generally speaking, and with some qualifications and exceptions to be discussed presently, the Cigoli letter does something very similar with respect to the arguments for and against sculpture vis-à-vis painting.

One immediate qualification is that critical reasoning in this sense is by no means unique to Galileo. Just to mention another classic example, the *Summa Theologica*

[23] Galilei 1890–1909, 11: 342, lines 87–91.

[24] For more details about this, see, for example, Galilei 1997, 309–335; and Finocchiaro (1980, 27–45, 167–179; 2010b, pp. xxxvii–xli, 132–134).

of St. Thomas Aquinas is full of critical reasoning. In particular, his discussion of the arguments for and against the existence of God fits this pattern.[25] Hence, this characteristic of the Cigoli letter is at least a weak reason for attributing it to Galileo, nevertheless it is a reason.

Ad Hominem Argumentation Another and stronger reason is that the letter contains several examples of a more characteristically Galilean manner of reasoning, which may be called *ad hominem* argumentation.[26] Here the term *ad hominem* argument must not be understood to have the common meaning prevalent today, namely that to argue *ad hominem* is to criticize a claim on the basis of the negative personal qualities or circumstances of the arguer advancing the claim, rather than on the basis of the inaccuracy, invalidity, or irrelevance of the reasons advanced by the arguer. Instead the notion of *ad hominem* argument must be taken in the seventeenth century sense of the term, which Galileo followed and practiced; that is, an *ad hominem* argument is one that derives a conclusion not accepted or not acceptable by an opponent largely on the basis of premises and reasons accepted or acceptable by the opponent but not necessarily by the arguer. This is a notion of *ad hominem* used by some philosophers even today. It corresponds to what is sometimes called internal criticism, namely criticism of a position or an opponent on the basis of ideas or facts that are parts of that same position or believed by that same opponent. Thus, one may, if one wishes, avoid using the often pejorative term *ad hominem* argument, and speak of internal logical criticism. The important point is that this type of argument or method of criticism is very powerful and effective, and can be sound if properly carried out.

The Cigoli letter contains at least two typical examples of *ad hominem* argumentation in this sense. One example occurs in the very first criticism in the first paragraph. There, as we have seen, the letter criticizes the pro-sculpture argument from relief by arguing that [P1] if and insofar as sculptures have relief but paintings do not, that would imply that *painting* is more wonderful than sculpture. Another example is provided by the sixth criticism in our sequence, occurring as the second criticism in the third paragraph. There the letter criticizes the pro-sculpture argument from the imitation of nature by arguing that [P6] if it were true that nature makes men like sculptures and not like paintings, this would be a sign of imperfection and demerit for sculpture.

These arguments are certainly powerful and impressive, as powerful and impressive as anything Galileo himself formulates. Thus, the occurrence of such *ad hominem* argumentation is a piece of evidence for attributing the letter to Galileo.

This feature of the Cigoli letter strikes me as a very strong reason for its authenticity because *ad hominem* argumentation is a characteristically Galilean technique. It is stronger than the feature of critical reasoning mentioned earlier insofar as it is a much more specific type of reasoning. Nevertheless, this reason is not decisive because *ad hominem* argumentation is obviously not unique to Galileo. Moreover

[25] Aquinas, *Summa Theologica*, part I, question 2, article 3; equivalent to Aquinas 1952, 1: 12–14.

[26] For more about this notion in general and in Galileo, see Johnstone (1952, 1959, 1978); Woods 1995; Finocchiaro (1980, 58, 131–132, 231–232, 368–370, 402–403; 2005, 277–291, 329–339).

and more importantly, as we shall soon see, the letter also contains considerable counter-evidence, which may very well outweigh the favorable evidence.

Experiment vs. Observation However, before revealing that counter-evidence, we have to mention other favorable evidence. In fact, the letter contains a third striking feature, which was briefly mentioned by Màrgani (1922, 561–562) and elaborated at length by Panofsky (1954, 8–9). This characteristic is the appeal to experimentation, as distinct from mere observation; that is, appealing to the results of active, interventionist manipulation of artificially produced and reproducible situations, as distinct from appealing to the passive observation of naturally occurring conditions. Such appeal might be called experimental reasoning. It occurs in the letter's first critical argument in the first paragraph. There, at one point it argues that [P112] sculptures show relief only insofar as some parts are shaded light and some dark, for [P1121] if we take a statue and darken its light surfaces so as to produce a single uniform shade, it will appear devoid of relief. Panofsky insightfully points out that a similar argument had been advanced by Leonardo da Vinci, but with the important difference that he was appealing to the observation of statues when they happened to be surrounded by a thick fog, creating a diffused light that evens out the differences in shades of light and dark visible in the statue. On the other hand, the letter is envisaging a situation that can be created at will by the investigator.

This difference is enormous. It is perhaps impossible to overestimate the importance in human history of the rise of experimentation as distinct from mere observation in the search for truth and the acquisition of knowledge. And there can be no doubt that Galileo was an indefatigable practitioner of actual experimentation who pioneered the art of experiment in natural science. Working scientists have always intuited this. And *pace* Koyré, eventually historical scholars were able to demonstrate this fact beyond any reasonable doubt, partly with documentary evidence such as that unearthed by Drake, and partly by the technique of re-enacting Galileo's experiments.[27]

This aspect of the Cigoli letter is important. Panofsky is correct to stress this point. However, although this point is correct and important as far as it goes, it does not go very far. For in the overall argumentation in the letter, this single instance of experimental reasoning is only a minute part, so much so that it is easy to miss. Moreover, if one did not already assume that the letter was authored by the father of modern experimental science, one would probably conflate this particular piece of reasoning with Leonardo's, and think that at this point the letter was referring to mere observations, rather than to active experimentation. Thus, the presence of this instance of experimental reasoning is a weak reason for the authenticity of the letter, although it is undeniably a reason.

[27] See Koyré (1939, 1966, 1978), Drake (1978, 1999), Settle 1961, Palmieri (2008, 2009, 2011), Finocchiaro 2010a. For attempts to elaborate and adjudicate some of the nuances and complications of the controversy between the apriorist and empiricist views of Galileo's work, see, for example, Segre (1980, 1991); Finocchiaro 1980, 202–223, 145–166.

10.4 Un-Galilean Aspects of the Letter's Method of Argument

So much for the evidence suggesting the authenticity of the letter. Let us not see whether there is evidence to the contrary.

Lack of Open-mindedness One of the most striking features of the Cigoli letter emerges at the very beginning, in the first two lines of the first paragraph. There the letter mentions the pro-sculpture argument from relief (S1), which it then goes on to criticize. However, the statement of this argument to be criticized is not very well developed, and indeed it is very cryptic, so much so that many readers may not even notice this contrary argument. The same applies to all the other pro-sculpture arguments criticized in the letter: the argument for the aesthetic value of relief (S2), in the middle of the first paragraph, at the beginning of the third criticism; the argument from the imitation of nature (S4), in the first two lines of the third paragraph; and the argument from eternity (S5), in the brief fourth paragraph.[28]

This inattention to contrary arguments is very much unlike Galileo's method of argument. In particular, in his activities pertaining to the Copernican controversy, he was keen on stating and explaining the anti-Copernican arguments very explicitly; indeed he usually formulated them more clearly and incisively than the anti-Copernicans themselves. For example, in the *Dialogue on the Two Chief World Systems*, before going on to criticizing the geostatic objection based on the ship experiment, he gave the following statement of the argument: "because when the ship stands still the rock falls at the foot of the mast, and when the ship is in motion it falls away from the foot, therefore, inverting, from the rock falling at the foot one infers the ship to be standing still, and from its falling away one argues to the ship being in motion; but what happens to the ship must likewise happen to the terrestrial globe; hence, from the rock falling at the foot of the tower, one necessarily infers the immobility of the terrestrial globe" (Galilei 2008, 228; cf. 1890–1909, 7: 169–170). And in the *Letter to the Grand Duchess Christina*, before undertaking a point by point refutation of the biblical argument against the earth's motion, Galileo stated it as follows: "the reason they advance to condemn the opinion of the earth's mobility and sun's stability is this: since in many places in the Holy Scripture one reads that the sun moves and the earth stands still, and since Scripture can never lie or err, it follows as a necessary consequence that the opinion of those who want to assert the sun to be motionless and the earth moving is erroneous and damnable" (Galilei 2008, 115; cf. 1890–1909, 7: 315). And Galileo's superior statement and understanding of contrary arguments is even more spectacular with regard to the anti-Copernican arguments from the extruding power of whirling and from the apparent dimensions and positions of the fixed stars.[29]

Moreover, Galileo did not just practice such a manner of arguing, but he was also reflectively aware of the importance and desirability of doing so. And so on

[28] Cf., respectively, Galilei 1890–1909, 11: 340–342 lines 4–5, 22–24, 58–59, and 80.

[29] See Galilei 1997, 171–212, 247–281; corresponding to Galilei 1890–1909, 7: 214–244, 385–399, 404–416. Cf. Finocchiaro 2010b, pp. xxxvii–xli, 99–102, 132–134.

many occasions he stressed the fact that whereas the Copernicans usually knew the arguments of their opponents, the Ptolemaics usually did not, and further that the Copernicans understood the contrary arguments even better than their opponents. In Galileo's own words: "the followers of the new system produce against themselves observations, experiments, and reasons much stronger than those produced by Aristotle, Ptolemy, and other opponents of the same conclusions" (Galilei 2008, 216–217; cf. 1890–1909, 7: 153–154).

The label "open-mindedness" may be used to refer to this feature of Galileo's *modus operandi*.[30] Open-mindedness is the willingness and ability to know and understand the arguments against one's own view. And to be open-minded is a normative requirement in the search for truth, especially in controversial situations. Of course, when stated generally, few would want to disagree with the principle of open-mindedness. However, such agreement is often mere lip service, and the actual practice of open-mindedness is a different story. In this regard, Galileo's deeds, besides his words, provide a model well worth emulating.

Here, it is useful to mention another characteristic of Galileo's manner of reasoning. It is related to open-mindedness in the sense that it presupposes open-mindedness, but adds a further normative requirement. The term fair-mindedness may be used to refer to this other element of Galileo's logical style.[31] It is the ability and willingness to appreciate the strength of arguments and reasons against one's own view, even when one is attempting to criticize or refute them. In Galileo's own words: "when one presents arguments for the opposite side with the intention of confuting them, they must be explained in the fairest way and not be made out of straw to the disadvantage of the opponent" (Galilei 2008, 283; cf. 1890, 1909, 19: 343).[32]

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[30] See Galilei 1997, 147–149, 154, 339–341, 388; Finocchiaro (1980, 134–135, 177; 2010b, pp. xxxvii–xli, 132–134).

[31] For more details, see, for example, Finocchiaro 2010b, pp. xxxvii–xli, 132–134. It should be noted that here the term fair-mindedness, and still less the term open-mindedness, do not have a moral connotation, but merely epistemological, methodological, and logical import. This is clear from the relevant literature on informal logic and argumentation theory, e.g..: Ennis 1996; Fisher 1991; Govier 1999, 155–180; Johnson 2000, 143–179; Paul 1990, 110, 111, 198; Scriven 1976, 166–167; Woods 1996, 650–662. More generally, discussions of values that are not moral but cognitive can be found in works such as Laudan 1984. It is also important to note that open-mindedness and fair-mindedness are not merely rhetorical principles, presumably or allegedly useful only in persuading opponents, winning arguments, and influencing opinions; they do have such a rhetorical and persuasive purpose, but their primary function is logical, methodological, and epistemological, in the sense that they are useful in the search for truth and the acquisition of knowledge.

[32] The context of this remark is complex, but does not undermine Galileo's commitment to the principle. The context is the Inquisition trial of 1633. In particular, Galileo asserted this sentence in the second deposition, dated April 30. This deposition was a confession of some wrongdoing, which he had denied at the first deposition on April 12. Between these two depositions, there had been some out-of-court plea-bargaining between Galileo and the Inquisition's commissary. Galileo was now formally admitting that the *Dialogue* was a defense of Copernicanism; and since he had never denied that the Church (through Cardinal Bellarmine's warning) had forbidden him to defend it, Galileo was thereby pleading guilty to having violated this warning. However, he was claiming this violation to be unintentional because he had meant to refute Copernicanism, but by following

By contrast, besides implicitly using a manner of arguing that conflicts with open-mindedness, and hence with fair-mindedness, the author of the Cigoli letter also explicitly disregards such Galilean traits in the remarks he makes at the end, in the penultimate (seventh) paragraph (Galilei 1890–1909, 11: 342, lines 92–99). There he tells Cigoli that this is all that came to his mind about these issues, after earlier that same day a certain Andrea conveyed to him Cigoli's request about how to answer the arguments of the pro-sculpture advocates. The author also advises Cigoli not to waste his time discussing this controversy any further with those people, because such discussions are better left to those who practice neither painting nor sculpture, which are both wonderful arts when practiced by people like Cigoli himself and the divine Michelangelo. Such ending may be flattering to Cigoli, but it does not speak well for the author of the letter since he is in effect advising Cigoli to close his mind. That author is also thereby implicitly describing himself as a busybody who has nothing better to do than speculate about such issues. Such an author is unlikely to be Galileo, who in this period was deeply involved in the observation and interpretation of sunspots, and also busy writing the second letter on the topic to Marc Welser and Christoph Scheiner, as well as putting the finishing touches on his tables of the periodic motions of Jupiter's satellites, not to mention that he was frequently in ill health.[33]

Such an ending of the Cigoli letter may also be emblematically contrasted with the more typical ending which we find in Galileo's letter to Father Christoph Grienberger, dated September 1, 1611. In this letter, Galileo is defending his theory of lunar mountains from various objections raised by a Jesuit astronomer in a lecture delivered in Mantua in May 1611. After a point by point refutation of them (amounting to about 25 pages of critical reasoning), in the penultimate paragraph Galileo makes a plea that he be sent the other objections alluded to but not discussed in the lecture:

> I want to end boring Your Reverence, but not without begging you again to intercede with the author of these objections to favor me by letting me see his other arguments against me, which he says are very great in number and weight. This is a favor which I hope to obtain all the more easily, inasmuch as Christian zeal and charity command that sinners be immediately warned, so that if they afterwards disregard the warnings and persevere in their errors, then they can be exposed and branded as criminals. Nor can it be taken lightly the fact that, to have my petition granted, I am spontaneously pleading, and indeed humbly begging, to be benefited with such warnings; if these are denied me, I could wonder whether this Father, by collecting and mentioning them, aimed more at shaming me than at reforming me. For this reason, therefore, and also because the general and proper intention of a true philosopher is to arrive at the understanding of hidden truths, I am confident that this desire of mine will be fulfilled, and I am in a state of eager anticipation. [Galilei 1890–1909, 11: 201–202, lines 824–841].

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the fairness principle, he had ended up giving the wrong impression. Here Galileo was perhaps being untruthful about his intention, but certainly not about the fairness principle. In fact, he had really intended to defend Copernicanism, and being committed to the fairness principle, he carried out his defense by criticizing all anti-Copernican arguments, but only after he had shown that he understood their meaning and appreciated their strength.

[33] Galilei 1890–1909, 5: 141, 11: 335; Favaro 1907, 29–30.

Here Galileo is not only declaring his open-mindedness, but also giving a justification for it. And the justification is two-fold: a longer and more elaborate one focusing on theological or pastoral issues, and a shorter meta-philosophical or methodological one. The contrast with the ending of the Cigoli letter could not be more striking.

It could be objected[34] that Galileo himself does not always exhibit open-mindedness. For example, in his "Letter to Castelli" Galileo elaborates a refutation of the biblical argument against Copernicanism without bothering to even state the argument, and this letter was a careful piece of writing on a very delicate and important topic; thus, the Cigoli letter could be a case like Galileo's "Letter to Castelli." Another alleged lapse against open-mindedness is Galileo's failure in the *Dialogue* to discuss the Tycho Brahe's system of the word, according to which the planets revolve around the sun, but the sun together with the planets revolves both daily and yearly around the motionless earth at the center; and such an anti-Tychonic bias would be fatal since it involves the most important theme (world systems) in Galileo's most important work.

Another objection is that open-mindedness is not an absolute requirement. In some cases, the context is such that to be open-minded is beside the point. For example, if a friend asks you how to refute an argument advanced by some opponent, it would be proper to focus on the refutation, without bothering to articulate the opponent's position. And this is precisely the context of the Cigoli letter, as its concluding remarks (just mentioned) make clear.

However, I do not think these objections undermine my argument. First, open-mindedness (like all similar traits and cognitive virtues) is a disposition defined in terms of probabilities or tendencies. To be open-minded means that one usually (typically, or more often than not) displays knowledge and understanding of the contrary arguments; it does not mean that one always does so, on each and every occasion. Thus, the question whether Galileo's work is characterized by open-mindedness has to be decided on the basis of whether this feature is present in most of his writings, or in his most important writings; and this can be decided affirmatively, as suggested above and as demonstrated elsewhere.[35]

Admittedly, Galileo's "Letter to Castelli" does not display open-mindedness, and so it is a work that deviates from the Galilean norm. However, this norm provides us with a probable reason that a disputed writing (such as the Cigoli letter) violating it should not be attributed to Galileo. As long as we do not think or pretend that this is a conclusive reason, there is no problem. It is merely a reason whose strength has to be weighed together with the other reasons on the same side and vis-à-vis the reasons on the other side.

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[34] This objection, its two examples, and the next objection have been advanced by Alfredo Damanti, in private communication. But the foundation of his remarks can be seen in Damanti 2010, 182–186, where one can find a demonstration of the well known the fact that Galileo's "Letter to Castelli" criticizes the biblical argument against Copernicanism without first elaborating it, and in that sense it contrasts with Galileo's *Letter to the Grand Duchess Christina.*

[35] Again, see, for example, Galilei 1997, 147–149, 154, 339–341, 388. Cf. Finocchiaro 1980, 134–135, 177; 2010b, pp. xxxvii–xli, 132–34.

On the other hand, Galileo's treatment of Tycho is different. It is not really true that Galileo neglects the Tychonic alternative. The only correct point to make that he fails to mention it by name. However, he does consider the relevant content and substance of Tycho's idea. For Galileo's primary interest is to discuss the physical reality of the earth's motion. From that point of view there is no difference between the Ptolemaic and the Tychonic systems: they are both fully geostatic and have only one alternative, the geokinetic one.

Moreover, the argument which Galileo regarded as his second most powerful one was based on the apparent motion of sunspots across the solar disk. This motion is such that they curve and slant upward for half a year and downward the other half, and such that both the curvature and slant are continuously changing so as to show straight paths twice a year when the slant is greatest and no slant twice a year when the curvature is greatest.[36] In arguing that this phenomenon is best explained as resulting from the earth's annual revolution around the sun, together with a monthly rotation by the sun on its axis, Galileo explicitly discusses the geostatic explanation (i.e., the not-so-labeled Tychonic explanation). He admits that one would be possible, if in addition to the diurnal and annual motions and monthly rotation, the sun were given a fourth motion whereby its inclined axis of rotation itself rotates yearly around the axis of the ecliptic. Galileo correctly points out, however, that this explanation would have two disadvantages compared with the geokinetic theory, namely, it would be less simple and more ad hoc.

Finally, several of the arguments which Galileo gives in favor of the earth's rotation apply with equal force against the Ptolemaic as against the Tychonic view. The best of these arguments is based on the law governing periods of revolutions, namely, that whenever a number of bodies revolve around a common center their periods increase with the distance.[37] Galileo was very confident of the truth of this law because he had found it to hold for Jupiter's satellites, besides the previously known case of the planets. However, he argued, in the geostatic system the diurnal motion violates this law since the sphere of the fixed stars or of the *primum mobile* circles the earth the fastest (every twenty-four hours) even though its distance from the center is the greatest. Galileo realized that the force of his argument is merely probable, but it is obvious that it works against the Tychonic as much as it does against the Ptolemaic system.

Thus, Galileo's open-mindedness cannot be impugned by his admitted inattention to contrary arguments in his "Letter to Castelli" or by his treatment of the Tychonic system in the *Dialogue*. Now, regarding the objection involving the context of the Cigoli letter, it is certainly true that open-mindedness is not supreme and that sometimes judiciousness can dictate some deviation from it. And Galileo was very much aware that all particular requirements (such as open-mindedness) are subject to regulation by judgment, which is the ultimate arbiter (cf. Finocchiaro 1980, 145–166). The issue then reduces to the question of exactly what was the context of the Cigoli

[36] Galilei (1890–1909, 7: 372–383; 1967, 345–356); cf. Finocchiaro 1980, 40–41, 129–130.

[37] Galilei (1890–1909, 7: 144–145; 1967, 18–19; 1997, 134–136; 2008, 206–207); cf. Finocchiaro 1980, 35–36, 113–14.

letter, and exactly what the friend was requesting. Here, this objection relies on and attempts to exploit the information given in the penultimate paragraph of the letter. Although its accuracy could be disputed, since the question of accuracy is dependent on the question of authenticity, let us take that information at face value and see what follows.

The relevant sentence states: "For now this is all I can think of for you to reply to the arguments of those advocates of sculpture, which were communicated to me this morning by our Mr. Andrea, as you requested" (Galilei 1890–1909, 11: 342, lines 92–94; cf. Panofsky 1954, 37). Thus, Cigoli is presumably requesting replies to the pro-sculpture arguments. From this point of view, the problem is that the Cigoli letter reads more like a series of pro-painting arguments than a series of replies to various pro-sculpture arguments. To be sure, there are *some* indications that the letter is elaborating refutations of arguments, but the indications are indirect, implicit, and cryptic, so much so that it is not always clear exactly which argument or which part of an argument is being refuted by which considerations. Thus, the letter is not really doing a very good job complying with the friend's request. A better display of open-mindedness would have actually provided clearer guidelines to a painter under attack by pro-sculpture advocates. And this brings us to the issue of clarity of exposition.

Lack of Clarity In fact, lack of open-mindedness is not the only reason for denying the authenticity of the letter. Another piece of evidence is that, from the point of view of clarity, the letter leaves something to be desired and falls short of the Galilean norm. With regard to clarity, there is no need to provide documentation and elaboration of the fact that Galileo's manner of exposition and of reasoning provides a valuable model, since this characteristic is widely recognized. Thus, here we can limit ourselves to analyzing the Cigoli letter from this point of view.

However, it is worth repeating that, like open-mindedness, clarity is a cognitive value, merit, or virtue that needs to be instantiated only most of the time not necessarily all the time, or in the typical and important cases but not in each and every instance. Thus, there are exceptions to the usual Galilean clarity, just as there are exceptions to his usual open-mindedness. For example, Galileo's *Letter to the Grand Duchess Christina* falls short of his usual clarity in many places,[38] although it is a model from the point of view of open-mindedness.

The Cigoli letter falls short of Galilean clarity in several respects. To begin with, the overall structure of the letter is hard to fathom. It is, of course, clear enough that it is arguing against the superiority of sculpture and in favor of the superiority of painting. However, it is not easy to determine how many distinct arguments or reasons the letter is advancing for its favored conclusion, and how many contrary arguments, if any, it is attempting to refute. My reconstruction above provides an answer to this question, by distinguishing five pro-sculpture arguments being criticized, and nine pro-painting arguments being advocated, and by interrelating the arguments both individually within each side and collectively for one side vis-à-vis the other. However, my reconstruction is obviously an interpretation, and although I believe it to be textually accurate, others might find reasons to question it. In any case, my

[38] As one may gather from Damanti 2010, 147–186, and from Finocchiaro 2010b, 79–89, 243–248.

reconstruction was not an easy task, and indeed I found myself struggling with the task more than for the usual Galilean text.

Moreover, within some of the identifiably distinct arguments, the structural interrelationships of the various claims and the chain of the reasoning are often unclear. The worst example in this regard is perhaps the third paragraph: above I reconstructed it as the fourth criticism in the sequence and a counter-argument to the sculpture thesis, and thus in favor of the painting thesis. In this paragraph too many sentences are basically repetitions of one another, and hence puzzle the reader about the purpose of the repetition. Too many illative connectives are used, giving the impression that they have been sprinkled almost at random to impress the inexperienced reader; but the experienced reader is instead likely to get perplexed.

Furthermore, the fifth paragraph remains a puzzle, as I indicated even in the course of the exposition of my reconstruction. Here I can add that two things, and not just one, are unclear. That is, it is not clear how this paragraph relates to the rest of the letter, whether it adds anything not said elsewhere, or whether it is just another instance of repetition. Secondly, it is unclear how the two sentences that make up the paragraph relate to each other; the illative term used to connect them, namely *perocchè* (which means 'because' or 'for') is unhelpful, since such a connection seems to make no sense.

Finally, with regard to clarity, there is a very difficult sentence in the third part of the first paragraph. In the original Italian it reads: "intendendo noi per ingannare l'operar sì che il senso da ingannarsi reputi quella cosa non qual'è, ma quella che imitar si volle" (Galilei 1890–1909, 11: 341, lines 31–32). It is clear that the assertion is a definition of deception, namely a statement of what it means to deceive. It is also clear that this sentence is part of the critical argument being advanced at the end of the first paragraph, and, in fact, my reconstruction above makes it a premise of the third criticism in the sequence. What is not so clear is what the sentence itself means. In my reconstruction, I interpreted it as the claim that [P3b1] to deceive means to operate in such a way that the sense to be deceived regards something not as it really is, but as it is intended by the representation. Panofsky translated it as follows: "provided that we understand by 'to deceive' to operate in such a manner that the sense to be deceived accepts the object not as what it is but as what it is intended to imitate" (Panofsky 1954, 35). The more I think about this sentence, the less sure I am that I understand it.

Illative Terms There is a third aspect of logical or argumentative style which enables us to compare and contrast the manner of reasoning displayed in the Cigoli letter with that of Galileo. It involves the linguistic expression of reasoning by means of illative words and phrases, whose use is indispensable when we are engaged in long and complex arguments of more than just a few sentences. The illatives I am referring to are words like the following: 'therefore', 'so', 'hence', 'thus', 'consequently', 'because', 'since', 'for', and their equivalents in Italian and other languages.[39]

[39] For the role and importance of such terms, see, for example, Angell 1964, 9–15; Eemeren et al. 2007; Finocchiaro 1980, 311–312; Hitchcock (2001, 2009); Scriven 1976, 41. But note that the labels used to refer to such terms vary from one author to another.

I myself would not have even thought of using this criterion to judge the letter's authenticity, were it not that at the very beginning of the letter (second sentence, line 3) I was struck by the illative *imperciocchè*. Its meaning (equivalent to the English 'for' or 'since') was obvious enough, both from the context and from guessing this word to be a variant of *imperocchè*. However, this illative did not seem to me to be one which Galileo would often, if ever, use. I had the same feeling when, at the beginning of the third paragraph (lines 61–62) I read a related illative meaning the same thing, namely *perciocchè*. The third surprise was occasioned by the one and only usage of the word *però* in the letter, near the end, in the penultimate paragraph; there the word is used in the modern Italian meaning of a conjunction equivalent to 'but' (*ma*). This led me to notice something that was *not* in the letter: there was no illative use of this word at all in the letter. This was striking because it so happens that the word *però*, preceded by the conjunction *e* ('and'), that is the phrase *e però*, is extremely frequent in Galileo's argumentative prose. The frequency of the Galilean usage of the illative *e però* is relatively easy to remember because this phrase is hardly ever used as an illative in the present-day Italian language, and so the careful reader of Galileo quickly learns that he is using this phrase with the now-archaic illative meaning of a conclusion indicator. Because of these impressions, I decided to do a systematic computer search of illatives in Galileo's writings. The results are tabulated in the table in the Appendix, which may be summarized and highlighted as follows.

Thirteen different illatives are used in the Cigoli letter with various frequencies, mostly just once, but one (*perchè*) six times. Three crucial patterns emerge. First, about one half (six) of the illatives used in the Cigoli letter are *never* used by Galileo in his major works and letters: *di qui è che*, *donde*, *imperciocchè*, *perciocchè*, *perocchè*, and *per prova di ciò*. Second, conversely, the one illative most frequently used by Galileo, namely *e però*, is *not* used *at all* in the Cigoli letter. Thirdly, of the other seven illatives that occur both in the Cigoli letter and in Galileo's major writings, there are three with a significantly much greater frequency in the former than in the latter, namely: *perchè*, *per questa ragione*, and *poichè*; for example, *poichè* occurs twice in the 1221-word Cigoli letter as well as in the 176,000-word *Dialogue*.

However, other aspects of this tabulation and computation turn out to be statistically insignificant, I would judge. For example, such is the case with regard to the four other illatives common to both this letter and the other works: *adunque*, *atteso che*, *dunque*, and *perciò*. Similarly, many other calculations could be made, but they are not necessary in the present case. For example, one could define the notion of relative frequency as the number of occurrences of a given illative term per page or per thousand words; and one could then say that the relative frequency of *adunque* in the Cigoli letter is about the same as its relative frequency in the *Dialogue* and in the "Reply to Ingoli"; but I would question the significance of this relative frequency of this illative because it probably reflects standard usage in the Italian language. Analogously, it may be noted that in his major writings Galileo does use illatives that are spelling variants of *imperciocchè*, namely *imperocchè* and *imperò che*, and so the possibility arises that the odd orthographical variants used in the Cigoli letter were introduced by the transcriber of the copy used by Favaro, which is not an original

manuscript (as mentioned earlier). This possibility underscores the desirability of further archival searches for the original manuscript, but in the present context I feel justified in assuming that, as a first approximation, each variant should be treated as a different illative; and the fact remains that the Cigoli letter does (but Galileo does not) use *imperciocchè*, and the letter does not (but Galileo does) use *imperocchè* and *imperò che*. Finally, there are many other features revealed by this computation that could give rise to various questions, but whose answers are either obvious, unimportant, or beyond the scope of this essay. For example, the illative *imperocchè* occurs ten times in the *Dialogue*, and not at all in the other works, and one might wonder why.

On the other hand, the three crucial patterns noted earlier represent, I believe, very strong evidence for suspecting the authenticity of the letter, because the differences with respect to them are enormous and striking. Moreover, the pattern of illative usage is relatively constant for any given writer. There are certainly minor variations depending on such things as the literary genre of a particular writing, the topic of discussion, and the stage of the writer's career. But these three crucial patterns would be inexplicable if Galileo were the author of this letter to Cigoli. The only available explanation is the hypothesis that it was not written by Galileo, but by someone else.

10.5 Aspects of the Letter's Substantive Content and Historical Context

For completeness sake, it is worth discussing several other considerations. Although they do not involve matters of argumentative or logical style and methodological procedure, they have been discussed by other scholars and are relevant and important for other reasons. They might be called substantive and historical considerations, to varying degrees.

Sculpture's Inability to Deceive First, there is an element of the letter's substantive content that could be taken to favor the letter's authenticity. Panofsky (1954, 7–8) stressed that one of the letter's arguments is an elaboration of an idea which can be attributed with certainty to Galileo because a formulation of it was written down by him and is found among his autograph fragments of uncertain dates. The sentence reads, "sculpture does not deceive at all, nor does it ever make you believe what later turns out not to be so" (Galilei 1890, 1909, 8: 642). In my reconstruction above, this corresponds to the third criticism in the sequence; there, the Cigoli letter criticizes the pro-sculpture argument from deception by arguing that [P3] it is not true that relief enables statues to deceive us more easily.

I admit that this fact has some probative weight toward claiming the authenticity of the letter, but that weight is not great for the following reason. Without questioning the undeniable authenticity of this fragment about sculpture's inability to deceive, the problem with it is that it is an isolated sentence; hence it could be part of a pro-sculpture or of a pro-painting view, and it is unclear which. It would be part

of a pro-sculpture view if Galileo meant that because sculpture cannot deceive but painting can, it is more truthful than painting, and hence it is superior to painting. It would be part of a pro-painting position if Galileo meant something along the lines of the Cigoli letter, namely that because sculpture cannot deceive but painting can, the relief possessed by statues and lacked by paintings is not an admirable quality, and hence the relief of sculpture does not make it superior to painting. Panofsky's interpretation is assuming that the sentence fragment is part of the latter position, but this is the issue being debated, and so he seems to be begging the question.

Galileo's Father on Vocal vs. Instrumental Music There is a second striking element of the letter's substantive content. As we have seen (in the sixth criticism in my reconstructed sequence), the letter claims that [P61c1] we admire an instrumental musician even more (than a vocal musician) if he achieves the same effect (of making us empathize with, for example, a suffering lover) without singing but by merely playing an instrument. This claim is used as a particular case which, together with other cases, entitles us to arrive at what I called the aesthetic principle of differential representation, namely to generalize that [P61] the more the means of representation are different from the things represented, the more the representation is challenging and potentially wonderful. This is certainly an interesting and nontrivial argument.

However, the fact to note at this point is that such a view contradicts the position of Galileo's own father, Vincenzo Galilei, who famously argued, in his *Dialogue on Ancient and Modern Music* (1581), that modern instrument-centered music is inferior to ancient vocal-centered music.[40] Panofsky mentioned this fact, using the colorful words that the father "must have turned in his grave when his great son anticipated what Jacob Burckhardt was to say 250 years later: 'Music, if we wish to penetrate the essence of its being, must be taken as instrumental music, detached from words and, above all, from dramatic representation'" (Panofsky 1954, 10–11). But Panofsky does not discuss the potential relevance of this fact to the question of the letter's authenticity.

The question here is whether Galileo happened to agree or would have agreed with his own father on this issue, or whether he developed a contrary view anticipating more-modern developments. If the former, then the Cigoli letter is un-Galilean to that extent; if the latter, then it is Galilean to that extent. Of course, it is fairly well-established that Vincenzo Galilei influenced his son with regard to the practice and value of experimentation in the search for truth,[41] be it about the acoustical problem of the harmonies produced by various vibrating strings (Vincenzo's concern), or be it about the mechanical problem of the distances traversed and speeds acquired by falling bodies (Galileo's concern). However, perhaps the paternal influence did not extend to the case of the relative merits of instrumental and vocal music; perhaps in this case filial rebellion prevailed. This question would seem to deserve further study, and at the moment I for one lack the information to resolve it.

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[40] V. Galilei (1581, 79–90, 137–149; 2003, 197–227, 340–372); cf. Palisca 2003, pp. xviii, xxi, liii–lxi, lxiv–lxv.

[41] See Coelho 1992; Drake (1975, 1992); Palisca 1992; Palmieri 2011, chapter 3. Cf. Chapter 9 of this book.

Galileo's Mastery of Aesthetics More generally, the claims and arguments in the Cigoli letter obviously deserve historical contextualization, above and beyond the context of Galileo's own work and of his father's views, and besides philosophical or logical analysis. This historical contextualization was provided in large part by Màrgani. She documented that the controversy over the relative merits of sculpture and painting had been widely discussed in many famous works by illustrious authors such as the following[42]: *The Courtier*, by Baldassare Castiglione (1478–1529); the *Treatise on Jewelry* and *Treatise on Sculpture*, by Benvenuto Cellini (1500–1571); the book on *Drawing*, by Antonio Francesco Doni (1513–1574); the book *On Paintings and on Statues*, by Leon Battista Alberti (1404–1472); the *Treatise on Painting*, by Leonardo da Vinci (1452–1519); and the *Two Lessons on Painting and on Writing*, by Benedetto Varchi (1502–1565). She concluded that "the author of the letter … combines in a graceful and original synthesis the most important things that had been said till then" (Màrgani 1922, 564). This was later echoed by Panofsky (1954, 8–9), who also stressed the important Galilean innovation of appealing to active experimentation, as distinct from passive observation, as we saw above.

As mentioned, such contextualization is interesting and important for its own sake, or to be more precise, it is necessary for historical understanding. However, after these historical connections have been sketched and elaborated, it is quite legitimate, indeed obligatory for the present problem, to try to determine what, if anything, they imply about the letter's authenticity. I believe they suggest a negative answer to this question. For it is simply incredible that Galileo would be so well acquainted with all this history of aesthetics that he would have at his fingertips all these arguments, and would be able to immediately write down such a polished piece of argumentation; here, recall that the penultimate paragraph of the letter states that the author just wrote these arguments down the same day that the intermediary named Andrea conveyed to him Cigoli's request for comment.

Here I am not questioning that Galileo had a certain amount of such historical knowledge of aesthetics. Nor am I questioning his practical acquaintance with the issues, which was keen, deep, broad, and well known and recognized by his contemporaries, especially with regard to perspective, chiaroscuro, vision, sensory observation, etc. (cf. Viviani 1654, 602); in fact, I shall soon exploit this fact. Nor am I denying his artistic background stemming from his brief experience in the 1580's with the Accademia del Disegno in Florence, and with its teachers Ostilio Ricci and Cigoli himself.[43]

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[42] Màrgani 1922, 560–561, 563–563. She gives the original titles as follows: Baldassare Castiglione, *Il cortigiano*; Benvenuto Cellini, *I trattati dell'oreficeria e della scultura*; Antonio Francesco Doni, *Disegno*; Leon Battista Alberti, *Della pittura e della statua*; Leonardo da Vinci, *Trattato della pittura*; and Benedetto Varchi, *Sopra la pittura e la scrittura lezioni due.*

[43] Note the guarded manner in which I am expressing this point, for I do not think it can be asserted with certainty that Galileo was actually a student at the Accademia del Disegno. For example, Heilbron (2010, 5) states merely that "Galileo may have been present at Buontalenti's and possibly attended the Disegno, where Cigoli taught." And I have been unable to confirm a more definite assertion after consulting: Banfi 1962, 17–20; Bredekamp 2001, 160; M. Camerota 2004, 43–55; DiCanzio 1996, 21–27; Drake 1978, 1–17; Fahie 1903, 6–14; Fantoli 2010, 45–52; Favaro 1907,

What I am saying is that none of these considerations show that Galileo had the kind of academic, professional, and technical proficiency with the arguments in the controversy over painting versus sculpture which would have enabled him to instantly write down a polished synthesis on the relative merits of painting and sculpture. Furthermore, I am saying that there seems to be no other evidence at all that he had such proficiency. He did have that kind of proficiency and competence with regard to the astronomical and physical arguments for and against the motion of the earth, although even in that case it may be too much to believe that he could immediately write them within a few hours.

Of course, Andrea's report from Cigoli and the immediate writing of the letter are used by the proponents of the letter's authenticity to explain away the fact that there is no trace of this discussion in the correspondence between Galileo and Cigoli either before or after this date of June 26, 1612. What I am arguing is that these same assertions (in the letter's penultimate paragraph) betray the fact that Galileo was not the author.

"De Visu et Coloribus" Besides Màrgani and Panofsky, at least one other scholar has explicitly treated the Cigoli letter as authentically Galilean, and to some extent argued for its authenticity. In 1959, on the occasion of the quatercentenary of the birth of Cigoli, Anna Matteoli (1959) published an edition of all letters exchanged by the two friends, twenty nine by Cigoli and two by Galileo. They were reprinted from the National Edition and included the disputed letter. Matteoli does acknowledge the issue, and Favaro's reasons for questioning the letter's authenticity. However, the only evidence in favor she mentions is that Galileo had written an essay "On Vision and Colors" ("De visu et coloribus"), never published and now lost. Matteoli (1959, 64) does *not* elaborate or even give a reference.

Let us do that work of elaboration. Let us follow the Galilean model of open-mindedness and fair-mindedness. It is indeed true that Galileo himself states having written such an essay.[44] He makes this statement in his famous letter to the Tuscan secretary of state, dated May 7, 1610, in which he is negotiating for a full-time research position, with the title of Philosopher and Chief Mathematician to the Grand Duke of Tuscany. In that letter, among other things, Galileo is stressing not only the startling astronomical discoveries he has been making in the past several months, and just published in *The Sidereal Messenger*, but also all the works he has written and not yet published, as well as those which he plans to write.

Such an essay would show that Galileo had reflected seriously and theoretically on topics involving vision and colors that are intimately related to the content of the Cigoli letter. Thus he did or could have had the knowledge of the aesthetic controversy over painting vs. sculpture required to write the Cigoli letter. And this would refute various incautious versions of my last counter-argument, which I was

10–11; Galilei 1890–1909, 20: 58, 154; Geymonat 1962, 13–20; Viviani 1654, 602; Wootton 2010, 14–18. In any case, even if Galileo had actually and formally studied at the Accademia del Disegno, that would not have ensured his mastery of the relevant aesthetic arguments, any more than Cigoli's experience there enabled him to master them.

[44] Galileo to Vinta, 7 May 1610, in Galilei 1890–1909, 10: 348–353, on p. 352.

careful to avoid above, when I clarified that I was not denying Galileo's general knowledge or practical understanding of the issues, but rather his academic mastery of the aesthetic controversy.

However, once we focus on his knowledge and understanding of these topics, we are led to a new difficulty for the authenticity thesis. The difficulty is that, insofar as Galileo's theory of vision can be reconstructed from other sources, it seems to point in a direction different from, if not opposite to, the account embodied in the Cigoli letter.

In fact, although Galileo's essay "On Vision and Colors" was lost, he incorporated some of its content and other related topics in numerous discussions on the theory of vision and perception. The most important of these can be found in the following[45]: in *The Sidereal Messenger*, especially those parts where he describes and interprets the telescopic appearance of the moon, and the sequence of appearances and disappearances of Jupiter's satellites; in the long letter to Grienberger dated September 1, 1611 (whose ending was discussed above), in which Galileo defends his claims about lunar mountains from the objections advanced by some Jesuit astronomers; in the *History and Demonstrations concerning Sunspots*, especially the parts where he argues, contra Christoph Scheiner, that the sunspots are phenomena occurring on the surface of the sun and rotate along with it because, as they move across the solar disk, there is a foreshortening of apparent size, shape, and speed; and in *The Assayer,* especially where Galileo discusses the distinction between primary and secondary qualities and sketches an interactionist and anti-Aristotelian theory of perception.

Studying such sources, a number of scholars have reconstructed Galileo's theory of perception in general, and vision in particular.[46] This is not the place to summarize this secondary literature or to undertake a new analysis of these Galilean primary sources. Rather the important point to stress is that some of the substantive content of the Cigoli letter seems to embody a theory of vision that disregards or contradicts Galileo's.[47] The best example of this disparity regards the claims made in the letter's second paragraph, which earlier I reconstructed as the fourth pro-painting argument (P4). It reads as follows:

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[45] Respectively in: Galilei 1890–1909, 3: 62–70; 11: 178–203; 5: 116–126; 6: 347–352.

[46] See, for example, F. Camerota 2004, Piccolino 2007, Piccolino and Wade (2008a, 2008b), Tongiorgi Tomasi 2007, Wade 2007.

[47] To my knowledge, Wade (2007, 301–306) deserves credit for being the only scholar who has noticed this discrepancy. Many others mention this passage in the Cigoli letter as if it corresponded to Galileo's own theory of vision: F. Camerota 2004, 154; Edgerton 1991, 224–225; Piccolino and Wade 2008b, 1330–1331; and Tongiorgi Tomasi 2007, 12–13. However, the context of Wade's investigation is that he takes for granted that the Cigoli letter was authored by Galileo, and so he says that here "Galileo's opinion is paradoxical" (Wade 2007, 303). Nevertheless, the reasons Wade gives to criticize this opinion are the same as mine; indeed I have adapted mine from his: "Galileo's opinion is paradoxical because it placed greater emphasis on the painterly powers of depth deception than on the direct perception of solidity. Moreover, it did not address the principal perceptual difference between viewing scenes and representations of them, namely binocularity. As such, it contrasted starkly with the opposite conclusion reached by Leonardo Da Vinci when he compared a painting of a scene with the direct perception of it" (Wade 2007, 303–304).

> A statue has relief not by being wide, long, and deep, but by being partly bright and partly dark. To prove this, note that of the three dimensions only two are exposed to the eye, namely length and width, which make up the surface (which the Greeks called *epifania*, namely periphery or circumference); for with regard to things that appear and are seen, we see nothing but their surface, and depth cannot be perceived by the eye because our vision does not penetrate inside opaque bodies. The eye thus sees only length and width, but not depth; that is, it never sees thickness. Depth not being exposed to sight, therefore, in a statue we can only perceive length and width; from this it is clear that we see only its surface, which is nothing but width and length without depth. Hence, we know depth not as an object of vision per se and absolutely, but only as a by-product and in relation to the bright and the dark. All this is present in painting no less than in sculpture, that is, the bright and dark, and length and width. But in sculpture the bright and the dark derive from nature, in painting they derive from art; therefore, here we have another reason why an excellent painting is more admirable than an excellent sculpture. [Galilei 1890–1909, 11: 341, my translation; cf. Panofsky 1954, 36].

The difficulty with the position embodied in this passage is that it ignores two aspects of vision which Galileo knew, understood, and exploited very well: binocularity and the dynamics of perspective. Binocularity refers to the fact that humans have two eyes, and thus are able to perceive some depth when they look at three-dimensional objects. By "the dynamics of perspective" I am referring to the fact that normally the visual observation of a three-dimensional object involves motions of the following sorts: looking from many observation points while the object and/or light source are fixed; looking from a fixed point while the object and/or light source are moving; looking as the object moves while observation point and/or light source are fixed; or looking while all three (observation point, object, and light source) are moving. It was Galileo's superior understanding of the dynamics of perspective that enabled him to prove beyond any reasonable doubt (and against his many critics) that there are mountains on the moon, dark spots on the surface of sun's body, and satellites revolving around Jupiter.[48]

And in so doing, Galileo was extending an insight that had been elaborated by Leonardo da Vinci, whose eloquent words contrasting painting and sculpture are worth quoting: "A Painting, though conducted with the greatest Art and finished to the last Perfection, both with regard to its Contours, its Lights, its Shadows and its Colours, can never show a *Relievo* [relief] equal to that of Natural Objects, unless these be view'd at a Distance and with a single Eye."[49] That is, Leonardo is saying that two-dimensional paintings would be comparable to three-dimensional statues only if perception were conceived statically rather than dynamically, and only if we lacked binocularity.[50]

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[48] For this aspect of Galileo's work, see, for example: Biagioli 2006, 142–143, 149–150, 216–217; Booth and van Helden 2001, 196–203; Bredekamp (2001, 170–184; 2007); Cavicchi 1991; Edgerton 1991, 223–253; Galilei and Scheiner 2010, especially pp. 107–168; Reeves 1997, 3–22, 138–183; Renn 2001; Winkler and van Helden 1992, 195–217. In particular, Biagioli (2006, 143) has used the eloquent and suggestive word "cinematic" to describe Galileo's evidence.

[49] Quoted in Wade 2007, 305, from Leonardo da Vinci 1721, 178.

[50] Similarly, Benvenuto Cellini had stressed the fact that a painting presupposes a single point of view of the observer, whereas a statue can be appreciated from several points of view, regarding which he distinguished eight important ones; I owe this information to Angelini 2010, 2–3.

By contrast, the Cigoli letter speaks as if the chiaroscuro were the only relevant factor—as if we were looking at paintings and statues with only one eye and in a static situation in which neither the observer, nor the object, not the light source moves. This is not what Galileo would have thought or said about this issue.

10.6 Conclusions and Conjectures

I began by calling attention to the letter to Cigoli dated June 26, 1612, published in the National Edition of Galileo's collected works. The letter is noteworthy for the aesthetic content of its discussion about the relative merits of sculpture and painting, and for the intensity and brilliance of its argumentation and reasoning. Favaro doubted its authenticity in part because of its un-Galilean style, but Màrgani, Panofsky, and Matteoli have defended its authenticity in part because of its allegedly Galilean logical or argumentative style (but also because of the substantive content of the views expressed). These facts provide the motivation for an analysis of the letter's argumentation that is deeper and more systematic than that provided by Màrgani or Panofsky, and that compares and contrasts its manner of reasoning with Galileo's. Moreover, my analysis is a case study of how informal logic or argument analysis can help decide historical issues, as well as a test case of the thesis on the Galilean correspondence of aesthetic attitude and scientific thinking, advanced by such scholars as Panofsky, Koyré, and Heilbron.

My analysis reconstructs the letter as a critical examination of five arguments in favor of the superiority of sculpture over painting, and an elaboration of a series of nine critical arguments designed to defend the reverse thesis, namely the superiority of painting over sculpture. The pro-sculpture arguments subject to criticism are the argument from the relief possessed by sculptures, the argument from sculpture's ability to deceive, a combination of the two arguments just mentioned, the argument from the imitation of nature, and the argument from durability. The nine critical arguments attempt partly to refute the truth of the premises of these arguments; partly to show that their conclusions do not follow from their premises, even if these premises were true; and partly to establish that the reverse of their conclusions are true.

The comparison and contrast with Galileo's manner of reasoning or method of argument yield the following results. The letter does have some Galilean characteristics: the letter as a whole is an exercise in critical reasoning; it contains two instances of *ad hominem* argumentation, in the seventeenth-century sense of internal criticism; and it contains one instance of appeal to experimentation, as distinct from mere observation. However, the letter falls seriously short of the open-mindedness and fair-mindedness typically practiced and advocated by Galileo; it also falls somewhat short of the typical Galilean clarity of exposition; and it uses several illative terms which Galileo never uses, and does not use the one which he uses more than any other. The latter features are, I believe, deeply un-Galilean, and they seem to me to considerably outweigh the former Galilean ones.

It should be noted that my comparison and contrast of the method of argument in the Cigoli letter with that of Galileo in general has included discussions of not only other private unpublished correspondence (such as the letters to Castelli and to Grienberger), but also semi-public or potentially published correspondence (such as the letter to Christina and the "Reply to Ingoli"), and published works (such as the *Dialogue*). I am assuming that the Cigoli letter can and should be legitimately compared to writings other than just the class of private unpublished letters, and one could object that this assumption is questionable. However, I think this assumption is correct. For, when we study the private unpublished letters in relation to other kinds of writings, I do not deny that there are *minor* differences in method of *argument*, or *major* differences in *literary* style, but I do deny that there are major differences in method of argument or manner of reasoning; and I have tried to characterize Galileo's method of argument at a level of generality as to be in principle relevant and applicable to all his writings. Moreover, if one wanted to focus on differences no matter how minor, the class of private unpublished letters should be subdivided into smaller groups depending on such factors as: whether the recipient is or is not a close acquaintance; whether the date of writing falls into a particular stage of Galileo's career; whether the topic is mathematics, astronomy, physics, etc.; whether the language used in Latin or Italian. And such further subdivisions start a slippery slope in which it becomes arbitrary or question-begging to ever stop.

I end with four substantive and historical considerations. The first is that the letter's criticism of the pro-sculpture argument from its ability to deceive is an elaboration of a Galilean fragment consisting of a sentence claiming that sculpture cannot deceive. Panofsky argued this to be very strong evidence for the letter's authenticity, but I object that his argument begs the question because the sentence fragment could be part of both a pro-sculpture and of a pro-painting position. Second, a consideration against authenticity might be that one of the letter's critical arguments is based on the premise that instrumental music is superior to vocal music, which contradicts a thesis famously held by Galileo's father Vincenzo. Although Vincenzo's influence on Galileo is unquestionable with regard to the practice and value of experimentation, nevertheless, whether that influence extended to the topic of the relative merits of instrumental and vocal music is unknown and requires further investigation. Thirdly, Màrgani and Panofsky provided an historical contextualization for the Cigoli letter, documenting the fact that the controversy over sculpture vs. painting had been widely discussed by such authors as Castiglione, Cellini, Alberti, and Leonardo; in this regard, the letter is interpreted primarily as an incisive synthesis of such background discussions. I argue that if this is true, what follows is the unlikelihood of Galileo having written the letter, since there is no biographical evidence that he had such an academic mastery of these discussions as to be able to write this letter without preparation and in a few hours. Finally, I report Matteoli's suggestion that the letter is authentic because Galileo had written an essay on vision and colors, and so was sufficiently acquainted with the topics of discussion. I argue that such a reason backfires because Galileo held a theory of vision that understood the role of the dynamics of perspective and the role of binocularity, which contrasts with the theory assumed and elaborated in the Cigoli letter.

Thus, my historical-erudite conclusion is that, in all likelihood, Galileo was not the author of the letter to Cigoli dated June 26, 1612. That is, Favaro seems to have

had the right intuition in judging the Cigoli letter to be apocryphal; his intuition can be justified anew by means of a systematic analysis of the letter's method of argument and a critical comparison with Galileo's logical and methodological style.

Above and beyond this matter of historical erudition, the analysis reinforces my novel version of the connection between history and philosophy. That is, the branch of logic variously called informal logic, applied logic, argument analysis, etc. can be instrumental in the understanding and evaluation of historical material and the solution of historical problems.

One logical idea I have exploited is the method of analyzing argumentation or reasoning into its constituent elements, in such a way that these elements (the individual propositions or claims) are identified as regards their content, but also interrelated as regards their inferential connections. And recall that this method involves not only the simpler and traditional distinctions between conclusions and premises, between major and minor premises, and between implicit and explicit premises, but also the subtler distinctions (widely elaborated and applied in this branch of logic) between intermediate and final premises, between linked and independent premises, and between pro and con reasons.

Another logical idea I have utilized is the notion of a conductive argument. Recall that this term is technical jargon for what is ordinarily called a pro-and-con argument or a balance-of-considerations argument. Now the definition given in the introduction above can be further clarified by pointing out that it conceives a conductive argument in terms of four conditions. (1) A conductive argument is one that attempts to justify a claim *non-conclusively*; that is, the conclusion is provided with some (more or less strong) support, but there is no pretense that it is established apodictically or beyond any reasonable doubt. (2) A conductive argument advances two or more *pro* reasons that are *mutually independent but cumulative*; that is, multiple reasons are given to support the conclusion, in such a way that each reason is separately favorable to the conclusion, and it adds some support above and beyond that provided by the other reasons. (3) A conductive argument acknowledges at least one *con* reason; that is, the argument explicitly includes a discussion of counter-evidence, counter-considerations, or objections that might undermine the truth or weaken the credibility of the conclusion. (4) Implicitly or explicitly, the argument assumes, states, or tries to show that the pro reasons *outweigh* the con reasons.

This essay has provided two significant examples of conductive arguments: the argument in the Cigoli letter trying to show that painting is superior to sculpture as an art form; and my own argument in this essay attempting to show that the Cigoli letter was not authored by Galileo. In light of the clarifications and elaborations in the previous paragraph, and of the exposition throughout the essay, I take it as relatively obvious that these two arguments are conductive, although the detailed and exhaustive proof that they satisfy the four conditions just stated would be an long exercise in informal logic, or argument analysis, too tedious to be carried out in this context. In any case, terminology and pedantry aside, the similarities between these two arguments are, I believe, striking and palpable, that is, the similarities from the point of view of logical structure, or method of argument; and this is the crucial point.

Thus, besides my earlier historical-erudite thesis, we may also conclude with a philosophical-methodological thesis: that the analysis provided in this essay enhances the explanatory power, the empirical-historical adequacy, and the practical utility of the method of argument analysis and the concept of conductive argument elaborated in that branch of logic variously called informal logic, applied logic, argumentation theory, etc. However, this conclusion should not be regarded as a claim of one-sided advantage which historical inquiry can enjoy by utilizing some such logical notions, for the advantage is mutual and the benefit also goes the other way; that is, this type of logical theorizing is thereby encouraged to study historical material like that examined here.

Finally, let us reflect on the implications of my analysis for the thesis of a correspondence between aesthetic attitude and scientific thought, mentioned earlier in the introduction and advanced by such authors as Panofsky, Koyré, and Heilbron. Clearly, if and to the extent that my analysis is correct, the aesthetics-science correspondence thesis cannot be grounded on the evidence of the Cigoli letter, by alleging that in it Galileo displays an aesthetic attitude that is essentially identical to the attitude he displayed in his scientific work; for in all likelihood, it was not Galileo who wrote the letter, and so we cannot attribute its aesthetic attitude to him. Equally clearly, this cannot invalidate these scholars' efforts to ground the general correspondence thesis on other evidence; for example, they support the correspondence thesis in part by finding similarities between Galileo's scientific work and the literary criticism he advanced in his critical comparison of Ludovico Ariosto's *Orlando Furioso* and Torquato Tasso's *Gerusalemme Liberata*.[51] It is also clear that my analysis may well support other theses of general cultural or historical significance regarding the relationship between science and other things. Let us engage in some speculations or conjectures along these lines.

Let us recall that in the course of arguing for the inauthenticity of the Cigoli letter, my analysis attributes to Galileo a number of logical or augmentative attitudes and practices which I have labeled: critical reasoning, *ad hominem* argumentation (in the sense of internal criticism), appeal to experimentation (above and beyond mere observation), open-mindedness, fair-mindedness, clear-mindedness, and a keen self-reflective sensitivity to argumentation (displayed by the careful use of illatives). I have claimed that such traits characterize Galileo's scientific thought in general. Now, if and to the extent that these attributions are correct, two questions immediately come to mind. First there is the question of their historical origin: how and why did Galileo acquire these traits, and what is their connection to the historical context and background? The other is the question of what such historical connections show about the relationship between Galilean or modern science and other cultural developments or activities. In fact, there already is a considerable body of relevant scholarship on these topics.

For example, following some of Wallace's (1992a; 1992b) work, I believe one could plausibly argue that this Galilean method of argument is not surprising in view of Galileo's early logical studies recorded in his notebook now known as manuscript

[51] Panofsky 1954, 13, 16–20; Heilbron 2010, 11–23.

MS27.[52] Moreover, Wallace also uses such evidence to argue for a version of the continuity thesis that attributes an Aristotelian origin to Galilean science; and whether or not one accepts this argument, it is certainly important and needs to be taken seriously.

Wallace argues that Galileo's manuscript MS27 is a set of notes based largely on the lectures notes for various logic courses taught at the Jesuit university in Rome (the Collegio Romano), and the indebtedness is so great that it is best to speak of a reworking of such lecture notes written probably for the purpose of preparing himself to teach logic. Now, these Jesuit sources subscribed to logical doctrines which were essentially Aristotelian but included some progressive modifications; thus, for example, logic was taken in a broad sense which included the theory of science; scientific knowledge was taken to possess the traits of certainty and demonstrativeness; such scientific certainty was deemed to be achievable on the basis of a type of argument called the "demonstrative regress"; mathematics was not excluded from natural science (as it was in some other versions of Aristotelianism), but the necessity and importance of mathematical analysis were admitted in what were called "mixed sciences." These historical points provide the basis for Wallace's continuity thesis; he rejects the view according to which modern science involved a radical break with the past, and argues that Galileo was logically (methodologically, epistemologically) in the Aristotelian tradition of mixed sciences, even as he rejected the substantive theories of Aristotle's cosmology, astronomy, and physics.

Other scholars have drawn other conclusions on the basis of Galileo's argumentation. For example, Moss has elaborated a rhetorical interpretation. Working within the Aristotelian framework that distinguishes demonstration, dialectical (probable) reasoning, and rhetorical persuasion, she plays down the demonstrative and dialectical aspects of Galileo's argumentation, and stresses the rhetorical dimension, so much so that she ends up viewing Galilean science primarily as a "rhetorical revolution" (Moss 1993, 328). Now, she conceives rhetoric partly as the realm of persuasion by means of appeals to the emotions of the audience and to the character of the speaker, and partly as the domain of verbal communication and the principles and practices for the organization of information, presentation of ideas, composition of speech and writing, and stylistic expression (Moss 1993, 9–19). However, rather than elaborating the Aristotelian roots of modern science, she is led to connect it with the tradition of scholastic and Renaissance humanism (Moss 1993, 329; Moss and Wallace 2003, 7–13).

The connection between science and humanism has been elaborated more explicitly by other authors. However, in this regard it is useful to keep in mind Mann's (1996, 2) definition of this word:

> Humanism is that concern with the legacy of antiquity—and in particular, but not exclusively, with its literary legacy—which characterizes the work of scholars from at least the ninth century onwards. It involves above all the rediscovery and study of ancient Greek and Roman

[52] To get a flavor of the late medieval, Renaissance, and early modern logic with which Galileo may have been acquainted, see also Ashworth 1988; L. Jardine 1988; Mack 1996; Marenbon 1987, 35–50. See also chapter 21 of this book.

> texts, the restoration and interpretation of them and the assimilation of the ideas and values that they contain. It ranges from an archaeological interest in the remains of the past to a highly focused philological attention to the details of all manner of written records—from inscriptions to epic poems—but comes to pervade, as we shall see, almost all areas of post medieval culture, including theology, philosophy, political thought, jurisprudence, medicine, mathematics and the creative arts. Grounded in what we would now think of as learned research, it rapidly found expression in teaching. And in this way it was to become the embodiment of, and vehicle for, that very classical tradition that is the most fundamental aspect of the continuity of European cultural and intellectual history.

When humanism is so defined, it would be difficult to deny its link with science, although one could question the specificity and informativeness of such a link.

However, such greater specificity and informativeness could be obtained by focusing on Renaissance humanistic logic, that is the theories of argumentation of such authors as Lorenzo Valla, Rudolph Agricola, and Petrus Ramus. The potential fruitfulness of such a focus is evident in light of Lisa Jardine's (1988, 175) conclusion that "humanistic treatments of logic … have a good deal in common with the interests of some recent, modern logicians, who have chosen to give a good deal of attention to non-deductive inference, and to 'good' arguments (arguments which can be counted on to win in debate), and the problematic nature of their validity." In fact, among such recent modern logicians, she mentions Hamblin (1970). Now Hamblin is indeed a leading and important figure in the branch of logical theory which in this essay I have labeled applied logic, informal logic, practical logic, argument analysis, etc. Next, it should be recalled that in this essay I have taken the point of view of this branch of logic to compare and contrast the argumentation in the Cigoli letter and in Galileo's work in general. The uncanny consequence from these considerations is that by and large Galileo's argumentation displays features that correspond to humanistic logic and may have originated from it.

Thus, Galileo's method of argument or manner of reasoning can also provide some evidence for linking Galilean and modern science with such traditions as Aristotelianism, rhetoric, and humanism. I have not stressed this aspect of the present investigation, as compared with using Galilean argumentation as evidence for the inauthenticity of the Cigoli letter and as an illustration of applied logic's relevance to historical inquiry. The reasons for the relative emphases of the present investigation stem partly from psychological, biographical, and incidental circumstances and preferences, but also from what might be called the logic of the situation. That is, on the one hand I feel that my argument for the inauthenticity of the Cigoli letter is strong (although not apodictic or conclusive), and that my illustration of the potential connection between applied logic and historical inquiry is significant (however unusual). On the other hand, the suggested connection between Galilean science and such cultural traditions as Aristotelianism, rhetoric, and humanism is relatively conjectural, and it is beyond the scope of this investigation to strengthen these conjectures and turn them into well supported theses. In this regard, my aim here has been merely to show that when the question of the authenticity of the Cigoli letter is approached by comparing and contrasting its argumentation with that characteristic of Galileo, the topic is of more than merely antiquarian interest, but has general cultural relevance.[53]

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[53] A shorter version of this paper was presented at the symposium on "Music and the Arts in the Thought of Galileo Galilei," at the Egida Sartori and Laura Alvini Seminar on Ancient Music, Fondazione Giorgio Cini, Venice, 8 May 2010. For the opportunity and the inspiration I thank the

Appendix: Table of Galilean Illatives

Italian illative	English	Cigoli Letter	*Dialogue*	*Two New Sciences*	*Assayer*	Reply Ingoli	Major Letters
total wordcount		1,221	176, 000	78,000	82,000	23,000	75,000
Adunque	therefore	1	129	88	41	22	23
atteso che	given that	1	12	10	12	5	4
di qui è che	from this it follows that	1	0	0	0	0	0
donde (illative)	hence	1	0	0	0	0	0
[*donde* (place)]	from where	0	9	0	1	0	2
Dunque	therefore	6	208	167	56	18	77
[*imperocchè*]	for / since	0	10	0	0	0	0
[*imperò che*]	for / since	0	3	54	10	11	8
imperciocchè	for / since	1	0	0	0	0	0
per prova di ciò	as a proof of this	1	0	0	0	0	0
per questa ragione	for this reason	3	1	1	0	0	0
Perchè	because	6	30	0	0	67	4
Perciò	therefore	1	19	42	20	1	0
Perciocchè	for / since	1	0	0	0	0	0
però/e però (illative)	therefore	0	351-α	120-β	24-γ	24-δ	122-ε
però (= ma)	but	1	α	β	γ	δ	ε
Perocchè	for / since	1	0	0	0	0	0
Poichè	because / since	2	2	0	0	1	0

This table records, for the major Galilean writings, the number of occurrences of all the illatives occurring in the Cigoli letter. The first column includes all illative terms or phrases that occur in the Cigoli letter, except for *dimostra* (line 11) and *questo argomento* (lines 24–25), which would not have yielded a meaningful comparison with their occurrence in other works. The first column also includes, inside square brackets, illatives that do not occur in the Cigoli letter, but are related to those that do

organizer Pedro Memelsdorff, and for their comments and suggestions I thank the other participants and attendees, especially Alfredo Damanti, Marco Piccolino, and Alessandro Angelini. In particular, Damanti also sent me written comments and bibliographical references, and many of his observations and objections have been incorporated into the present version. I also thank Albert DiCanzio, Mariapiera Marenzana, and Michael Segre for their feedback.

in various ways deserving note. Thus, I have listed the word *donde* twice, because its single occurrence in the Cigoli letter has the meaning of an illative, but it normally is an adverb of place. I have also listed two spelling variants of *imperciocchè*, namely *imperocchè* and *imperò che*, in order to stress that the letter does not use these variants, whereas in other cases it does use variants, for example *adunque* as well as *dunque*, *perciocchè* as well as *perocchè*, and *perchè* as well as *poichè*. And I have listed the word *però* twice, because its single occurrence in the Cigoli letter has the modern Italian meaning synonymous with 'but' (*ma*), whereas in Galileo's time, and certainly in his works, the word (especially when preceded by the conjunction *e*) usually has the meaning of the illative 'therefore'. For the word *però*, my computer counted the total number of occurrences without distinguishing the two possible meanings; although I estimate that almost all these occurrences have the illative meaning, as I believe to be the case in Galileo's writings, I am designating with Greek letters (α-ε) the vanishingly small number of occurrences of the non-illative *però*. To convey a sense of the relative frequency of occurrence, as distinct from the total number of occurrences, the second row gives the length of the various works in number of words, rounded to the nearest thousand, except for the case of the Cigoli letter; thus, one can easily see that, approximately speaking, the *Dialogue* is about 150 times longer than the Cigoli letter, and the "Reply to Ingoli" about 20 times longer.

The last column labeled *Major Letters* refers to most of Galileo's longest letters, spanning the years 1610–1640, including the letters on sunspots of 1612 and those on Copernicanism and Scripture of 1613–1615; they are available in a single volume in an electronic edition found at the website: http://www.liberliber.it/biblioteca/g/galilei/index.htm, consulted on April, 5 and September 28, 2010. The volume also includes the Inquisition's trial sentence and Galileo's abjuration, which however I have discounted both for the total wordcount and for occurrence of illatives. Specifically, the collection includes the following 27 letters, identified by addressee and date: Belisario Vinta, 7-May-1610; Matteo Carosi, 24-May-1610; Giuliano de' Medici, 13-November-1610; Benedetto Castelli, 30-December-1610; Christoph Clavius, 30-December- 1610; Giuliano de' Medici, 1-January-1611; Paolo Sarpi, 12-February-1611; Marc Welser, on sunspots, 4-May-1612; Marc Welser, on sunspots, 14-August-1612; Marc Welser, on sunspots, 1-December-1612; Maffeo Barberini, 2-June-1612; Paolo Gualdo, 16-June-1612; Benedetto Castelli, 21-December-1613; Piero Dini, 16-February-1615; Piero Dini, 23-March-1615; Grand Duchess Christina, 1615; Elia Diodati, 16-August-1631; Andrea Cioli, 6-October-1632; Francesco Barberini, 13-October-1632; Cesare Marsili, 16-October-1632; Andrea Cioli, 19-February-1633; Geri Bocchineri, 23-April-1633; Andrea Cioli, 23-July-1633; Elia Diodati, 7-March-1634; Elia Diodati, 25-July-1634; Fortunio Liceti, 15-September-1640; Prince Leopold de' Medici, on the moon's secondary light, 1640.

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Part III
For or Against Galileo or the Church

Chapter 11
The Berkeley Para-Clerical Approach

Abstract Recently, the Galileo affair has been studied by several scholars whom I label "Berkeley para-clericals," chiefly philosopher Paul Feyerabend and historian John Heilbron. Their approach is distinctive: it views controversial topics involving the relationship between science and religion from a perspective that is secular-minded, but appreciative of religion, and yet conducted in the belief that such topics are too important to leave to religious believers. This approach also characterizes the work of other Berkeley para-clericals, such as Ronald Numbers on the controversy over creationism and evolutionism; they stress such attitudes as impartiality, judiciousness, and even-handedness. This approach is also followed by the present author.

11.1 Introduction

The main aim of this essay is to call attention to the existence of a distinctive approach to the study of the Galileo affair, and more generally to questions of science vs. religion. Such an approach can be detected in the works of philosopher Paul Feyerabend and historian John Heilbron. I shall argue that, despite the many other differences between Feyerabend and Heilbron, they both approach the affair with a perspective which is secular-minded, but appreciative of religion, and yet conducted in the belief that the affair is one of those topics that are too important to leave to religious believers. I label such an approach the "Berkeley para-clerical" approach because it is practiced not only by these two luminaries of the University of California, Berkeley, but also by other scholars who are graduates of the same institution. One of these is the distinguished historian Ronald Numbers, who has studied the controversy over evolution and creationism. The analysis of the work of these other scholars leads us to a deeper understanding of this para-clerical approach in terms of a family of notions such as impartiality, judiciousness, objectivity, non-partisanship, even-handedness, and balanced judgment.

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11.2 A Summary of the Galileo Affair

It will be useful to begin with a succinct summary of the Galileo affair.[1] In 1543, Copernicus published an epoch-making book, *On the Revolutions of the Heavenly Spheres*. In it, he advanced an argument in favor of the idea that the earth rotates daily on its axis and revolves yearly around the sun. The argument amounted to showing that the known facts about the motion of heavenly bodies could be explained better on the basis of the heliocentric geokinetic hypothesis than on the basis of Ptolemy's geocentric geostatic theory.

Although novel and significant, Copernicus's argument was hypothetical and inconclusive. Moreover, there were many arguments against the earth's motion, stemming from astronomical observation, Aristotelian physics, traditional epistemology, and scriptural interpretation. These objections were advanced by astronomers, mathematicians, and natural philosophers, as well as theologians and churchmen, and by Protestants as well as Catholics. They can be summarized as follows.

The earth's motion seemed philosophically and epistemologically absurd because it contradicted direct sense experience; in fact, neither Copernicus nor anyone else could see, feel, or otherwise perceive the earth's motion. From the perspective of the science of motion, the motion of the earth seems physically impossible because the available laws of motion (stemming from Aristotle) implied that bodies on a rotating earth would, for example, follow a slanted rather than vertical path in free fall, and would be thrown off by centrifugal force. From the point of view of astronomy, the earth's motion seemed to be empirically false because it had consequences that could not be observed; for example, terrestrial and heavenly bodies would have to have similar physical properties; the planet Venus would have to exhibit phases similar to those of the moon; and the fixed stars would have to undergo a yearly shift in their apparent position, called annual stellar parallax. Finally, the earth's motion seemed theologically heretical because it contradicted the words and the traditional interpretations of Scripture, such as the passage in Joshua 10: 12–13.

Thus, Copernicanism attracted few followers. Galileo himself, in the first twenty years of his career (1589–1609), was *not* one of them. His stance toward Copernicanism then was one of *indirect pursuit*, an attitude that is not only weaker than *acceptance*, but also weaker than *direct pursuit*: his research focused on physics rather than astronomy; he was critical of Aristotelian physics and favorably inclined toward an Archimedean approach; he had intuited that the Copernican hypothesis of the earth's motion was more consistent with the new science of motion he was developing than was the geostatic theory; but at that time he felt that, overall, the arguments against Copernicanism were stronger than those in favor of it.

However, in 1609–1610, by means of the newly invented telescope, Galileo made several startling discoveries, which he immediately published in a book entitled *The Sidereal Messenger*: that the moon's surface is rough, full of mountains and valleys;

[1] For more details, see, e.g., Beltrán Marí 2006, Blackwell (1991, 1998a, 1998b, 2006), Bucciantini (1995, 2003), Camerota 2004, Fantoli (2003, 2012), Finocchiaro (1980, 1989, 2005, 2010, 2014a, 2014b, 2015), Heilbron (1999, 2010), Mayer 2015, and Speller 2008.

that innumerable other stars exist besides those visible with the naked eye; that the Milky Way and what had been traditionally called nebulas are dense collections of large numbers of individual stars; and that the planet Jupiter has four moons revolving around it at different distances and with different periods. Soon thereafter, he also discovered the phases of Venus and sunspots; and in 1613, he published a book *On Sunspots*.

The new telescopic evidence removed most of the observational-astronomical objections against the earth's motion and added new evidence in its favor. Galileo now believed not only that the geokinetic theory had greater explanatory coherence than the geostatic theory (as Copernicus had shown); not only that it was physically and mechanically more adequate (as Galileo's new physics suggested); but also that it was empirically and observationally more accurate in astronomy (as the telescope now revealed). His assessment was now that the arguments and evidence for the earth's motion were collectively stronger than those for the earth being at rest; in other words, that Copernicanism was more likely to be true than the geostatic world view. However, he realized that this strengthening of Copernicanism was not equivalent to settling the issue, because there was still some astronomical counter-evidence (mainly, the lack of annual stellar parallax); because the physical objections had not yet been explicitly refuted and the physics of a moving earth had not yet been published; and because the scriptural objection had not yet been answered.

Besides realizing that the pro-Copernican arguments were still not absolutely conclusive, Galileo must have also perceived the potentially explosive character of the scriptural objection. In fact, for a number of years he did not get involved despite the fact that his *Sidereal Messenger* had been attacked by several authors on biblical grounds, among others. Eventually, however, he was dragged into the theological discussion. He was careful enough not to publish his criticism of the scriptural objection, but to circulate it privately, in the form of letters. The first one, in 1613, was addressed to his former student Benedetto Castelli, professor of mathematics at the University of Pisa, while a more elaborate version, in 1615, was addressed to the Grand Duchess Christina, mother of Cosimo II de' Medici, grand duke of Tuscany.

Galileo's criticism, although complex and liable to misunderstanding, was logically compelling, rhetorically persuasive, and theologically sophisticated. And in this context, it should be stressed that his efforts were parallel and complementary with those of other progressive Catholic theologians and philosophers, such as Paolo Antonio Foscarini and Tommaso Campanella.

However, despite winning this intellectual argument, Galileo lost the practical struggle. In 1615, after some formal complaints were filed against him, the Inquisition launched an investigation. The proceedings lasted about a year, and the results were the following. In 1616, the Congregation of the Index issued a decree declaring that the doctrine of the earth's motion was physically false and contrary to Scripture; it also condemned and permanently banned Foscarini's book entitled *Letter on the Pythagorean Opinion* (1615), which had argued that the earth's motion was probable and not contrary to Scripture; and it temporarily prohibited Copernicus's book *On*

the Revolutions, until and unless it was revised. Although Galileo was not mentioned at all in the decree, in private he was given a warning.

This warning exists in two versions. One is written on a certificate given to Galileo and signed by Cardinal Robert Bellarmine, who was an authoritative member of both the Congregations of the Index and of the Inquisition; it states that Bellarmine informed Galileo that, in light of the Index's decree, the earth's motion could not be held or defended. The second version is in an unsigned note written by a notary and found in the file of Inquisition trial proceedings; it states that the commissary general of the Inquisition gave Galileo the special injunction not to hold, defend, or discuss in any way the earth's motion. The difference between Bellarmine's friendly warning and the commissary's special injunction is that the latter adds a more stringent prohibition to the one mentioned in the former: besides being prohibited, like other Catholics, to hold and defend the Copernican opinion, Galileo in addition was specially forbidden to discuss it in any way whatever.

For the next several years, Galileo behaved as if he was bound by Bellarmine's warning, but as if he had no knowledge of the special injunction. That is, he refrained from supporting or defending the earth's motion, although he discussed it incidentally in the context of a controversy over comets. The situation changed in 1623, when an admirer of Galileo, Cardinal Maffeo Barberini, became Pope Urban VIII. From several indications, Galileo came to the conclusion that if he exercised the proper care, he could publish a book on the dangerous topic.

Thus, in 1632, Galileo published a work entitled *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican.* The book was obviously a discussion of the earth's motion, but the discussion took the form of a critical examination of all the arguments for and against the idea; the arguments on both sides were presented, analyzed, and evaluated. He tried his best to carry out his evaluation fairly and validly. The arguments for the earth's motion turned out to be much better than those against it. This was at worst an *implicit* defense of Copernicanism.

Galileo's hope and gamble was that friendly Church officials would not blame him for this; that they would recognize that the defense was not explicit; and that therefore they would judge that he had acted within the spirit of Bellarmine's warning. Galileo's attempt misfired not because it was foolhardy or unreasonable, but because in 1632 the special injunction came to the surface, and from its point of view any discussion of the earth's motion by Galileo was prohibited, whether or not it amounted to a defense. Thus, in the fall of that year he was summoned to Rome to face the Inquisition.

The proceedings did not begin until April. At the first hearing, Galileo was asked about the events of 1616 and the *Dialogue* of 1632. He admitted receiving from Bellarmine the warning that the earth's motion could not be held or defended, but only discussed hypothetically. He denied receiving a special injunction not to discuss the topic in any way whatever, and in his defense he introduced the certificate he had obtained from Bellarmine in 1616 which only mentioned the prohibition to hold or defend. Galileo also claimed that the book did not really defend the earth's motion, but rather suggested that the favorable arguments were inconclusive, and so did not violate Bellarmine's warning.

The special injunction must have surprised Galileo as much as Bellarmine's certificate surprised the inquisitors. In fact, it took three weeks before they decided on the next step. The inquisitors opted for what might be called out-of-court plea-bargaining: they would not press the most serious but most questionable charge (namely, violation of the special injunction), but Galileo would have to plead guilty to a lesser and more provable charge (namely, transgression of the warning not to defend Copernicanism). He decided to cooperate, but requested a few days to devise a dignified way of pleading guilty to the lesser charge.

Thus, at later hearings, he stated that the first deposition had prompted him to re-read his book. He was surprised to find that it gave readers the impression that the author was defending the earth's motion, even though this had not been his intention. He attributed his error to wanting to appear clever by making the weaker side look stronger. He was sorry and ready to make amends.

The trial ended on June 22, 1633 with a harsher sentence than Galileo had been led to believe. The verdict found him guilty of a category of religious crime intermediate between the most and the least serious, called "vehement suspicion of heresy"; the suspiciously heretical beliefs were the cosmological thesis that the earth moves and the methodological principle that the Bible is not a scientific authority. Thus, he was forced to recite a humiliating "abjuration." And the *Dialogue* was banned.

The sentence also states that he was to be held in prison indefinitely. However, this particular penalty was immediately commuted to house arrest. Accordingly, for about one week he was confined to Villa Medici, a sumptuous palace in Rome belonging to the Tuscan grand duke. Then for about five months he was sent to the residence of Siena's archbishop, who was a good friend of Galileo's. Finally, in December 1633 he was allowed to live in seclusion at his own villa in Arcetri, near Florence.[2]

While the Inquisition's condemnation in 1633 ended the original Galileo affair, it gave rise to a new one that continues to our own day. To begin to make sense of it, I stress that the subsequent affair has three principal aspects: the historical aftermath; the critical issues; and the reflective commentary.

The historical aftermath consists of facts and events directly stemming from the trial. Here, we can just highlight the actions taken by the Church. In 1740 to 1758, Pope Benedict XIV allowed the partial unbanning of Galileo's *Dialogue* and of Copernican books in general. In 1820 to 1835, there was a total repeal of the prohibition of the Copernican doctrine. In 1893, Pope Leo XIII published an encyclical containing an implicit theological vindication of Galileo's biblical hermeneutics, although his name was not even mentioned. In 1942, during the commemoration of the tricentennial of Galileo's death, several Church officials started publicizing an appreciation of Galileo-the-person, as a model of harmony between science and religion. And in 1979–1992, Pope (now Saint) John Paul II undertook a further "rehabilitation"[3] of Galileo.

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[2] For details about Galileo's non-imprisonment, see Finocchiaro 2009.

[3] The quotation marks around this word are meant as scare quotes, for it is unclear whether there really was a rehabilitation, and if so what kind and to what extent. This will become apparent in the discussion below, where I will usually speak of rehabilitation efforts, although I will avoid the

The critical issues of the subsequent controversy in part reflect the original issues. However, the subsequent controversy has also acquired a life of its own, with debates over new issues, such as whether the condemnation of Galileo illustrates the incompatibility between science and religion. Indeed, traditionally this condemnation has been viewed as epitomizing such a conflict. Here, it is important to note that this view has been advanced not only by relatively injudicious authors such as John Draper (1875) and Andrew White (1896), who have recently been widely discredited (cf. Brooke 1991; Lindberg and Numbers 1987), but also by such scientific, philosophical, and cultural icons as Albert Einstein (1953), Bertrand Russell (1997), and Karl Popper (1956).

The reflective commentary on the original trial consists of countless interpretations and evaluations advanced in the past four centuries by astronomers, physicists, theologians, churchmen, historians, philosophers, cultural critics, playwrights, novelists, and journalists.

Although distinct, these three principal aspects of the subsequent affair are obviously interrelated. For example, much of the reflective commentary consists of attempts to formulate or resolve one or more critical issues, and such formulations often represent important developments of the historical aftermath.

In this essay, I shall focus on a particular development in the very recent historical aftermath. This involves primarily the accounts advanced by philosopher Paul Feyerabend and by historian John Heilbron. They raise relatively novel issues in the ongoing subsequent controversy.

11.3 Feyerabend on the Galileo Affair

Let us begin with the account advanced by Feyerabend (1985; 1987, 247–264; 1988, 129–138; 1993, 125–134). He portrays Galileo's trial as involving a conflict between two philosophical attitudes toward, and historical traditions about, the role of experts. That is, Galileo allegedly advocated the uncritical acceptance by society of the views of experts, whereas the Church advocated the evaluation by society of the views of experts in the light of human and social values. Feyerabend extracts this principle from Cardinal Bellarmine's letter to Foscarini (cf. Finocchiaro 2014b, 78–80). Moreover, he judges the principle favorably, in the sense that "the Church would do well to revive the balance and graceful wisdom of Bellarmine, just as scientists constantly gain strength from the opinions of … their own pushy patron saint Galileo" (Feyerabend 1985, 164). More generally, Feyerabend claims that "the Church at the time of Galileo not only kept closer to reason as defined then and, in part, even now; it also considered the ethical and social consequences of Galileo's views. Its indictment of Galileo was rational and only opportunism and a lack of perspective can demand a revision" (Feyerabend 1988, 129; 1993, 125).

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pedantry of constantly using the scare quotes. On this issue, besides the references given below, see also Segre 1999.

In my opinion, Feyerabend's account is untenable. In part, it is not really supported by the texts to which he refers. However, the principal difficulty is that he seems to commit a fallacy of equivocation. For the principle in question could mean either that *social and political leaders* should evaluate the *use* of experts' views in light of human and social values, or that *scientists* should evaluate the *truth* of each other's views in light of human and social values.

Now under the first interpretation, Galileo did not reject the principle, but rather would have agreed with it. Moreover, when Feyerabend attributes this principle to Bellarmine, the documentation is unclear and unconvincing. In any case, in this regard, the difference between Galileo and Bellarmine was not one of principle but of application. For example, they would have disagreed on who the relevant experts were, in particular whether theologians should be counted as experts in physics and astronomy; another disagreement would have been whether the views of theological experts should be subject to the same requirement.

Under the second interpretation, the principle was indeed rejected and criticized by Galileo. However, it is in fact untenable. For this version of the principle cannot survive the objections (which we moderns have inherited from Galileo) against teleological and anthropomorphic ways of thinking; such thinking reduces to arguing that something is true because it is useful, beneficial, or good, and false because it is useless, harmful, or bad.

However, whether untenable or not, Feyerabend's account is important, for at least two reasons, one historiographical, the other cultural.

Historiographically speaking, Feyerabend's account may be appreciated as an updated and revised conflictual thesis about the relationship between science and religion.[4] It was advanced in an essay with the revealing title of "Galileo and the Tyranny of Truth"; and this essay was a contribution to a 1985 conference on "The Galileo Affair: A Meeting of Faith and Science," sponsored by the Cracow Pontifical Academy of Theology and the Vatican Astronomical Observatory (cf. Coyne et al. 1985). The conference thus appears to have had an apologetic or pro-clerical aim, in the sense that it was meant to substantiate and elaborate Pope John Paul II's harmony thesis regarding the relationship between science and religion; that is, the thesis that science and religion are generally in harmony with one another, and that in particular the Galileo affair really proves this harmony, rather than their incompatibility, as commonly thought. Feyerabend did contribute an account which is in one sense apologetic and pro-clerical, but which remains conflictual, and so is critical and anti-clerical in another sense. This, of course, is the kind of irony and iconoclasm at which Feyerabend was a master.

Note that Feyerabend is reversing not the traditional type of *interpretation*, but rather what may be called the traditional *evaluation*. In fact, at the interpretive level, he sees a conflict between Galileo and the Church. However, at the evaluative level, he is (in the historical context) siding with the Church and against Galileo, insofar as he

[4] For some good examples of the voluminous literature on this problem, see Brooke 1991, Brooke and Cantor 1998, Das 2011, Finocchiaro (2010, 291–314; 2012, 14–25; 2014a, 311–314), Lindberg and Numbers (1986, 1987, 2003), Machamer 1998, and Numbers 2009.

thinks that the principle advocated by the Church was sounder than the one advanced by Galileo. At the same time, since the Church in the meantime has herself switched sides, the result is that Feyerabend is upholding the past Church against the present-day Church. The content and nature of Feyerabend's evaluation became more obvious later, in the 1988 edition of his book *Against Method*, where he explicitly criticized the rehabilitation efforts of Pope Saint John Paul II with the (in)famous words I have already quoted, to the effect that the Church was right to condemn Galileo, and would be wrong to rehabilitate him now, because she was upholding principles of rationality, morality, and social well-being that are sounder than Galileo's. And this substantiates my earlier remark that Feyerabend's account is apologetic and pro-clerical in one sense, but critical and anti-clerical in another sense. However, a clarification about these terms is in order.

Here, the labels *pro-clerical*, *anti-clerical*, *apologetic*, *pro-Galilean*, and *anti-Galilean* are intended to have a descriptive, informative, and piecemeal connotation, rather than a loaded, inflammatory, holistic, or name-calling meaning. Thus, note that I apply these terms primarily to theses and not to persons, and that in my account authors often advance views that are a mixture of such orientations; moreover, pro-clerical and pro-Galilean are not meant to be opposite. For example, note that here I am describing Feyerabend's account as pro-clerical in one sense, but anti-clerical in another. The non-invidious and nonloaded character of these terms may also be seen from the fact that I would have little difficulty describing certain parts of my own account as pro-clerical, and certain other parts as pro-Galilean.

My main point here is that Feyerabend's account provides a good illustration of how an ingenious scholar can formulate an interesting thesis, which is an updated and sophisticated version of the traditional, otherwise discredited, conflictual account.

A second reason for the importance of Feyerabend's account is cultural, in the sense of the historical repercussions it has had. In fact, it has become involved in one of the latest twists to the subsequent Galileo affair, which brings the story to our own day.

On the one hand, Feyerabend's apologia was politely rejected in 1989–1990 by Cardinal Joseph Ratzinger, who at the time was the chairman of the Congregation for the Doctrine of the Faith (the new name of the Inquisition); who in 2005 became Pope Benedict XVI; and who then resigned his position in 2013, thus becoming "Emeritus" pope. In a scholarly essay, in the context of an analysis of the role of faith in the revolutionary geopolitical changes happening in 1989–1990, Cardinal Ratzinger quoted several anti-Galilean critiques, including Feyerabend's. However, Ratzinger went on to criticize such views as expressions of skepticism and philosophical insecurity, asserting that "it would be foolish to construct an impulsive apologetic on the basis of such views; faith does not grow out of resentment and skepticism with respect to rationality, but only out of a fundamental affirmation and a spacious reasonableness … I mention all this only as a symptomatic case that permits us to see how deep the self-doubt of the modern age, of science and of technology goes today" (Ratzinger 1994, 98).

On the other hand, there seems to be a very widespread tendency to confuse or conflate Feyerabend's view with Ratzinger's. Some authors (Socci 1993, 62; Sinke

Guimarães 2005, 6) have claimed simply that Cardinal Ratzinger or Pope Benedict XVI *accepts* Feyerabend's view. Other authors (Machamer 2005; Saka 2006) have gone so far as to attribute this claim directly to Cardinal Ratzinger or Pope Benedict, without giving any indication that he was quoting Feyerabend. There have been some attempts to clarify the situation (Accattoli 1990, 15; Feyerabend 1993, 133–134 n. 20; Finocchiaro 2008, 274 n. 19), but apparently to no avail.

In fact, in January 2008 such confusion triggered the following clash (cf. Cini 2007; Anonymous 2008). A few months earlier, Pope Benedict XVI had accepted an invitation by the rector of the University of Rome to deliver the keynote address at the formal ceremony inaugurating the new academic year. This plan, however, triggered protests by students and faculty, especially in the university's distinguished department of physics. They objected primarily on the grounds of the principle of separation of Church and State, but also in part because, as they stated, they felt offended and humiliated by the pope's view of Galileo's condemnation, expressed some twenty years earlier when the pope was still a cardinal; that is, by his sharing Feyerabend's view. In the light of such opposition, and the potential for unrest and violence, the pope cancelled his speech.

This controversy is not helped, but rather exacerbated, by what seems to be a recurrent pattern of thinking or lecturing on the part of Benedict XVI, namely flirting with equivocation by means of quoting a controversial view. For example, an analogous issue arose as a result of a lecture he delivered at the University of Regensburg on September 12, 2006, in which he quoted a remark made by Byzantine emperor Manuel II Paleologus in 1391 regarding Islam and holy war (Ratzinger 2006). Now, given the post-nine-eleven geopolitical situation, Benedict did make a sustained effort to clear up the latter misunderstanding. But it appears that he has made no such effort regarding the approval of Galileo's condemnation.

In sum, Feyerabend elaborated an account of the Galileo affair that is conflictualist, anti-Galilean, and partly pro-clerical and partly anti-clerical. He interpreted the controversy as rooted in a conflict between Galileo's ideal of a value-free natural science, and the Church's ideal of a morally and social responsible natural philosophy. And Feyerabend evaluated the two sides by arguing that the Church was right and Galileo wrong, or at least that she was more nearly right than he was. In my judgment, Feyerabend's account is historically and philosophically untenable. However, from a methodological or historiographical point of view, his account represents an ingenious and clever version of the conflict thesis regarding the problem of science vs. religion. Moreover, from a cultural point of view, his account is significant and consequential insofar as it has become injected and involved into the latest developments and the highest levels of that continuing cause célèbre which is the Galileo affair.

Interesting as all this may be, in the present context I want to focus on something which is presumably even more intriguing. That is, Feyerabend exemplifies an unusual but important cognitive attitude, or scholarly approach, which combines a secular perspective and a clerical focus. However, before describing and analyzing it further, let me hasten to discuss another example.

11.4 Heilbron on the Galileo Affair

In 2010, John Heilbron published a massive and impressive biography of Galileo. Although the occasion seems to have been the International Year of Astronomy, commemorating the 400th anniversary of Galileo's telescopic discoveries, there is no question that the book was rooted in Heilbron's long and distinguished career, during which he has studied almost every aspect of the history of modern physics and astronomy, from the seventeenth to the twentieth century. The book ends with a section discussing the prospects for the Galileo affair coming to an end (Heilbron 2010, 358–365). This discussion embodies two theses: the main and more explicit thesis is the prediction that sooner or later the Catholic Church will canonize Galileo and make him a saint; a corollary and less explicit thesis is an endorsement of such an action to the effect that the canonization of Galileo would be proper.

These conclusions are no mere afterthought designed to give the book an interesting and provocative ending. In fact, Heilbron first discussed these ideas in a book published eleven years earlier, entitled *The Sun in the Church* (1999), and subtitled *Cathedrals as Solar Observatories*. In the middle of that book, in a chapter relating the centuries-long story of how the Church accommodated herself to Copernicus's doctrine of a moving earth, there is a section entitled "Galilaeus Sanctificatus" (Heilbron 1999, 207–211); and this is essentially an earlier version of the 2010 discussion that makes up the ending of the Galileo biography. Moreover, about halfway between these two books, Heilbron (2005) contributed a chapter to a collective volume on *The Church and Galileo*, edited by Ernan McMullin. In that chapter, entitled "Censorship of Astronomy in Italy after Galileo," although Heilbron does not advance the canonization thesis, or even discuss the issue, he does give a polished, synthetic, and neutral account of the story; and these historical developments are what provide him with the elements from which to argue in support of his prediction and endorsement in his 2010 book. In any case, that book's ending does indeed contain a supporting argument, which is certainly not an afterthought. Let us examine that argument.

To begin with, Heilbron wisely distinguishes two strands in the history of the subsequent Galileo affair: one pertains to the Church's attitude toward the Copernican doctrine of the earth's motion, which Heilbron (2010, 358–362) discusses in a section entitled "Off the Index"; the other strand involves the condemnation of Galileo the person and the Church's attitude toward him, which Heilbron (2010, 362–365) discusses in a section entitled "On the Rota?". The astronomical-physical part of the story consists of a series of small and gradual steps designed to moderate or retract the Church's opposition to Copernican astronomy. That story lasted about two centuries, beginning in 1616 when the Congregation of the Index declared the earth's motion false and contrary to Scripture, and ending in 1835 when Galileo's *Dialogue* was taken off the *Index*, after the Congregation of the Inquisition had decreed in 1822 that Catholics were free to hold the theory of the earth's motion in accordance with modern astronomy. This may strike one as the dark story of the commission of a monumental mistake, followed by a slow and reluctant recognition of the fact, and by various actions designed to repair the damage. However, Heilbron manages to find

a value and a positive lesson, as we can see from the following critical appreciation of ecclesiastic behavior:

> The policy of tolerating violations of the law when enforcement would do more harm than good, and annulling the law when violation has become ordinary practice, is often employed in church and state. A good administrator, like a good judge, knows when to be implacable and when to be lenient. Church officials who connived at ways to elude the force of the decrees against Copernicanism deserve notice and credit. Historians have ignored them because in not doing their jobs—that is, in not attempting to enforce a ridiculous and injurious ruling—they made no noise and because the imputed immobility of the Church over the two hundred years or so between the condemnation and the reprieve of the *Dialogue* makes too good and simple a story to ruin with facts. The Roman Catholic Church itself does not claim the wise inaction of its censors as a contribution to science in Italy. The Galileo Commission created at the instigation of Pope John Paul II missed an opportunity to blunt criticism of the Church by noticing officials who found a practical way out of the predicament into which Urban VIII and his Holy Office had plunged it. [Heilbron 2005, 280].

Let us now examine the other strand of the subsequent Galileo affair, involving the personal condemnation of Galileo. Heilbron stresses a number of milestones, as he calls them. The first occurred in 1893, with the encyclical *Providentissimus Deus* by Pope Leo XIII (Heilbron 2010, 362–363; cf. Finocchiaro 2005, 263–266). It elaborates a view of the relationship between scientific investigation and biblical interpretation that corresponds to the one advocated by Galileo; the authority of Scripture is explicitly limited to questions of faith and morals, and carries no weight for questions of physical truth and the world of nature.

The second milestone was occasioned by the tricentennial of Galileo's death in 1942 (Heilbron 2010, 363; cf. Finocchiaro 2005, 275–294). A number of Church officials commemorated the event by advancing or publishing views that portrayed Galileo as a Catholic hero who understood and practiced the very important doctrine that science and religion are in harmony because they both derive from God: science studies the Work of God, religion studies His Word. They also credited Galileo for his spirit of sacrifice and piety when at the trial, in deference to unambiguous Church commands, he abjured his scientific beliefs.

A third milestone occurred on the 400th anniversary of Galileo's birth in 1964, during the Second Vatican Council (Heilbron 2010, 363; cf. Finocchiaro 2005, 326–330). In response to various churchmen's proposals for a rehabilitation of Galileo, two related actions were taken. First, the Pontifical Academy of Sciences published an important pro-Galilean biography of Galileo, written two decades earlier by a clergyman named Pio Paschini (1964). Second, in the Council's constitution *Gaudium et spes* (approved on December 7, 1965), the Church affirmed explicitly the autonomy of science in general and deplored her wrongful interference in some cases in the past; and the text had a footnote mentioning the case of Galileo and referring the reader to Paschini's book.

Finally, there was the rehabilitation of Galileo by Pope John Paul II in the period 1979–1992 (Heilbron 2010, 363–364; cf. Finocchiaro 2005, 338–358). Recognizing the first milestone (of 1893), John Paul was explicit that Galileo's hermeneutical principles, unlike those of his ecclesiastical opponents, were correct, and that they

correspond to those which the Church herself adopted starting with Leo XIII's *Providentissimus Deus*. Recognizing the second milestone (of 1942), John Paul reiterated that Galileo was also right about the harmony between science and religion, and that this doctrine also corresponds to the one which the Church herself holds. And John Paul went beyond the earlier cryptic admission of wrongdoing at the Second Vatican Council (of 1964–1965); he was clearer and more explicit that the condemnation of Galileo had been not only a mistake, but also been an injustice.

Heilbron is well aware that this strand of the affair, unlike the strand involving the Copernican theory, is not over yet. Indeed he is quite realistic about how long the resolution will take: in his 1999 book, he spoke of about 100 years; in his 2010 book, he spoke of about 400 years, a period equivalent to that which has already elapsed. Nevertheless, Heilbron suggests that such rehabilitations of Galileo will continue, until sooner or later they will reach the level of canonization. The key part of his argument amounts to the following steps: describe the milestones just mentioned; point out that they obviously constitute a trend or tendency; postulate that the trend will continue; and thus reach a conclusion about the end result. Moreover, Heilbron's case for canonization has three subsidiary parts.

One subsidiary part is the accumulation of judgments and statements about Galileo's extraordinary and superhuman virtues on the part of his many followers and admirers, during the past four centuries (Heilbron 2010, 364–365). Then there is the business of his relics: just as for the case of other saints, various people and institutions are keen to find, collect, display, and revere various parts of Galileo's body (Heilbron 2010, 365). Thirdly, Heilbron sees it fit to defend Galileo's canonization prospects from a crucial possible objection, involving miracles. Here, Heilbron's words are worth quoting:

> It might be objected that Galileo performed no miracles. What then were the miracles of Thomas Aquinas? In fact, Galileo performed a stupendous miracle. He obliterated the ancient distinction between the celestial and terrestrial realms, raised the earth to the heavens, made the planets so many earths, and revealed that our moon is not unique in the universe. Not since the creation had there been such a refashioning. Then there was the miracle of himself, a rare combinations of talents and personalities, who, despite mania and depression, arthritis, gout, hernias, blindness, and overindulgence in wine and wit lived to write three books—the [*Sidereal*] *Messenger*, the *Dialogue*, and the *Discourse*—any one of which would have given him enduring fame. [Heilbron 2010, 365].

Heilbron ends his book with the following rhetorical question: "According to Galileo's mechanics, the slightest force can move the greatest weight given sufficient time. The direction of motion is clear. Who can doubt that within another 400 years the church will recognize Galileo's divine gifts, atone for his sufferings, ignore his arrogance, and make him a saint?" (Heilbron 2010, 365).

I believe that Heilbron's prediction cannot be dismissed. I have tried to reconstruct his argument in a plausible and sympathetic light. The fact that he gives such an argument means that it would be irrelevant to object that he is not serious, and does not really mean what he says. For even if that were the case, it would not invalidate the argument and evidence presented. Nevertheless, I am not sure I share Heilbron's

prediction, and I do not find his argument convincing. The key weakness is the following.

If we study the milestones of the Church's rehabilitation efforts described above, they each had a dark, negative, anti-Galilean side. Leo's encyclical *Providentissimus Deus* of 1893 does not even mention Galileo, let alone credit him with the right hermeneutics. The tricentennial rehabilitation of 1942 included the censorship and non-publication of the biography commissioned to Paschini by the Pontifical Academy of Sciences, on the grounds that the book was too pro-Galilean and anti-Jesuitical. The actions at the Second Vatican Council of 1964–1965 included the publication of Paschini's book in a censored version, consisting of the deletion or revision of the more pro-Galilean and anti-Jesuitical passages. John Paul's rehabilitation of 1979–1992 included the following paradox spread by the chairman of the Vatican Commission on Galileo (Cardinal Paul Poupard) and uncritically accepted by the pope: that although Galileo was right, and his ecclesiastical opponents wrong, with regard to biblical interpretation and hermeneutical questions, the reverse was the case with regard to scientific, methodological, and epistemological questions; Galileo was not aware of the weaknesses of his pro-Copernican arguments and evidence and of the epistemological limitations in general of scientific arguments for establishing the truth, whereas Pope Urban VIII and Cardinal Bellarmine were, and they attempted without success to enlighten Galileo about it. This is an old apologetic strategy first elaborated by Pierre Duhem (1908, 1969), and recently updated by a German clergyman named Walter Brandmüller (1982, 1992).

I do not want to give the impression that Heilbron is unaware that the various rehabilitation milestones have such anti-Galilean aspects. There is no question that he knows about them, since his discussion mentions and describes them. However, what I am saying is that Heilbron does not seem to realize that the existence of this other side in the Church's rehabilitation efforts casts doubt on what we can predict about the end result. For this anti-Galilean side amounts to the existence in this story of another trend opposing the one on which he focuses and which he wants to extrapolate into the future. I think both conflicting trends must be taken into account. If we do that, the safer and more plausible prediction to make is that in the next rehabilitation effort something will be done to credit Galileo in some way and admit some ecclesiastic wrongdoing, but in such a way as to come up short of a full-fledged Galilean exoneration and clerical retraction, perhaps on account of some previously neglected, or recently updated, or newly invented Galilean flaw. Extrapolating this pattern onto the final end result 100 or 400 or 1000 years from now, the chances are that we will have the following outcome. To echo the words with which Heilbron concludes his book, "the church will recognize Galileo's divine gifts, atone for his suffering, ignore his arrogance" (Heilbron 2010, 365) but *refuse* (I say) "to make him a saint" (Heilbron 2010, 365).

For example, the issue of miracles, which Heilbron tries to pre-empt and defuse, could easily provide such a pretext. In fact, miracles are a very serious business for the Church, and must be conceived in a material sort of way that involves concrete deliverance from physical ailments afflicting real living persons; they cannot be conceived as intellectual or mental achievements, as Heilbron seems to do. And the

case of Saint Thomas Aquinas, which Heilbron attempts to utilize, confirms this and underscores the difficulties which Galileo's canonization would have. Aquinas died in 1274 and was canonized by Pope John XXII, at Avignon, within fifty years (in 1323). As Aquinas's latest biographer states, the bull of canonization, which "extolled the 300 miracles by the new saint … is not … explicit on Thomas's intellectual work" (Torrell 1996, 1: 321). I conclude that if feats like the Christianization of Aristotle did not help Aquinas become a saint, it is unlikely that Galileo's raising the earth to the heavens would facilitate his canonization.

In a sense, Heilbron's argument is a typical example of the pitfalls to which almost everyone is irresistibly drawn in the business of making predictions about the future based on the existence of demonstrated or demonstrable past tendencies or trends. The pitfalls involve the assumption that the past trends will continue, and the neglect of countervailing trends. The most spectacular and culturally significant such prediction with such pitfalls was Karl Marx's prediction in the nineteenth century of the downfall of capitalism. One could conceptualize this problem by defining a corresponding logical fallacy and exploring general principles for avoiding it, but such logical theorizing and criticism are beyond the scope of the present inquiry. Instead, my aim here is to conceptualize Heilbron's account (as well as Feyerabend's) in general methodological terms. To that theme I now turn.

11.5 The Berkeley Para-Clerical Approach

Recall that, for the case of Feyerabend's account of the Galileo affair, I had not only some substantive criticism, but also some methodological appreciation. Similarly, for the case of Heilbron's canonization thesis, I have just articulated a substantive criticism of its supporting argument. However, this criticism of Heilbron should not make you forget that I devoted even more time and space to a sympathetic reconstruction of his position, on the firm belief in the scholarly originality and cultural importance of his canonization thesis and the canonization issue in general. Still, all this is relatively preliminary, and however interesting it may be, it does not yet touch something else which is potentially even more important, namely the similarities between Feyerabend and Heilbron.

To be sure, there are many differences between Feyerabend and Heilbron, disciplinary as well as biographical, general as well as particular, formal as well as substantive. For example, Feyerabend is a philosopher, Heilbron a historian; also, note that their positions are at almost opposite ends of the evaluative spectrum. However, here I want to stress the characteristics they share.

There are at least four things which they seen to have in common. First, they both hail from Berkeley. Heilbron studied and was trained at the University of California, Berkeley; he taught there from 1967 to 1994; he founded and directed its Office for the History of Science and Technology; he served as university Vice-Chancellor (of academic affairs) for several years, until he retired; and he has continued his association with this institution. As for Feyerabend, he taught at Berkeley for more

than 30 years, even longer than Heilbron, from 1958 to 1989. Note, in particular, that they were colleagues for about 22 years.

Second, neither Feyerabend nor Heilbron are Galilean specialists; that is, Galileo is *not* their first, main, or only scholarly concern. On the contrary, they were drawn to Galileo by other interests. In Feyerabend's case, he began working in the philosophy of physics, quantum mechanics in particular; then, partly under the influence of Karl Popper, Feyerabend made some contributions to general methodology. The key link was provided by the historical approach to the philosophy of science, that is, the use of the history of science to formulate and test philosophical claims about the nature of science, especially the utilization of significant episodes or great figures such as the Copernican Revolution and Galileo. In Heilbron's case, his primary interest may be said to be the history of physics, modern physics of the past four centuries; his earlier work dealt with atomic physics in the twentieth century, and with electricity in the seventeenth and eighteenth centuries. Whether or not one accepts Albert Einstein's and Stephen Hawking's judgment of Galileo as the father of modern physics (Einstein 1954, 271; Hawking 1988, 179), it is not surprising that a historian of physics with broad interests would sooner or later develop views about Galileo and find reasons to write them down. The importance of this shared trait is that it gives them a perspective on Galilean topics, relatively free of vested interests.

Third, both Feyerabend and Heilbron are master literary stylists, whose prose is anything but prosaic, but rather full of wit, irony, sarcasm, humor, double entendre, playfulness, and many other figures of speech that are beyond my own repertoire. This shared characteristic may seem irrelevant, but is important for at least two reasons. First, they share such a literary and rhetorical dimension with Galileo himself, and so in part they may have learned it from him and may thereby be trying to emulate him. Second, such a practice raises the question of whether what they say can or should be taken literally or at face value. In Feyerabend's case, there are well-known examples when he was pressed to elaborate and defend his published claims, and responded by saying that he had been playacting, being an iconoclast and a provocateur. And in Heilbron's case, I raised the issue earlier, resolving it by pointing out that, playacting or not, in his writings on the issue of Galileo's canonization there is argumentation and evidence which is real and consequential, even if introduced in a playacting mode. Still, the rhetorical dimension of the writing and work of Feyerabend, Heilbron, and Galileo may lend itself to a twist, as we shall see later.

Be that as it may, a more important trait of Feyerabend's and Heilbron's accounts of the Galileo affair is the following. Both Feyerabend and Heilbron are secular-minded scholars, and yet they are keenly concerned with questions about the relationship between science and religion. Moreover, their writings on this topic often contain frank advice to the Catholic Church about the conduct of her Galilean affairs. Furthermore, they often credit the Church, or defend her, in ways which Catholics themselves, clergymen or not, would not dare to do. It's as if Feyerabend and Heilbron believe that some Catholic affairs are too important to be left to Catholics themselves. This sort of attitude and practice takes intellectual courage, strong self-confidence, analytical subtlety, and experienced judgment.

Such an attitude and approach are clearly reminiscent of Galileo's own. In this regard, one revealing remark is found in a letter Galileo wrote just after the condemnation of Copernicanism in 1616 (cf. Finocchiaro 2014b, 108–109). Heilbron mentions it on more than one occasion, and describes the situation by saying that "Galileo occasionally referred to himself as a saint in his self-appointed mission to enlighten the Church" (Heilbron 1999, 211; cf. 2010, 364). This Galilean saintly self-image is part of Heilbron's case for the canonization thesis. However, I take it as evidence of the approach of secular-minded concern with, and service to, religion and the Church. Another important occasion when Galileo expressed his secular-minded concern with religion is in the classic *Letter to the Grand Duchess Christina*, in which he defends himself from the clerical criticism that he is a heretic because he believes in the earth's motion, which contradicts Scripture. In the introductory part of this essay, he gives some background information and makes a number of clarifications. One of these is the following contrast between the attitude of his enemies and his own:

> They always shield themselves with a simulated religious zeal, and they also try to involve Holy Scripture and to make it somehow subservient to their insincere objectives; against the intention of Scripture and of the Holy Fathers (if I am not mistaken), they want to extend, not to say abuse, its authority, so that even for purely physical conclusions which are not matters of faith one must totally abandon the senses and demonstrative arguments in favor of any scriptural passage whose apparent words may contain a different indication. Here I hope to demonstrate that I proceed with much more pious and religious zeal than they when I propose not that this book [Copernicus's *Revolutions*] should not be condemned, but that it should not be condemned without understanding, examining, or even seeing it, as they would like. [Galilei 2008, 113–114].

This approach which I have extracted from Feyerabend and Heilbron, and which I have also attributed to Galileo, has more cultural resonance and presence than it may appear at first. I believe it corresponds to the fundamental inspiration, motivation, and aim of the International Society for Science and Religion, the scholars associated with it, and the works that represent it. Here, as supporting evidence I would cite a book entitled *A Companion to the ISSR Library of Science and Religion*, edited by Pranab Das (2011), consisting of reviews of 224 books on the topic.

Furthermore, despite the indisputable Berkeley connection, it goes without saying that many scholars not associated with Berkeley practice such a methodological approach. In this regard, I would mention Joseph Agassi (1971), Richard Blackwell (1991, 1998a, 1998b, 2006), Albert DiCanzio (1996), Annibale Fantoli (2003, 2012), Marcello Pera (1998), and Michael Segre (1997, 1998), just to limit myself to good examples from the field of Galilean studies, which I know best.[5]

On the other hand, I would not say that this approach constitutes a school. For I do not think that the number of practitioners is sufficiently large or that their critical mass is great enough; nor has their self-reflective awareness and articulation of the

[5] It should be noted that a high proportion of such scholars (namely, Feyerabend, Agassi, Pera, and Segre) seem to be followers of Karl Popper, in one fashion or another. This fact may be significant, and there may indeed be an important connection between a Popperian orientation and the para-clerical approach. This topic deserves further study and reflection, but is obviously beyond the scope of the present investigation.

approach reached a sufficient degree of explicitness; and the same applies to the degree of scholarly and academic organization. In this regard, I would contrast this situation with the historical approach to the philosophy of science, which does seem to have all the requisites to be a school.

Similarly, it should be added that many Berkeley scholars have done excellent work on the interaction between science and religion, but do not practice this methodological approach; and this applies even to some outstanding Galileo scholars, such as Mario Biagioli (1993, 2006). My point is that this approach, important and fruitful as I believe it is, has no monopoly on truth and goodness.

Nevertheless, the Berkeley connection cannot be dismissed lightly, for it cannot be a fortuitous coincidence that another distinguished practitioner of this approach holds a history Ph.D. from there, namely Ronald Numbers (1993, 2009; cf. Das 2011, 133–136). Numbers, who was awarded the Sarton Medal in 2008, studies the history of scientific creationism, and manages to follow a secular-minded approach on questions about the relationship between evolutionary biology and biblical creationism. More generally, he conceived the project of bringing together scholars who have contributed to dispelling myths about science and religion, from the ancient Greeks to the twenty-first century; and then he edited a collection of resulting essays under the title *Galileo Goes to Jail and Other Myths about Science and Religion.* Numbers's concluding words in the book's introduction give a good flavor of the approach I am trying to articulate:

> The contributors to this volume have no obvious scientific or theological axes to grind. Nearly half, twelve of twenty-five, self-identify as agnostic or atheist (that is, unbelievers in religion). Among the remaining thirteen there are five mainstream Protestants, two evangelical Protestants, one Roman Catholic, one Jew, one Muslim, one Buddhist—and two whose beliefs fit no conventional category (including one pious Spinozist). Over half of the unbelievers, including me, grew up in devout Christian homes—some as fundamentalists or evangelicals—but subsequently lost their faith. I am not sure exactly what to make of this fact, but I suspect it tells us something about why we care so much about setting the record straight. [Numbers 2009, 6–7].

What I would add is that we are dealing with a cognitive phenomenon important enough to deserve a name. Partly echoing Numbers, and retaining the Berkeley connection, we could call this approach and these scholars "Berkeley clerical fact-checkers." But such emphasis on setting the record straight and fact checking strikes me as too naïve, uncritical, or positivistic. Perhaps we could label it the "Berkeley clerical pundit syndrome"; but the term syndrome would carry too negative a connotation. A more positive description would be "Berkeley clerical rescue service"; but that is perhaps too positive and somewhat crass. I think it may be best to drop all the terms introducing extra or extraneous connotations, and retain the bare essentials. Perhaps that would be accomplished by calling it simply the "Berkeley para-clerical" approach.

At this point someone might object to my label by suggesting that the word *Berkeley* be replaced by the word *Galilean* or by the term *Feyerabendian-Heilbronian*, or be dropped altogether. However, this semantical question is relatively unimportant, and I shall not pursue it here. It is more important to try to describe

and analyze this approach more clearly, more precisely, and more deeply, to which I now turn.

11.6 Numbers's Version of the Para-Clerical Approach

In the context of the Galileo affair, and more generally of the interaction between science and religion, I have illustrated and introduced what I call the Berkeley para-clerical approach. So far I have characterized this approach in terms of being secular-minded, offering advice to the Church, crediting the Church for uncommon reasons, defending the Church from common criticism, believing in the superiority of religious outsiders over insiders, and intellectual courage. These traits require some elaboration.

Let us focus on just two things. First, although the term *secularism* can be given various nuances of meaning, I believe a common core of all is the connotation of some kind of "indifference to or rejection or exclusion of religion and religious considerations."[6] Having said this, the next question that immediately arises for the analyst is: what exactly are indifference, rejection, and exclusion. Still, to have said this is not totally opaque or unhelpful. Secondly, to believe or act as if Church affairs and history are too important to be left to churchmen or religious believers themselves, whatever else it may imply, certainly implies that on such topics the secular or nonreligious investigator has an advantage over an investigator who is a religious believer.

Thus, here we have two specific attitudes that are anti-clerical, in the sense of being critical of or opposed to the Church. On the other hand, in the Berkeley para-clerical approach, one displays such an attitude in order to perform a service to the Church by offering useful advice, giving her due credit, and defending her from unfair criticism; and here we have three distinct pro-clerical attitudes. I believe that in so doing the para-clerical scholar is trying to properly combine the pros and the cons, in the belief that the topic is so important that one must avoid being one-sided or going too far along any one side. In other words, we are talking about committed and critical impartiality or judiciousness.

Such clarifications are useful partly because some of Ron Numbers's self-reflections explicitly bring out such a perspective. To begin with, Numbers's choice of topic is precisely what one would expect from such impartiality or judiciousness. Referring to his history of scientific creationism, Numbers insightfully says: "In writing this history, I have chosen to concentrate on those creationists who possessed, or claimed to possess, scientific credentials. This might strike some readers as an odd choice of topic for a historian of science, but I would submit that one of the best ways to learn about the history of 'science' is to explore how interested parties have contested its boundaries. Many books in recent years have sought to

[6] Merriam-Webster Dictionary online, at: http://www.merriam-webster.com/dictionary/secularism, consulted on 30 December 2014.

discredit creationism scientifically or theologically, but only a few have examined the movement historically …" (Numbers 1993, p. xiv).

Here, Numbers is using one connotation of the term *historical* to clarify his own approach. According to this connotation, to be historical is to be objective. In turn, objectivity does not mean being merely descriptive and avoiding evaluation altogether, which is impossible and self-defeating when the topic is a contested or controversial one. Rather, in such contexts, objectivity means being accurate in one's descriptions and fair in one's evaluations. And fairness in turn means avoiding one-sidedness and taking all sides properly into account, although of course this is not a mechanical task that can be reduced to following simple and precise rules, such as splitting the difference between the opposite sides; rather, it involves essentially the exercise of judgment.

Next, one of the major theses which Numbers elaborates may be regarded as an illustration of the para-clerical approach. This thesis is a substantive claim about the history of scientific creationism, and it asserts, in his own words:

> Rather than finding clerics arrayed in simple opposition to scientists, we discover conflicts of a different sort: psychological, as creationists struggled to reconcile the apparently conflicting claims of science and Scripture; and social, as they quarreled with one another over competing scientific and biblical interpretations or contested the boundaries of science and religion with evolutionists in courthouses, legislative halls, and school-board rooms. In virtually every public battle, even when creationists squared off against evolutionists, scientists and preachers could be found on both sides, and sometimes in unexpected numbers. For example, … the Arkansas creation-evolution trial of 1981 … prompted the Protestant theologian Langdon Gilkey … to observe that the only "warfare" in Little Rock found "liberal religion and liberal science on the one side, and absolutist religion and its appropriate 'science' on the other." [Numbers 1993, pp. xiv–xv].

Finally, Numbers's criticism of alternative approaches adds another variation on the same theme:

> For too long now students of science and religion have tended to grant the former a privileged position, often writing more as partisans than historians and grading religious "beliefs" by how much they encouraged or retarded the growth of scientific "knowledge." Recently we have heard persuasive calls for a more even-handed treatment. But even academics who would have no trouble empathetically studying fifteenth-century astrology, seventeenth-century alchemy, and nineteenth-century phrenology seem to lose their nerve when they approach twentieth-century creationism and its fundamentalist proponents … In other words, although many scholars seem to have no trouble respecting the unconventional beliefs and behaviors of peoples chronologically and geographically removed from us, they substitute condemnation for comprehension when scrutinizing their own neighbors. I think it is profitable to get acquainted with the neighbors, especially so if we find them so threatening. [Numbers 1993, pp. xvi–xvii].

Here, Numbers uses the notions of "non-partisanship" and "even-handedness" as two additions to that family of terms which I have been extracting: impartiality, judiciousness, objectivity, and judgment calls.

It seems to me that Numbers's self-reflective pronouncements are not only important in themselves, but also revealing with regard to the para-clerical approach. In fact, such self-reflections may be usefully compared to those we find in a work which

was produced by another Berkeley Ph.D., and which was published in 1989 by the University of California Press as the inaugural volume of the California Studies in the History of Science. This book is a collection of the most important documents pertaining to the trial and condemnation of Galileo from 1613 to 1633. Here is how he summarizes the approach he is advocating, at the end of a methodological and historiographical discussion in the Introduction:

> To summarize, a balanced approach to the study of the Galileo affair must avoid the two opposite extremes exemplified by the anti-Galilean and the anti-Catholic interpretations. There is no easy way of doing this, but it may help to distinguish scientific from epistemological issues, factual correctness from rational correctness, essential correctness from total correctness, the several epistemological issues from each other, intellectual from external factors, and the several external factors from each other (personal, psychological, social, economic, and political). However … these distinct entities are also interrelated, so the point is not to deny their interaction, but to make sure they are not confused with one another.[7]

Let me add here the usual caveat about theory vs. practice; that is, more important than saying such things and expressing such self-reflective pronouncements is to actually do them and to put them into practice in one's investigations. Indeed, just as Numbers practices in the course of his investigation what he preaches in the pronouncements quoted earlier, so does this other scholar. This is evident from the reactions of readers, who easily recognize that the book does follow this approach. One such reader is a distinguished outsider, legal scholar Alan Dershowitz, who writes: "*The Galileo Affair* should be required reading for everyone who values freedom and fears censorship. The extraordinary virtue of this collection of documents … is that it presents both sides of the dispute. 'Both sides?' you are probably thinking. Is there really a procensorship side of this particular debate that is worth reading? In answering that question, it must be recalled that at the time Galileo published his arguments, there was no dispositive empirical evidence that he was correct" (Dershowitz 1991).

Finally, besides the methodological similarity (at both the reflective and practical levels) between Numbers and this other Berkeley graduate, it is simply uncanny to find another similarity with regard to a main substantive thesis that illustrates the approach. I am referring to Numbers's thesis (mentioned above) that, in the history of the creation vs. evolution controversy, the real conflict has been not between science and religion, but between liberal science and religion on one side and absolutist science and religion on the other. Here is what this other author has written regarding this topic: "Because the Galileo Affair involved a conflict between one of the founders of modern science and one of the world's great religious institutions, it has traditionally been taken as an example of the warfare between science and religion … [However] even a cursory reading of the relevant documents shows that many churchmen were on his side and many scientists were on the opposite side; thus, there was a split within both science and religion, along the lines of what may be

[7] Finocchiaro 1989, 10.

called conservation and innovation; so the real conflict was between a conservative and a progressive attitude."[8]

11.7 Conclusion

I have been arguing that the methodological self-reflections of Numbers and this other scholar not only correspond to each other in tone and content, but also correspond to the methodological practices of Feyerabend and Heilbron in their accounts of the Galileo affair. Those self-reflections correspond to one another insofar as they both stress notions such as impartiality, judiciousness, objectivity, non-partisanship, even-handedness, and balanced judgment in the handling of controversial topics such as the Galileo affair, creation vs. evolution, and science vs. religion in general. And they correspond to the Feyerabend-Heilbron approach to the Galileo affair, which attempts to combine a generally secular and clerically external perspective with a concern to help, appreciate, and defend the Church when appropriate.

I have named this the Berkeley para-clerical approach, because it has been pioneered by these Berkeley professors and graduates. However, I have indicated that it is much more widespread than such a label might suggest. For example, I have argued that it is reminiscent of the approach which Galileo himself followed in matters of science vs. religion. I have also pointed out that this approach is practiced by a number of non-Berkeley scholars. And I have suggested that this approach is important and fruitful, and deserves even greater adoption. At the same time I have made it clear that this approach, like any other, is not infallible; and my substantive criticism of Feyerabend's and Heilbron's accounts may be taken to show this. Indeed infallibility is one of those religious doctrines toward which the para-clericals can only show indifference, rejection, or exclusion, to echo the canonical dictionary definition of secularism quoted above.

Finally, some personal clarifications and qualifications are in order. First, it is obvious that the fourth scholar cited above is the present writer; thus, the references and quotations given above are offered as evidence that I do indeed practice the para-clerical approach. Moreover, there is no doubt that I learned this approach at Berkeley; that is, in part from Feyerabend and Heilbron, who were my teachers and dissertation advisors. Additionally, my doctoral dissertation was not on Galileo, but on the historiography of science, in the sense of the philosophy and methodology of the history of science (Finocchiaro 1973); like them, only later was I drawn to Galileo, partly because one of my historiographical case studies was Alexandre Koyré's work on Galileo, and that led me to learn more about Galileo. However, another reason for my attraction to Galileo was that I felt he could serve as a good model to emulate in the search for the truth and acquisition of knowledge; and of course, the emulation could only be judicious and critical, and not mechanical or blind.

[8] Finocchiaro 1997, 3; cf. 2005, 3–4; 2010, pp. xxix–xxx; 2012, 22–24; 2014a, 311–314.

On the other hand, recall that, as presented above, the Berkeley para-clerical approach includes (as a minor component) the literary style and rhetorical flourish which Feyerabend and Heilbron, and indeed Galileo, like to use and display. And in this regard, for better or worse, I am not sure I have yet mastered, or ever learned, this Feyerabendian-Heilbronian style and rhetoric, especially the double-entendre and playfulness. In any case, even if I had, I might not want to use or display it on the present occasion. Thus, I hope readers will resist the temptation to misinterpret my rhetoric of Berkeley para-clericalism; they should not just dismiss it, or take it as mere rhetoric; but also they should not take that rhetoric more seriously than the substance of how this approach actually studies the relationship between Galileo and the Church, creationism and evolutionism, and more generally science and religion. In short, readers should be even-handed and impartial in their analysis of my rhetoric.

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Chapter 12
Prison and Torture as False but Well-Founded Myths

Abstract This essay discusses two common myths about the Inquisition trial of Galileo: that he was tortured during the proceedings, and that he was imprisoned as punishment for his alleged crime. The truth is that although Galileo underwent an interrogation with the verbal threat of torture, he did not undergo actual torture; and although he was under house arrest during the 1633 trial and for the subsequent 9 years of his life, he was never held in an actual prison. However, it would be a historical and philosophical error to think that these myths are, or have always been, without foundation. On the contrary, for 150 years after the 1633 trial, the publicly available evidence indicated that Galileo had been imprisoned, and so it was then plausible and reasonable to hold the prison thesis. Similarly, for an even longer period (250 years), the available evidence indicated that he had been tortured, and so the torture thesis was then rationally defensible.

> [T]he great Galileo, at the age of fourscore, groaned away his days in the dungeons of the Inquisition, because he had demonstrated by irrefragable proofs the motion of the earth. —Voltaire, "Descartes and Newton" (1728, 167)

> [T]he celebrated *Galileo* … was put in the inquisition for six years, and put to the torture, for saying, that *the earth moved.*—Giuseppe Baretti, *The Italian Library* (1757, 52)

> [T]o say that Galileo was tortured is not a reckless claim, but it is simply to repeat what the sentence says. To specify that he was tortured about his intention is not a risky deduction, but it is, again, to report what that text says. These are observation-reports, not magical intuitions; proved facts, not cabalistic introspections.—Italo Mereu, *History of Intolerance in Europe* (1979, 385)

12.1 Introduction

In the early years of the seventeenth century the Italian mathematician and natural philosopher Galileo Galilei (1564–1642) openly advocated the theory of the earth's motion elaborated in Nicolaus Copernicus's book *On the Revolutions of the Heavenly*

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_12

Spheres (1543). As a result, he was persecuted, tried, and condemned by the Catholic church. He spent the last nine years of his life under house arrest in his villa outside Florence. But was he imprisoned and tortured as the authors above, and countless others, have alleged?[1]

12.2 Galileo's Trial

Galileo did not begin advocating Copernicanism until 1609. Before then, he was acquainted with Copernicus's work and appreciated the fact that it contained a novel and significant argument for the earth's motion. Galileo had been working on a new theory of motion and had intuited that the Copernican theory was more consistent with the new physics than was the geostatic theory. But he had not published or articulated this intuition. Moreover, he was acutely aware of the considerable evidence against Copernicanism stemming from direct sense experience, astronomical observation, traditional physics, and scriptural passages. Accordingly, he judged that the anti-Copernican arguments far outweighed the pro-Copernican ones.

In 1609, however, he perfected the newly invented telescope, and in the next few years by its means he made several startling discoveries: mountains on the moon, innumerable stars besides those visible with the naked eye, dense collections of stars in the Milky Way and nebulas, four satellites around Jupiter, the phases of Venus, and sunspots. He described them in *The Sidereal Messenger* (1610) and *Sunspots Letters* (1613).

As Galileo started showing that the new telescopic evidence rendered Copernicanism a serious contender for real physical truth, he came increasingly under attack from conservative philosophers and clergymen. They argued that he was a heretic because he believed in the earth's motion and the earth's motion contradicted Scripture. Galileo felt he could not remain silent and decided to refute the biblical arguments against Copernicanism. He wrote his criticism in the form of long, private letters, in December 1613 to his disciple Benedetto Castelli and in spring 1615 to the grand duchess dowager Christina.

Galileo's letter to Castelli provoked the conservatives even further, and so in February 1615 a Dominican friar filed a written complaint against Galileo with the Inquisition in Rome. The resulting investigation lasted about a year. Galileo

[1] For other statements of the prison thesis, see Holste 1633; Milton 1644, 24; Bernini 1709, 4: 615; Moreri 1718, 1: 196; Delambre 1821, 1: 671; Draper 1874, 171–172; Haeckel 1878–1879, 33; White 1896, 2: 142; and Russell 1935, 40. On the torture thesis, see also Frisi 1775, 71; Nelli 1793, 2: 542–554; Libri 1841, 34–37; Gherardi 1870, 52–54; Wohlwill 1877; Scartazzini 1877–1878; and Genovesi 1966, 232–282. A version of the prison myth can also be found in the context of recent popular culture. It occurs in the PBS two-hour program titled *Galileo's Battle for the Heavens,* first aired on 29 October 2002. Near the end of the program, there is a scene in which Galileo arrives at his house in Arcetri after the condemnation, and the door to the house is shown being closed and locked with a key from the outside. This suggests that he was not free to go in and out of his house, whereas in fact he was: he was free to walk in the villa's gardens and to travel the few blocks to the nearby convent where his daughter was a nun.

himself was not summoned to Rome, partly because the key witnesses exonerated him, partly because his critical letters had not been published, and partly because his publications contained neither a categorical assertion of Copernicanism nor a denial of the scientific authority of Scripture.

In December 1615, however, Galileo went to Rome of his own accord to defend the Copernican theory. Despite winning the intellectual arguments, his practical effort failed. In February 1616, Cardinal Robert Bellarmine (in the name of the Inquisition) gave Galileo a private warning forbidding him to hold or defend the view that the earth moved. Galileo agreed to comply. In March, the Index of Prohibited Books (the department charged with book censorship) published a decree, without mentioning Galileo, that declared that the earth's motion was physically false and contradicted Scripture and that Copernicus's book was banned until revised.

Until 1623, when Cardinal Maffeo Barberini became Pope Urban VIII, Galileo kept quiet about the forbidden topic. Since Barberini was an old admirer, Galileo felt freer and decided to write a book that would defend Copernicanism indirectly and implicitly. Thus he wrote a dialogue featuring three characters engaged in a critical discussion of the cosmological, astronomical, physical, and philosophical aspects of Copernicanism but avoiding the biblical or theological ones. Published in 1632, this *Dialogue* showed that the arguments favoring the earth's motion were stronger than those favoring the geostatic view. Galileo apparently felt that the book did not "hold" the theory of the earth's motion because it was not claiming that the favorable arguments were conclusive; it was not "defending" this theory because it was a critical examination of the arguments on both sides.

Galileo's enemies nevertheless complained that the book defended the earth's motion and so violated Bellarmine's warning and the Index's decree. A new charge also emerged: that the book violated a special injunction issued personally to Galileo in 1616, prohibiting him from discussing the earth's motion in any way whatever. Such a document had just been discovered in the file of the earlier proceedings. Thus he was summoned to Rome for trial, which began in April 1633.

At his first hearing Galileo admitted receiving from Bellarmine the warning that the earth's motion could not be held or defended. But he denied receiving a special injunction not to discuss the topic in any way whatever. In his defense he introduced a certificate he had obtained from Bellarmine in 1616, which mentioned only the prohibition to hold or defend. Galileo also claimed that the *Dialogue* did not defend the earth's motion but rather showed that the favorable arguments were not conclusive and so did not violate Bellarmine's warning.

In light of Bellarmine's certificate and of various irregularities with the special injunction, the Inquisition's officials tried out-of-court plea-bargaining: they promised not to press the most serious charge (violation of the special injunction) if Galileo would plead guilty to a lesser charge (transgression of the warning not to defend Copernicanism). Galileo agreed, and so at subsequent hearings (on April 30 and May 10) he admitted that the book was written in such a way as to give readers the impression of defending the earth's motion. However, he denied that this had been his intention and attributed his error to conceit.

The trial ended on June 22, 1633, with a harsher sentence than Galileo had been led to expect. The verdict found him guilty of a category of heresy intermediate between the most and the least serious, called "vehement suspicion of heresy." The objectionable beliefs were the astronomical thesis that the earth moves and the methodological principle that the Bible is not a scientific authority. He was forced to recite a humiliating "abjuration" retracting these beliefs. But the *Dialogue* was banned.[2]

12.3 Origin of, and Evidence for, These Two Myths

The lengthy sentencing document also recounted the proceedings since 1613, summarized the 1633 charges, and noted Galileo's defense and confession. In addition, it provided two other extremely important details. The first described an interrogation: "Because we did not think you had said the whole truth about your intention, we deemed it necessary to proceed against you by a rigorous examination. Here you answered in a Catholic manner, though without prejudice to the above-mentioned things confessed by you and deduced against you about your intention." The second imposed an additional penalty: "We condemn you to formal imprisonment in this Holy Office at our pleasure."[3]

The text of the Inquisition's sentence and Galileo's abjuration were the only trial documents publicized at the time. Indeed, the Inquisition sent copies to all provincial inquisitors and papal nuncios, requesting them to disseminate the information. Thus news of Galileo's fate circulated widely in books, newspapers, and one-page flyers. This unprecedented publicity resulted from the express orders of Pope Urban, who wanted Galileo's case to serve as a negative lesson to all Catholics and to strengthen his own image as an intransigent defender of the faith.[4]

The prison clause in the sentence clearly stipulated that Galileo was to be imprisoned in the jail at the Inquisition palace in Rome for an indefinite period, as long as the authorities wanted. Anyone reading or hearing of the sentence would naturally assume that the Inquisition had carried out the sentence it had imposed.

Although the sentence did not use the word *torture,* it did speak of a "rigorous examination," a technical term connoting torture. Moreover, the passage gave a reason why the judges had decided to subject Galileo to a rigorous examination: after the various interrogations, including his confession (to having defended Copernicanism), they had doubts about whether his transgression had been intentional (thus aggravating the crime) or inadvertent (as he claimed). In Inquisition practice (as well as in lay criminal courts) such doubts justified the administration of torture

[2] The simplified account of Galileo's trial given in the preceding paragraphs is distilled from the following standard works: Santillana 1955; Drake 1978; Finocchiaro 1980, 1989; Biagioli 1993, 2006; Feldhay 1995; Bucciantini 1995; Beretta 1998; Fantoli 2003; Shea and Artigas 2003; Camerota 2004; McMullin 2005; Blackwell 2006; and Beltrán Marí 2006.

[3] Quoted from Finocchiaro 1989, 290, 291; see also Galilei 1890–1909, 19: 405, 406.

[4] Galilei 1890–1909, 15: 169, 19: 411–415; Finocchiaro 2005, 26–34.

(in order to resolve them). The passage informed readers that Galileo had passed the rigorous examination when it stated that he "answered in a Catholic manner." That is, Galileo had answered like a good Catholic, who would not intentionally do something the church had forbidden. Finally, the passage clarified, again in accordance with inquisitorial practice, that Galileo's denial of a malicious intention (his "Catholic answers") did not undermine the other incriminating evidence coming from his confession and other sources (for example, opinions on the *Dialogue* written by three consultants). Readers of the sentence acquainted with legal terminology and practice understandably concluded that Galileo had suffered torture at the hands of his inquisitors.[5]

The impression that Galileo had been imprisoned and tortured remained plausible as long as the principal evidence available about Galileo's trial came from these documents, the sentence and abjuration. The story remained unchanged until—after about 150 years for the prison thesis and about 250 years for the torture thesis—relevant documents came to light showing that Galileo had suffered neither.

12.4 Subsequent Evidence Against the Prison Thesis

The new information about imprisonment comes from correspondence in 1633, primarily from the Tuscan ambassador to Rome (Francesco Niccolini) to the Tuscan secretary of state in Florence, and secondarily that to and from Galileo himself. The Tuscan officials were especially interested in Galileo because he was employed as the chief mathematician and philosopher to the grand duke of Tuscany, had dedicated the *Dialogue* to him, and had successfully sought his help in publishing the book in Florence. Thus the Tuscan government treated the trial like an affair of state, with Niccolini constantly discussing the situation directly with the pope at their regular meetings and sending reports to Florence. Moreover, Galileo was on very friendly terms with Niccolini and his wife.[6]

The 1633 correspondence, which surfaced in 1774–1775, shows that Galileo, answering the Inquisition's summons, left Florence on January 20 and arrived in Rome on February 13. The Inquisition allowed him to lodge at the Tuscan embassy (which served also as the Niccolinis' residence) on condition that he remain in seclusion until the proceedings started. On April 12 Galileo went to the Inquisition

[5] Masini 1621, 121–151; Scaglia 1616, 133; Eymerich and Peña 1578, 158–164, 207–212; Beretta 1998, 214–221.

[6] Some crucial letters were published in Florence in 1774, as reported in Nelli 1793, 2: 537–538. A larger collection was published in Fabroni 1773–1775. A good and accurate summary of this evidence was given by Tiraboschi 1793, 382, translated in Finocchiaro 2005, 171. The 1633 correspondence can now be found in Galilei 1890–1909, vol. 15; the trial depositions in vol. 19: 336–362. Translations of the most important such correspondence and of all four deposition are given in Finocchiaro 1989, 241–255, 256–287, respectively. For the distinction between the Tuscan embassy (Palazzo Firenze) and Villa Medici, see Shea and Artigas 2003, 30, 74, 106–107, 134–135, 179–180, 195.

palace for his first interrogation. He stayed there for the next eighteen days while undergoing further interrogations, but he was put up in the prosecutor's six-room apartment, together with a servant, who brought him meals twice a day from the Tuscan embassy. On April 30, after his second deposition was recorded and signed, Galileo returned to the embassy, where he remained for fifty-one days, interrupted by a visit to the Inquisition palace on May 10 to give a third deposition. On Monday, June 20, he was summoned to appear in court the following day. On Tuesday he underwent the rigorous examination—and remained at the Inquisition palace until the evening of June 24. It is unclear whether he was held in a prison cell or permitted to use the prosecutor's apartment. On June 22 he appeared at the convent of Santa Maria sopra Minerva for sentencing and abjuration. Two days later Galileo moved from the Inquisition palace to Villa Medici in Rome, a sumptuous palace owned by the grand duke of Tuscany. On June 30 the pope granted Galileo permission to travel to Siena to live under house arrest at the residence of the archbishop, a good friend of Galileo's. The archbishop hosted him for five months. In December 1633 Galileo returned to his own villa in Arcetri, near Florence, where he remained—except for a brief period in 1638, when he resided within the city limits of Florence—under house arrest until his death in 1642.

With the possible exception of three days (June 21–24, 1633), Galileo was never held in prison, either during the trial (as was universal custom) or afterward (as the sentence decreed). Even for those three days he likely lodged in the prosecutor's apartment, not in a cell. The explanation for such unprecedentedly benign treatment is not completely clear but includes the following factors: the protection of the Medici, Galileo's celebrity status, and the love-hate attitude of Pope Urban, an erstwhile admirer.

12.5 Actual Torture vs. Threat of Torture

The evidence for staying out of jail tells us nothing about Galileo's success in avoiding torture. The resolution of this question had to wait until the trial proceedings were published and assimilated in the late nineteenth century.[7] Two documents proved crucial.[8] The first was the minutes of the Inquisition meeting of June 16, 1633, chaired by the pope. After various reports and opinions were heard and after considerable discussion,

> His Holiness decided that the same Galileo is to be interrogated even with the threat of torture; and that if he holds up, after a vehement abjuration at a plenary meeting of the Holy

[7] The trial proceedings were published by Épinois 1867, 1877, Berti 1876, 1878, and Gebler 1877. Besides the proponents of the torture thesis mentioned in note 1, essential works in the assimilation process included Marini 1850, 54–68; Martin 1868, 123–131; Pieralisi 1875, 227–246; Berti 1876, cv–cxvii; Épinois 1878, 197–216; Gebler 1879, 252–263; Garzend 1911a, 1911b; and Giacchi 1942.

[8] The first is quoted from Finocchiaro 2005, 246; see also. Galilei 1890–1909, 19: 282–283; and Épinois 1867, 129 n. 4. The other is from Finocchiaro 1989, 287; see also Galilei 1890–1909, 19: 362.

> Office, he is to be condemned to prison at the pleasure of the Sacred Congregation, and he is to be enjoined that in the future he must no longer treat in any way (in writing or orally) of the earth's motion or sun's stability, nor of the opposite, on pain of relapse; and that the book written by him and entitled *Dialogo di Galileo Galilei Linceo* is to be prohibited.

This preview of the actual sentence mentions a novel procedure: interrogation under the threat of torture. The minutes of the interrogation, dated June 21 and signed by Galileo, reveal that the commissary asked him several times whether he held the Copernican theory of the earth's motion; each time Galileo denied having done so after the condemnation of that doctrine in 1616. The exchange between Galileo and his inquisitors is worth quoting in full:

> *Q*: Having been told that from the book itself and the reasons advanced for the affirmative side, namely that the earth moves and the sun is motionless, he is presumed, as it was stated, that he holds Copernicus's opinion, or at least that he held it at the time, therefore he was told that unless he decided to proffer the truth, one would have recourse to the remedies of the law and to appropriate steps against him.
>
> *A*: I do not hold this opinion of Copernicus, and I have not held it after being ordered by injunction to abandon it. For the rest, here I am in your hands; do as you please.
>
> *Q*: And he was told to tell the truth, otherwise one would have recourse to torture.
>
> *A*: I am here to obey, but I have not held this opinion after the determination was made, as I said.
>
> And since nothing else could be done for the execution of the decision, after he signed he was sent to his place.
>
> I, Galileo Galilei, have testified as above.

This deposition leaves no doubt that Galileo was *threatened* with torture during the June 21 interrogation. But there is no evidence that he was *actually* tortured, or that his accusers planned actually to torture him. Apparently, the "rigorous examination" mentioned in the sentence meant interrogation with the threat of torture, not interrogation under actual torture.

12.6 Varieties of Torture

The most common and relevant torture in Rome at the time was "torture of the rope." This consisted of tying the victim's wrists together behind the back, then tying the joined wrists to the end of a long rope that went around a pulley hung from the ceiling. The executioner held the other end of the rope in such a way that the victim could be hoisted in midair and left hanging for different periods (a standard rule specified a maximum of one hour). To increase the strain, weights of various amounts could be attached to the victim's feet. Alternatively, the victim would be dropped from various heights, just short of hitting the ground; the greater the height of the drop,

the greater the pain in the victim's arms and joints (in fact, the numerical values of the distance dropped provided a quantitative measure of the severity of the torture).[9]

Because of the severity of the torture of the rope, we can be fairly certain that Galileo was not tortured this way. Given his advanced age of sixty-nine years and his frailty, he would have suffered permanent injury to his arms and shoulders, but there is no evidence of this. Moreover, if he had been tortured, it would have happened on June 21, leaving him in no condition to attend the sentencing and recite the abjuration on the twenty-second. Furthermore, Inquisition rules required that the torture session, including the victim's cries and groans, be recorded, but the proceedings contain no such minutes. Inquisition rules also stipulated that confessions obtained during torture be ratified twenty-four hours later, outside the torture chamber, but there is no record of ratification. And before a defendant could be tortured, there had to be a formal vote by the Inquisition consultants recommending it, as well as a decree to that effect by the inquisitors; but no minutes indicate that these steps were taken in Galileo's case.[10]

In addition, Inquisition authorities in Rome rarely practiced torture, further reducing the likelihood that Galileo experienced this punishment. Inquisitorial rules exempted old or sick people (along with children and pregnant women) from torture, and Galileo was not only elderly but suffering from arthritis and a hernia. The rules also spared clerics, and we now know that Galileo had received the clerical tonsure (a ceremonial haircut given to men being inducted into the clergy) on April 5, 1631, in order to benefit from an ecclesiastical pension. For reasons that may easily be guessed, the rules of torture stipulated that defendants could not be tortured unless a period of ten hours had elapsed since their last meal; but the known pace of the trial did not leave a gap of this length. Finally, another rule held that defendants could not be tortured during the investigation of an alleged crime unless the transgression was serious enough to require corporal punishment. Galileo's alleged crimes fell short of formal heresy, which would have justified corporal punishment; therefore, torturing him would have been inappropriate.[11]

Of course, all of the aforementioned rules and practices were subject to exception. For example, although old men could not be subjected to the rope torture, they could undergo the torture of fire to the feet. And although clerics could not be tortured by laymen, they could be tortured by other clerics. Moreover, the rules were often abused or disregarded by individual officials.[12] Additionally, several intermediate steps existed between the two extremes of threat during an interrogation outside the torture chamber and actual torture with the infliction of physical pain in the torture

[9] See Scaglia 1616, 133; Genovesi 1966, 79–81; Mereu 1979, 226–227; Beretta 1998, 216; and Beltrán Marí 2006, 797.

[10] See Masini 1621, 120–151; Berti 1876, pp. cv–cxvii; Gebler 1879, 256–257; and Beretta 1998, 214–221.

[11] See especially Garzend 1911a, 1911b, citing an impressive array of treatises on canon law, civil law, theology, and inquisitorial practice from Galileo's time. For Galileo's clerical tonsure, see Galilei 1890–1909, 19: 579–580.

[12] On abuses of the system that occurred in 1604 in the Paduan Inquisition's investigations of Galileo and Cesare Cremonini, see Beltrán Marí 2006, 25–45.

chamber—from showing the defendant the instruments of torture, to undressing the victim and then tying him to the instruments in preparation, and so on. The term *territio realis* (meaning "real intimidation," as distinct from *territio verbalis,* or "verbal threat") was used to refer to these intermediate steps. Some scholars have speculated that Galileo was subjected to *territio realis.* This version of the torture thesis is not incompatible with the June 16 papal orders or with the fact that Galileo showed no signs of shoulder dislocation after June 21, that he had enough physical strength to attend the sentencing and abjuration on June 22, that there was no ratification of the confession under actual torture, and that there was no consultants' vote or Inquisitors' decree for torture. However, it is inconsistent with the June 21 deposition, which contains no description of those intermediate steps. Hence this version of the torture thesis presupposes the inauthenticity of that deposition.[13]

One could object that even if Galileo was not tortured physically, the treatment he received amounted to moral (or psychological) torture, that is, the threat of torture in the last interrogation and the perpetual house arrest after the condemnation. Indeed, since the middle of the nineteenth century many authors have maintained the moral-torture thesis.[14] But the argument for moral torture starts down a semantic slippery slope with no end in sight.

12.7 Conclusion

In view of the available evidence, the most tenable position is that Galileo underwent an interrogation with the threat of torture but did not undergo actual torture or even *territio realis.* Although he remained under house arrest during the 1633 trial and for the subsequent nine years of his life, he never went to prison. We should keep in mind, however, that for 150 years after the trial the publicly available evidence indicated that Galileo had been imprisoned, and for 250 years the evidence indicated that he had been tortured. The myths of Galileo's torture and imprisonment are thus genuine myths: ideas that are in fact false but once seemed true—and continue to be accepted as true by poorly educated persons and careless scholars.

[13] On degrees of torture, see Masini 1621, 120–151; Limborch 1692, 322; Scartazzini 1877–1878, 6: 403–404; Gebler 1879, 256, n. 2; Genovesi 1966, 252–255; Eymerich and Peña 1578, 209; and the original sources to which most of these authors refer: Grillandi 1536, question 4, number 11; and Clarus 1640, question 64. For support of the *territio-realis* thesis, see Wohlwill 1877, 25–28. For criticism, see Gebler 1879, 254–256; and Finocchiaro 2005, 252.

[14] Proponents of the moral-torture thesis include Biot 1858, 3: 42–43; Chasles 1862; Trouessart 1865, 110. For criticism of the moral-torture thesis, see Pieralisi 1875, 242–246. See also Finocchiaro 2005, 234, 236.

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Chapter 13
Galileo Under Fire and Under Patronage

Abstract This essay explores three challenging ironies in Galileo's career, involving the viewpoints of leisure, adversity, and intellectual achievement. Before 1610, as a university professor at Padua and employee of the Venetian Republic, he had the liberty and protection to conduct significant and unorthodox research, but lacked the leisure and financial comfort to bring it to completion. After 1610, as philosopher and chief mathematician to the grand duke of Tuscany, Galileo had the leisure and comfort for full time research, but was unable to do it effectively because of opposition by churchmen and rivals. Also after 1610, he was both under fire and under patronage from the same institution—the Church—which both paid (indirectly) his salary and created many obstacles for his Copernican research program.

13.1 Introduction

In a famous passage, Aristotle remarked that "the sciences which do not aim at giving pleasure or at the necessities of life were discovered … first in the places where men first began to have leisure. This is why the mathematical arts were founded in Egypt; for there the priestly caste was allowed to be at leisure."[1] It is clear that Aristotle is limiting the scope of his generalization to the theoretical sciences, as distinct from the recreational and practical arts, and that his talk of leisure refers to the existence of a socio-economic class of mental workers, rather than to the individual availability of idleness and free time. However, it is less clear whether Aristotle means to claim that leisure is a necessary, or a sufficient, or merely an important condition for intellectual achievements. It would be difficult to hold that it is necessary, for as Jonathan Lavery has pointed out in the Introduction, there are many important examples of superior intellectual contributions that were made in conditions of such adversity that they could not be regarded as leisure, however defined. Similarly, there are many historical

[1] Aristotle, *Metaphysics*, book 1, chapter 1, 981b20–24.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_13

cases where an individual's leisure or a leisure class produced only recreational arts, barren idleness, or wasted time, but no intellectual achievements.

In fact, there is a non-Aristotelian tradition that makes intellectual accomplishments dependent on things which are almost the opposite of leisure, and which may be subsumed under the label of "adversity": crises, criticism, obstacles, problems, and difficulties. This tradition includes thinkers as diverse as Georg W.F. Hegel, Karl Marx, John Dewey, Benedetto Croce, Karl Popper, and Thomas Kuhn.[2] In Croce's words, "every mental act … emerges from a contrast, from some labor, from a war that seeks peace, from an obscurity that seeks light."[3] Among such authors, the consensus is that adversity is merely a necessary condition and that equally necessary is the *overcoming* of adversity. Then the issue becomes that of what are the adversities that constitute proper challenges, and what conditions facilitate the overcoming of such challenges.

If we combine the two traditions, we can see that both leisure and adversity are important factors in bringing about intellectual achievements. This is the hypothesis I should like to explore with reference to the case of Galileo. To be more precise, I plan to examine Galileo's career in the context of these questions and this hypothesis; I leave to others or to some future investigation of mine the derivation from this case study of appropriate conclusions about the exact role and the precise blend of leisure and adversity in intellectual achievements.

Furthermore, Galileo's career was so long, complex, intense, and eventful that it would be also beyond the scope of this paper even to merely narrate it, let alone analyze it from the point of view of the present problem.[4] However, it can be divided into two main periods which are strikingly different in terms of the categories of our problem; the year that divides his earlier and later life is 1610. Moreover, the change that occurred in that crucial year constitutes one of the root causes for Galileo's trial and condemnation by the Inquisition in 1633. Thus, it will be very instructive to structure my account in accordance with this periodization.

13.2 Paduan Industriousness (1592–1610)

Galileo was born in 1564 in Pisa, which at that time was part of the grand duchy of Tuscany ruled by the House of Medici in Florence. His was an impoverished aristocratic family that originated from Florence and moved back there when he

[2] Hegel 1969, 54, 442–443; cf. Finocchiaro 1988, 192–194, 224–226; Croce (1917, 198–217; 1941, 17–18; 1967a, 121–131); cf. Finocchiaro (1973, 201–206; 1988, 47–50); Dewey 1960; Popper 1968, 66–96, 128–129, 198–199, 312–335; Kuhn 1962; cf. Brown 1979, 129–144.

[3] Croce 1967, 126–127; here and in the rest of this essay, all translations are my own, unless otherwise noted. Cf. Croce 1917, 208; Finocchiaro 1988, 47–50.

[4] See, for example, Camerota 2004; Drake 1978; Galilei 2008; and Heilbron 2010.

was about ten. His father Vincenzo was a musician and musical theorist who made significant contributions to the field and influenced the son's experimental approach. In 1581 Galileo enrolled at the University of Pisa to study medicine, but soon switched to mathematics, which he also studied privately. In 1585, he left the university without a degree and began doing private teaching and research. In 1589, he became professor of mathematics at that university. Then from 1592 to 1610, he was a mathematics professor at the University of Padua, which was a public institution in the Republic of Venice and much more prestigious and internationally renowned than Pisa.

As a mathematics professor, Galileo was required to teach, and did teach, courses on such subjects as Euclidean geometry, spherical astronomy, Ptolemaic planetary astronomy, and Aristotelian mechanics[5] (which meant the theory of machines, mechanical instruments, and labor saving mechanical devices).

During his university career, Galileo was also actively engaged in research, and had an ambitious and original program. He researched primarily the nature of motion. He was critical of Aristotelian physics and favorably inclined toward Archimedean statics and mathematics; and he pioneered the procedure of combining empirical observation with quantitative mathematization and conceptual theorizing. Following this approach he formulated, justified, and to some extent systematized various kinematical principles: an approximation to the law of inertia; the composition of motion; the laws that in free fall the distance fallen increases as the square of the time elapsed and that the velocity acquired is directly proportional to the time; the isochronism of the pendulum; and the parabolic path of projectiles. However, he did not publish any of these results during that earlier period.

Galileo was also acquainted with Copernicus's theory of a moving earth (published in 1543) and appreciative of the fact that it advanced a novel argument. Galileo intuited that the geokinetic theory was more consistent in general with his new physics than was the geostatic theory; in particular he was attracted to Copernicanism because he felt that the earth's motion could best explain why the tides occur. But at that time he did not publish this general intuition and this particular belief. Moreover, he was acutely aware of the considerable evidence against Copernicanism: the earth's motion seemed epistemologically absurd because it contradicted direct sense experience; astronomically false because it had consequences that could not be observed (e.g., the similarity between terrestrial and heavenly bodies, Venus's phases, and annual stellar parallax); physically impossible because the available laws of motion implied that bodies on a rotating earth would, for example, follow a slanted rather than vertical path in free fall, and would be thrown off by centrifugal force; and theologically heretical because it contradicted the literal meaning and patristic interpretation of Scripture. At that time, Galileo judged that the anti-Copernican arguments far outweighed the pro-Copernican ones.

Besides formal teaching and physics research, during his Paduan years Galileo became increasingly busy with three other types of activities, mostly to round out

[5] Actually this referred to a work entitled *Questiones mechanicae*, then mis-attributed to Aristotle, and now attributed to an unknown author conventionally designated pseudo-Aristotle.

his meager university salary.[6] He gave private lessons to wealthy students, often on various aspects of mechanical and military engineering. He used his own residence as a boarding house, providing lodging and meals for students. And he ran a business manufacturing and selling a calculating device of his own invention, called the geometrical and military compass; and he taught buyers how to use it. Although these activities were distinct from his official university position, they were all indirectly connected with it: Galileo was responding creatively to students' needs and entrepreneurially using the authority of his official position.

To get some perspective on these activities, it should be noted that Galileo's only publications during his university career were a booklet of instructions on how to use the compass, printed in 1606, and a pamphlet defending the priority and originality of this invention from a plagiarist, published in 1607.[7] Moreover, a comparison of the finances underlying such activities will be even more instructive.

At Pisa, Galileo's annual salary was 60 scudi, or the equivalent of 720 Venetian lire[8]; this may be contrasted to the 500 scudi received by the philosophy professor Jacopo Mazzoni. At Padua, Galileo's initial salary was slightly higher: 180 florins, or 900 Venetian lire.[9] This was increased to 320 florins, or 1600 lire, in 1599[10]; and to 520 florins, or 2600 lire, in 1606.[11] Then in 1609, in the wake of Galileo's construction and improvement of the telescope, and of his offering it the Venetian government, he was given lifetime tenure and his salary was raised to 1000 florins, or 5000 Venetian lire; however, the raise was not supposed to become effective until the beginning of the 1610–1611 academic year.[12]

Regarding Galileo's non-university income, we can give the following selected figures. In 1603, his income from private lessons and tutoring was 3212 lire; in 1604, 2605 lire.[13] In the same year of 1604, his boarding house received payments of 7539 lire from twenty guests[14]; this sum represented gross income, and in a letter where Galileo summarized his financial situation, he stated that its profit was equal at least to his last annual Paduan salary.[15] Finally, from the manufacture and sale of

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[6] For more details on these other aspects of Galileo's life, relating mostly to practical knowledge, see Valleriani 2010.

[7] Galilei 1890–1909, 2: 363–424, 513–601; 1978; 2004.

[8] Here and below, I take the conversion rates between scudi, florins, and lire from Favaro 1883, 1: 61–62; cf. Galilei 1890–1909, 19: 124; cf. Camerota 2004, 55, 79. Favaro points out that such conversion rates were incorrectly reported by previous Galileo scholars, starting with Nelli 1793, 1: 50. Favaro also points out that a Venetian lira was worth one-half an Italian lira; in my discussion I use the Venetian lira as a common denominator.

[9] Galilei 1890–1909, 19: 111–112, 122; Favaro 1883, 1: 61–62, 2: 142; Camerota 2004, 79.

[10] Galilei 1890–1909, 19: 112–113, 123; Favaro 1883, 2: 144; Fantoli 1996, p. 150 n. 6.

[11] Galilei 1890–1909, 19: 114–115, 124; Favaro 1883, 2: 145; cf. Fantoli 1996, p. 150 n. 6.

[12] Favaro 1883, 2: 132, 295–296; Galilei 1890–1909, 19: 115–117.

[13] Galilei 1890–1909, 19: 153–156; cf. Biagioli 2006, 7.

[14] Galilei 1890–1909, 19: 161–163; cf. Biagioli 2006, 7.

[15] Galileo to Vinta, 7 May 1610, in Galilei 1890–1909, 10: 348–353, at p. 350.

compasses, his gross income was 1081 lire in a one-year period from July 1600 to August 1601.[16]

Summarizing these finances, by the academic year 1609–1610, Galileo could quadruple his university salary with roughly equal income from each of his other three extra-curricular activities: private teaching; boarding house; and compass manufacturing. It is obvious that they, plus the formal teaching, must have kept Galileo quite busy and left him little time for scholarly research. So it is not surprising that (until 1610) he did not publish any scholarly works. What is surprising is that he managed to do any research at all, and that he accomplished so much in his research.

13.3 Crucial Transition (1610)

In the summer of 1609, after hearing about the invention in Holland of an optical instrument that made distant objects appear as if they were near, Galileo was able to build such an instrument on his own. He soon gave a spectacular demonstration of the instrument to Venetian aristocrats from St. Mark's campanile. Then he presented it to the Venetian government, offering to help manufacture it. The republic showed its appreciation by doubling his university salary (to 1000 florins, or 5000 lire) and giving him lifetime tenure, as previously stated.

In the fall of the same year, Galileo improved the instrument to such an extent that it could not be duplicated by others for some time; and he started using it to observe the heavenly bodies. In the next several months, such telescopic observations yielded a number of astronomical discoveries: that the moon's surface is full of mountains and valleys; that innumerable other stars exist besides those visible with the naked eye; that the Milky Way and the nebulas are dense collections of large numbers of individual stars; and that the planet Jupiter has four moons revolving around it at different distances and with different periods. These discoveries were so startling and consequential that he decided to publish them immediately. The short book appeared in Venice in March 1610 with the title *Sidereal Messenger*.[17]

The significance of these discoveries was threefold. Methodologically, the telescope implied a revolution in astronomy insofar at it was a new instrument that enabled the gathering of a new kind of data transcending the previous reliance on naked-eye observation. Substantively, those discoveries suggested a revolution in cosmology insofar as they significantly strengthened the case in favor of the physical truth of Copernicanism by refuting many of the empirical astronomical objections and providing new supporting observational evidence. Finally, this reinforcement did not settle the issue because there was still some astronomical counter-evidence (mainly, the lack of annual stellar parallax), and because the physical objections had not yet been answered and the physics of a moving earth had not yet been articulated;

[16] Galilei 1890–1909, 19: 147–149; cf. Biagioli 2006, 7.

[17] Galilei 1890–1909, 3: 51–96; 1989, 2008, 45–84.

thus, those discoveries also suggested the need and urgency to engage in a substantial program of telescopic observation, as well as to polish and publish the results of Galileo's two decades of physical research.

Galileo dedicated the book to the grand duke of Tuscany, Cosimo II de' Medici. He also named Jupiter's satellites after this Tuscan ruling family, and so the book calls them the "Medicean stars". Both of these gestures were taken with the knowledge, advice, and consent of the grand duke and Tuscan government. The book immediately caused a sensation in university, diplomatic, and governmental circles throughout Europe; and Galileo immediately became a celebrity. As a result of his acquired stature, he tried to obtain something he had unsuccessfully sought before: a patronage position that would give him the leisure and the income to work full time doing and publishing his research.

As early as 1604, Galileo had explored the possibility of such a position with the duke of Mantua, Vincenzo Gonzaga. The prospective salary was 300 ducats, which converts to about 2800 Venetian lire, and which was thus almost double Galileo's university salary. He declined the offer, stating that he would have needed a salary of at least 500 ducats, or 4650 lire, or approximately triple his university salary.[18]

Furthermore, in February 1609, Galileo had written to a well-connected Florentine friend a long letter describing his situation in Padua and his desire for a court position with the grand duke.[19] The timing of this letter is especially significant because it was immediately after Cosimo II had succeeded his just-deceased father Ferdinando I, and in previous years Galileo had tutored Cosimo in mathematics during summer vacations. Galileo had made it clear that he wanted to be under the patronage of his "natural ruler," in a position that would give him enough "free time and tranquillity"[20] to complete three great works. He indicated that at Padua the situation would have been ideal if he could have made a living by limiting himself to teaching (and research), but he had found it necessary to get involved in several other activities to supplement his university salary. This earlier indirect attempt to secure a patron had produced no results.

But now, after the telescope, after the *Sidereal Messenger*, after discovering the "Medicean stars," the situation was very different. Thus during the Easter recess of April 1610, Galileo traveled to Tuscany to discuss his position and to give a demonstrations of the "Medicean" stars to the Medici court. These discussions and demonstrations were fruitful, for upon his return to Padua, Galileo wrote a long memorandum to the grand duke's secretary of state, recording the details of his request, and containing references to the discussions they had had in person.[21] This important letter, dated May 7, 1610 may be highlighted as follows.

Galileo begins by reporting another triumph, namely that he has just received a long letter from the Mathematician to the Holy Roman Emperor (Johannes Kepler)

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[18] Galileo to Gonzaga, 22 May 1604, in Galilei 1890–1909, 10: 106–107; I take the conversion rate between ducats and scudi from DiCanzio 1996, 79.

[19] Galileo to S. Vesp. [unknown], February 1609, in Galilei 1890–1909, 10: 231–234.

[20] *Otio et quiete*, in Galilei 1890–1909, 10: 232, line 30.

[21] Galileo to Vinta, 7 May 1610, in Galilei 1890–1909, 10: 348–353.

endorsing his telescopic discoveries; this was a letter which Kepler also published soon thereafter, under the title of *Dissertation with the Sidereal Messenger*.[22] Galileo stresses that his greatest need is to have enough "free time and comfort"[23] to continue his research and complete his works, primarily three major writings on the system of the world, on a new science of motion, and on the theory of mechanical devices. He reports that his teaching load at the University of Padua is merely "60 half hours per year"[24]; this very revealing information is part of Galileo's argument that his problem at Padua is not the burden of his university duties but all the extra-curricular activities he is involved with in order to make a living.

After the exchange of several other letters, on 10 July 1610 the grand duke signed the following letter of appointment:

> The eminence of your doctrines; the worth of your intelligence; your singular excellence in mathematics and in philosophy; the great respect, affection, deference, and readiness to serve which you have always displayed toward us; all these things have made us desirous of having you near us. And you, for your part, have always let us know that if you were to return home, you would be most pleased and honored to serve us on a permanent basis, not only as Chief Mathematician of our University of Pisa, but also as Chief Mathematician and Philosopher of our own person. Thus, having decided to have you here, we have appointed you Chief Mathematician of our above mentioned University, as well as our own personal Chief Mathematician and Philosopher. And we have ordered and we do hereby order our ministers in charge to provide you with a salary of one thousand scudi of Florentine currency for each year, with payments to begin on the first day you arrive here in Florence to serve us, and to be disbursed in semiannual installments. You have no obligation to reside in Pisa, nor to teach there, except on an honorary basis, whenever you should so wish, or whenever we should explicitly and extra-ordinarily command it for our own pleasure or for the benefit of visiting foreign princes and dignitaries. Ordinarily you will reside here in Florence, pursuing and completing your studies and your works. But you will be obliged to come to us, wherever we are, even outside Florence, should we call you. May the Lord God keep you healthy and happy.[25]

It is important to note that Galileo's official titles were to include that of philosopher. He had explicitly requested it, and there is no question that it represented one of Galileo's principal reasons for his move from Padua to Florence. In part, he was acutely aware that his (unpublished) physical research and the just-published telescopic discoveries had deep implications for natural philosophy, and that the transition from a geostatic to a geokinetic world view required a struggle that had to be fought on the logical, methodological, and epistemological plane.[26] In part, the social status of philosophers was higher than that of mathematicians, and so his

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[22] Kepler to Galileo, 19 April 1610, in Galilei 1890–1909, 10: 319–340; Kepler, "Dissertatio cum nuncio sidereo," in Galilei 1890–1909, 3: 96–126; and Kepler 1965.

[23] *Otio et comodità*, in Galilei 1890–1909, 10: 350, line 73.

[24] Galilei 1890–1909, 10: 350, line 66. This teaching load is so light that some scholars have questioned its accuracy and conjectured that Galileo made a slip of the pen and meant to say, perhaps, "per month" rather than "per year"; see Drake 1957, p. 62 n. 2. Most scholars seem to take the statement at face value; cf. Cochrane 1973, 171; Shank 2005, 65; Biagioli 2006, 11.

[25] Cosimo II to Galileo, 10 July 1610, in Galilei 1890–1909, 10: 400–401.

[26] Cf. Finocchiaro 1980, 103–166; 1997, 28–69, 335–356; 2010, pp. xv–xviii, xxxvii–xliii, 121–134.

philosophical title would contribute to the social legitimation of the new world view and facilitate his confrontations with philosophical opponents.[27] On such grounds (and others), the importance of Galileo's new philosophical title has been universally acknowledged by scholars.

On the other hand, there is another equally important feature of Galileo's new title that has seldom, if ever, been noted; that is, the fact that he was appointed mathematician (as well as philosopher) to the grand duke. This was also not an accident and also corresponded to Galileo's request. This fact emerges from the spring 1610 correspondence between Galileo and Belisario Vinta (the Tuscan secretary of state) that followed Galileo's Easter visit. On May 7, the last point in Galileo's long letter was that he would like the title of "philosopher" as well as "mathematician."[28] On June 5, Vinta wrote that the court was ready to give him the title of "Chief Mathematician of the University of Pisa" and "Philosopher to the Grand Duke."[29] On June 18, Galileo stated that although he was pleased with the first title, he would like his other title to include the designation of "Mathematician" besides that of "Philosopher" to the grand duke.[30] As we have seen, this request was indeed approved. The significance of this detail is that Galileo regarded himself, and wanted to be regarded, as someone who combined mathematics and philosophy. He was a philosopher, but not a mere philosopher, any more than he was a mere mathematician. He was a special kind of philosopher and a special kind of mathematician, a philosopher-mathematician, or a mathematician-philosopher.

There is a third significant detail in Galileo's appointment whose importance has also been usually overlooked.[31] He was also given a title he did not request, although he was pleased to hold it, namely chief mathematician at Pisa. As I will document below, the significance of this was financial, administrative, and cultural, and it generated a situation of great irony. For this title enabled the grand duke to pay Galileo's salary from funds of the budget of the University of Pisa, rather than from the Medici family treasure. The irony stems from the fact that, by law, the university budget derived from a tax on Church property in Tuscany authorized by a previous pope. The cultural significance stems from the fact that in 1629 a complaint was raised about the legitimacy of paying Galileo's salary from such funds, and a formal legal opinion was written, arguing that the arrangement was legitimate.[32]

But this brings us to another financial aspect of Galileo's new appointment, namely the size of his salary. The sum of 1000 scudi was indeed an astronomical amount. It translated to approximately 12,000 Venetian lire, which was about two and one-half times his final Paduan salary. It was roughly equivalent to the total income he had been

[27] Cf. Biagioli 1993, 1–10, 103–158; 2006, 1–44, 77–134.

[28] Galilei 1890–1909, 10: 353.

[29] Galilei 1890–1909, 10: 369.

[30] Galilei 1890–1909, 10: 373.

[31] Among the few scholars who have *not* overlooked this detail are Favaro 1883, 1: 465, and Biagioli 1993, 159.

[32] See Castelli to Galileo, 1629, in Galilei 1890–1909, 14: 62–63; and Cini 1629.

earning in Padua from all four of his sources. Comparing it to other Tuscan salaries,[33] it was about three times higher than that of highly paid artisans or engineers; one and one-half times the salary of the secretary of state; about the same as that of the so-called "chief majordomo," the highest-paid court official; and inferior only to the salaries of the commanders-in-chief of the infantry, artillery, or cavalry, which could reach as much as 2500 scudi.

The job description in Galileo's new position is even more relevant. He was simply to pursue his research and disseminate the results, free of teaching duties. This had been the main part of his request. Of course, he could be asked to give special lectures, and he was always on call for his patron, but such special assignments were not meant to interfere with his main job.

Galileo was supposed to live in Florence, but it should be noted that this did not mean at court. This constitutes a third reason why it would not be correct to describe his new position as that of "court mathematician and philosopher": not only he was not paid from court funds; not only did his day-to-day job not have to be performed at court; he also did not reside there. As Mario Biagioli has eloquently phrased it, "Galileo was an honorary university professor at Pisa and an honorary courtier in Florence."[34]

In June 1610, Galileo resigned his professorship at Padua, and in September he moved to Florence. His departure from Padua was strongly criticized by his friends and by Venetian authorities. We will see later how prophetic this criticism was, after we see what happened in the later phase of his life.

13.4 Florentine Leisure (1610–1642)

No sooner had Galileo's *Sidereal Messenger* left the printing press (March 1610) than Martin Horky published *A Very Short Excursion Against the Sidereal Messenger* (June 1610).[35] A few months later, Lodovico delle Colombe compiled an essay "Against the Earth's Motion" (1610–1611) that included theological objections; it circulated widely, but was left unpublished.[36] The following year, Francesco Sizzi published in Venice his *Dianoia astronomica, optica, physica* (1611), questioning on scriptural grounds Galileo's discovery of Jupiter's satellites.[37]

In 1612, Giulio Cesare Lagalla, professor of philosophy at the University of Rome, published in Venice a book *On the Phenomena in the Orb of the Moon*, disputing Galileo's lunar discoveries.[38] By the summer Galileo was worried enough

[33] For these figures and comparisons, see Biagioli 1993, 104.

[34] Biagioli 1993, 169.

[35] Galilei 1890–1909, 3: 127–145.

[36] Galilei 1890–1909, 3: 12, 251–290.

[37] Galilei 1890–1909, 3: 12, 201–250; cf. Gebler 1879, 39; Müller 1911, 86–87.

[38] Galilei 1890–1909, 3: 13, 309–399.

that he asked cardinal Carlo Conti for advice on whether Scripture really favors Aristotelian natural philosophy.[39] On November 2, in a private conversation in Florence, Dominican friar Niccolò Lorini attacked Galileo as a heretic for believing ideas (such as the earth's motion) that contradict Scripture; but on November 5 Lorini wrote Galileo a letter of apology.[40]

In the autumn of 1613, Ulisse Albergotti published in Viterbo a *Dialogue … in Which It Is Held …That the Moon Is Intrinsically Luminous …*, containing biblical criticism of Galileo's theories.[41] In December 1613, incited by Cosimo Boscaglia, special professor of philosophy at Pisa, the grand duchess dowager Christina of Lorraine, mother of Cosimo II, questioned Galileo's disciple Benedetto Castelli, a Benedictine friar who was the mathematics professor at Pisa; the question was whether Galileo's ideas were compatible with Scripture. Castelli gave satisfactory answers, but he also informed Galileo of the incident. So on 21 December 1613, Galileo wrote a long letter to Castelli giving a multi-faceted refutation of the scriptural objection to Copernicanism, including a discussion of the miracle in Joshua 10: 12–13, which had been advanced as an especially troublesome passage.[42]

Exactly a year after Galileo's Letter to Castelli, on 21 December 1614 at the Church of Santa Maria Novella in Florence, Dominican friar Tommaso Caccini preached a sermon against mathematicians in general and Galileo in particular, because their beliefs and practices allegedly contradicted the Bible and were thus heretical.[43] Some scholars have alleged that Caccini, besides explaining that the Joshua passage contradicts the earth's motion and thus renders belief in it heretical, also discussed the suggestive verse "Ye men of Galilee, why stand ye gazing up into heaven?" (Acts 1:10).[44]

On February 6, 1615, Christopher Scheiner sent Galileo, together with a courteous letter, a book (*Disquisitiones mathematicae de controversiis et novitatibus astronomicis*) by one of his disciples (Johannes Locher) violently attacking the proponents of the earth's motion[45]; Scheiner was a German Jesuit with whom Galileo was engaged in a dispute about the priority of discovery and the proper interpretation of sunspots. The following day, on February 7, Lorini sent the Roman Inquisition a written complaint against Galileo, enclosing his Letter to Castelli as incriminating evidence.[46] A month later, on March 20 Caccini gave a deposition to the Inquisition in Rome, charging Galileo with suspicion of heresy, based on the content of the Letter

[39] Conti to Galileo, 7 July and 18 August 1612, in Galilei 1890–1909, 11: 354–355, 376.

[40] Galilei 1890–1909, 11: 427.

[41] Galilei 1890–1909, 11: 598–599; cf. Drake 1957, 190.

[42] Galilei 1890–1909, 11: 605–606, 5: 281–288; Finocchiaro 1989, 47–54.

[43] Galilei 1890–1909, 12: 123, 19: 307.

[44] Fabroni 1773–1775, 1: 47 n. 1; for the mythological character of this claim, see Finocchiaro 2005, 115.

[45] Martin 1868, 42–43; Camerota 2004, 338–342.

[46] Galilei 1890–1909, 19: 297–298; Finocchiaro 1989, 134–135.

to Castelli and of his book on *Sunspots* (1613) and on hearsay evidence.[47] The issue was now in the hands of the Inquisition.

In December 1615, Galileo went to Rome of his own accord to defend his views. He was able to talk to many influential Church officials and was received in a friendly manner; he may be credited with having prevented the worst, insofar as the Inquisition did not issue a formal condemnation of Copernicanism as a heresy or of Galileo as a heretic. Instead two milder consequences followed. In February 1616, Galileo was given a private warning by Cardinal Robert Bellarmine (in the name of the Inquisition) forbidding him to hold or defend the truth of the earth's motion; Galileo agreed to comply. And in March, the Congregation of the Index (the cardinals in charge of book censorship) published a decree which, without mentioning Galileo, declared that the earth's motion was physically false and contradicted Scripture; that a 1615 book supporting the earth's motion as physically true and compatible with Scripture was condemned and permanently banned; and that Copernicus's 1543 book was banned until appropriately revised. Published in 1620, these revisions amounted to rewording or deleting a dozen passages suggesting that the earth's motion was or could be physically true, so as to convey the impression that it was merely a convenient hypothesis to make mathematical calculations and observational predictions.

For the next several years, Galileo kept quiet about the forbidden topic, until 1623 when Cardinal Maffeo Barberini became Pope Urban VIII. Since Barberini was an old admirer and patron, Galileo felt freer to express his own opinions and decided to write the book on the system of the world conceived earlier, adapting its form to the new restrictions. Galileo wrote the book in the form of a dialogue among three characters engaged in a critical discussion of the cosmological, astronomical, physical, and philosophical arguments, but he avoided theological arguments. This *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*[48] was published in 1632 and its key thesis is that the arguments favoring the geokinetic theory are stronger that those favoring the geostatic view, and in that sense Copernicanism is more probable than geostaticism. When so formulated, the thesis is successfully established. In the process, Galileo managed to incorporate into the discussion the new telescopic discoveries, his conclusions about the physics of moving bodies, a geokinetic explanation of the tides, and various methodological reflections. From the viewpoint of the ecclesiastic restrictions, Galileo must have felt that the book did not "hold" the theory of the earth's motion because it was not claiming that the geokinetic arguments were conclusive; that it was not "defending" the geokinetic theory because it was merely a critical examination of the arguments on both sides; and that it was an hypothetical discussion because the earth's motion was being presented as an hypothesis postulated to explain observed phenomena.

However, Galileo's enemies complained that the book did not treat the earth's motion as an hypothesis because he regarded it as a potentially true description of physical reality; and that the book defended the earth's motion because it criticized the pro-geostatic arguments; these features allegedly amounted to transgressions of

[47] Galilei 1890–1909, 19: 307–311; Finocchiaro 1989, 136–141.

[48] Galilei 1890–1909, 7: 23–520; 1953, 1997.

Bellarmine's warning and the Index's decree. And there was a third charge: that the book violated a special injunction issued personally to Galileo in 1616 prohibiting him from discussing the earth's motion in any way whatever; a document describing this special injunction had been found in the file of the earlier Inquisition proceedings. Thus Galileo was summoned to Rome to stand trial, which began, after various delays, in April 1633.

At the first hearing, Galileo was asked about the *Dialogue* and the events of 1616. He admitted receiving from Bellarmine the warning that the earth's motion could not be held or defended, but only discussed hypothetically. He denied receiving a special injunction not to discuss the topic in any way whatever, and in his defense he introduced a certificate he had obtained from Bellarmine in 1616; this certificate only mentioned the prohibition to hold or defend. Galileo also claimed that the book did not defend the earth's motion, but rather suggested that the favorable arguments were inconclusive, and so did not violate Bellarmine's warning.

The special injunction surprised Galileo as much as Bellarmine's certificate surprised the inquisitors. Thus it took three weeks before they decided on the next step. The inquisitors opted for some out-of-court plea-bargaining: they would not press the most serious charge (violation of the special injunction), but Galileo would have to plead guilty to a lesser charge (unintentional transgression of the warning not to defend Copernicanism). He requested a few days to devise a dignified way of pleading guilty to the lesser charge. Thus, at later hearings, he stated that the first deposition had prompted him to re-read his book; he was surprised to find that it gave readers the impression that the author was defending the earth's motion, even though this had not been his intention. He attributed his error to wanting to appear clever by making the weaker side look stronger. He was sorry and ready to make amends.

The trial ended on 22 June 1633 with a harsher sentence than Galileo had been led to expect. The verdict found him guilty of a category of heresy intermediate between the most and the least serious, called "vehement suspicion of heresy"; the objectionable beliefs were the cosmological thesis that the earth moves and the methodological, theological principle that the Bible is not a scientific authority. The *Dialogue* was banned. He was condemned to house arrest for the rest of his life. And he was forced to recite a humiliating "abjuration."

One of the ironic results of this condemnation was that before his death in 1642, to keep his sanity, Galileo went back to his earlier research on the theory of machines and on the physics of motion. He organized and developed his notes, wrote them up, and in 1638 published a book entitled *Two New Sciences*.[49] This contained the new science of motion and part of the mechanical theory he had promised in 1610. It was his most important book from a purely scientific point of view. Without the tragedy of the trial, he might have never done it.

[49] Galilei 1890–1909, 8: 41–318; 1974.

13.5 The Loss of Republican Liberty

Thus, in the end, Galileo did complete most of the major works which he had conceived in the earlier phase of his career and whose completion was the main reason for his move from Padua to Florence: the works on the system of the world, on the science of motion, and on the theory of machines. However, one might object that it took him a rather long time to accomplish this: twenty two years (from 1610 to 1632) to complete the system of the world; and twenty eight years to complete the science of motion and the mechanical theory. Moreover, he never completed some parts of his mechanics and other minor works.

On the other hand, he did conceive and complete other works along the way, besides the two major ones already mentioned. In 1612, he published the *Discourse on Bodies in Water*; in 1613, the *History and Demonstrations concerning Sunspots*; and in 1623, the *Assayer*, dealing with comets.[50] Moreover, he did write and circulate in the form of letters some essays criticizing the scriptural objection to Copernicanism and advocating the limitation of biblical authority in matters of astronomy, physics, and natural philosophy: the Letter to Benedetto Castelli and the Letter to the Grand Duchess Christina.[51] And although, as we have seen, the ideas advanced in these essays were grounds for his persecution by conservative clergymen and by the Inquisition, in the end (centuries later) even the Church herself accepted Galileo's principle that Scripture is not a scientific authority and appreciated his argument on its behalf.[52]

Still, one may wonder whether the move from the money-seeking industriousness of Padua to the leisure and financial security of Florence was worth it. I have already mentioned that even his friends had been skeptical at the time. So let us listen to the words of Giovanfrancesco Sagredo, a Venetian aristocrat and his best friend. Sagredo had been away from Venice during the crucial year of 1610, on a long voyage that took him to the Middle East and India. Thus they had not been able to discuss the issue in person at the time it happened. However, when Sagredo returned in the summer of 1611, he was shocked not to find his friend and after regaining his composure, he wrote Galileo a letter dated August 13, 1611 that may be highlighted as follows.[53]

Having just visited many cities of the world, Sagredo feels he is in a good position to compare them with Venice. More than ever, he feels fortunate to have been born in Venice. He thinks that Galileo's own personal situation there was no less fortunate:

> Here the liberty and the way of life for all segments of the population seem to me something that is marvelous and perhaps unique in the world … Here your salary and your other earnings were not, in my opinion, at all negligible; I believe your expenses were, of your own choice,

[50] Respectively, in Galilei 1890–1909, 4: 57–141; 5: 71–249; 6: 197–372. For English translations, see Galilei 1960; Galilei and Scheiner 2010; and Drake and O'Malley 1960.

[51] Respectively, in Galilei 1890–1909, 5: 281–288, 309–348; and in Finocchiaro 1989, 49–54, 87–118.

[52] See Finocchiaro 2005, 263–266, 338–358.

[53] Sagredo to Galileo, 13 August 1611, in Galilei 1890–1909, 11: 170–172; translated in Drake 1957, 66–68.

> very small; and your needs were certainly not such that you had anything to worry about for the future. Where will you be able to find liberty and self-determination as in Venice? Especially in light of the connections you had, which day by day became more considerable with the growth of the age and authority of your friends … Here you ruled over those who rule over others and govern them, and you had to serve only yourself, like the king of the universe.[54]

To this glowing and accurate portrayal of the situation in the Venetian republic, Sagredo contrasts Galileo's situation in the Tuscan grand duchy:

> You now serve your natural ruler, great, full of virtues, and a youth of singular promise … The virtue and magnanimity of that prince give one hope that your devotion and merits will be appreciated and rewarded. But in the tempestuous sea of the court, who can be sure not to be submerged, or at least burdened and troubled, by the fierce winds of resentment? … Who knows what can be produced by the infinite and incomprehensible vicissitudes of the world, aided by the lies of evil and envious men, who can take advantage of the fairness and virtue of a prince to ruin a good man by sowing false and slanderous ideas in the prince's mind? … Moreover, I am greatly troubled by your being in a place where, I understand, the authority of Berlinzone's friends [the Jesuits] counts a great deal.[55]

We have already seen how much Galileo's Florentine leisure and security were jeopardized by court intrigue, human envy, and clerical conservatism. So Sagredo's words must be regarded as deeply insightful and spectacularly prophetic.[56]

Actually, during his Florentine years Galileo experienced even more adversity than that on which we have already focused. There were at least two other episodes which are admittedly less important but deserve to be mentioned; they are especially significant in the present context because, unlike the Inquisition trial and the Copernican controversy, they were resolved in Galileo's favor, and so they show that even when he was able to prevail, he still had to face difficulties. The first of these episodes will be briefly mentioned, the second one will be discussed at greater length.

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[54] Galilei 1890–1909, 11: 170–172, lines 21–22, 43–49, 52–54.

[55] Galilei 1890–1909, 11:170–172, lines 51–52, 54–57, 61–65, 83–84.

[56] Here one could object that even in the Venetian republic Galileo's geokinetic research program might have gotten into trouble with the Inquisition, as suggested by the case of Giordano Bruno, who was first arrested in Venice and tried by the Venetian Inquisition, and then extradited to Rome for trial and eventual execution there. However, Bruno's and Galileo's cases are very different, because the charges against Bruno were almost all theological, and because Bruno's situation in Venice was not only that of a noncitizen but also that of a vagrant or "homeless" person; cf. Finocchiaro 2002. The proper analogy is that between Galileo and Cesare Cremonini, colleague and friend of Galileo at Padua, and philosophy professor whose Aristotelian doctrine of the *mortality* of the soul ran afoul of the Inquisition; but he was protected by the Venetian government and the Inquisition proceedings against him were ineffectual; cf. Beretta 2005. More generally, Venice had successfully proved its independence from ecclesiastical interference by expelling the Jesuits from its territory in 1606 and by resisting a papal interdict in 1606–1607; cf. Shank 2005.

Another objection might be that universities were very conservative institutions and professors were very tradition-bound, and so the revolutionary character of Galileo's geokinetic research program would have been difficult to carry out at Padua. Such a claim is often advanced as an important part of the explanation of why Galileo left Padua; cf. Drake 1957, 69–71; and Banfi 1979, 95–96. However, I would respond that this issue should not be discussed in absolute terms: Padua was relatively progressive, as compared to other universities, and the Venetian republic was relatively liberal, as compared to the Tuscan grand duchy.

The first of these episodes involved the atomistic theory of matter and the Catholic dogma of the Eucharist. Galileo's *Assayer* of 1623 contains passages that indicate his endorsement of atomistic ideas, especially regarding the distinction between primary and secondary qualities; according to this doctrine, whereas properties like motion, weight, and shape really belong to physical bodies, properties like colors, sounds, and taste are phenomena that occur only in the sensory apparatus of the perceiving subject. About a year later, a written complaint was sent to a Church official arguing that this doctrine is incompatible with the doctrine of transubstantiation, according to which during mass, after bread and wine are consecrated, their substance changes into the body and blood of Christ (although their accidents do not). After the charge was investigated and the book was read by a consultant, the complaint was dropped. A similar complaint was drafted just before the 1633 trial, but again it did not even find its way into the proceedings. These incidents have only come to light since 1983 and have generated some heated controversy and a voluminous literature.[57]

The second episode, on the other hand, has been known to a few specialists for a long time,[58] but they have not given it the attention it deserves, and so it remains widely unknown. Yet its potential scholarly fruitfulness is considerable since it sheds light on the Galileo affair and on the issue of the relationship between science and religion. And its general relevance is striking since it requires little elaboration to be applied to present-day discussions about the relationship between teaching and research in universities and about the funding of research by government and non-profit foundations. I have already alluded to the complication arising from the source of his salary above; it is now time to elaborate.

13.6 The Ecclesiastic Funding of Heretical Research

In 1629, Galileo was informed by friends in Rome that a complaint had been raised questioning the legitimacy of the salary he had been receiving since he moved to Florence in 1610.[59] The complaint was that Galileo's salary was paid from the budget of the University of Pisa, which in turn derived from a tax on Church property granted to the Tuscan government by a bull of an earlier pope; and the problem was that, as we have seen, Galileo had no obligation to teach or even reside at Pisa, although he had been given the title of chief mathematician of that university. The informants were Castelli, a former student and then mathematics professor at the University of Rome, and Michelangiolo Buonarroti, nephew of the great artist and a well-connected Florentine politician. Their brief letter also sketches some rebuttals to the complaint.

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[57] Cf. Redondi 1983, 1987, 2004, 465–485; Pagano (1984, 43–48; 2009, pp. lxxxvi–xc); Ferrone and Firpo 1986; Finocchiaro 1989, 202–204; Westfall 1989, 84–99; Cerbu 2001; and Artigas et al. 2005.

[58] For example, Favaro 1883, 1: 465; Biagioli 1993, 159.

[59] Castelli to Galileo, 1629, in Galilei 1890–1909, 14: 62–63.

However, the incident did not end there, for the Tuscan government consulted various Florentine experts for a formal legal opinion on the question. A lengthy and detailed opinion was written by Niccolò Cini, canon of the Florence cathedral, and dated November 8, 1629.[60] It was also endorsed and signed by two Jesuits, two Dominicans, two regular priests, and two lawyers. The opinion refutes the complaint and justifies the legitimacy of Galileo's salary arrangement.

The opinion begins with a statement of the complaint.[61] Then it goes on to state that the complaint is groundless and that Galileo's salary arrangement is legitimate.[62] And then it elaborates several reasons for this conclusion. We may glimpse at these reasons in the following summary, in which I quote or paraphrase the text of the opinion.

"First, it is customary for all universities to excuse from teaching those who for a long time have honorably discharged their duties but are no longer able to continue doing so due to ill health or old age."[63] But Galileo has honorably discharged his duties, first in 1589-1592 at Pisa, then for 18 years at Padua, and now since 1610 at Florence; since then, he has given private lessons to grand duke Cosimo II, to princes Francesco and Lorenzo de' Medici, to many other gentlemen of Pisa, as well as to his disciples Castelli and Niccolò Aggiunti; moreover, Galileo is presently in poor health and very old.[64] Thus, "His Highness can excuse him from actual teaching.[65]

"Secondly, in the arts and sciences, professors are hired not only for the particular benefit of the particular students attending their classes, but also for the reputation and honor of a university, which thus tries to have the most distinguished and famous professors … This honor or reputation acquired by a university from distinguished scholars is to be esteemed and prized more than the particular lessons in the classroom."[66] But everybody knows that Galileo's scholarly reputation is very high, perhaps higher than anyone else's of our time; and everybody who wants to know can learn that he is about to complete three scholarly works that are even more important than his previous ones and that will bring to the students and to the reputation of the University of Pisa incomparably greater benefits than his classroom lectures could.[67] "And to put the finishing touches on these works, he needs uninterrupted tranquillity.[68]

Additionally, it would be strange "for a university to pay someone a salary of 1000 scudi to teach a class which countless people would teach for 100 scudi; so it is clear that such a high salary is not given on account of the teaching but on account

[60] Cini 1629.

[61] Cini 1629, p. 487, lines 1–14.

[62] Cini 1629, p. 487, lines 15–20.

[63] Cini 1629, p. 487, lines 21–24.

[64] Cini 1629, pp. 487–488, lines 24–44.

[65] Cini 1629, p. 488, lines 44–45.

[66] Cini 1629, p. 488, lines 46–49; pp. 488–489, lines 66–68.

[67] Cini 1629, p. 488, lines 49–65.

[68] Cini 1629, p. 488, lines 58–59.

of the great fame, name, and reputation which universities value so much and try to acquire in every possible way."[69]

There might be a difficulty if the relevant papal bull said that the ecclesiastical tax is to be used only for teachers at the university. Instead the bull says generally that is it to be used for the *benefit* of the university. But what greater benefit is there than that which produces luster and reputation?[70]

Furthermore, the grand duke has the discretion of setting a teacher's salary and raising it. So he could raise Galileo's salary by 200 or 300 scudi, and then Galileo could, with the grand duke's approval, give this amount to a substitute teacher. However, such a formality would be necessary only for the sake of appearances, not for the merits of the case.[71]

Finally, the grand duke could reason that this arrangement was devised by his late father Cosimo II, whose prudence, piety, and religiousness are well known. So one can presume that Cosimo must have considered the matter and must have convinced himself that it was legitimate.[72]

13.7 Conclusion

During his university career (except for the final, crucial year), Galileo lived a relatively quiet, if busy, life. Besides teaching his official courses, he was increasingly occupied with tutoring students in private lessons, providing lodging and meals to student boarders, and manufacturing and selling his calculator compass. From each of these three extracurricular activities, he could earn an amount roughly equal to his university salary. He also found time to do important scientific research, but not enough time to polish and publish his findings.

Then in the frantic, crucial year of 1609–1610, Galileo improved the just invented telescope; turned it into a scientific instrument; made revolutionary astronomical discoveries; published the *Sidereal Messenger*; became a scientific and philosophical celebrity, as well as a controversial figure; gained the favor of the Tuscan ruling family of the Medici by naming Jupiter's satellites after them; was appointed Chief Mathematician of the University of Pisa and Chief Mathematician and Philosopher to the Grand Duke of Tuscany; was given a generous and unprecedented salary equivalent to his Paduan income from all sources; and was assigned the sole duty of continuing his research and finishing his works. He finally had the financial comfort and the leisure to do research and writing full time.

Alas, the result of his patronage was not as Galileo had hoped. The cultural significance of his discoveries and the visibility of his patronage position inevitably drew him into several time consuming and distracting controversies. The main one was

[69] Cini 1629, p. 489, lines 82–87.

[70] Cini 1629, p. 489, lines 90–94.

[71] Cini 1629, pp. 489–490, lines 99–107.

[72] Cini 1629, p. 490, lines 107–113.

the Copernican controversy over the twin issues of the physical reality of the earth's motion and the astronomical authority of Scripture. The resulting affair severely limited his freedom in research and writing. The earlier proceedings were occasioned by Galileo's letter to Castelli of 1613, in which he refuted the scriptural objection to Copernicanism and argued that Scripture is not an astronomical authority; as a result, in 1616 the Inquisition privately warned Galileo to stop defending the earth's motion, and the Index publicly decreed the geokinetic doctrine to be false and contrary to Scripture and Copernicus's work banned unless and until revised. The later proceedings were occasioned by Galileo's *Dialogue* of 1632, which discussed the scientific and philosophical evidence for and against the earth's motion, and showed that the geokinetic arguments were stronger than the geostatic ones; for this, Galileo was formally tried by the Inquisition, found guilty of vehement suspicion of heresy, and condemned to abjure and to indefinite house arrest, and his book was banned. Consequently, Galileo was not able to do much of the research he had outlined in 1610, and did not publish the works then promised until 1632 for the system of the world, and until 1638 for the theory of mechanics and the new science of motion.

It is ironic that when in the second part of his life Galileo finally had the leisure and the financial security which he had been seeking throughout his university career, other obstacles intervened to retard or prevent his research, writing, and accomplishments. These obstacles fell into two groups: (1) ecclesiastic and theological opposition and (2) controversies with philosophical and scientific rivals. However, such obstacles could have been predicted and should have been expected; indeed Sagredo prophesized them in his 1611 letter to Galileo, and there is evidence that Galileo himself was aware of the risk,[73] however much he wanted to believe otherwise. And in this tension between wishful thinking and realistic expectation, we have a second great irony. Finally, there is a third striking irony stemming from the fact that during his patronage appointment in the second half of his life, the Church was ultimately paying for Galileo's research, insofar as his salary derived from the tax on Church property authorized by papal bull to finance the University of Pisa; such funding of the research of an alleged suspected heretic was of course unintentional, but it was neither secret nor hidden, and it came to light with the filing and resolution of the 1629 complaint.

In short, from the point of view of the themes of leisure, adversity, and intellectual achievement problematized in the introduction, Galileo's career exhibits three challenging ironies. Before 1610, as a university professor at Padua in the employment of the Venetian republic, he had the liberty and the protection to conduct significant research, but lacked the leisure and financial comfort to bring it to completion. After 1610, as the grand duke's chief mathematician and philosopher, Galileo had the leisure and comfort for full time research and writing, but was unable to do it effectively because of the opposition of churchmen and rivals. Also after 1610, he was both under fire and under patronage from the same institution, the Church, which both paid (indirectly) his salary and created all kinds of obstacles for his geokinetic research program.

[73] In the above mentioned letter of February 1609, in Galilei 1890–1909, 10: 232–233.

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Chapter 14
From Religion vs. Science to Science vs. Religion: McMullin on the Church and Galileo

Abstract This essay is a critical examination of the volume *The Church and Galileo* (2005), edited by Ernan McMullin and containing thirteen contributions by various scholars. I provide a description of the origin and content of the volume, and a brief account of its minor blemishes. I then focus on two issues stressed by McMullin, reflected in the book's title, and discussed in his editorial introduction. One is the issue of the similarities and differences between the original trial of Galileo (which can be characterized in terms of the Church's persecution of a scientist) and the subsequent and continuing controversy about it (which can be characterized in terms of science's criticism of the Church); the other issue is the so-called rehabilitation of Galileo by Pope Saint John Paul II in 1979–1992. Although I give McMullin and other contributors credit for calling attention to these two issues and for some sound suggestions, I argue that much more remains to be done, and I undertake to sketch a satisfactory account.

14.1 The Condemnation of Galileo

In 1633, at the end of one of the most famous trials in history, the Inquisition found Galileo "vehemently suspected of heresy"[1] for holding and defending the thesis that the earth revolves around the sun and for thinking "that one may hold and defend as probable an opinion after it has been declared and defined contrary to the Holy Scripture."[2] Vehement suspicion of heresy was a technical term meaning much more than it may sound to modern ears; in fact, it was a specific category of religious crime intermediate in seriousness between formal heresy and mild suspicion of heresy. The content of Galileo's "suspected heresy" was twofold. The first was an astronomical or cosmological claim about physical reality, which Galileo had supported and defended in his *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican* (1632). The second was a methodological principle or rule about how to proceed in the search

[1] Galilei 1890–1909, 19: 405, line 119.

[2] Galilei 1890–1909, 19: 405, lines 122–123.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_14

for physical truth or the acquisition of natural knowledge; it may be rephrased as the principle that Scripture is not an authority and may be disregarded as irrelevant in astronomy and natural philosophy; Galileo had practiced this principle in this book and had justified it explicitly in privately circulated essays in 1613–1616.

A number of penalties accompanied this verdict. First, after the sentence was read to Galileo at the conclusion of the trial, he had to recite immediately an "abjuration" of the "above mentioned errors and heresies."[3] Second, the *Dialogue* was banned. Third, he was condemned to house arrest to the end of his life (1642). Finally, he had to recite the seven penitential psalms once a week for three years.

14.2 The Trial of Galileo

This condemnation was the climax of a series of events[4] that started in 1543, when Nicolaus Copernicus published an epoch-making book, *On the Revolutions of the Heavenly Spheres*, in which he advanced a new argument in favor of the idea that the earth revolves around the sun. The Copernican theory immediately came under attack for reasons stemming from astronomical observation, Aristotelian physics, traditional epistemology, and scriptural interpretation. These objections were advanced by astronomers, mathematicians, and natural philosophers, as well as theologians and churchmen, and by Protestants as well as Catholics. Thus, Copernicanism attracted few followers. Galileo, in the first twenty years of his career (1589–1609), was not one of them. His stance toward Copernicanism then was one of *indirect pursuit*, an attitude that is not only weaker than *acceptance* but also weaker than *direct pursuit*: his research focused on physics rather than astronomy; he was critical of Aristotelian physics and favorably inclined toward an Archimedean approach; he had intuited that Copernicanism was more consistent with the new physics he was developing than was the geostatic theory; but at that time, he felt that the arguments against Copernicanism were stronger than those in favor of it.

However, in 1609, by means of the newly invented telescope, he made several startling discoveries, which he published in *The Sidereal Messenger* (Venice, 1610): that the moon's surface is rough, full of mountains and valleys; that innumerable other stars exist besides those visible with the naked eye; that the Milky Way and the nebulas are dense collections of large numbers of individual stars; and that the planet Jupiter has four moons revolving around it at different distances and with different periods. Soon thereafter, he also discovered the phases of Venus and sunspots, and he published the *Sunspot Letters* (Rome, 1613). The new telescopic evidence removed most of the observational-astronomical objections against the earth's motion and added new evidence in its favor. Galileo now believed not only that the geokinetic theory had greater explanatory coherence than the geostatic theory (as Copernicus had shown) and that it was physically and mechanically more adequate (as Galileo's

[3] Galilei 1890–1909, 19: 407, line 168.

[4] For the details and the documentation of the story sketched in this section, see Finocchiaro 1989.

new physics suggested), but also that it was empirically and observationally more accurate in astronomy (as the telescope now revealed). His assessment was now that the arguments for the earth's motion were stronger than those for the earth being at rest; that Copernicanism was more likely to be true than the geostatic worldview. However, he realized that this strengthening of Copernicanism was not equivalent to settling the issue because there was still some astronomical counter-evidence (mainly, the lack of annual stellar parallax); because the mechanical objections had not yet been explicitly refuted and the physics of a moving earth had not yet been published; and because the scriptural objection had not yet been answered.

Besides realizing that the pro-Copernican arguments were still not absolutely conclusive, Galileo must have also perceived the potentially explosive character of the scriptural objection. In fact, for a number of years, he did not get involved despite the fact that his *Sidereal Messenger* book had been attacked by several authors on biblical grounds, among others. Eventually, however, he was dragged into the theological discussion. He was careful enough not to publish his criticism of the scriptural objection, but to circulate it privately, in the form of letters. The first one (1613) was addressed to his former student Benedetto Castelli, professor of mathematics at the University of Pisa, while a more elaborate version (1615) was addressed to the Grand Duchess Christina, mother of Cosimo II de' Medici, grand duke of Tuscany.

Galileo's criticism, although complex and liable to misunderstanding, was logically compelling, rhetorically persuasive, and theologically sophisticated. In this context, it should be stressed that his efforts were parallel and complementary with those of other progressive Catholic theologians and philosophers, such as Paolo Antonio Foscarini and Tommaso Campanella. Moreover, Galileo elaborated views about scriptural interpretation and its relationship to scientific and philosophical investigation that were later implicitly accepted by Pope Leo XIII in his encyclical *Providentissimus Deus* (1893) and explicitly appreciated by Pope John Paul II in his speeches on the Galileo affair (1979, 1992).

However, despite winning the intellectual argument, Galileo lost the practical struggle. In 1615, after some formal complaints were filed against him, the Inquisition launched an investigation that lasted about a year. In 1616, the Congregation of the Index issued a decree declaring that the doctrine of the earth's motion was physically false and contrary to Scripture; condemning and permanently banning Foscarini's book *Letter on the Pythagorean Opinion* (1615), which had argued that the earth's motion was probable and not contrary to Scripture; and temporarily prohibiting Copernicus's *Revolutions* until and unless it was revised. Although Galileo was not mentioned at all in the decree, he was given a warning in private. This warning exists in two versions. One is written on a certificate given to Galileo and signed by Cardinal (now Saint) Robert Bellarmine, who was an authoritative member of both the Congregations of the Index and of the Inquisition; it states that Bellarmine informed Galileo that the earth's motion could not be held or defended. The second version is in an unsigned note written by a clerk and found in the file of Inquisition trial proceedings; it states that the commissary general of the Inquisition gave Galileo the special injunction that he must not hold, defend, or discuss in any way the earth's

motion. The difference between Bellarmine's warning and the commissary's special injunction is that the latter adds a more stringent prohibition to the ones mentioned in the former: besides being prohibited, like other Catholics, to hold and defend the Copernican opinion, Galileo, in addition, was specially forbidden to discuss it in any way whatsoever.

These prohibitions of 1616 connect with the 1633 condemnation as follows. Galileo behaved as if he was bound by Bellarmine's warning, but also as if he had no knowledge of the special injunction. For the next seven years, he refrained from supporting or defending the earth's motion. The situation changed in 1623 when an admirer of Galileo, Cardinal Maffeo Barberini, became Pope Urban VIII. From several indications, Galileo came to the conclusion that if he exercised the proper care, he could publish a book on the forbidden topic. This was the *Dialogue* of 1632.The book was obviously a discussion of the earth's motion, but the discussion took the form of a critical examination of all the arguments for and against the idea; the arguments on both sides were presented, analyzed, and evaluated. He tried his best to carry out his evaluation fairly and validly. The arguments for the earth's motion turned out to be much better than those against it. This was at worst an implicit defense of Copernicanism. Galileo's hope and gamble was that friendly church officials would not blame him for this, would recognize that the defense was not explicit, and therefore would judge that he had acted within the spirit of Bellarmine's warning.

Galileo's attempt misfired not because it was foolhardy or unreasonable, but because in 1632, the special injunction came to the surface, and from its point of view, any discussion of the earth's motion by Galileo was prohibited, whether or not it amounted to a defense. The publication of the *Dialogue* thus led to the verdict and penalties previously mentioned.

14.3 The Subsequent Galileo Affair

Although the Inquisition's condemnation in 1633 ended the original Galileo affair, it gave rise to a new one that continues to this day.[5] The subsequent affair is much more complex than the original one because of the longer historical span, the broader interdisciplinary relevance, the greater international and multilinguistic involvement, and the ongoing cultural import. To begin to make sense of it, it is useful to stress that the subsequent affair has three principal aspects: the historical aftermath, the reflective commentary, and the critical issues.

The historical aftermath consists of facts and events directly stemming from the trial and condemnation of Galileo. Some of these involve actions taken by the Church, such as the partial unbanning first of Galileo's *Dialogue* and later of Copernican books in general during the papacy of Benedict XIV (1740–1748); the total repeal of

[5] For the details and the documentation of the account sketched in this section, see Finocchiaro 2005.

the condemnation of the Copernican doctrine in the period 1820–1835; the implicit theological vindication of Galileo's hermeneutics by Pope Leo XIII's encyclical *Providentissimus Deus* (1893); the beginning of the rehabilitation of Galileo himself, occasioned by the commemoration in 1942 of the tricentennial of his death; and most recently, the further rehabilitation of Galileo by Pope John Paul II (1979–1992). The historical aftermath also includes actions by various non-ecclesiastic actors, such as René Descartes's decision (in 1633) to abort the publication of his own cosmological treatise *The World*; Gottfried Leibniz's indefatigable efforts (1679–1704) to convince the Church to withdraw its condemnation of Copernicanism and Galileo; the Tuscan government's reburial of Galileo's body in a sumptuous mausoleum in the church of Santa Croce in Florence (1737); Napoleon's seizure of the Vatican file of the Galilean trial proceedings and his plan to publish its contents (between 1810 and 1814); the publication of those proceedings by lay scholars in France, Italy, and Germany between 1867 and 1878; and the attempts in the latter part of the twentieth century by various secular-minded and Left-leaning intellectuals (e.g., Bertolt Brecht, Arthur Koestler, and Paul Feyerabend) to blame Galileo for such things as the abuses of the industrial revolution, the social irresponsibility of scientists, the atomic bomb, and the rift between the two cultures.

The reflective commentary on the original trial consists of countless interpretations and evaluations advanced in the past four centuries by astronomers, physicists, theologians, churchmen, historians, philosophers, cultural critics, playwrights, novelists, and journalists. These comments have appeared sometimes in specialized scholarly publications, sometimes in private correspondence or confidential ecclesiastical documents, and sometimes in classic texts. Among the latter are Descartes's *Discourse on Method*, John Milton's *Areopagitica,* Blaise Pascal's *Provincial Letters,* Leibniz's *New Essays on Human Understanding,* Voltaire's *Age of Louis XIV,* Denis Diderot and Jean D'Alembert's French *Encyclopedia,* Auguste Comte's *Positive Philosophy,* John Henry Newman's writings, Pope Leo XIII's *Providentissimus Deus,* Brecht's *Galileo,* and Koestler's *Sleepwalkers.* Here we have a historiographical or metahistorical labyrinth in which it is easy to get lost unless some guidelines are employed. For example, it is useful to distinguish the following types of account: surface-structural versus deep-structural, circumstantial versus principled, one-dimensional versus multi- dimensional, pro-Galilean versus anti-Galilean, pro-clerical versus anti-clerical, and neutral versus evaluatively overcharged.

The critical issues of the subsequent controversy in part reflect the original issues, which involved questions such as the following: whether the earth is located at the center of the universe; whether the earth moves around its own axis daily and around the sun annually; whether and how the earth's motion can be proved, experimentally or theoretically; whether the earth's motion contradicts Scripture; whether a contradiction between terrestrial motion and a literal interpretation of Scripture would constitute a valid reason against the earth's motion; whether Scripture must always be interpreted literally; and, if not, when Scripture should be interpreted literally and when figuratively. However, the subsequent controversy has also acquired a life of its own, with debates over new issues such as whether Galileo's condemnation was right, why he was condemned, whether science and religion are incompatible, how

science and religion do or should interact, whether individual freedom and institutional authority must always clash, whether cultural myths can ever be dispelled with documented facts, whether political expediency must prevail over scientific truth, and whether scientific research must bow to social responsibility.

Although distinct, these three principal aspects of the subsequent affair are obviously interrelated. For example, much of the reflective commentary consists of attempts to formulate or resolve one or more critical issues, and such formulations often represent important developments of the historical aftermath.

14.4 Origin and Content of Present Volume

With this information in mind, we are now in a better position to comment on *The Church and Galileo*. In 2002, a conference was held on "Galileo and the Church" at the University of Notre Dame, organized by Ernan McMullin. Some of the presentations there are included in this volume,[6] together with specially commissioned essays and other pieces reprinted and adapted from various sources. Two essays deal mostly with the background to Galileo's trial, seven examine its details in depth, and four discuss some aspects of the subsequent affair. The individual papers deserve a more detailed description, and I discuss them below in the order in which they are presented in the book.

Beginning with the trial's background, Michel-Pierre Lerner gives a very useful and highly informative account of the scriptural criticisms of heliocentrism from just a few years before Copernicus's *Revolutions* to its prohibition by the Index's decree of 1616; although specialists will know this essay from the original French version published in 1999, its erudition is impressive enough to deserve further dissemination in English. Irving Kelter covers the same topic and the same period, but focuses on Jesuit authors and on their reluctance to apply the principle of accommodation, according to which biblical statements accommodate themselves to the language, beliefs, and capacity of common people at the time of their writing; this is a revised version of an article first published in 1995.

In the main group of essays, which deal with the trial itself, Michael Shank discusses its cultural and political context, stressing the situation in the three Italian regions that were directly involved—the Grand Duchy of Tuscany, the Republic of Venice, and Rome and the Papal States; one of his more interesting and important theses is that the anti-astrological bull issued by Pope Urban VIII in 1631 is likely to have played a role in the condemnation of Galileo. In the first of two contributions, McMullin examines Galileo's views on the role and interpretation of Scripture, stressing their similarity to those of St. Augustine; he attributes to them a set of mutually inconsistent principles about how to handle apparent conflicts between Scripture literally interpreted and natural philosophy, namely both the irrelevance of Scripture to astronomy and the priority of literally interpreted Scripture to contrary claims about

[6] McMullin 2005.

nature lacking demonstration; although there is no question of Galileo's reliance on Augustine and although Augustine's views may have been incoherent, to attribute such incoherence to Galileo is questionable. Annibale Fantoli considers the role of the special injunction—the much-discussed and highly controversial document dated February 26, 1616, stating that the Inquisition was forbidding Galileo not only to hold and defend Copernicanism but also to discuss it in any way: Fantoli argues that the document is authentic and not a forgery; that its content is likely to be accurate; but that its legitimacy is questionable; and that in any case, its primary role was not to convict Galileo but to exculpate Church officials for allowing the publication of the *Dialogue*. In his second essay, McMullin focuses on explaining the 1616 ban on Copernicanism without trying to justify it, but rather after explicitly admitting that it was a great error: he criticizes a large number of one-sided explanations that blame it on Aristotelian professors of philosophy (Stillman Drake), on the after-effects of the execution of Giordano Bruno in 1600 (Alexandre Koyré), on Galileo's excessive zeal and imprudence (Arthur Koestler), and so forth. He argues plausibly that the two key factors were the Catholic Counter-Reformation, which upheld patristic interpretations of Scripture, and the personal influence of Bellarmine, who was supremely influential and held an especially conservative version of biblical literalism or fundamentalism. McMullin's explanation is cogently argued, judiciously articulated, and worthy of greater dissemination, and its only blemish is the minor oversight of not giving credit to another Catholic author who first adumbrated the essential points of such an explanation.[7] In the first of two essays, Francesco Beretta discusses the documents of Galileo's trial: there exists in the Vatican Secret Archives a special file consisting mostly of the Inquisition proceedings and related correspondence; from 1867 to 1878, the file was examined and published in various critical editions by four scholars and intensely discussed by many others. Subsequently, a consensus emerged that the file is authentic but incomplete; but there are lingering questions, especially about their origin, and Beretta examines some recent hypotheses and sheds new light on the subject. An essay by Mariano Artigas, Rafael Martínez, and William Shea examines a new document, recently discovered by Artigas (among others) as a result of the Church's decision in 1998 to open to scholars the archives of the Roman Inquisition, or the Congregation for the Doctrine of the Faith, as it is now called; the document is a consultant's report arguing that Galileo accepted an atomistic theory of matter that contradicted the doctrine of the Eucharist. The document is thus similar in content to the one sensationalized in Pietro Redondi's *Galileo Heretic* (1987) and was probably written during the Inquisition's inquiries generated by the Redondi document; but the authors of this essay do not follow Redondi and instead soberly examine the issue, reaching conclusions that add useful refinements to the 1633 trial. In his second essay, Beretta discusses several loosely connected topics, but the main thread seems to be an attempt to connect Galileo's trial to the Fifth Lateran Council's decree of 1513; this council condemned the thesis of the human soul's mortality and the principle of double truth and affirmed the primacy of theology over philosophy; one connection is philosopher Cesare Cremonini, a colleague of Galileo at the

[7] Soccorsi 1947.

University of Padua who was prosecuted by the Inquisition for defending the soul's mortality, although the prosecution was largely unsuccessful because Cremonini did not cooperate and was protected by the Republic of Venice. Another connection is the Index's condemnation of Copernicanism in 1616, which Beretta argues was essentially an application of the principle of the priority of theology over philosophy; and he also tries to show that the Inquisition's condemnation of Galileo in 1633 was a consequence of the same principle. This essay is a good example of the novelty, originality, and seriousness for which Beretta has become known among experts, and it has convinced me that this connection is part of the explanation of Galileo's trial, but not that it alone is sufficient.

In the third group of papers, which focuses on the subsequent affair, Stéphane Garcia argues that after 1633 Galileo violated the oath in his abjuration and relapsed into heresy by helping some friends publish his letter to the Grand Duchess Christina; in fact, in 1636, a booklet appeared in Strasbourg with the Italian text of this letter, a Latin translation, a justificatory editorial introduction, and a revealing Latin title (rendered in English as *New and Old Doctrine of the Most Holy Fathers and Esteemed Theologians on Preventing the Reckless Use of the Testimony of the Sacred Scripture in Purely Natural Conclusions That Can Be Established by Sense Experience and Necessary Demonstrations*). Galileo's involvement in this publication is unquestionable, but whether this amounted to a "relapse" is more speculative. John Heilbron contributes an account of censorship of astronomy in Italy after Galileo that is essentially a long and substantial summary of the relevant parts of his book *The Sun in the Church* (1999), but he also adds new details reflecting new research; this paper is not only an incomparable combination of witty prose, scholarly erudition, and interpretive insight but also an admirable example of judicious balance with respect to the controversy of the interaction between the Catholic Church and scientific progress. In regard to this aspect of Heilbron's work, I am inclined to describe him as a non-Catholic who feels that the issue of the Church's role in scientific history is too important to be left to either Catholic apologists or secular anti-clericals. Michael Sharratt criticizes the 1992 report of Cardinal Paul Poupard, chairman of the Vatican Commission on Galileo; the criticism is well deserved since the report is full of historical inaccuracies and of the traditional anti-Galilean apologetics. More constructively, Sharratt intersperses his criticism with pleas for freedom of thought and quotations to that effect from Cardinal John Henry Newman; one of Newman's judgments is particularly insightful and striking, namely that at the time of Galileo, although the earth's motion had not yet been conclusively proved, the educated class deserved the freedom to discuss and teach it because a prohibition would have been and was "a real scandal in the true meaning of the word, an occasion of their falling" (quoted on p. 336). Finally, George Coyne, S.J, director of the Specola Vaticana (the papal astronomical observatory), also criticizes the Vatican Commission on Galileo and the conclusion in 1992 of the Vatican re-examination of the Galileo affair. Coyne was a member of the commission and co-chairman of its scientific and epistemological subcommittee, and so his criticism is particularly significant. Moreover, his essay is extremely informative and useful because he reconstructs the chronology and operation of the commission via his personal records; for example, although the

commission was formed in 1981 and dissolved in 1992, it held no meetings from 1983 to 1990.

14.5 Minor Blemishes

Like almost all such anthologies, this one brings together papers of uneven quality; for example, in the more substantial papers, their mere endnotes are about as long as the bulk of the shorter essays. Similarly, this collection fails to include works by many specialists whose contributions would have enriched the discussion; for example, no Italian scholars are included (although there is one Italian-Swiss author of works in French). Several important subtopics and issues are left out, such as the absence of the astronomical, mechanical, and epistemological arguments against Copernicanism. Further, more than half of the chapters are not novel to specialists: four are revised versions of articles published in 1995–2001, and three others are reworkings of relatively well-known recent publications. Finally, only two or three of the thirteen essays approach the ideal of giving a masterful or definitive account of the subtopic they treat.

However, these are minor blemishes. The more important question is the book's key aim, which is implicit in the title and further amplified in the introduction.

14.6 The Original vs. The Subsequent Affair

The order of the two nouns in the title is deliberate and is meant to make a statement. McMullin suggests this in the Introduction, after his brief account of the trial climaxing with the 1633 condemnation, and states, "the Galileo affair, however, did not end there; 'Galileo and the Church' might be over, but 'The Church and Galileo' was only just beginning" (p. 5). Although McMullin proceeds to provide a brief account of the aftermath, he does not really elaborate this cryptic but interesting remark.

Some terminological clarifications may be useful here. The term *Galileo affair* can mean the sequence of events that began in 1613 when Galileo defended Copernicanism from the scriptural objection in his letter to Castelli, climaxing in 1633 when the Inquisition condemned Galileo. However, *Galileo affair* can also mean the controversy about the facts, causes, issues, and implications of that trial, which began immediately after Galileo's condemnation and continues to the present day. The phrase can also refer jointly to both of these controversies. Finally, the two can be distinguished through the terms, *original affair* and *subsequent affair*, or by calling the former *the trial of Galileo* and the latter just *the Galileo affair* (in a narrow sense). Thus, the contrast between *Galileo and the Church* and *the Church and Galileo* is, first of all, a contrast between the original and the subsequent affairs.

Second, a logical clarification may be useful. Strictly speaking, and from the point of view of formal deductive logic, the word *and* is a binary operation (conjunction) that connects two entities or propositions (the conjuncts) in accordance with the commutative law, that is in such a way that the order of the two conjuncts does not affect the meaning or truth value of the conjunction. Obviously, McMullin knows this, and so if he suggests there is a difference when the order of *Galileo* and *the Church* is changed, there must be more beneath the surface than it literally appears from these words. What can that be?

When the original affair is described as *Galileo and the Church*, I believe that *Galileo* must refer to something like Galileo's science, astronomy, or defense of Copernicanism, and *the Church* to something like the Church's opposition to it, condemnation of it, or prohibition of it. On the other hand, when the subsequent affair is described in terms of *the Church and Galileo*, I think that *the Church* refers to something like the Church's life and history of the past four centuries, and *Galileo* to something like Galileo's trial and condemnation. The former conjunction yields something like "Galileo and the problems and difficulties he had to face due to the Church," whereas the latter conjunction yields something like "the Church and the problems and difficulties it has had to face due to its condemnation of Galileo."

Although this is a plausible interpretation of this book's title and of McMullin's intended contrast, my main claim would be that this is an essentially correct historical interpretation of one important difference between the original and the subsequent Galileo affair. But note my qualification "essentially correct." For there is more in the history of the original affair than opposition to Galileo by institutions such as the Index and the Inquisition, by conservative Dominicans such as Niccolò Lorini and Tommaso Caccini, and by officials such as Cardinal Bellarmine and Pope Urban VIII. Indeed, the opposition of Bellarmine and Urban was not total, constant, and monolithic; in various ways and on various occasions, they supported, encouraged, and favored Galileo. In addition, Galileo received support and encouragement by clergymen such as Jesuit Christopher Clavius, Benedictine Benedetto Castelli, Carmelite Paolo Antonio Foscarini, and Dominican Tommaso Campanella. In such a climate of support and opposition due to contingent circumstances that implied nothing inevitable, crucial actions were taken against Galileo in the end: Bellarmine in 1616 with the condemnation of the Copernican theory of the earth's motion as false and contrary to Scripture, and Urban in 1633 with the condemnation of Galileo for vehement suspicion of heresy. Such qualifications to the Church's opposition to Galileo represent important nuances and refinements to the history of the original affair, but should not and cannot wash away the basic antagonism and conflictual character of the original story, which remains as an essentially correct first approximation.

Similarly, nuances must to be added to the description of the subsequent affair as the story of the Church's problems in the last four centuries that resulted from Galileo's trial and condemnation. It may very well be true, as many authors have advocated,[8] that such problems and difficulties are an aspect of the past, and there is no longer any good reason for them to continue. According to one argument,

[8] Gemelli 1942, 2, 27; John Paul II 1979.

the view was developed during the Enlightenment that Galileo's trial embodied the inherent incompatibility between science and religion, and later this view became widely accepted.[9] But this view was the result of inadequate historical knowledge about the trial and of philosophical and ideological biases. For example, it overlooks the pro- and anti-Galilean split within Catholicism at the time of the original affair. It also overlooks the crucial fact that despite the opposition Galileo experienced at the practical level, at the reflective level he believed in the harmony between science and religion; his letters to Castelli and the grand duchess provide very good arguments to justify such harmony. The view also presupposes the Platonist principle that science and religion are eternal, unchanging, self-subsisting entities—which, by definition, places them at war with each other—rather than elements that function as historical dynamic entities that are sometimes at war and sometimes in harmony.

However, although this pro-harmony argument is important and consequential, it does not undermine the essential correctness of the *Church and Galileo* idea for the following reasons. First, the view of Galileo's trial as epitomizing the conflict between science and religion was not an Enlightenment invention but began after the 1633 condemnation, spurred by the international group of liberals who translated into Latin Galileo's banned *Dialogue* and the incriminating letter to the grand duchess and published them in Strasbourg.[10] Second, even if the conflictual view of Galileo's trial is incorrect and a thing of the past that should now be replaced by the harmony view, it would be naïve and wrong to deny the truth and the consequences of the historical fact that for nearly four centuries, such an incorrect view has been the most popular interpretation of the episode. Third, although the Platonist, static conception of science and religion may be inadequate, and there are periods when science and religion are in harmony, the case of Galileo may be one of those where science and religion happened to be in conflict. Fourth, Galileo's trial does exhibit such a conflict if science is interpreted in that context as Copernicanism and religion as Scripture; for although Galileo believed and argued that Copernicanism is compatible with Scripture, his opponents (Bellarmine, Urban, the Index, and the Inquisition) claimed that Copernicanism is contrary to Scripture; hence, there is an irreducible historical conflict between those such as Bellarmine who affirmed, and those such as Galileo who denied, that Copernicanism was contrary to Scripture.

Next, another qualification should be added to my claim that the contrast between *Galileo and the Church* and *the Church and Galileo* represents an essentially correct historical interpretation of one important difference between the original and the subsequent Galileo affair. That is, it should be clear that this is not the only difference and that there are other important differences. First, the conflict between science and religion is a striking feature of both the original and the subsequent Galileo affair: in the original episode, it takes the form of Copernicanism versus Scripture; in the subsequent controversy, it takes the form that Galileo's trial was widely perceived to epitomize the conflict between science and religion. The important difference involves the deep structure that underlies them. For the original affair, that deep structure is

[9] D'Alembert 1751, p. xxiv; White (1896, 1965).

[10] See Finocchiaro 2005, 72–76.

the conflict between conservation and innovation, which generated the split within the Catholic Church (as well as within Protestantism, and within astronomy and natural philosophy) with regard to Copernican astronomy and scriptural interpretation. For the subsequent controversy, the deep structure lies in the phenomenon of the rise, evolution, and fall of cultural myths, because the trial of Galileo became a great occasion for mythologizing not only (more obviously) on the part of anti-clerical and pro-Galilean elements but also on the part of pro-clerical and anti-Galilean forces.

In fact, mythmakers on both sides have been busy for four centuries. On the anti-clerical side, John Milton, recalling his 1638–39 visit to Tuscany in the *Areopagitica* (1644), noted: "there it was that I found and visited the famous Galileo grown old, a prisoner to the Inquisition, for thinking in astronomy otherwise than the Franciscan and Dominican licensers thought."[11] In the *Essay on the Customs and Spirit of Nations* (1753), Voltaire opined that

> the manner in which this great man was treated by the Inquisition toward the end of his days would bring eternal disgrace on Italy, were not this disgrace erased by the very glory of Galileo. In a decree issued in 1616, a congregation of theologians declared Copernicus's opinion, so well brought to light by the Florentine philosopher, 'not only heretical in the faith, but also absurd in philosophy'. This judgment against a truth later proved in so many ways is clear testimony of the force of prejudice. It should teach those who have nothing but power to be silent when philosophy speaks and not to interfere by deciding what is not within their jurisdiction. Then in 1633, Galileo was condemned by the same tribunal to prison and to do penance, and he was obliged to recant on his knees. In truth, his sentence was milder than that of Socrates; but it was no less disgraceful to the reason of the judges of Rome than the condemnation of Socrates was to the enlightenment of the judges of Athens.[12]

In 1841, in a book widely translated and circulated in Italian, French, and German, Guglielmo Libri concluded his account with these words:

> The persecution of Galileo was odious and cruel, more odious and more cruel than if the victim had been made to perish during torture. For by nature all human individuals have the same rights, and there are no privileges as regards physical suffering; and when tortured, Galileo did not then deserve any greater compassion than other less famous victims of the Inquisition. But they were not intent only on Galileo's body; they wanted to strike him morally; they forbade him to make discoveries. Enclosed in a circle of iron, blind, and isolated, he was left to be consumed by the anguish of a man who knows his strength but who is prevented from using it. This ill-fated vengeance, which Galileo had to endure for such a long time, had the aim of silencing him; it frightened his successors and retarded the progress of philosophy; it deprived humanity of the new truths which his sublime mind might have discovered. To restrain genius; to frighten thinkers; to hinder the progress of philosophy; that is what Galileo's persecutors tried to do. It is a stain which they will never wash away.[13]

Finally, in 1953, Albert Einstein, in his foreword to an English translation of the book (*Dialogue*) that occasioned Galileo's condemnation, expressed this judgment: "a man is here revealed who possesses the passionate will, the intelligence, and the

[11] Milton (1644, 24; 1953–1982, 2: 537–538); I have modernized the spelling.

[12] Voltaire 1877–1883, 12: 249.

[13] Libri 1841a, 46–47; Cf. Libri 1841b, 1842.

courage to stand up as the representative of rational thinking against the host of those who, relying on the ignorance of the people and the indolence of teachers in priest's and scholar's garb, maintain and defend their position of authority."[14]

On the anti-Galilean side of the mythmaking genre, there was an attempt after the condemnation to discredit Galileo's ideas by taking his abjuration at face value; a good example appears in Alexander Ross's *The New Planet no Planet, or the Earth No Wand[e]ring Star Except in the Wand[e]ring Heads of Galileans* (1646), which rebutted John Wilkins's *A Discourse Concerning a New Planet, Tending to Prove that'tis Probable Our Earth Is One of the Planets* (1640). Taking issue with Wilkins's claims about how many astronomers followed Copernicus, Ross claimed, "And yet of these five you muster up for your defense, there was one, even the chiefest, and of longest experience, to wit, Galileus, who fell off from you; being both ashamed, and sorry that he had been so long bewitched with so ridiculous an opinion."[15] In 1784, Jacques Mallet du Pan started the myth that "Galileo was persecuted not at all insofar as he was a good astronomer, but insofar as he was a bad theologian"[16]; the bad theology that Mallet misattributed to Galileo was the use of Scripture to prove astronomical propositions (the opposite of what Galileo preached and practiced); less absurd versions of this myth claim that Galileo was a bad theologian in the sense of being a nontheologian who intruded into hermeneutical controversies. In *The Martyrs of Science, or the Lives of Galileo, Tycho Brahe, and Kepler* (1841), David Brewster portrayed Galileo as a coward:

> In the ignorance and prejudices of the age—in a too literal interpretation of the language of Scripture—in a mistaken respect for the errors that had become venerable from their antiquity—and in the peculiar position which Galileo had taken among the avowed enemies of the church, we may find the elements of an apology, poor though it be, for the conduct of the Inquisition. But what excuses can we devise for the humiliating confession and abjuration of Galileo? … Galileo cowered under the fear of man, and his submission was the salvation of the church. The sword of the Inquisition descended on his prostrate neck; and though its stroke was not physical, yet it fell with a moral influence fatal to the character of its victim, and to the dignity of science.[17]

In *To Save the Appearances* (1908), Pierre Duhem tried to portray Galileo as a bad logician and epistemologist, claiming "that logic was on the side of Osiander, Bellarmine, and Urban VIII, and not on the side of Kepler and Galileo; that the former had understood the exact import of the experimental method; and that, in this regard, the latter were mistaken."[18] Finally, in *Against Method* (1988), Paul Feyerabend claimed that "the Church at the time of Galileo not only kept closer to reason as defined then and, in part, even now; it also considered the ethical and

[14] Einstein 1953, p. vii.

[15] Ross 1646, 9.

[16] Mallet du Pan 1784, 122.

[17] Brewster 1841, 93–95.

[18] Duhem 1908, 136; cf. 1969, 113.

social consequences of Galileo's views. Its indictment of Galileo was rational and only opportunism and a lack of perspective can demand a revision."[19]

I claim that whereas such two-sided mythmaking forms the deep structure underlying the science-religion conflict of the subsequent Galileo affair, the dialectic of conservation and innovation forms the deep structure under the science-religion conflict of the original affair. Further, this deep-structural difference is another essential difference between the two affairs, besides the difference between *Galileo and the Church* and *the Church and Galileo*. Third, besides the important substantive issues raised by the various views of Galileo's trial, they also deserve study through the lens of the rise, evolution, and fall of cultural myths. This brings me to McMullin's second aim that appears in his introduction.

14.7 Pope John Paul's Rehabilitation of Galileo

McMullin ends his introduction with these words:

> Against this background, Pope John Paul's call in 1979 'to go beyond the stand taken by the [Second Vatican] council' and to initiate a major collaborative study of the Galileo affair 'in loyal recognition of wrongs from what-ever side they come' sounded a welcome new note. There has admittedly been disappointment, grave disappointment indeed, in the interim. But it is in the spirit of that original invitation that this collection of essays was first conceived and is now presented. [P. 7]

The editor may be commended for the conception and presentation of this book with such a spirit of objectivity and bipartisanship. Moreover, the execution of the project goes a long way toward actually accomplishing such judiciousness, as I have already suggested in my comments on Heilbron's account of astronomical censorship in Italy after Galileo and on McMullin's own historical explanation of the admittedly erroneous ban on Copernicanism in 1616.

On the other hand, many of the other essays fall short of this ideal, especially in the coverage of the Vatican re-examination of the Galileo affair in 1979–1992. As previously noted, two essays are very critical of the operation and results of the Vatican commission on Galileo, and they substantiate McMullin's disappointment

[19] Feyerabend 1988, 129. There is a growing myth that confuses or conflates Feyerabend's view with the view of Pope Benedict XVI. Some authors have claimed simply that Pope Benedict accepts Feyerabend's view: for example, Socci 1993, 62; and Sinke Guimarães 2005, 6. Other authors have gone so far as to attribute this claim directly to the pope: Machamer 2005, 58; and Saka 2006. The truth is that in one essay, originally presented as a lecture at various venues in 1989–90, the then-Cardinal Ratzinger quoted Feyerabend's view along with similar anti-Galilean accounts, not to endorse but to criticize them, in the context of an analysis of the role of faith in the revolutionary geopolitical changes happening in 1989–90. In fact, after quoting them, Ratzinger said, "it would be foolish to construct an impulsive apologetic on the basis of such views; faith does not grow out of resentment and skepticism with respect to rationality, but only out of a fundamental affirmation and a spacious reasonableness. I mention all this only as a symptomatic case that permits us to see how deep the self-doubt of the modern age, of science and of technology goes today" (Ratzinger 1994, 98). For an account of some of the issues, see Accattoli 1990.

thesis. However, neither these essays nor others in the book present much awareness of several crucial distinctions necessary for a proper understanding and assessment of this episode. One is the distinction among the various facets of Galileo's trial—scientific, theological, philosophical, legal, pastoral, and so forth. Another is the contingent distinction between John Paul's own views and those of Cardinal Paul Poupard, the last chairman of the commission. It may be useful here to give a more nuanced account of this episode.[20]

In 1979, at a meeting of the Pontifical Academy of Sciences commemorating the centennial of Einstein's birth, Pope John Paul II gave a speech in which he talked about the Galileo affair. John Paul not only admitted errors on the part of ecclesiastic individuals and institutions but also acknowledged some wrongdoing on their part. He spoke[21] of Galileo having been caused "suffering," of his treatment as an instance of unwarranted interference into the autonomy of scientific research, and of the fact that the Second Vatican Council had "deplored" such interferences. The pope also issued a call for further studies of the Galileo affair that would be guided by three goals: bipartisan collaboration between the Galilean scientific side and the ecclesiastic religious side, open-mindedness to the wrongs of one side and the merits of the other side, and validation of the harmony between science and religion. Although the third goal was in some tension with the other two, it was the one dearest to the pope's heart. For he argued that Galileo believed that science and religion are harmonious and that Galileo conducted his scientific research in the spirit of religious service and worship; Galileo also elaborated important epistemological principles about Scriptural interpretation, which correspond to the correct ones later clarified and formulated by the Church.

John Paul was aware that his account did not solve all the problems of the Galileo affair, but felt it held the key for properly understanding the rest. The pope was keen on reversing the traditional interpretation of the trial as epitomizing the conflict between science and religion. He was reviving parts of the favorable reassessment occasioned by the tricentennial (in 1942) of Galileo's death,[22] updating it, and placing it in a more authoritative context. It is not surprising that the speech was widely reported at the time, and continued to be commonly interpreted later, as a "rehabilitation" of Galileo.

However, characterizing the pope's speech as a rehabilitation was problematic, because the speech was not a papal pronouncement *ex cathedra*, but rather was a personal opinion; it also was not a formal, official action by the same tribunal (the Inquisition) that had condemned the suspected heretic. Nonetheless, the pope's speech was an important and revealing action that might be termed an "informal" rehabilitation, if this phrase is not a contradiction in terms.

After the speech, John Paul appointed a commission to study the Galileo affair in 1981. It was headed by a cardinal and subdivided into four subcommittees: exegetical, cultural, scientific-epistemological, and historical-juridical. The memorandum of

[20] For more details and documentation, see Finocchiaro 2005, 338–357.

[21] John Paul II 1979, Sect. 6.

[22] See Finocchiaro 2005, 275–294.

appointment states that its charge was not the review of the trial, the revision of the trial, or the rehabilitation of Galileo, but rather the "rethinking" of the Galileo affair. The "rethinking" of the affair was to be free and objective, unprejudiced and open-minded; but it was also to be guided by John Paul's ideas in his speech—namely, that the Galileo affair illustrates the harmony between science and religion.

After the pope's speech and the subsequent media coverage about a retrial of Galileo and rehabilitation, such a clarification and definition of the commission's purpose were essential. But this remained unknown at the time, and thus the talk, perception, and expectation of a rehabilitation or retrial continued. Moreover, the commission's appointment repeated the Einstein-speech's equivocation between an open-minded inquiry and a validation of the harmony thesis; indeed, it heightened the tension with a subcommittee dedicated to historical and juridical issues, yet warned to avoid the issue of retrials and rehabilitations. Finally, despite the commission's high profile, its members included only two laymen and did not involve any experts in Galilean scholarship or any non-Catholics; this reflected the Church's traditional approach to such questions.[23]

During the next decade, most of the commission appointees actively studied and published on the Galileo affair, organized conferences, and sponsored and produced relevant works. The Vatican Observatory started a monograph series, and by 1992, five works had been published. The Pontifical Academy of Sciences commissioned, sponsored, and published a number of works, some of which are discussed below.

In 1983, Mario D'Addio (a member of the historical-juridical subcommittee) started publishing a series of articles, "Considerations on Galileo's Trial," which were later published as a book; its most crucial and revealing conclusion was probably its endorsement of the thesis that the special injunction transcript was legally worthless and inadmissible at the trial (although authentic and not a forgery).[24] In 1984, Coyne (a contributor to the McMullin volume) and the historian Ugo Baldini published a monograph containing two new documents: the first was the text of Cardinal Bellarmine's lectures at Louvain in 1571, which explicitly argued for the anti-Aristotelian thesis of the fluidity of the heavens on biblical grounds and thus implicitly displayed such a biblical literalism as to make him more of a conservative than previously thought; the second was a handwritten draft of Bellarmine's certificate to Galileo dated May 1616, which revealed that the cardinal's revised wording placed Galileo in a better light.[25] In 1989, the Vatican Observatory published a collection of essays on Galileo's trial by Richard S. Westfall, a distinguished non-Catholic historian of science; the most important one argued clearly and explicitly that Bellarmine was primarily a biblical literalist and traditionalist in scientific

[23] For example, on the occasion of the tricentennial commemoration of 1942, the task of writing a re-examination of Galileo had been assigned to Msgr. Pio Paschini, a respected Church historian but not a Galileo specialist.

[24] D'Addio 1983, 1984, 1985, 51–52.

[25] Baldini and Coyne 1984.

methodology and thus neither an epistemological instrumentalist nor an hypothetico-deductivist; consequently, Duhem's interpretation was a one-sided oversimplification.[26] In 1992, German priest-professor Walter Brandmüller and the German scholar Egon Greipl published the voluminous documents and an historical interpretation of the so-called Settele affair—an 1820 episode in which the Inquisition overruled the chief censor in Rome and allowed the publication of an astronomy textbook treating the earth's motion as a scientific fact. This massive volume sponsored (and copyrighted) by the Pontifical Academy, although extremely valuable for the documentation and historical information provided, is less valuable for its interpretative thesis that the Settele affair of 1820 ended the controversy.[27]

There is no question that the Vatican Commission generated a considerable body of work in Galilean studies and that many of these works contained valuable and useful contributions. However, one may question whether such work as a whole amounted to a rethinking of the Galileo affair, let alone a retrial and rehabilitation. By and large, such work amounted to a reaffirmation, repetition, and reinforcement of the thesis that science and religion can be in harmony, and Galileo's work and even Galileo's trial can help us see such harmony.[28]

Pope John Paul II heard the Vatican Commission's report on the Galileo affair at an October 1992 meeting of the Pontifical Academy of Sciences. Cardinal Poupard,[29] president of the Pontifical Council for Culture, presented the report, which included coverage of the original controversy, Bellarmine's role, and the subsequent affair.

According to Poupard, Bellarmine deserves credit for appreciating the importance of asking whether Copernicanism was demonstrably true and whether it was compatible with scriptural statements. Bellarmine realized that Galileo had not provided a conclusive proof of the earth's motion; by contrast, Galileo allegedly did not realize that he lacked such proofs and in particular that the argument from tides which he regarded as conclusive was not so. By portraying Bellarmine as a shrewd methodologist in this manner, Poupard was relying on and following the apologetic tradition of Duhem (noted earlier) and recently updated by Brandmüller.[30] However, in so doing, Poupard was ignoring and indeed contradicting two other relevant studies stemming from the commission—the work by Baldini and Coyne on Bellarmine's Louvain lectures of 1571 and one of Westfall's *Essays on Galileo's Trial*. Whether one praises Poupard for relying on Brandmüller or blames him for discarding Westfall's interpretation and the Baldini-Coyne documents, it is perhaps more important to note that Poupard was in the eternal predicament of non-experts who rely on specialists: often the specialists disagree, and then the non-expert can pick and choose among them to justify the desired conclusion.

In contrast to Bellarmine, Poupard portrays Galileo as incompetent in his theory and practice of demonstration; here again, Poupard repeats the positions of Duhem

[26] Westfall 1989.

[27] Brandmüller and Greipl 1992.

[28] See, for example, Coyne et al. 1985.

[29] Poupard 1992.

[30] Brandmüller (1982, 1987, 1992); cf. Finocchiaro 2005, 348–350.

and Brandmüller. Among other aspects, Poupard ignored the point that Galileo's lack of conclusive proof was true but irrelevant, since such lack could not (and should not) have motivated a condemnation or prohibition. He also ignored the point that if heliocentrism lacked demonstration, so did geocentrism, and hence the question was reduced to which side had the better and stronger arguments. But with respect to Galileo's view of his demonstrations, the situation is different from Bellarmine's view of the logical situation, because among the studies produced by the Vatican commission, none followed the interpretive line I have discussed here. However, the tradition exists, with roots in the works of Adrien Auzout (1665), and includes works by Pio Paschini (1943) and Filippo Soccorsi (1947).[31] The failure to assign, sponsor, or encourage any study along these lines could be taken as a sign that the commission had not been as objective and bipartisan in its investigations as it was envisioned.[32]

Regarding the subsequent affair, Poupard gave an account claiming that the 1633 condemnation of Galileo had been "reformed" several times: in 1741–1744, with the Church's imprimatur for the publication of the *Dialogue*; in 1757–1758, with the abolition of the general prohibition of Copernican books; in 1820–1822, with the explicit permission of books advocating the earth's motion as a thesis; and in 1835, with the removal from the *Index* of Copernicus's and Galileo's books. Poupard's account was largely an abstract of Brandmüller's interpretation, augmented by imprecisely reported dates and carelessly described events. Poupard was also gratuitously extrapolating from considerations that affected the 1616 condemnation of the Copernican doctrine to those that affected the 1633 condemnation of Galileo.

In general, Poupard's report was an uncritical appropriation of Brandmüller's work, both his historical interpretation of the aftermath of the trial and his epistemological account stemming from Duhem. Thus, Poupard's report (together with the commission's actions and inactions in general) was perhaps an attempt to reaffirm the conviction of Galileo and undo the rehabilitation attempt of Pope John Paul II.

After Poupard presented the report, the pope delivered his own speech. As in the case of his 1979 speech, although the Galileo affair was the main topic of a plenary session of the academy, it was not the only one. This time, the other more current and routine topic was the nature of complexity as studied in mathematics, physics, chemistry, and biology. The pope, however, shrewdly connected the two topics, essentially stating that although the facts of complexity lead to the fragmentation of knowledge and to the philosophical problem of keeping such fragmentation under control, they also lead us to appreciate the need and importance of methodological pluralism—that is, the idea "that the different branches of knowledge call for different methods."[33] But for John Paul, a key lesson from the Galileo affair is precisely methodological pluralism—what Galileo advocated with his principle that "the intention of the Holy Spirit is to teach us how one goes to heaven and not how heaven goes."[34] In contrast,

[31] Auzout 1665; Paschini (1943, 1965); Soccorsi 1947. Cf. Finocchiaro 2005, 93–99, 280–294.

[32] Admittedly, such failure could also (or instead) be the sign of lack of proper bureaucratic organization or administration.

[33] John Paul II 1992, Sect. 2.

[34] Galilei 1890–1909, 5: 319; Finocchiaro 1989, 96.

his theological opponents were committed to a misplaced cultural unitarianism that led them to fail to distinguish scriptural interpretation from scientific investigation and so to illegitimately transpose one domain into the other.

In the central part of his speech dealing explicitly with Galileo, the pope expressed the proper thanks and appreciation to Poupard, but did not simply endorse his report. John Paul was expressing gratitude to the commission members and the experts who had participated in its projects. He explicitly mentioned the commission's publications and its conclusions in general, but not Poupard's report as such. To be sure, later in his speech, the pontiff mentioned and endorsed some specific theses from the report, but he was not just rubber-stamping the whole report. Thus, the pope was acknowledging that the commission had finished its work, but he was drawing his own conclusions.

John Paul went on to reiterate the theme of the science-religion harmony that had appeared in his 1979 speech; it had been studied by the commission, but had hardly been mentioned in Poupard's report. In addition, the pope reflected that "one day we shall find ourselves in a similar situation,"[35] so the lessons of the Galileo affair may be useful, relevant, and applicable in the future; the pope also mentioned the worrisome future area of biology and genetics. Another papal reflection was the memorable judgment that "Galileo, a sincere believer, showed himself to be more perceptive in this regard than the theologians who opposed him."[36] Moreover, John Paul tried to generalize the lesson from this aspect of the episode in a way different from methodological pluralism; this second, more general lesson involved the epistemology of interdisciplinary interaction, namely that "the birth of a new way of approaching the study of natural phenomena demands a clarification on the part of all disciplines of knowledge."[37]

The most novel part of the speech was perhaps the discussion of the pastoral dimension of the affair. On this question, Catholic as well as non-Catholic authors have usually argued that, although Galileo may have been right in astronomy and biblical hermeneutics, he was definitely wrong from the pastoral point of view; this requires that the mass of believers not be scandalized or misled by new discoveries, and so the dissemination of truth (if not its pursuit) must be careful not to upset popular beliefs too suddenly and must be mindful of the social and practical consequences of truth. Instead of siding with Galileo's opponents, John Paul's solution to the pastoral issue was to declare "that the pastor ought to show a genuine boldness, avoiding the double trap of a hesitant attitude and of hasty judgment, both of which can cause considerable harm."[38] He was not reversing the traditional anti-Galilean solution, but rather he was denying it, and pointing out that the correct pastoral position is one of arriving at a judicious mean between the two extremes of too much conservation and too much innovation. Thus, although he was not really siding with Galileo on

[35] John Paul II 1992, Sect. 4.

[36] John Paul II 1992, Sect. 5, paragraph 4.

[37] John Paul II 1992, Sect. 6, paragraph 1.

[38] John Paul II 1992, Sect. 7, paragraph 2.

the pastoral issue, his rejection of the opposite side was contextually a pro-Galilean position.

The pope went on to accept some of Poupard's specific conclusions. One was the thesis about the unity of culture in Galileo's age, together with the explanation that "this unitary character of culture, which in itself is positive and desirable even in our own day, was one of the reasons for Galileo's condemnation."[39] John Paul also seemed to endorse Poupard's reference to Bellarmine, but the pope traced Bellarmine's key point to St. Augustine, and so the endorsement was partial and apparently diluted. Similar remarks apply to the Poupard-Brandmüller thesis that the 1633 sentence was "reformed" in subsequent history, and that "the debate … was closed in 1820."[40] This could be taken as an instance of uncritical acceptance by the pope of an untenable and misleading thesis. But he was so cursory about it that the impression is that he mostly used it to add further support to his own historical cultural thesis: that the Enlightenment fabricated the myth that Galileo's trial illustrates the conflict between scientific progress and the Catholic Church, but that this conflict is a thing of the past.

Thus, in this speech, the pope was acknowledging the completion of the commission's work, as reported by Poupard, and reiterating his own earlier view that a key lesson of the Galileo affair is the harmony between science and religion. He was clearly and explicitly praising Galileo's biblical hermeneutics, thus finalizing what might be called the theological rehabilitation of Galileo. John Paul was placing such a theological rehabilitation in the context of a broader philosophical appreciation, one along the lines of the epistemology of interdisciplinary relations, the other in line with methodological pluralism. In addition, he was giving an unprecedented pastoral interpretation of the affair, which, while not implying that Galileo was right along the pastoral dimension, did suggest that he was no more wrong than his ecclesiastic opponents.

John Paul did not explicitly endorse Poupard's report. Although he accepted some particular conclusions, in the context of the papal speech, those theses lost the anti-Galilean flavor and implications they possessed in Poupard's speech. If this interpretation of John Paul's speech is correct, and if it is correct to say that under the influence of people such as Poupard and Brandmüller, the Vatican commission studies had been acquiring an increasingly anti-Galilean tone and apologetic flavor, then perhaps it can be conjectured that the pope was closing the Galileo re-examination because he wanted to close the reconviction of Galileo at the hands of such people.

On the one hand, I sympathize with the disappointment so well exemplified by several authors in McMullin's book about the latest episode in the Galileo affair—Pope John Paul's "rehabilitation" of Galileo from 1979 to 1992; further, I believe much more remains to be done. On the other hand, I would say that a rehabilitation did occur, but that it was informal, partial, incomplete, not unopposed, and not unprecedented; indeed, it was the sort of rehabilitation to be expected, given the

[39] John Paul II 1992, Sect. 9, paragraph 1.

[40] John Paul II 1992, Sect. 9, paragraph 3.

previous four centuries of Galileo affair and the re-assessment occasioned by the tricentennial in 1942.

14.8 Conclusion

McMullin and the other contributors to this volume should not be blamed for the absence of the critical interpretation I have just sketched of Pope John Paul II's "rehabilitation" of Galileo. Indeed, a book reviewer should not blame the authors under review for not writing the books he has written. Suffice it to say that several aspects of this volume's content do point in the direction I have discussed. Similarly, if I made much of McMullin's contrast between *Galileo and the Church* and *the Church and Galileo*, my main purpose was not to find fault but to draw out a line of investigation that deserves to be pursued. The fact that McMullin's volume has elicited the re-exposition of such a critical interpretation and the re-articulation of such an ongoing project may be added to the list of its other merits.

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Chapter 15
Legal Formalities and Improprieties: Mayer on the Inquisition Trying Galileo

Abstract This essay is a critical examination of Thomas Mayer's *The Roman Inquisition: Trying Galileo* (2015). I argue that Mayer's book does have a few small merits: it displays considerable diligence and hard work in archival research; its legal orientation is potentially fruitful; its prosopographical approach provides some useful information; and its central thesis shows ingenuity and is challengingly provocative. However, I also argue that Mayer's book has many major flaws: its account of the 1616 Inquisition orders to Galileo is one-sided, insofar as it focuses arbitrarily on the order by the Inquisition's commissary, to the exclusion of those by cardinal-inquisitor Bellarmine and by Pope Paul V; Mayer's account is too formalistic, insofar as it stresses the form of the commissary's order and neglects the content; it lacks conceptual clarity and precision, insofar as it ignores the important differences in the contents of these orders; it commits a fallacy of equivocation, insofar as it fails to properly distinguish among the documentary authenticity, the historical accuracy, and the legal validity of the commissary's order; and it is hyper-legalistic, insofar as it perverts the concept of law, which Mayer pretends to articulate.

15.1 Introduction

This is a deeply flawed book, although it is not completely without merit. Mayer, who died (in January 2014) as this book went to press, may have been an accomplished scholar of ecclesiastical history,[1] but was a relative novice in Galilean scholarship. To explain and justify this judgment, I shall first highlight Galileo's trial, then summarize Mayer's book, and finally elaborate my criticism.

[1] The book under review here is Mayer 2015, references to which will be given in the course of my exposition simply by indicating the page numbers in parenthesis; his other related books are Mayer (2012, 2013, 2014).

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_15

15.2 The Trial of Galileo: Highlights and Approaches

The trial of Galileo involved two sets of proceedings by the Roman Inquisition.[2] They began in 1615 when a Dominican friar filed a written complaint and another testified in person against Galileo. These friars advanced two principal charges, among many: Galileo advocated the Copernican doctrine of the earth's motion, which they thought was religiously dangerous and probably heretical; and he defended this doctrine from the objection that it was contrary to Scripture, by means of arguments that involved novel exegeses of biblical passages and unorthodox hermeneutical principles. Indeed, the first friar attached to his complaint a long letter by Galileo to his former student Benedetto Castelli, in which Galileo defended the earth's motion from the scriptural objection.

The proceedings were concluded in June 1633, with a sentence finding Galileo guilty of a religious crime technically labeled "vehement suspicion of heresy"; this was an intermediate transgression, short of the most serious crime, labeled "formal heresy". According to the sentence, Galileo had committed the crime by writing and publishing (in 1632) a book entitled *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*; in it, he advocated the Copernican doctrine of the earth's motion and implicitly denied the astronomical authority of Scripture.

What connects the two sets of proceedings is an order which Galileo received from the Inquisition in February 1616. In fact, in those earlier proceedings he was not prosecuted or even interrogated, for several reasons: his letter to Castelli was judged in its essence to reflect traditional Catholic doctrine; Galileo could not be found to have advocated the earth's motion explicitly and publicly; and other more serious and theologically pertinent charges could not be confirmed. Instead, the Inquisition decided to privately order Galileo to abandon Copernican ideas, and he promised to comply. Another ecclesiastic decision was that in March 1616 the Congregation of the Index issued a decree, declaring the doctrine of the earth's motion contrary to Scripture, and temporarily banning Copernicus's book *On the Revolutions of the Heavenly Spheres* (1543); but Galileo was not mentioned at all.

Now, for 16 years Galileo kept his promise, more or less, at least to the extent that no difficulties arose. However, the publication of the *Dialogue* re-opened the proceedings. The reason why he published this book stems from the fact that in 1623 a cardinal named Maffeo Barberini was elected pope Urban VIII. Barberini was an admirer of Galileo, and had reservations about the 1616 decisions. In 1624, Galileo went to Rome to pay homage to Urban, and was able to have at least six audiences. From these discussions, Galileo got the impression that if he was careful and did not appear defiant, he could publish on the topic.

Thus, Galileo decided to write a book that was a critical examination of all the evidence for and against the idea. The arguments on both sides were presented, analyzed, and evaluated. He tried his best to carry out the evaluation fairly. The arguments for the earth's motion turned out to be stronger than those against it, although admittedly not conclusive. This was at worst an implicit advocacy of the

[2] See Finocchiaro (1989, 2014).

probability of Copernicanism. Galileo's gamble was that friendly Church officials would not blame him for this, because they would recognize that his advocacy was not explicit or absolute, and so was within the spirit of the Inquisition's 1616 order.

Galileo's effort got him into trouble because of some issues pertaining to the precise wording and content of that Inquisition order. In 1632, a document dated 1616 emerged to the effect that the Inquisition's commissary Michelangelo Seghizzi had ordered Galileo not to support, defend, or teach Copernicanism in any manner whatever. Galileo had been under the impression that the order was the one which had been conveyed to him by cardinal-inquisitor Robert Bellarmine; it stated that Galileo was not supposed to support or defend Copernicanism, but said nothing more than this. Seghizzi's order was obviously more stringent than Bellarmine's: a critical examination of pro- and anti-Copernican arguments is clearly a manner of teaching Copernicanism (and so a violation of Seghizzi's order), whereas it might not be regarded as an instance of supporting or defending Copernicanism (and so not a violation of Bellarmine's order).

Galileo's trial can be studied from several points of view: the history of physics and astronomy, since the earth's motion was a key issue of the Copernican Revolution; methodology and the philosophy of science, since the denial of scientific authority to Scripture was an epoch-making principle about proper procedure; and general cultural history, since the episode embodies lessons about the relationship between science and religion (apparently implying their incompatibility).

15.3 Mayer's Approach and Account

However, Mayer's book approaches Galileo's trial from the point of view of law, specifically ecclesiastic or canon law. For the episode was also a legal event. Mayer finds the relevant law to some extent in formal ecclesiastic decrees establishing the tribunal of the Inquisition and governing its procedures, but to an even greater extent in two other sources: inquisitorial practice as found in archival trial records and minutes of Inquisition meetings; and treatises by jurists and theologians attempting to clarify and systematize rules and procedures, and manuals by experienced inquisitors reflecting on their experience with the aim of improving practice and providing useful information to less experienced inquisitors.

Another aspect of Mayer's method is the so-called prosopographical approach. He himself describes it (p. 2) as an emphasis on the biographies of the historical agents, aiming to ascertain their motives and thus help us understand the complexity of events.

Using these two approaches, Mayer elaborates a substantive account of Galileo's trial that may be summarized as follows. Mayer uses as a blueprint the ideal type of an Inquisition trial, as one can reconstruct it from inquisitorial practice, scholarly jurisprudence, and ecclesiastical decrees. He is clear (p. 214) that such a "typical" trial is a theoretical construct not likely to be fully instantiated by any actual trial,

so that we also need to ascertain the actual proceedings of the real trial, and then compare and contrast the two.

Mayer distinguishes no fewer that eleven steps in a typical trial: (1) denunciation, by plaintiffs or witnesses; (2) preliminary investigation by officials; (3) citation of the accused or defendant; (4) further interrogation of witnesses; (5) formulation of charges; (6) "*repetitio*", meaning re-examination of witnesses to clarify discrepancies or follow up newly-emerged leads; (7) defense by the accused or his lawyers, after they have been given copies of all testimony; (8a) consultants' reports, providing expert (legal or theological) opinions, and (8b) "*summarium*", meaning that the chief legal officer (called "assessor") compiles a summary of the evidence, including important documents as attachments; (9) deliberation and verdict (including penalties) by the judges (in Rome, the cardinal-inquisitors presided by the pope), with the whole being written up in a "sentence"; (10) "abjuration" by the defendant; and (11) "publication" of the sentence, which usually meant merely reading of the sentence to the defendant at a private meeting with Inquisition officials, or at a public meeting in a church.

With some exceptions, Galileo's trial fitted this scheme, according to Mayer. One exception is the complete absence of the *repetitio* step (no. 6). Another is the incomplete adherence to the defense step (no. 7), since Galileo never received written copies of the evidence. A third deviation is that the degree of publicity of the sentence (no. 11) was unprecedented, for in Galileo's case copies were sent to all provincial inquisitors and papal nuncios in Europe, with orders to read it to all professors of philosophy and mathematics. Finally, during the 1633 proceedings, "Galileo's trial went most seriously off the rails with Commissary Vincenzo Maculano's extrajudicial move, apparently designed to secure Galileo's admission of guilt" (p. 216).

Now, with regard to these irregularities, Mayer is willing to call them "legal improprieties" (p. 217). And he is even willing to blame them on the officials, on the cardinal-inquisitors, and most of all on pope Urban. In particular "Urban's penchant for increasingly autocratic behavior, including his housecleaning of the papal administration beginning in mid-1632, and blithe disregard for the law … should not be downplayed" (pp. 217–218). However, Mayer is keen on pointing out that he does *not* include among the irregularities and improprieties one item that has been widely discussed and commonly alleged to constitute the main and most serious impropriety in the trial: commissary Seghizzi's order, which emerged in 1632, which is found in a document dated 1616, and which Galileo (in the first deposition dated April 12, 1633) denied receiving. In fact, the main thesis in Mayer's book is that Seghizzi's order was really administered; that the 1616 document where it is recorded truthfully reflects was happened; that it was legitimate; and that it ties together the earlier and later proceedings. This issue deserves further elaboration.

At a meeting of the Inquisition on 25 February 1616, Pope Paul V decided the following. Cardinal-Inquisitor Bellarmine should summon Galileo and give him the friendly warning to abandon the Copernican doctrine. If Galileo refused, then commissary Seghizzi should give him the formal injunction to abstain completely from teaching, defending, or discussing the doctrine. If Galileo did not acquiesce at Seghizzi's injunction, he should be arrested and prosecuted.

At the following week's Inquisition meeting (3 March), Bellarmine reported that Galileo acquiesced when he was given the warning to abandon the Copernican doctrine.

However, in the file of Galilean trial proceedings, there is a document dated 26 February 1616, stating the following: Bellarmine summoned Galileo to his residence and warned him to abandon the Copernican doctrine; immediately thereafter, commissary Seghizzi ordered Galileo to completely abandon the Copernican doctrine and to abstain from supporting, defending, or teaching it in any manner whatever; Galileo acquiesced and promised to obey.

Finally, there is another crucial document, a certificate written by Bellarmine for Galileo and dated May 26, 1616. Its origin lies in the fact that rumors began circulating to the effect that Galileo had been tried, convicted, forced to abjure, and given penalties; Galileo became increasingly disturbed by these rumors, and they may have also led him to wonder about the precise content of the Inquisition's orders to him at Bellarmine's residence. Thus, he asked Bellarmine to write a brief and clear statement of what happened and what the orders meant. Bellarmine's certificate states that there was no truth to those rumors, but rather Galileo was only informed that Copernicanism was contrary to Scripture and thus could not be defended or supported.

Ever since these documents became accessible in the 1860s, scholars have discussed the issue that the Seghizzi injunction document seems to conflict with the other three: Pope Paul's instructions, Bellarmine's report to the Inquisition, and Bellarmine's certificate to Galileo. From this conflict and other evidence, various conclusions have been advanced, which can be listed in the following sequence of decreasing strength or extremism: the Seghizzi injunction document is a forgery perpetrated in 1632; a forgery perpetrated in 1616; materially authentic but historically inaccurate; authentic and accurate but legally invalid. Some of these stronger claims, or some versions of them, were conclusively refuted already in the 1870s, but other weaker versions are well-founded and may very well be correct.

Mayer rejects all such claims undermining the legitimacy of the Seghizzi injunction. Instead he argues that the document is materially authentic, historically accurate, and legally valid. His argument is based on a lengthy (pp. 80–120) analysis of the kinds of orders issued by the Inquisition, according to the legal practice, the jurisprudence, and the ecclesiastic decrees. This analysis tries to show that there was no clear distinction between the notion of a "warning" and the notion of a "precept". The critics presuppose this distinction when they argue that Pope Paul had ordered Bellarmine to give Galileo a warning, and Seghizzi to give him a precept if he refused the warning; and that Seghizzi deviated from the papal order when he administered his precept "immediately after" Bellarmine's warning, and thus without giving Galileo the opportunity to accept the warning.

And here we come to another part of Mayer's argument, pertaining to the meaning of two Latin phrases. One is used in the February 26 document to connect Bellarmine's action with Seghizzi's. The phrase is "*successive ac incontinenti*", which is usually taken to mean "immediately thereafter". In an erudite semantic

analysis (pp. 67–71), Mayer argues that "the translation of *successive ac incontinenti* as 'immediately thereafter' is open to doubt" (p. 71). He does not explicitly tell us what it means, other than to implicitly suggest that it just means "thereafter".

The other phrase occurs in the Inquisition minutes of February 25 that contain the pope's instructions for Bellarmine's initial action and Seghizzi's possible intervention. The instructions stipulate that Seghizzi's intervention is contingent on "if he [Galileo] refuses to obey", which is the literal meaning of the Latin "*si recusaverit parere*". Mayer interprets this phrase in the weak sense meaning "if he is reluctant" (p. 261, n. 149), namely, if "Galileo objected is some way, perhaps in so mild a fashion as looking grumpy" (pp. 73–74). This facilitates Mayer's justification, validation, and legitimization of Seghizzi's "precept".

15.4 Criticism

In my judgment, Mayer's argument supporting his key precept thesis is unconvincing. His semantic analyses strike me as arbitrary. His conflation of Bellarmine's warning and Seghizzi's precept seems to be a legalistic exercise that fails to take into account the different content of the two orders: not to advocate—presumably as true or as compatible with Scripture; and not to advocate in any way. Mayer also fails to appreciate the conceptual difference between Pope Paul's conditional order (not to discuss) and Seghizzi's order (not to advocate in any way, which would allow discussion aimed to refute). And Mayer tends to conflate and thus to equivocate among the material authenticity, the historical accuracy, and the legal validity of an order.

Moreover, Mayer fails to appreciate that Bellarmine's report to the Inquisition and certificate to Galileo contradict the historical accuracy and legal validity of Seghizzi's order. In this regard, he has a tendency to dismiss these conflicting documents arbitrarily. For example, he generally holds what may be labeled the Inquisition sloppiness thesis: "Ideally, the Inquisition under Urban and his brother Antonio's guidance would have done its work more carefully … That it did not should cause no surprise. In sloppiness, creative record-keeping, and inventive jurisprudence the Inquisition treated Galileo no differently than most of the rest of those who underwent trial before it" (p. 5; cf. pp. 54–55). However, Mayer uses this sloppiness in a prejudicial manner, to apply it primarily to documents he wants to reject, and not to those he wants to accept.

This flaw with Mayer's central substantive thesis is merely the tip of an iceberg of difficulties. Many others are methodological or more general.

Consider Mayer's legal approach. Generally speaking, it is quite proper since Galileo's trial is, among other things, a legal event. The legal approach is also valuable because it has been unduly neglected. However, Mayer betrays a constant misappreciation of previous scholarly contributions to the legal aspects of the episode.

Three examples deserve mention.[3] Mayer has a half-page critical discussion (p. 118) of Léon Garzend's 1912 distinction between a formal theological concept of heresy and a disciplinary Inquisitorial concept; but he completely misses its explanatory potential, especially to help solve the problem of what Mayer himself (pp. 3–5, 219–221) sees as the two main alternative interpretations of the trial, the precept theory and the heresy theory. And regarding another important legal contribution, by Orio Giacchi in 1942, Mayer mostly ignores his many insights; instead, he endorses one of Giacchi's theses which is not only untenable but also perverse: that Galileo should have been subjected to actual torture, and the fact that he did not undergo such treatment was "the only serious irregularity" (p. 205) in the trial. A third more recent scholar also receives shoddy treatment: Mayer frequently cites the works of Francesco Beretta and accepts or adapts many of his conclusions, but also criticizes him for at least two theses which he does *not* hold; one is a crass version of the precept forgery thesis (pp. 58–59), and the other claims (pp. 154–155) that the Inquisition's assessor authored both parts (instead of only the first part) of a report on the *Dialogue* compiled in the summer of 1632; here we have Mayer twice committing the fallacy of straw-man criticism.

Additionally, Mayer's legal approach is too one-sided: he displays an excessive neglect of the intellectual aspects of the episode.[4] For example, the book contains no explicit discussion of the content and structure of the *Dialogue*, which was the crucial work that triggered the 1632–1633 proceedings. And concerning Galileo's letter to Castelli, which occasioned the 1615–1616 proceedings, Mayer does have a few pages of discussion (pp. 22–25), but it never rises above the level of free-association commentary and pseudo-rhetorical analysis.

Thirdly, in his legal approach, Mayer seems to operate with a peculiar conception of law, which I would describe as hyper-legalistic, hyper-formalistic, and self-defeatingly meticulous. That is, some crucial parts of his account of the legal proceedings are such that the legal officials involved did not know what they were doing; and such an account strikes me as a *reductio ad absurdum* of his notion of what it is for a proceeding to be "legal". Consider, for example, the 1616 Inquisition's orders to Galileo: it was not just Galileo who was confused; according to Mayer's own account, the confusion was also in the mind of the chairman of the Inquisition (pope Paul V) and of the most authoritative cardinal-inquisitor (cardinal Bellarmine); in Mayer's own words, "by seeing that Galileo got both a warning and a precept, Paul V took no chances that he could wiggle out of papal clutches. Roberto Bellarmino gladly helped, acting on behalf of both pope and Inquisition in summoning Galileo. Paul and Bellarmino could be excused if they were somewhat confused about exactly what to do. The lawyers had not achieved much more clarity" (p. 84). Or consider the 1632 Inquisition's order to Galileo summoning him to Rome: at the Inquisition meeting of 23 September 1632, pope Urban decreed to have Galileo summoned to Rome; two days later, cardinal-inquisitor Antonio Barberini, the Inquisition's secretary and pope's brother, transmitted the order to Florence's provincial inquisitor; on

[3] Garzend 1912; Giacchi 1942; Beretta 1999.

[4] On such issues, cf. Finocchiaro (1980, 2010).

the same date, cardinal-inquisitor Francesco Barberini, the Vatican secretary of state and pope's nephew, with the help of a legal assistant, transmitted the order to the papal nuncio in Florence; in Mayer's own account, they were all confused about whether and how Galileo was to be simply given a precept, or first an informal order and then a precept only if he refused; and so Mayer himself describes the whole situation as "tangled" (p. 116), Antonio Barberini's action as afflicted by a "discrepancy between decree and letter" (p. 116), and the intervention of Francesco Barberini as a "disaster" (p. 117) and a "spectacular mess" (p. 156).

The other part of Mayer's method, his prosopographical approach, is also flawed, although in a different way. The point is that biographical information is valuable, but Mayer too often includes too many details that are irrelevant or distracting. For instance, the Inquisition meetings were usually held at the Quirinale palace when the pope once a week presided the meeting, or at the residence of a cardinal-inquisitor at other times; on 25 November 1615, it met at the palace of cardinal Paolo Sfondrato, concerning which we are told the following cultural-tourism trivia: Sfondrato's palace is "the present Palazzo Sacchetti, one of the grandest in Rome, which shortly afterward sold for the colossal sum of 55,000 *scudi*" (p. 43). Another example concerns Pietro Paolo Febei, who was appointed assessor by pope Urban in January 1633 to replace Alessandro Boccabella (whose surname means literally "beautiful mouth"); now, one detail about Febei is found by Mayer to be noteworthy, i.e., "he cut a good figure. In his home town of Orvieto, they called him 'Bellafaccia', 'Pretty Face'" (p. 174); this enables Mayer to speak of a change from Boccabella to Bellafaccia (p. 173)! Finally, during the 1633 proceedings, some decisions suffered delays due to the fact that pope Urban spent a few weeks from late April to early May at the papal country residence of Castel Gandolfo; on no fewer than three occasions (pp. 187, 196, 198), Mayer feels the need to tell us that Urban was "purging" himself.

There is another limitation of Mayer's work that deserves mention. The issue involves archival research and is relatively arcane in general, but becomes highly revealing in the present context. The present context is one in which Mayer was able to exploit the fact that in 1998, for the first time ever, the Catholic Church regularly and generally opened to scholars the Inquisition archives in Rome, officially known as the Archives of the Congregation for the Doctrine of the Faith (the new name of the Inquisition). The single most important source of information is the minutes of Inquisition meetings, recording discussions and decisions. And the book's endnotes (pp. 229–329) show that Mayer made a diligent study of them, constantly citing them. This is, of course, commendable and in some ways impressive. Now, for the study of Galileo's trial, an even more crucial set of documents is the original manuscript proceedings, which have survived, and are kept in a different location, known as the Vatican Secret Archives (ASV). Despite such a name, the latter archives are also generally open to scholars with the proper credentials; they have been scholarly accessible for much longer than the Inquisition archives; and indeed Mayer also consulted the ASV. Of the many documents held in the ASV, the dossier of Galilean trial manuscripts is one of the most precious. The Church began to make it accessible to lay scholars in the 1860s, and as a result complete critical editions of the dossier were

published, evolving with increasing editorial sophistication.[5] The dossier consists of about 228 folios, grouped into various bundles of folded sheets, some with writing on both sides, some on only one side, and some blank; these manuscripts accumulated as the trial proceedings occurred. Unfortunately, Mayer never consulted or accessed this original dossier, as he himself reports at one point: "The original is now in the Reserva of the ASV and allegedly inaccessible for consultation, although Frajese was allowed to see it after I made my request. The photocopy made available to me is useless for most questions of paleography and obviously all those of codicology" (p. 255, n. 54). Frajese is the author of a book published in 2010, and holds a version of the forgery thesis regarding Seghizzi's 1616 precept to Galileo; and he does so in part based on his examination of the original dossier.[6] And Frajese is not the only recent scholar who has had access to the original dossier: Beretta has also done so; and the present reviewer can report having consulted it first in June 1986 and then again in April 2004. Mayer's lack of access is consequential because direct access is crucial for the resolution of some issues, and even for an informed discussion of them. For example, apropos of the possible forgery of Seghizzi's 1616 document, a first-hand examination of the handwriting in the manuscript is crucial (cf. p. 61); and it is important to also reach an informed conclusion about the fact that in the dossier, between the two folios on which the Seghizzi document is written, another folio has been cut out and disappeared; now, although the multiple paginations of the dossier exclude a 1632 forgery, they leave open the possibility of one perpetrated in 1616; however, not surprisingly, Mayer is completely unaware of this issue.[7] Moreover, apropos of the authorship of the two parts of the special commission report on the *Dialogue* in the summer of 1632 (mentioned earlier), it is important to examine directly the original handwriting as well as the water marks on the folio sheets (cf. pp. 155, 304 n. 32).

One of the most damaging features of Mayer's book is that his understanding of the Italian language seems inadequate and his translations of Italian texts are often inaccurate. For example, consider Bellarmine's certificate, concerning which Mayer examines not only the final version, but also an earlier draft, in order to discuss the possible significance of the differences; one change involved the connectives used by Bellarmine to go from a description of what had not happened (a trial) to what had happened (a warning); the phrase "*ma si bene*" (meaning "but rather") in the first draft was changed to "*ma solo*" (meaning "but only") in the final version; however, Mayer (p. 65) tells us that the earlier phrase means "but although"!!! Or consider the (already-mentioned) letter dated 25 September 1632 by Inquisition secretary Antonio Barberini to Florence's provincial inquisitor, for the purpose of summoning Galileo to Rome to stand trial; the letter states that Galileo should come to Rome "*per tutto il mese di ottobre*", which means "for the whole month of October"; Mayer thinks that Galileo is being ordered "to come to Rome before the end of October" (p. 116). Finally, consider the letter dated 14 February 1633 by the Tuscan ambassador

[5] Épinois 1867; Berti 1876; Gebler 1877. Cf. Finocchiaro 2005.

[6] Frajese 2010, 57, 103–106.

[7] On this issue, see Finocchiaro 2005, 251–258.

Francesco Niccolini in Rome to the Tuscan secretary of state in Florence: Niccolini reports that Galileo had arrived the night before, and that today he went to see the previous assessor Boccabella to thank him for his previous support and to seek advice; Galileo also went to see the current assessor Febei; and he tried to see but did not find commissary Maculano; however, Mayer tells us (pp. 175, 321 n. 166) that it was Boccabella who contacted and thanked Galileo, and who tried unsuccessfully to see commissary Maculano, and that it was Niccolini who had "addressed himself to the new assessor"!!!

Last, but not least, there is a problem with the English language used by Mayer. It's not anything stemming from inadequate linguistic competence; rather, it may stem from an opposite characteristic: a mastery so ingrained as to engender a total unawareness of the effect on readers of the writer's many user-unfriendly stylistic practices. Their combined effect generates an idiolect which I shall dub "Pidgin English 2.0". One of these practices is the ubiquitous usage of Latin words, such as: *censura* (censure), *decretum* (decree), *denunciatio* (denunciation), *expeditio* (concluding phase), *monitio* (admonition), *praeceptum* (precept), *processus* (proceedings or trial), *repetitio* (re-examination of witnesses), *socius* (associate), and *summarium* (summary). Then there is the excessive use of Italian terms, such as: *avviso* (news or announcement), *compagno* (associate), *fede* (affidavit or certificate), *giunta particolare* (special commission), *imbreviatura* (abbreviation), *nipote cugino* (relative or second cousin), *processo* (proceedings or trial), *sede vacante* (vacancy), and *staffetta* (courier). In addition, Mayer too often uses English words which are very rare, so much so that I am not embarrassed to report that I had to look them up in my unabridged English dictionary, for example: *accrete, benefice, brevet, chirograph, doceur, fettle, feudatory*, and *sequela*. Here, I am referring to terms whose usage could have been easily avoided, and not to technical jargon, which is unavoidable in almost all scholarship, for example (in this book): *breve* (official papal letter), *fiscal* (prosecutor), *precept* (judicial injunction), *process* (for proceedings or trial), and *rehabilitation* (commutation of some sentence or punishment). As if all these things were not enough, Mayer almost always gives hyper-literal translations whenever he quotes some original Latin or Italian passage; that is, even when they are essentially accurate, his translations are so literal that they are basically unintelligible to laypersons, and useless to specialists who know the original languages. Finally, to compound all these obstacles to normal reading, Mayer often includes (in parenthesis) the original Italian or Latin text, and he does so in the course of his exposition, although other times he relegates the original to the notes.

The general and methodological flaws illustrated above are not the only ones afflicting Mayer's book. There are others which, while still general, are less important. Thus, here for lack of space I will just mention a few others, mostly without illustrating them, just as for the same reason in my critiques above I limited myself to just giving a few examples for each criticism.

The book displays a pervasive anti-Galilean animus, expressed in language that is emotionally charged and full of negative connotations. The clarity of the exposition leaves much to be desired, and frequently I had to re-read various passages to fathom Mayer's unclear grammatical references and chronological sequences.

Mayer completely skips any discussion of the period 1616–1630, which is admittedly devoid of Inquisition proceedings, but contains important developments, such as: the 1620 Index decree detailing the corrections to Copernicus's book; the 1623 election of pope Urban; and Galileo's decision to write the *Dialogue*. Mayer practices without acknowledgment a questionable rhetorical approach: he often seeks to find in the texts or actions being examined instances of such things as the art of making the worse argument appear stronger, and the art of unscrupulously winning friends and influencing people; but he applies this technique one-sidedly only to people he wants to portray negatively and texts he does not want to take seriously. Finally, this book contains more than a proper share of factual errors; for example, Galileo left Florence for Rome to stand trial *not* on 21 January 1633 (p. 173), but on January 20; and Giovanfrancesco Buonamici was not (in 1633) Galileo's "future son-in-law" (p. 210), but rather was already (since 1629) brother-in-law of Galileo's daughter-in-law.

15.5 Conclusion

This essay has been a critical examination of Thomas Mayer's *The Roman Inquisition: Trying Galileo* (2015). This book displays considerable diligence and hard work in archival research, but, unfortunately, such research was crucially limited; for, as Mayer himself confesses, he was unable to consult the dossier of Galileo's trial proceedings kept in the Vatican Secret Archives. Mayer's prosopographical approach does sometimes provide useful biographical information, but it mostly includes excessive or trivial details. The book's legal orientation is relatively novel and potentially fruitful, but deeply flawed: it is too one-sided, because of an appalling disregard of the intellectual issues; and it uses a conception of law that is too formalistic and legalistic; for example, it implies that most of the time most of the Inquisition officials involved were not cognizant of the relevant laws or ignored them when they were.

Furthermore, Mayer's central substantive thesis shows ingenuity and is challengingly provocative; that is, the thesis that Commissary Seghizzi's special injunction to Galileo of February 26, 1616 is authentic as a document, historically accurate, and legally valid. Unfortunately, Mayer's supporting argument is highly deficient: his semantic and conceptual analyses of this and other documents are arbitrary, naïve, or superficial; he also tends to conflate, and thus to equivocate among, documentary authenticity, historical accuracy, and legal validity; and he fails to appreciate that fact that Seghizzi's February 26 injunction contradicts the words and actions of the key official of those proceedings, Cardinal-Inquisitor Robert Bellarmine.

These failings are such that laypersons and scholars can ignore the book, if they are trying to learn about Galileo's trial. Nevertheless, a few specialists could benefit from it by exploiting it to sharpen their skills: how to interpret opaque texts, how to evaluate challenging theses, how to avoid historical and philosophical errors.

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Chapter 16
Charity, Logic, and Para-Clericalism: Agassi on Explaining Galileo's Trial

Abstract This essay is a critical examination of Joseph Agassi's article "On Explaining the Trial of Galileo," which was a critical examination of Arthur Koestler's account. It focuses partly on several of Agassi's insightful substantive theses. These involve Galileo's skill in the art of reasoning and critical thinking, reflected in Agassi's claim that Galileo was "the greatest logical mind of his age"; Galileo's technique of strengthening opponents' arguments before criticizing them; and his efforts as a Catholic reformer. The essay also focuses on several of Agassi's fruitful methodological approaches. These concern the principle of "charity," when interpreting actions and arguments; an emphasis the role of logic in science and culture; and an approach to questions of science vs. religion that can be labeled "para-clerical," meaning a secular orientation holding that some religious questions are too important to leave to religious believers.

16.1 Introduction

In 1971, Joseph Agassi published an essay entitled "On Explaining the Trial of Galileo" (Agassi 1971b). At the time, I did not read it; nor, indeed, did I know of its existence or publication. The main reason for my oversight was that at the time I had no special interest in the trial of Galileo. Yet, there were other reasons why I could and should have learned about it.

In fact, I was already well acquainted with some of Agassi's works, especially his historiography of science. Indeed, I had written my doctoral dissertation on the problem of explanation in historiography of science, and several chapters in it consisted of a critical examination of his views on the subject, as found in his *Towards an Historiography of Science* (Agassi 1963; cf. 1987, 2008, 119–242). And in 1971, I was in the process of revising and polishing this dissertation, for publication as a book, which occurred soon thereafter (Finocchiaro 1973; cf. 1974, 1976a, 1987).

There were thus two principal reasons for the great relevance of Agassi's essay to my own work. One was that the essay is largely a critical examination of the account

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of Galileo's trial advanced by Arthur Koestler in *The Sleepwalkers* (1959); and this critical examination is largely a critique of Koestler's historiographical practices. The second reason was that, as the title of Agassi's essay explicitly suggests, the main historiographical practice examined by him is that of explanation: that is, how Koestler goes about explaining why Galileo's trial took place, why in 1633 the Roman Inquisition tried and condemned him; and how one could give a better explanation than Koestler's.

I did not discover Agassi's 1971 essay until about ten years later, that is, not until I myself developed a special interest in Galileo's trial. What happened was the following.

In my work on history-of-science explanation, one of the case studies I had examined was Alexandre Koyré's (1939) explanation of Galileo's initial failure and eventual success in discovering the law of falling bodies. Following up on this case study, I became more involved in Galilean scholarship. Moreover, another one of my scholarly specialties was logical theory, and in this field I followed an approach which stresses practical reasoning and argument in natural language, and which goes under such labels as informal logic, applied logic, practical logic, critical thinking, and argumentation theory (cf. Finocchiaro 2005a). It did not take me long to discover, both for pedagogical and theoretical reasons, a uniquely important and importantly unique source for such material; that is, Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*, which is full of arguments and counter-arguments for and against the motion of the earth. Combining these two lines of research, in the 1970's I became very involved in analyzing this Galilean work from the point of view of informal logic and critical thinking. This led me to publish in 1980 a book whose content is well described by the title, *Galileo and the Art of Reasoning: Rhetorical Foundations of Logic and Scientific Method*.

Now, in 1980, a world-historical development added a twist to the just-mentioned personal development. In the fall of that year, Pope John Paul II announced the appointment of a Vatican commission to re-examine Galileo's trial and the Galileo affair; and it was clear that in so doing the pope was acting on some of what he had said on the topic a year earlier, on the occasion of his speech to the Pontifical Academy of Sciences, at its commemoration of the centennial of Albert Einstein's birth (cf. Finocchiaro 2005b, 338–358). I knew, of course, that the 1633 condemnation of Galileo was occasioned by his publication of the *Dialogue* a year earlier. But this Galilean work was the subject of my decade-long research project and of my just-published book; and my book could boast of being the most detailed and comprehensive analysis of Galileo's *Dialogue* ever attempted or published. As a result, I felt that my deep understanding of the *Dialogue* enabled me to make some contribution to the interpretation and evaluation of the trial and the affair. Thus began my long-standing scholarly involvement with this topic.

When, finally, in the early 1980's, I read Agassi's 1971 essay, two things impressed me favorably. One was what may be called the principle of charity, which he advocated, practiced, and applied in interpreting and evaluating Galileo's trial, as well and in criticizing Koestler's account. Independently of Agassi, I had myself become a follower of the principle of charity, having learned it primarily from Michael Scriven

(1976, 71–73), in the context of informal logic and argumentation theory; and I had extensively applied the principle in my approach to the analysis of fallacies in general, and in my analysis of Galileo's *Dialogue* (Finocchiaro 1981, 1980, 340–341, 378–379). Thus, in my first paper on Galileo's trial, presented orally in 1982, and then published in 1986, I was eager, pleased, and excited to note this convergence (Finocchiaro 1986, 246).

A second convergence between myself and Agassi was the appreciation of Galileo from the point of view of informal logic, critical thinking, and argumentation. I could not help but being impressed upon reading in Agassi's essay so many explicit statements and examples to this effect (to be elaborated below). And as mentioned earlier, my *Galileo and the Art of Reasoning* had independently articulated such a critical interpretation of Galileo's work.

There is a third aspect of Agassi's 1971 essay that converges with my own work. This concerns what I call the para-clerical approach to questions of the relationship between science and religion in general, and of the Galileo affair in particular. This is an approach that is secularist-minded and yet tries to provide an intellectual service to religion, based on the belief that topics like the Galileo affair are too important to be left to religious believers themselves. I did not become aware of this convergence until very recently, for the simple reason that I myself became aware of this aspect of my own work only in the past five years roughly (Finocchiaro 2015b). However, this is something which I now find striking in Agassi's 1971 essay, which is significant, and which I am ready to elaborate. But before elaborating this convergence, and the others mentioned earlier, it will be useful to have a summary of Galileo's trial.

16.2 Galileo's Trial

In 1633, at the conclusion of one of history's most famous trials, the Roman Inquisition found Galileo guilty of "vehement suspicion of heresy."[1] This was a specific category of religious crime intermediate in seriousness between formal heresy and mild suspicion of heresy. He had committed this alleged crime in a book that defended Copernicus's hypothesis of the earth's motion and implicitly denied the scientific authority of Scripture. This work had been published the previous year, under the title *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican.*

This verdict was accompanied by several penalties: Galileo had to immediately recite an "abjuration" of his erroneous beliefs; the book was banned; and he was condemned to house arrest for the rest of his life.

This condemnation was the climax of a series of events starting in 1543, when Copernicus published an epoch-making book, *On the Revolutions of the Heavenly Spheres.* Copernicus advanced a novel argument for the idea that the earth moves, with daily axial rotation and yearly heliocentric revolution. His argument amounted

[1] For details, documentation, and references on the story sketched here, see Finocchiaro (1989, 2005b, 2014b).

to showing that the known facts about the motion of heavenly bodies could be explained better (and in quantitative detail) on the basis of the geokinetic, heliocentric hypothesis, as compared with the geostatic, geocentric view.

Although novel and significant, Copernicus's argument was hypothetical and inconclusive. Moreover, the idea faced powerful objections, which had been elaborated for two millennia by Aristotle, Ptolemy, and many others. That is, the earth's motion seemed epistemologically absurd because it contradicted direct sense experience. It seemed empirically false because it had astronomical consequences that were not observed, e.g., the phases of Venus and the annual parallax of fixed stars. It seemed mechanically impossible because it contradicted the available laws of motion and the clearest observations of moving bodies, such as the vertical direction of free fall. And it seemed religiously heretical because it contradicted biblical texts, such as the Joshua miracle, when God stopped the sun to prolong daylight (Joshua 10:12–13).

Thus, Copernicanism attracted few followers. Galileo himself, in his early career, was *not* one of them.[2] His research focused on physics rather than astronomy; he was critical of traditional physics and was constructing a new one; he had intuited that the Copernican hypothesis was more consistent with the new science of motion than was the geostatic theory; but he felt that, overall, the arguments against Copernicanism were stronger than those in favor.

However, in 1609–1610, with the newly invented telescope, Galileo made several startling discoveries: the moon's surface was full of mountains and valleys; innumerable other stars existed besides those visible with the naked eye; and the planet Jupiter had four moons revolving around it. Soon thereafter, he also discovered the phases of Venus and sunspots.

The new telescopic evidence removed most observational-astronomical objections against the earth's motion and added new evidence in its favor. Galileo's assessment was now that the arguments for the earth's motion were collectively stronger than those for the earth's rest. However, he realized that this did not mean that the issue was settled, because there was still some astronomical counter-evidence (mainly, the lack of annual stellar parallax), and because the physical–mechanical objections had not yet been explicitly refuted.

Furthermore, Galileo perceived the potentially explosive character of the scriptural issue. In fact, for several years he did not get involved. When he finally did, he was careful enough not to publish his criticism of the scriptural objection, but to circulate it privately, in the form of letters: in 1613 to his former student Benedetto Castelli, and in 1615 to the grand duchess Christina.

Galileo's criticism, although complex and liable to misunderstanding, was logically compelling, rhetorically persuasive, and theologically sophisticated.

However, despite winning the intellectual argument, Galileo lost the practical struggle. In 1615, after some formal complaints were filed against him, the Inquisition launched an investigation. The proceedings lasted about a year, and the results were

[2] For details and documentation of this thesis, and criticism of alternatives, see Finocchiaro 2010, 37–64.

the following. In March 1616, a decree was issued by the Index, the department of the Church in charge of book censorship: it declared that the doctrine of the earth's motion was scientifically false and theologically contrary to Scripture; and it temporarily prohibited Copernicus's book.

Although Galileo was not mentioned at all in the decree, in private he was given a warning. This warning exists in two versions. One is in a certificate given to Galileo by Cardinal Bellarmine, a member of the Inquisition and one of the most authoritative churchmen of the time; it states that he had informed Galileo that the earth's motion could not be defended as true, but merely as a convenient hypothesis to save the appearances. The second version is in an unsigned note written by a clerk and found in the file of Inquisition trial proceedings; it states that the Inquisition's commissary (Michelangelo Seghizzi) gave Galileo the special injunction not to hold, defend, or teach in any way the earth's motion. The difference between these two versions is that the special injunction adds a more stringent prohibition to the one mentioned in Bellarmine's warning: besides being prohibited, like other Catholics, to defend the physical truth of Copernicanism, Galileo was also forbidden to defend it in any way whatever.

For the next several years, Galileo kept relatively quiet. Then a new opportunity arose in 1623, when one of his admirers became Pope Urban VIII. From several indications, Galileo got the impression that, if he exercised the proper care, he could publish a book on the earth's motion. Thus, he wrote the *Dialogue.*

The book was obviously a discussion of the earth's motion, but Galileo had done several things to avoid trouble. For example, as the title suggests, he wrote the book in the form of a dialogue among three characters: a Copernican named Salviati, a Ptolemaic named Simplicio, and an intelligent and curious layperson named Sagredo. Moreover, the conversation amounted to a critical examination of all the evidence for and against the earth's motion; the arguments on both sides were presented, analyzed, and evaluated. Galileo tried his best to carry out the evaluation fairly. The arguments for the earth's motion turned out to be stronger than those against it. This was at worst an *implicit* defense of the *probable* truth of Copernicanism. Galileo's gamble was that friendly Church officials would not blame him for this, because they would recognize that the book could be interpreted as a *hypothetical discussion* of the geokinetic opinion, and that its defense was not explicit, and so that is was within the spirit of Bellarmine's warning.

Galileo's effort misfired not because it was foolish, but because in 1632 the special injunction came to the surface, and from its point of view any defense of the earth's motion by Galileo was prohibited. The book's publication thus led to a trial during which he denied receiving the special injunction, but admitted receiving and violating Bellarmine's warning, insisting that his violation was unintentional. This admission spared Galileo more drastic punishments, such as being burned at the stake.

16.3 Koestler's Account

There is much in Agassi's account of Galileo's trial that is correct, insightful, and important. And the same applies to his critique of Koestler's account. Let us highlight some of these themes, beginning with the latter.

As Agassi points out, according to Koestler, "Galileo fought not for the sake of truth, but because he was pathologically unable to avoid any quarrel, accept any compromise, or admit any error" (138).[3] In other words, allegedly, Galileo was "an extremely unpleasant and arrogant person" (142). This psychological interpretation provides Koestler with the explanation of the tragic outcome; that is, "Galileo's character was the cause of the trial" (143). Koestler tries to support this explanation with two main allegations.

The first allegation pertains to the first phase of the trial, in 1615–1616. According to Koestler, after some heresy charges against Galileo were formally filed in February and March 1615, the Inquisition investigated the matter, and by the autumn of that year it had found them to be baseless and had essentially dismissed them. However, in December 1615, Galileo went to Rome on his own initiative, and ended up staying till June. His goal was to defend himself and Copernicanism, and indeed for about two months he was very active, meeting as many officials and laypersons as he could, and attending or organizing many social gatherings to discuss pro- and anti-Copernican arguments. Koestler claims that "his method was to make a laughing stock of his opponent—in which he invariably succeeded, whether he happened to be in the right or in the wrong … before answering the opposing reasons, he amplified them and fortified them himself with new grounds which appeared invincible … It was an excellent method to score a moment's triumph, and make a lifelong enemy. It did not establish his own point, but it destroyed his opponent's" (Koestler 1959, 452).

Koestler's second allegation pertains to the 1633 proceedings. These were triggered by the 1632 publication of the *Dialogue*. For Koestler, in this work Galileo was resuming his (previously failed) pro-Copernican campaign with the same tactics and techniques that had been unsuccessful earlier. That is, this book supposedly insulted the intelligence of Galileo's contemporaries with fallacies, sophistries, confusions, and errors, and offended their sensibilities with insults, lies, deception, dishonesty, hypocrisy, and false promises. For example, Koestler thinks that Galileo's simplified presentation of the Copernican doctrine "is downright dishonest … was not a simplification but a distortion of the facts, not popular science, but misleading propaganda … Moreover, he keeps silent about the fact that the Tychonic system fits the phenomena equally well" (Koestler 1959, 476–477).

Agassi does show some appreciation of Koestler's account, primarily because his image of Galileo is lively, de-mythologized, and forceful (138, 163). However,

[3] Here and elsewhere, references to Agassi 1971b are given by means of page number(s) in parenthesis.

Agassi is mostly critical of Koestler, and here I can only focus on the criticism.[4] Let us look at the points that are most telling.

16.4 Agassi's Criticism

Agassi is absolutely right that Koestler's explanation of the trial is "an exaggeration" (143) of the truth that Galileo had quarrelsome tendencies which caused him difficulties. Now, exaggeration is a serious flaw by any standard, and so there might be no obvious need to elaborate. However, it turns out that this criticism has some revealing ramifications, which should not be ignored.

To begin with, it is worth quoting an aphorism formulated in 1785 by one Girolamo Ferri, in the context of his criticism of a new Galilean myth that was emerging then: "the exaggeration of truth is a lie" (Ferri 1785, 54; cf. Finocchiaro 2005b, 161). However, to go back to Agassi, it is important to know and understand that, in the words of Jarvie and Laor (1995, p. xix), "one theme that recurs in his [Agassi's] writings and his discussions is that of steering the middle way and keeping a sense of proportion … for example, he spells out and rejects polarized views of the nature of man … he sets out the extreme views on sociological method and shows they share an underlying premiss which, when negated, permits moderate mediation between the extremes. Agassi's use of this *via media* is confessedly Lévi-Straussian. He invites us to break through intellectual myths that come in poles, sweeping our searchlight over the whole area of reality between the poles." One work where Agassi focuses on such an approach is his *Towards a Rational Philosophical Anthropology* (Agassi 1977; cf. Finocchiaro 1978).

More to the point, this approach is not merely Lévi-Straussian, but also Galilean, at least as I interpret Galileo. In fact, in a number of works, I have argued that one of the most central methodological features of Galileo's work is something which I call judicious-mindedness, or judiciousness, or, more simply, judgment. This concept means the willingness and ability to be impartial, balanced, and moderate; that is, to avoid one-sidedness (by properly taking into account all distinct aspects of an issue) and to avoid extremism (by properly taking into account the two opposite sides of any one aspect). For example, Galileo was judicious in this sense with regard to the contrast between the two chief world systems (Ptolemaic and Copernican), between religion and secularism, between epistemological apriorism and empiricism, between mathematicism and qualitative considerations, between authority and independent-mindedness, and so on (cf. Finocchiaro 1980, 145–166; 2010, 121–134; 2014a, 259–280).

[4] I have elaborated my own critical appreciation of Koester's account in Finocchiaro 2005b, 306–317. Similarly, but in a reverse manner, it would be beyond the scope of the present essay for me to elaborate a critical appreciation of Agassi's (1971b) essay, that is for me to give an elaboration that was both critical and appreciative; rather, I can only focus on a positive appreciation of Agassi's account. If I had to be critical, I would elaborate some difficulties involving dates, names, and documents.

Another criticism advanced by Agassi is that Koestler's portrayal of Galileo is one-dimensional and static rather than multi-faceted and dynamic, insofar as Koestler attributes to Galileo a character than does not change, whereas it underwent significant developments corresponding to the three main stages of his life. That is, according to Agassi, "Galileo held three different positions in different times: first, he concealed his Copernicanism; then—when he was about fifty—he defended it with some caution; his great battle for it took place when he was about seventy. Koestler's view of Galileo's motives and purpose is offered as one explanation of all three phases" (138). More generally, "it seems that Koestler's historical figures suffer from the same defect as his fictional figures: they never change their characters" (142).

This criticism is perceptive, correct, and important. And the same applies to the positive and constructive thesis that underlies the criticism. Methodologically speaking, Koestler's one-dimensionality regarding Galileo's character may be viewed as an instance of injudiciousness. And substantively speaking, such a three-fold development in Galileo's life is a crucially important historical fact. I myself have had occasion to elaborate it elsewhere (cf. Finocchiaro 2010, 37–64), and have summarized it above. It is worth repeating here, with some further encapsulation.

First, there is the pre-telescopic stage, when Galileo's cognitive stance toward Copernicanism may be described as partial, indirect, and implicit pursuit (which is also to say, qualified rejection). Then we have the full-blown middle period from 1609 to 1616, which constitutes a qualitative change from the preceding one; which may be described as full-fledged, direct, and explicit pursuit of Copernicanism (which is also to say, qualified acceptance); and which contains varying and increasing degrees of endorsement or commitment. Thirdly, there is a problematic post-1616 stage, which may be described as again one of indirect and implicit pursuit; now the main stumbling blocks were external, namely the warning he had personally received from cardinal Bellarmine, and the public condemnation of Copernicanism issued in the Index's decree. As suggested earlier, the *Dialogue* fits this third stance, for it can be interpreted as a critical examination of all the astronomical, physical, and philosophical (but *not* the theological) arguments for and against the opinion.

An additional flaw which Agassi finds in Koestler's account is what may be called bias. For example, with regard to the scientific aspect of the story, the pattern displayed by Koestler is the following: "every time Galileo's defenders are apologetic, Koestler stresses the point which caused them discomfort; and every time the Catholic apologists show a weak point in Galileo's scientific views, Koestler follows suit" (140). And with regard to the political context, "the chief Catholic technique used on the political side of the story is that of pooh-poohing one document and stressing another; and Koestler follows suit" (140).

This is a perceptive, accurate, and fair criticism. The only thing I would want to add here is that this is precisely what we would expect from an injudicious thinker like Koestler. Indeed bias is an aspect of injudiciousness.

Similar considerations apply to another one of Koestler's failings, namely unfairness. However, here the issues get more profound and far-reaching. For, the underlying principle, presupposed and advocated by Agassi, and violated by Koestler, is extremely important. It deserves a label: the principle of fair-mindedness.

As Agassi points out, "Koestler … can be unbelievably unfair to Galileo, to the point of viewing even his (alleged) anticipation of criticism—the thing which every good author does—as a vice" (151, n. 2). Indeed, in discussing the controversy over floating bodies in 1611–1612, Koestler complains that Galileo "employed his favorite trick of anticipating the peripatetics' arguments, building them up in a mock-serious manner, and then demolishing them with glee" (Koestler 1959, 428). This is the same complaint which, as we saw above, Koestler advances in his account of Galileo's discussions in Rome in the winter of 1615–1616.

Now, at the interpretive level, Koestler is giving an accurate construal of this element of Galileo's methodology. Indeed, I would add that this is an important feature of the approach he follows in the *Dialogue* (cf. Finocchiaro 2014a, 259–264). And Galileo was even reflectively aware of this fact, for he had the occasion to explicitly formulate the principle of fair-mindedness with words that are eloquent and memorable: "when one presents arguments for the opposite side with the intention of confuting them, they must be explained in the fairest way and not be made out of straw to the disadvantage of the opponent" (cf. Finocchiaro 1989, 278; 2014b, 129).

However, as Agassi correctly suggests, at the evaluative level Koestler's complaint, which portrays this technique as illegitimate, is not only wrong, but perversely wrong; that is, it is the opposite of the truth. Let me elaborate.

This technique is a very sophisticated and powerful method of argument. It has nothing to do with what is imagined by Koestler, namely with the method of ridiculing opponents by means of the trick of first persuading them of one thing and then proving to them the opposite. On the contrary, the Galilean technique of strengthening objections before refuting them shows that the objections, although invalid, are serious, important, and plausible, and therefore that the opponents who believe the contrary thesis are reasonable people. Far from ridiculing opponents, this is a way of ennobling them, paying them respect, and enhancing their standing and credibility. Galileo's technique is, in fact, the anti-thesis of the widespread practice of demonizing one's opponents, or differently expressed, it is the anti-dote to the straw-man fallacy. Finally, fair-mindedness may be regarded as an application of judiciousness to the contrast between a proponent and an opponent in the context of the practice of argumentation.

I am adapting this (meta)argument, as well as the label "fair-mindedness," from the literature on informal logic, critical thinking, and argumentation theory (Scriven 1976, 166–167; Fisher 1991; Fisher and Scriven 1997, 90–91, 137–143; Finocchiaro 2013, 89–91,117–122, 153–154). However, the key idea of this principle has also been elaborated by many others under different terminology, for example, by John Stuart Mill under the heading of freedom of thought, discussion, and argument, and the role of such freedom in the search for truth (cf. Finocchiaro 2013, 178–191).

So far, I have elaborated and strengthened several of Agassi's major criticisms of Koestler. However, it should be obvious that my discussion has not been merely

a negative exercise, but rather has revealed several constructive and positive principles which may be properly attributed to Galileo; which Agassi seems to advocate; and which appear to be highly plausible. That is, we have seen that, firstly, Agassi faults Koestler's account in general as full of exaggerations, and this criticism may be conceptualized as revealing Koestler's injudiciousness, as well as the value of the principle of judiciousness. Secondly, Agassi criticizes Koestler's account of Galileo's character as one-dimensionally static, rather than dynamically three-fold, and such one-dimensionality may be seen as an instance of injudiciousness. Thirdly, Agassi questions Koestler's account as biased with regard to the issue of Galileo vs. the Church, and hence as one-sided, and thus again as a violation of the principle of judiciousness. Fourthly, Agassi objects to Koestler's unfairness in assessing Galileo's method of argument, with regard to the technique of anticipating and strengthening the objections to one's own views; this reveals Koestler's own violation of the principle of fair-mindedness, and thus again of the ideal of judiciousness; and we also have a reaffirmation of the importance of this ideal.

16.5 Agassi's Approach

Despite this positive and constructive value of Agassi's criticism of Koestler, at this point it is useful to focus more directly and explicitly on Agassi's own account of Galileo's trial. However, it will be obvious that, just as the critiques discussed above have underlying positive counterparts, the positive account discussed below has critical implications. These would take the form that Koestler fails to appreciate various aspects of Galileo and his trial which Agassi's account utilizes, elaborates, and appreciates.

A key theme in Agassi's explanation of Galileo's trial is the following. It occurs too frequently to be ignored. For example, with regard to the first phase in 1615–1616, consider,

> his theological campaign for Copernicanism. Galileo had a very good chance of winning it; but the point to stress is that we ought … to notice that he thought he had a good chance to win the battle. (Indeed at one point he thought the battle was already won.) Trying to explain his behavior thus may be more interesting than viewing it as irrational, as Koestler does, by saying that he was obsessed with his need to quarrel regardless of the consequences. In brief, Koestler does not attempt a rational reconstruction of the battle as it appeared before it was over. [157]

And with regard to the second phase of the trial in 1632–1633, "the story of the writing and the publication of the *Dialogue* becomes amply clear and reasonable if we do not use hindsight, if we forget the ensuing catastrophe when trying to reconstruct the way Galileo, the Pope, and others, looked at the situation before they knew of the grave consequences of all they did. (Even the fact that they were apprehensive, all of them, indicates that none of them acted unreasonably.) But it all becomes clear when one assumes their intentions to have been honorable" (159). Furthermore, "as long as there is no evidence against Galileo's confessions, they ought to be taken

very seriously as possibly true; and the same ought to be said of Pope Urban VIII and of all others involved. It is quite possible that both Galileo and Urban VIII were irresponsible rascals, as Koestler asserts; but this should not be our starting point" (158). Or again, "the people involved knew what they were doing—they were too cautious and apprehensive not to—and were acting in good faith and not from any personal motive" (160).

This is a very commendable and fruitful approach. Although Agassi does not use this terminology, I think that here we are dealing with the principle of charity.

In the context of the theory of reasoning and argument, Scriven (1976, 71–72) formulated this principle as follows: "The Principle of Charity requires that we try to make the best, rather than the worst, possible interpretation of the material we are studying … It doesn't mean letting people off the hook entirely … What the Principle of Charity does mean is that 'taking cheap shots' is something we shouldn't waste much time doing … It tells you that you want to interpret the argument's meaning in whatever way makes the most sense and force out of it, because otherwise, *it can easily be reformulated slightly in order to meet your objections*."

And in the context of my own critical analysis of the arguments in Galileo's *Dialogue*, I have had the occasion to formulate it as follows: "The principle of charity states that in criticizing an opposing argument or view, one should first formulate a charitable interpretation and then make the latter the target of one's criticism; a charitable interpretation is one that portrays the original argument or view in a reasonably favorable light, as possessing some strength that must be taken seriously, and as free of insignificant or trivial errors which do not affect the main issue" (Finocchiaro 1997, 376).

More generally, I have also had the opportunity to formulate and apply this principle in the context of my criticism of a recent account of Galileo's trial that reaches new heights of uncharitableness. This account is that advanced in Thomas Mayer's *The Roman Inquisition: Trying Galileo* (2015). Besides many other objections, I also point out that whereas my account (along the lines of the one sketched above) conforms to the principle of charity, Mayer's account violates it:

> my account is also confirmed by the fact that it can help us understand Galileo's subsequent behavior and the Inquisition's subsequent proceedings, up to the sentence and abjuration of June 22, 1633. This is in accordance with a general principle of charity that aims to portray all parties as more or less reasonable, or at least to maximize the total reasonableness displayed on all sides. On the other hand, Mayer's account is further disconfirmed by the fact that his … thesis forces him to attribute all kinds of administrative incompetence and legal improprieties to the officials involved, in both the earlier (1615–1616) and the later (1632–1633) proceedings, and a nearly-insane arrogance and recklessness to Galileo. In short, for Mayer, Galileo as well as almost all clerical officials (except for commissary Seghizzi) were unreasonable, incompetent, and/or unlawful, whereas in my account Galileo and almost all officials were reasonable, competent, and lawful (the only and main exception being Seghizzi). [Finocchiaro 2016, 57; cf. Finocchiaro 2015a]

A second fruitful theme found in Agassi's account is the thesis of Galileo as a skillful practitioner of applied logic, critical thinking, and argumentation. In fact, in the context of his discussion of Galileo's methodology as a synthesis of apriorism

and observation, it is impossible to overlook Agassi's talk of Galileo's "clear thinking" (149); of his "critical mode of thinking" (152); and of "his realization of the importance of logic; his considering criticism and clarity to be essential for scientific discourse" (153). Similarly, Agassi regards Galileo as "a forerunner of Descartes in viewing clarity and distinctness … but also simplicity … as criteria of truth" (150); as "a teacher of critical thinking" (151, n. 2); as a "clear and critical thinker" (162); and as "the greatest logical mind of his age" (155).

Agassi finds such applied logic and critical thinking primarily in Galileo's early works *On Motion* and *On Mechanics*; in his *Sidereal Messenger*; and in his *Discourse on Floating Bodies*. The only thing I would add here is that such Galilean traits are also predominant in the *Dialogue* and the *Two New Sciences*.

This Agassian thesis is not just a substantive thesis about the nature of Galileo's scientific work. It also reflects a methodological orientation, according to which a scholar seeks to interpret and assess scientific works from such a point of view. In fact, Agassi himself, in a completely different context, has made a contribution that reflects such a point of view. I am referring to his book on Michael Faraday (1791–1867), the English scientist best known for his discovery in 1831 of electromagnetic induction; that is, the phenomenon of generating an electric current in a wire by moving it relative to a magnet (Agassi 1971a; cf. Finocchiaro 1976b). This work may be interpreted as a case study in the general interpretation of the history of science as the history of arguments. In fact, the book is full of discussions seeking to ascertain the reasons which Faraday and his contemporaries had for their frequently conflicting views; and Agassi also attempts to evaluate those views in terms of those reasons, rather than by reference to the latest physics textbook. And at the end of the book, Agassi expresses explicitly his awareness that he is practicing and advocating the history of science as the history of arguments.

A third key aspect of Agassi's account is a methodological and historiographical orientation which I shall label and conceptualize as para-clericalism, or a para-clerical approach. Again, this is not Agassi's terminology, and so this orientation is not as explicit as the theme of critical thinking, which we have just seen to be very obvious; nor is such para-clericalism as easily discernible as the charity principle, which we saw earlier is not explicitly discussed by him but corresponds closely to his talk of rationality and reasonableness. Nevertheless, para-clericalism may be seen to be no less central, once we understand what is meant by this term.[5]

To begin with, the para-clerical approach should not be confused with either the pro-clerical or anti-clerical orientations, which are very common, as Agassi's account points out. It is also clear that he is critical of both of these approaches, and such double-edged criticism is a hint of para-clericalism. Second, it is useful to point out that the term para-clericalism is meant to bear some analogy (with regard to the prefix) to para-medical and para-military contexts.

Additionally, conceptually speaking, as I have had occasion to elaborate before, the para-clerical approach "views controversial topics involving the relationship

[5] See also Chapter 11 in this book.

between science and religion from a perspective that is secular-minded, but appreciative of religion, and yet conducted in the belief that such topics are too important to leave to religious believers" (Finocchiaro 2015b, 125). In the context of the Galileo affair, para-clericalism commonly involves "offering advice to the Church, crediting the Church for uncommon reasons, defending the Church from common criticism, believing in the superiority of religious outsiders over insiders, and intellectual courage" (Finocchiaro 2015b, 137).

Finally, by way of illustration, I have had occasion to elaborate a methodological appreciation of the work of two scholars, Paul Feyerabend and John Heilbron (Finocchiaro 2015b). However, I must add that in their case my methodological appreciation happens to be combined with substantive criticism, whereas with regard to Agassi my position seems to be a combination of both methodological and substantive appreciation. In fact, Feyerabend has (in)famously argued, on secularist grounds, that "the Church at the time of Galileo not only kept closer to reason as defined then and, in part, even now; it also considered the ethical and social consequences of Galileo's views. Its indictment of Galileo was rational and only opportunism and a lack of perspective can demand a revision" (Feyerabend 1993, 125). And Heilbron ends his secularist biography of Galileo with an argument predicting that sooner or later the Catholic Church will canonize Galileo as a saint. Indeed the book's last sentence is the following rhetorical question: "Who can doubt that within another 400 years the church will recognize Galileo's divine gifts, atone for his sufferings, ignore his arrogance, and make him a saint?" (Heilbron 2010, 365).

In light of such a framework, several aspects of Agassi's account become very revealing. The most general and significant one is his thesis that, with regard to the science vs. religion issue, Galileo was a "Catholic reformer" (139, 156). That is, as Agassi formulates it elsewhere, Galileo "was an ardent Catholic who sincerely wished to cooperate with the Church as far as his conscience permitted him … The Church wished to embrace Copernicus' system, in order to retain its intellectual hegemony … it wanted to make the transition from Aristotelianism as smooth as possible and … Galileo was supposed to help in this transition" (Agassi 1957, 238).

More specifically, Agassi makes and applies a crucial theological distinction that helps to make sense of the Church's warning to Galileo in 1616 and of his subsequent behavior resulting from it. This is the distinction between believing or accepting a doctrine as true on the one hand, and discussing or supporting or criticizing it on the other. The distinction is reflected in one of the most important documents of Galileo's trial, namely the minutes of the Inquisition meeting of February 25, 1616, when Pope Paul V issued some conditional orders with regard to the resolution of the Galileo case:

> The Most Illustrious Lord Cardinal Millini notified the Reverend Fathers Lord Assessor and Lord Commissary of the Holy Office that, after the reporting of the judgment by the Father Theologians against the propositions of the mathematician Galileo (to the effect that the sun stands still at the center of the world and the earth moves even with the diurnal motion), His Holiness ordered the Most Illustrious Lord Cardinal Bellarmine to call Galileo before himself and warn him to abandon these opinions; and if he should refuse to obey, the Father Commissary, in the presence of a notary and witnesses, is to issue him an injunction to

> abstain completely from teaching or defending this doctrine and opinion or from discussing it; and further, if he should not acquiesce, he is to be imprisoned. [Finocchiaro 1989, 147; 2014b, 102]

And to this we should add that a week later Bellarmine reported to the Inquisition that he had given the warning and that Galileo had acquiesced; thus, there had been no need for the commissary to intervene and issue the more stringent special injunction.

On this particular issue, Agassi speaks precisely as one would expect from a para-clerical scholar: "Now (being an agnostic and a Jew) I am a person poorly qualified to explain the fact that Catholics are permitted by their Church simultaneously to believe in a doctrine and to criticize it; yet I wish to state categorically and most emphatically that such is the case, no matter what is the doctrine in question, no matter how far-reaching its consequences" (157–158).

Similarly, Agassi's para-clerical orientation emerges with regard to the issue of Galileo's criticism of the biblical objection to Copernicanism, which started the first phase of the proceedings. According to Agassi, "had Galileo's theology been accepted by the Church directly from him, he might have become a saint, rather than Bellarmine. There was a chance that this could happen, both in 1616 … and after the death of Bellarmine and the Pope, when the new Pope, Urban VIII, encouraged Galileo to write his *Dialogue*. But even if he had no chance against Bellarmine, his sincerity and courage, as well as his important contribution to Catholic theology, ought to be appreciated (and may be appreciated in the future, even by Rome; remember Joan of Arc!)" (156). Some of these words (from 1971) are prophetic in light of what started happening about a decade later, with the so-called rehabilitation of Galileo by Pope John Paul II in 1979–1992 (cf. Finocchiaro 2005b, 338–358).

Finally, with regard to the 1633 proceedings, occasioned by the *Dialogue*, Agassi's para-clerical explanation is the following: "I do think that throughout the *Dialogue* he meant what he said and that the constrictions he accepted were those he freely chose as a religious man. It seems to me that the Church was ready to grant more freedom to scientists provided it could save its face, and that Galileo, the ardent Catholic and leading scientist, was eager to help. Why then did the plan misfire? Because, I suggest, of the enthusiastic reception of he book: the Copernicans took the permission to publish the *Dialogue* as the Church's admission of failure" (Agassi 1957, 239). Moreover, "Galileo's enemies successfully spread the rumour that Simplicio was [a caricature of] the Pope; and then, even though the Pope knew it not to be true, he had to do something about it: this was not a period in which jokes against the Pope were harmless to the Church. So the Church had to assert its authority against Galileo" (161).

16.6 Conclusion

In this essay, I have examined Agassi's 1971 article on explaining the trial of Galileo. I have found it to be full of substantive and methodological insights.

Substantively speaking, Agassi is essentially correct in advancing the following important theses, and in criticizing Koestler for his failure to appreciate them. Galileo had quarrelsome tendencies, but not a compulsive obsession to get into trouble. His career displays three importantly different behavior patterns, and not a single one. A characteristically Galilean method of argument was the technique of acknowledging and strengthening objections against his own views, before criticizing them; and such a technique is highly commendable, and not at all deplorable. More generally, Galileo was a highly skilled and effective practitioner of the art of reasoning, argument, applied logic, and critical thinking, and not at all a sophistical or incompetent arguer. And Galileo was also a kind of Catholic reformer, whose arguments on the separation of Church and science, and on the non-scientific authority of Scripture, could have prevailed in his own time, but had to wait centuries before they received the proper recognition by the Church.

Methodologically speaking, Agassi displays a number of fruitful historiographical orientations and approaches; and these are attitudes which by and large he shares with Galileo and which are constantly disregarded or violated by Koestler. Judiciousness is the avoidance of one-sidedness and of extremes. Charity is the ideal of maximizing the rationality or reasonableness of one's own opponents. Critical thinking is the skill of reasoning focused on the identification, interpretation, evaluation, analysis, and self-reflective presentation of arguments. And para-clericalism is an approach to questions of science vs. religion that is secular-minded, but appreciative of religion, and yet conducted in the belief that such questions are too important to leave to religious believers.

It should be obvious from the preceding that I share these substantive theses and methodological attitudes. Indeed, my scholarly work on Galileo seems to have been largely an elaboration of such theses and attitudes. However, despite such convergence, my Galilean scholarship was created independently of Agassi's influence. On the other hand, it is perhaps because of such a convergence that I have been able to extract these themes from his essay. All this strikes me as strange and curious, especially in light of my early acquaintance with and study of his historiography of science. However, I shall postpone to some other occasion any further elaboration and embellishment of this biographical and autobiographical curiosity.

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Chapter 17
Authenticity vs. Accuracy vs. Legitimacy: Pagano on the Vatican Documents

Abstract This essay is a critical appreciation of the scholarly work on Galileo's trial by Mons. Sergio Pagano (Prefect of the Vatican Secret Archives since 2007). Pagano has argued convincingly that the 1616 precept to Galileo by Inquisition commissary Seghizzi is authentic and not a forgery. This is an important accomplishment because it settles a controversy that has raged since the 1860's, with wide-ranging cultural repercussions. However, the precept's *documentary authenticity* does not prove its *factual accuracy*, nor its *juridical legitimacy*. Indeed, it can be shown that the precept was illegitimate, because it contradicts Pope Paul V's orders and cardinal-inquisitor Robert Bellarmine's testimony. Pagano apparently conflates, and equivocates among, these three concepts, as well as among the contents of four distinct orders to Galileo regarding Copernicanism: Pope Paul's orders not to believe and not to discuss, Bellarmine's warning not to hold or defend as true or as biblically compatible, and Seghizzi's precept not to hold or teach in any way.

17.1 Pagano's Contributions to Galilean Scholarship

My acquaintance with the scholarly work of Mons. Sergio Pagano is rather limited, but it does concern one of the most important and controversial episodes in ecclesiastical history, namely the trial of Galileo Galilei. I am referring to the fact that in 1633 the Roman Inquisition tried and condemned Galileo for a crime labeled "vehement suspicion of heresy." He had allegedly committed this transgression by publishing the year before a book entitled *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican.* This book defended Nicolaus Copernicus's theory that the earth is a planet rotating on its own axis every day and circling the sun once a year, and it implicitly advocated the principle that Holy Scripture is not a scientific authority; and in 1616, this theory and this principle had undergone various censures and prohibitions by the Church. However, Galileo happened to make such crucial contributions to physics, astronomy, instrumentation, and scientific method that he came to be regarded, by scientists themselves,[1] as the Father of Modern Science.

[1] Einstein 1954, 271; Hawking 1988, 179.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_17

Thus, it is not surprising that the 1633 condemnation of Galileo generated a new controversy that continues to our own day; this subsequent and continuing controversy is about such issues as whether his condemnation was just, whether science and religion are incompatible, and what is the proper relationship between individual freedom and institutional authority.

Pagano's first contribution to this topic came in 1984, when he published (with the assistance of Antonio G. Luciani) an edition of the documents of Galileo's trial.[2] This volume was basically a critical edition of the special Galileo file of trial proceedings (now kept in the Vatican Secret Archives) and of the relevant Inquisition documents known as *Decreta*, consisting of minutes or summaries of decisions reached at official meetings of the inquisitors (now kept in the Archive of the Sacred Congregation for the Doctrine of the Faith). Both sets of documents are essentially identical to those published in 1908 in Volume 19 of the National Edition of Galileo's *Opere*, edited by Antonio Favaro[3]; but there are a few minor editorial improvements and documentary additions. The book also contains a long useful introduction in which the editors give a history of the special Vatican file on Galileo and report on their activities and searches.

Here, we should recall that in 1979, Pope Saint John Paul II had started what some people came to regard (whether accurately or not) a process of rehabilitation of Galileo, which was not brought to an end until 1992.[4] For example, in 1980 John Paul appointed a commission to study the Galileo affair, whose main aim was the clarification of the relationship between science and the Catholic Church; the pope distinguished several aspects of the problem, each of which was the responsibility of a member of the commission. And during the next dozen years many studies, conferences, and publications were directly or indirectly encouraged, supported, or sponsored by this Vatican Commission.

Pagano's 1984 book was welcomed and appreciated by many scholars, including the present author, who at the time was beginning his scholarly study of the Galileo affair. In fact, I too had been inspired by the epoch-making and world-cultural aspects of John Paul's rehabilitation of Galileo, although my background was in logic and the philosophy and history of science. The main connection was that after about a decade of research, in 1980 I had published a detailed critical analysis of Galileo's *Dialogue* as a case study in logical theory and scientific methodology.[5] But the *Dialogue* was also the book that occasioned the trial and condemnation of Galileo, and so I felt I had at least some preparation for the task. However, I had barely examined other aspects of the topic, such as the documents of the Inquisition proceedings. Fortunately, Pagano's collection of documents could help fill such a lacuna. And so perhaps I may be allowed to quote the conclusion of my review of his book: "This work is to be welcomed for its great intrinsic documentary value, for being immensely more readable and affordable than the Favaro volume, and as a sign of an

[2] Pagano 1984.

[3] Galilei 1890–1909, 19: 272–407.

[4] John Paul II (1979, 1992); cf. Finocchiaro 2005, 338–357.

[5] Finocchiaro 1980.

admirable Vatican commitment to objective scholarship. It invites more determined interpretative efforts to resolve the Galileo puzzle."[6]

As my own efforts in this field continued, so did my appreciation of, and the profit I could derive from, Pagano's scholarship. A noteworthy example refers to a 1992 volume also stemming from Vatican Commission on Galileo, and edited by Walter Brandmüller and Egon Johannes Greipl.[7] This volume dealt with an important episode of the subsequent Galileo affair; it occurred in 1820, when a controversy raged in Rome about whether or not to grant the imprimatur to an astronomy textbook authored by Giuseppe Settele, which taught the Copernican theory of a moving earth. It is noteworthy and interesting that the two opposing sides of this controversy were the Master of the Sacred Palace, who wanted to deny the imprimatur, and the Congregation of the Holy Office, which was favorably inclined. After a year of intense debates, the Holy Office prevailed, and Settele's textbook was published. The volume in question is a collection of all the documents generated by this controversy, preceded by a lengthy introduction.

In 1994, Pagano published an extremely valuable essay review of Brandmüller and Greipl's volume on the Settele affair.[8] The essay is in part a useful and informative introduction to the Settele affair and the issues it raises from the points of view of historical interpretation and philosophical evaluation. Pagano's review is also, in part, a meticulous analysis and correction of the documents published in the Brandmüller volume, from the point of view of erudition; that is, from the point of view of someone who can access the original archival documents and compare and contrast them with the published texts. Thus, Pagano points out errors of transcription, involving abbreviations, punctuation, insertions, omissions, etc.

Now, if I may add a personal note, I can report that, utilizing Pagano's corrections, I corrected my own copy of the Brandmüller volume, to make sure that when I studied it I would be reading a correct version of the original documents. I felt that this was important not only from the point of view of the subsequent Galileo affair, which I was researching, but also from the point of view of Galileo's trial, for the following reason. The Brandmüller volume is a collection of the documents of the 1820 Settele controversy, as well as an attempt to construct an historical interpretation of that episode, and to evaluate its philosophical significance. Such a scholarly challenge is very similar to the problem of collecting, interpreting, and evaluating the documents of Galileo's trial, on which I had been working for some time, and to which Pagano had already contributed with his 1984 volume and would contribute again with another volume published in 2009, to which I now turn.

In 2009, Pagano published another volume of Galilean trial documents similar to his 1984, but with a much more ambitious scope.[9] The newer volume includes more documents and more critical apparatus, thus doubling the length from about 300 pages to about 600 pages. The additional documents, although not new, are

[6] Finocchiaro 1985, 381.

[7] Brandmüller and Greipl 1992.

[8] Pagano 1994; cf. Finocchiaro 2005, 347, 425 n. 44.

[9] Pagano 2009.

valuable, and it is useful to have them included in a single volume. Besides the special Galileo file from the Vatican Secret Archives and the relevant Inquisition minutes from the Archive of the Congregation for the Doctrine of the Faith, Pagano now included some documents from the Vatican Library and some from the archive of the just mentioned congregation that are not minutes of Inquisition meetings. This last mentioned addition calls for some further comments, however brief.

As is well known, in 1998, the Archive of the Congregation for the Doctrine of the Faith was finally opened to all scholars. Since then, it has been searched for material relevant to Galileo's trial, and a few dozens new documents have been found and published. The new documents do not change the basic points of previous knowledge, but they fill in gaps and sometimes raise new questions. The largest group of such documents was published in a journal article by Ugo Baldini and Leen Spruit in 2001, and other contributions have been published by Francesco Beretta, Rafael Martínez, and Mariano Artigas.[10] Clearly, the inclusion of such documents enhances significantly the value of Pagano's 2009 volume.

With regard to the critical apparatus of this volume, the introduction and notes were expanded as follows. The monograph-length introduction (258 pages) contains an historical account not only of the origin and vicissitudes of the special Galileo file, which is largely repeated from Pagano's earlier edition, but also of the Inquisition proceedings against Galileo from 1615 to 1633. The notes now contain biographical sketches of every person named in the documents, and if I may be allowed to quote from my review of this 2009 volume, "I can report having already profited from this novel feature of the book".[11]

Finally, one should mention a very revealing aspect of the 2009 volume. Pagano explicitly mentions that, with regard to two items, he changed his mind in light of scholarly criticism, and so the 2009 edition corrects two errors found in the 1984 edition.

The first concerns a new document which Pagano published in 1984, claiming it to be an original report of both an Inquisition decision made on February 25, 1616 and of the action taken by Cardinal-inquisitor Robert Bellarmine the following day to implement that decision. Pagano's claim had been criticized by several scholars, on the grounds that the document in question did not seem an original report drafted in 1616, but a copy drafted at some later time. Pagano now candidly admits that the document is an eighteenth-century copy.[12]

The second item concerns the origin of the already-mentioned special Galileo file now kept in the Vatican Secret Archives. Pagano now rejects his earlier view that this file originated as a selected collection of documents from the regular volumes of seventeenth-century Inquisition trial proceedings, the criterion of selection being the relevance to questions about the prohibition of books of concern to the Congregation of the Index in the eighteenth century. He now adopts the more tenable view defended

[10] Baldini and Spruit 2001; Beretta 2001; Artigas 2001; Martínez 2001.

[11] Finocchiaro 2010, 652.

[12] Pagano 2009, 176. Cf. Pagano 1984, 222–223; Camerota 2004, 317; Beretta (1999, 467 n. 95; 2005); Beltrán Marí 2006, 307–313.

by Beretta and Baldini that the file is the complete set of documents used by the pope and the cardinal-inquisitors to arrive at their sentence and condemnation of Galileo in 1633; and it was in the eighteenth century that these documents were moved out of the regular Inquisition archive and collected into a separate free-standing file.[13]

Such revisions are refreshing, not merely or primarily in a psychological sense, but also and more importantly in the methodological sense of being instructive. They suggest the possibility that Pagano could perhaps be persuaded to change his mind and revise his views with regard to another issue, which he has studied more intensely and which is the subject of a more recent contribution by him.[14] This is the topic of the Inquisition orders that were issued to Galileo in 1616. For I happen to have also studied this topic carefully, and reached conclusions which agree with his only in part, and which disagree in important ways.[15] And this is the main problem which I want to discuss in this essay, and to which I now turn.

17.2 The Inquisition's 1616 Orders to Galileo

Let us start with some context, however brief and cryptic.[16] In February 1615, a Dominican friar filed a written complaint against Galileo with the Roman Inquisition. In March, another Dominican testified in person before this body. They accused Galileo of heresy, because he believed in the earth's motion, which contradicted many biblical passages, such as Joshua 10:12–13. These accusations were exacerbated by the fact that in 1613 Galileo had written a refutation of the biblical objection to the earth's motion. He was prudent enough not to publish this essay, but to write it as a letter to a former student named Benedetto Castelli; nevertheless, the letter was circulating widely.

The Inquisition launched an investigation. Galileo's letter to Castelli was evaluated by a consultant, who found nothing really wrong with its main argument, but only a few minor blemishes. Other witnesses were interrogated, who did not corroborate the charges of the two Dominicans. However, in the process, the Inquisition officials started wondering about the status of heliocentrism, and so they consulted a committee of advisors.

On February 24, 1616, the consultants reported the unanimous assessment that heliocentrism was philosophically (namely, scientifically) false and theologically heretical or at least erroneous. Although the Inquisition did not formally endorse the heresy recommendation, it accepted the judgments of scientific falsity and theological error, and decided to prohibit the theory. This prohibition was twofold: one

[13] Pagano 2009, pp. ccxvi–ccxix. Cf. Pagano 1984, 4–10; Beretta (1999, 460–469; 2005); Baldini 2000, 332–343.

[14] Pagano 2010.

[15] Finocchiaro 2005, 241–258; 2015, 2016.

[16] For more details, see, for example, Finocchiaro (1989, 1–43; 2014, 1–30); Pagano 2009, pp. xvii–ccx.

part involved Galileo personally, the other involved the Copernican doctrine and Copernican books in general.

On February 25, there was a meeting of the Inquisition, presided by Pope Paul V. Their decision on the Galileo case was communicated to the Inquisition's commissary and the assessor by the cardinal secretary. This communication is recorded in a document included in the Vatican file of Galilean proceedings.[17]

These papal orders envisaged three possible steps. First, Cardinal-inquisitor Bellarmine would give Galileo an informal and friendly warning to abandon the heliocentric geokinetic doctrine. The content of this intended warning is expressed in terms of the notion of abandonment, which means to stop believing, accepting, or holding this doctrine. This, in turn, refers to an internal mental state.

The second step would be taken in case Galileo rejected Bellarmine's warning. Then the Inquisition's commissary would intervene and issue Galileo a formal injunction or precept.

The content of this intended injunction would be much more stringent than the warning: Galileo would be completely prohibited to teach, defend, or even discuss the doctrine. Discussion is here meant in the general sense, so as to include teaching and defending. Teaching refers to two relatively distinct activities: one is the explanation of a doctrine for the sake of conveying an understanding of it; the other is a discussion of the doctrine to render it plausible or acceptable, which would be essentially equivalent to supporting it. Similarly, defending can refer to two things: primarily, answering or refuting objections or counter-arguments; but secondarily, supporting a doctrine with evidence or reasons. Thus, the content of the pope's intended injunction was meant to prohibit Galileo from discussing Copernicanism, understanding that discussion can include explanatory teaching, supporting with reasons, and defense from objections, among other activities.

Note, however, that discussion does not encompass belief or acceptance. One may discuss a doctrine without necessarily accepting or rejecting it, and conversely one may accept or reject a doctrine, and yet refrain from discussing it. Accordingly, the intended injunction does not include the abandonment of the opinion, but rather refers to relatively public activities that are relatively distinct from internal mental states.

The third step mentioned in the document refers to what would happen in case Galileo rejected the commissary's injunction, namely imprisonment. It's unclear how literally one should take the notion of imprisonment, if that meant indefinite prison without a trial. But it is meant to convey at least the notion of being arrested, so as to be prosecuted and tried.

In short, on February 25, Pope Paul V ordered Cardinal Bellarmine to give a friendly warning to Galileo to stop believing in the geokinetic doctrine; if, and only if, Galileo rejected this warning, the Inquisition's commissary was to give him a formal injunction to completely refrain from discussing the doctrine; and if, and only if, Galileo rejected this injunction, he was to be arrested.

[17] Pagano 2009, 45; Galilei 1890–1909, 19: 321; Finocchiaro (1989, 147; 2014, 102).

The following day, February 26, these papal orders were carried out. One description of what happened is given in a memorandum written by a notary, now found among the documents in the Vatican file of Galilean trial proceedings.[18] This document says that Bellarmine warned Galileo that he should abandon the heliocentric doctrine, and, immediately thereafter, the Inquisition's Commissary Michelangelo Seghizzi enjoined Galileo to abandon completely the doctrine and henceforth not to hold, teach, or defend it in any way whatever.

The most striking thing about this document is that it reports the occurrence of the first two steps of the papal orders, without motivating the second step. Bellarmine first issued his friendly warning, but we are told nothing about a refusal on Galileo's part, and yet, allegedly, Commissary Seghizzi immediately went on to issue him a formal injunction, and we are told that Galileo accepted the latter. Of course, Seghizzi's injunction presupposes that Galileo rejected Bellarmine's warning; but if that happened then this memorandum should have reported that development. The fact that the memorandum does not report it suggests that it did not happen—that Galileo accepted Bellarmine's warning; but then Commissary Seghizzi's injunction would be a deviation from the papal orders.

Another question concerns the conceptual content of this injunction. Seghizzi ordered Galileo, among other things, to completely abandon the Copernican opinion. But the pope's intended injunction said nothing about abandonment. The papal orders did mention abandonment as the main point of Bellarmine's warning, but not as part of the commissary's potential injunction. Thus, Seghizzi's precept is in one sense more stringent that the pope's intended injunction, insofar as the former prohibits a private mental state, which the latter had not planned to be the subject of a formal injunction, but rather of a friendly warning.[19]

To make this situation even more problematic, it also turns out that Seghizzi's injunction is in another sense *less* stringent than the pope's intended precept. To be sure, Seghizzi's injunction does forbid Galileo to hold, teach, or defend heliocentrism in any way, and such activities do include the presentation of arguments in favor, the elaboration of explanations of its meaning, and the refutation of arguments against. However, Pope Paul had envisaged that if Galileo rejected Bellarmine's friendly warning to stop accepting Copernicanism, then Galileo would be issued the formal injunction to refrain from *discussing* it. The key point now is that a prohibition of discussion clearly includes a prohibition of refutation and criticism, but the latter are *not* excluded by Seghizzi's injunction. If Galileo had wanted to publish a technical discussion of why Copernicanism was false, or otherwise flawed, this would not be a way of supporting, teaching, or defending Copernicanism, and so would be allowed by Seghizzi's injunction; but it would still be a discussion, and hence banned by the pope's intended injunction.

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[18] Pagano 2009, 45–46; Galilei 1890–1909, 19: 321–322; Finocchiaro (1989, 147–148; 2014, 102–103).

[19] For a legal and theological discussion of the distinction between internal mental states and public cognitive activities, and an application to some important cases, see Kelly (2015, 2016).

Of course, it may seem far-fetched and unlikely that Galileo would want to write or publish such a refutation, but that does not matter. In any case, a plausible example can be considered. Copernicus had attributed to the earth a third motion, besides daily axial rotation and annual heliocentric revolution; this third motion was an annual precession of the terrestrial axis of rotation, supposedly needed to explain the fact that the earth's axis always remains parallel to itself. Now, Galileo believed that such a third motion did not exist, and in the *Dialogue* he argued plausibly and effectively that the parallelism of the earth's axis is an instance of rest or inertia.[20] A technical discussion of this issue would be banned by Paul's injunction, but not by Seghizzi's.

Finally, there is a fourth discrepancy in Seghizzi's precept vis-à-vis the one intended by the pope. This involves the consequences of Galileo's rejection of the injunction. For the pope, the stated threat was imprisonment, which perhaps could be taken to mean arrest; but in the February 26 document the stated threat is prosecution by the Inquisition. And the latter is certainly weaker than the former.

In short, it seems clear that there are four ways in which Commissary Seghizzi's precept does not correspond to the pope's intended injunction. However, it's unclear what conclusion we should draw from this discrepancy, and that is where the controversy arises. But before we tackle that problem, we need to point out other discrepancies.

Seghizzi's injunction also conflicts with two things which Cardinal Bellarmine said and did after meeting with Galileo and Seghizzi on February 26. They are known to us from two crucial documents in which Bellarmine explicitly addresses the question of what happened that day. However, these documents also mention a public decree published by the Congregation of the Index. So it is better to discuss the Index decree first.

On March 5, the Index issued a decree which did not mention Galileo at all, but contained various censures of the geokinetic doctrine and prohibitions of Copernican books.[21] The following points deserve attention.

First, although this decree deals mostly with books (as usual for the Index), it also contains a doctrinal pronouncement (which is quite unusual for the Index, and is usually handled by the Inquisition); that is, the doctrine of the earth's motion is declared scientifically false and theologically contrary to Scripture. Second, a book published by one Paolo Antonio Foscarini in 1615 is condemned and permanently banned, because it argued that the earth's motion is true and compatible with Scripture; that is, this book was trying to do something that goes explicitly against both parts of the Index's doctrinal declaration. However, thirdly, Copernicus's own book is merely banned temporarily until appropriately revised; this decree does not explain why.

These revisions were explained in a subsequent decree by the Index, published four years later.[22] The gist of the 1620 decree was that Copernicus's book was

[20] Galilei 1890–1909, 7: 287, 382, 424–425; 1967, 262–263, 355, 398–399; 2001, 304–305, 412, 462–463.

[21] Pagano 2009, 47; Galilei 1890–1909, 19: 322–323; Finocchiaro (1989, 148–150; 2014, 103–104).

[22] Galilei 1890–1909, 19: 400–401; Finocchiaro 1989, 200–202.

valuable from the viewpoint of astronomical calculation and prediction; that the book was treating the earth's motion primarily as a hypothetical construct, and not as a description of physical reality; that one could easily delete or rephrase the few passages where the book treated the earth's motion as physically real or compatible with Scripture; and that specific instructions were given about how to delete or rephrase the dozen passages where this happened. Such clarifications and corrections may be taken as being implicit already in the 1616 decree.

Thus, we can now better understand a fourth and final point about books in general, explicitly made in the 1616 decree. Books like Foscarini's were also condemned and permanently banned; whereas books like Copernicus's were suspended until and unless revised, to render them hypothetical. This meant presumably that no Catholic could do what Foscarini had tried to do, namely to support the physical reality of the earth's motion, or to defend it from the objection that it contradicts Scripture. On the other hand, Catholics were free to discuss, support, and defend the earth's motion as an hypothesis for the convenience of saving the appearances and making mathematical calculations and astronomical predictions, as Copernicus had allegedly done.

Let us now go back to the two pieces of evidence from Bellarmine. The first pertains to the next Inquisition meeting presided by the pope, after the one of February 25. On March 3, Bellarmine reported what he had done. The minutes of this meeting have been preserved and are found in the Inquisition archive.[23]

Here, several things are worth noting. First, Bellarmine says that Galileo accepted his warning to abandon the geokinetic doctrine. Second, the cardinal says nothing about any injunction issued to Galileo by Commissary Seghizzi; and of course, this is in accordance with the pope's earlier orders, which made the injunction contingent on Galileo's rejection of the warning. Third, the pope ordered the publication of the Index decree, which is summarized in terms of the distinction between the temporary suspension of books like Copernicus's and the complete prohibition of books like Foscarini's.

The other action by Bellarmine that conflicts with Seghizzi's injunction involves a certificate, dated May 26, 1616. Bellarmine wrote it at the request of Galileo, who in April received letters from friends saying that there were rumors to the effect that he had been put on trial, condemned, forced to recant, and given appropriate penalties by the Inquisition.[24] The document was presented by Galileo in his own defense at the 1633 trial, and so it is found in the special Vatican file.[25]

This short (one paragraph) document deserves careful analysis. First of all, Bellarmine speaks only of his warning, and not of a formal injunction by Commissary Seghizzi. But here Bellarmine elaborates the content of his own warning. Previously (March 3), he had described it merely as ordering Galileo to abandon Copernicanism, meaning to stop believing in it. Now (May 26), Bellarmine gives a clarification and

[23] Pagano 2009, 177; Galilei 1890–1909, 19: 278; Finocchiaro (1989, 148; 2014, 103).

[24] Galilei 1890–1909, 12: 254, 257–259.

[25] Pagano 2009, 76, 78–79; Galilei 1890–1909, 19: 342, 348; Finocchiaro (1989, 153; 2014, 105).

an elaboration involving a reference to the Index decree and an explicit mention of the notions of defending and holding.

The meaning of these notions of defending and holding here may be taken to be the same as the meaning we took them to have in our earlier discussion; that is, they refer to believing, supporting with reasons, and defending from objections. However, the reference to the Index decree specifies their meaning even further. Bellarmine is saying partly that what is prohibited is to believe, support, or defend the earth's motion as true or as compatible with Scripture, in line with the condemnation and complete prohibition of Foscarini's book. But Bellarmine is also saying that there is no prohibition on believing, supporting, or defending the earth's motion as a convenient hypothesis for saving the appearances and making calculations and predictions. This additional permissive clarification is in accordance with the mere suspension of Copernicus's book, published by the Index two months earlier, and in accordance with the subsequent correction of that book, in the 1620 decree. In short, the May 26 certificate embodies what is perhaps the most important and authoritative Inquisition order to Galileo.

However, from the point of view of rigor and stringency, we should note the following sequence. First, the mildest order is the warning intended in the pope's February 25 decision: namely, to abstain from believing the earth's motion. The second-weakest order is the one described by Bellarmine in his May 26 certificate: namely, to abstain from believing, supporting, or defending the doctrine as true or as compatible with Scripture. Third, there is the formal injunction presumably delivered by Commissary Seghizzi according to the February 26 document: namely, to abstain from believing, supporting, defending, or teaching the doctrine in any way whatever. Fourth, the most stringent order is the injunction intended by Pope Paul in case of Galileo's rejection of Bellarmine's warning: namely, to completely abstain from discussing the doctrine.

As mentioned earlier, publishing a refutation of Copernicanism would violate Paul's intended injunction, but not any of the other three orders. Now we can add that an explanation of the content, the arguments in favor, and the arguments against Copernicanism would violate Seghizzi's injunction, as well as the one intended by the pope, but neither of the other two orders; such an explanation would be a so-called disputation, in the traditional sense. The same would be true if one were to give a demonstration that the geokinetic doctrine is better than the geostatic one as an hypothesis for saving the appearances and making calculations and predictions; it would violate the two stronger orders, but not the two weaker ones. A simple and clear violation of the last three orders, but not of the first, would be a defense of Copernicanism from the objection that it contradicts Scripture; and this of course is found in Foscarini's book and in Galileo's unpublished letter to Castelli.

17.3 Critical Review of Pagano's Account

As mentioned above, in 2010, Pagano published a long journal article elaborating an account of the Inquisition's 1616 orders to Galileo. The most valuable part of Pagano's account is his summary and reinforcement of the arguments and evidence showing that Commissary Seghizzi's special injunction is authentic and not a forgery. This is important because for about a century and one-half, since the February 26 document was first published,[26] there has been a persistent controversy about this issue, that is, the question of the authenticity of this document. Many attempts have been made to show that this document is a forgery, and one of Pagano's accomplishments is to undertake a critical examination of such arguments and evidence; to show their untenability or implausibility; and to strengthen the case for authenticity. In a moment, I shall discuss, some of these arguments and counter-arguments. This will have to be brief, not only because of space limitations, but also because I want to advance a number of criticisms of Pagano's account, and these are no less important than his authenticity thesis. In fact, it may be useful to preview and highlight these criticisms.

One difficulty with Pagano's account is his failure to distinguish clearly, or at all, between Bellarmine's and Seghizzi's orders. For example, he entitles his 2010 article "Cardinal Bellarmine's Precept to Galileo: A Forgery? …"; and yet it is obvious that what Pagano is discussing there is whether Seghizzi's precept was a forgery. Similarly, in his 2009 collection of documents, the title which Pagano gives to the February 26 document is "The Admonition of Cardinal Robert Bellarmine to Galileo Galilei"; and yet this is the only document that explains Seghizzi's precept. All this is misleading and confusing.

It's not so much that Bellarmine's order is a friendly warning and Seghizzi's is a formal precept. Nor is the crux of the difficulty that Bellarmine's order is much less stringent and inclusive than Seghizzi's, the difference being that between the prohibition to hold as true or as biblically compatible and the prohibition to hold in any manner. More consequential is the fact that whereas Galileo freely admitted receiving Bellarmine's order, he essentially denied receiving Seghizzi's; this emerged in his first deposition (April 12, 1633).[27] Moreover, no scholar has ever questioned the authenticity of Bellarmine's warning, whereas many have argued that Seghizzi's precept is a forgery. Nor have any scholars questioned the factual accuracy of the Bellarmine part of the February 26 document, whereas many have argued that the Seghizzi part is inaccurate. And similar remarks apply with regard to the juridical legitimacy of the two orders: no one has questioned the legal validity of Bellarmine's admonition, but many have argued that Seghizzi's injunction was illegitimate.

Up to a certain point, Pagano's indiscriminate talk of "Bellarmine's precept" and "Bellarmine's admonition" to refer to Seghizzi's injunction may be merely a semantical or linguistic infelicity, and so this first difficulty is relatively minor. A more

[26] Épinois 1867, 98–99 n. 2. Cf. Pagano 2010, 144–156; Finocchiaro 2005, 241–258.

[27] Pagano 2009, 66–72; Galilei 1890–1909, 19: 336–342; Finocchiaro (1989, 256–262; 2014, 122–127).

substantive and substantial difficulty is one that emerges once we have agreed and are clear that we are talking about the order which the February 26 document says was delivered by Commissary Seghizzi, and which amounts to a prohibition to believe, support, defend, or teach the earth's motion in any way. This precept or injunction was, as we have seen, a deviation from Pope Paul V's orders of February 25, and it contradicts Cardinal-inquisitor Bellarmine's testimony given on two occasions, on March 3 and on May 26. Thus, I would argue, Seghizzi's injunction was legally invalid or judicially illegitimate. That is, I think that the discrepancies between Seghizzi's injunction and the pope's orders and the cardinal's reports imply that it had no legal standing. And this implication holds even if the February 26 document is authentic. Unfortunately, Pagano seems to focus on the question of authenticity, and argues plausibly in its favor, but then he thinks he is thereby also showing its legitimacy.

Actually, not only must we distinguish between authenticity and legitimacy, but also between each of them and factual accuracy. The issue of factual accuracy involves the question of what really happened on February 26; whether what the document says really took place; whether it is factually or historically true. On this issue, Pagano also holds that Seghizzi's injunction really took place as described in the February 26 document; and he seems to think that the same arguments that prove its authenticity also prove its accuracy. However, I don't think that such reasoning is sound; it involves a misconceived conflation of authenticity and accuracy. On the other hand, it is also not right to infer inaccuracy from illegitimacy; thus, although I claim that Seghizzi's injunction was illegitimate (as mentioned above), I believe the question of its factual accuracy is open.

In short, with regard to Bellarmine's warning, both Pagano and I (like everyone else) agree that it is authentic, accurate, and legitimate. With regard to Seghizzi's precept, Pagano claims that it is also authentic, accurate, and legitimate; and this thesis betrays a confusion among these three concepts. On the other hand, I hold primarily that Seghizzi's injunction is illegitimate, but that it is authentic, and that it remains undecided whether it is accurate.[28] For I also hold that it is as incorrect to infer forgery or inaccuracy from illegitimacy (as many anticlerical scholars have done), as it is to infer accuracy or legitimacy from authenticity (as Pagano is inclined to do).

[28] It is worth pointing out that Pagano is not the only scholar who confuses these three concepts; another example is Mayer 2015, as shown in Finocchiaro 2015 (chapter 15 in this book), and in Finocchiaro 2016, 52–56. Conversely, I do not want to give the impression that I myself have just discovered the wheel by elaborating and stressing this distinction, for it has been made in works such as the following: Gebler 1879, 334–340; D'Addio 1985, 49–52; Beretta (1998, 277; 1999, 481); M. Camerota 2004, 318; Fantoli 2005, 133. And there is also at least one borderline case, that is, Frajese 2010: for on the one hand, like Pagano, Frajese seems to succumb to the linguistic ambiguity of the Italian term "falso"; but on the other hand, Frajese sometimes (pp. 56, 75) seems to make the distinction.

17.4 Some Particular Arguments

One of the oldest questions regarding the authenticity of Seghizzi's injunction also happens to be one of those most conclusively disposed of. Starting in 1870, some scholars have claimed that the February 26 document was altered by erasing the original wording (which was consistent with Paul V's February 25 orders and with Bellarmine's later reports) and overwriting what can be read today. Let us recall that the February 26 document is the only evidence for the occurrence of Seghizzi's special injunction and that it conflicts will all other original documents. In part, such a forgery claim is an attempt to explain these facts; and, in part, it is also invalidly inferring inauthenticity from illegitimacy.

However, in 1926, German scholar Rudolf Lämmel requested and received permission from the Vatican to view this document under ultraviolet light, and the following year he obtained an ultraviolet photograph of the document. The experiment revealed no traces of erasure and rewriting, which refuted that version of the forgery conjecture. In his 2010 article, Pagano summarizes this discussion and relates the previously unknown details of the story of Lämmel's experiment. It is a fascinating story, which includes the following interesting detail. Originally, Lämmel wanted to subject the document to X-rays; but after Vatican officials consulted several experts, there was a consensus that this would have damaged the document, and so the X-ray experiment was denied.[29]

Another question with the February 26 document stems from the fact that it is written on two different folios, the bottom third of one folio (f. 43v) and the top third of the following one (f. 44r). The problem here is that the writing on the two folios looks different: on f. 43v, the writing is relatively small, full of abbreviations, and with little space between words and between lines; whereas the writing on f. 44r is larger, with fewer abbreviations, and with more space throughout. The relevance of such graphological and paleographical considerations is that it is on f. 44r that is written the conceptual content of Seghizzi's injunction, in terms of a prohibition to believe, support, defend, or teach Copernicanism in any way, which prohibition is much stronger than Bellarmine's warning. This version of the forgery thesis claims that the second part of the February 26 document was written at some later time by some other person, who replaced the original wording with the current one.[30] Such a replacement was accomplished not by erasure, but by replacing the original folio with another one that was already in the file but blank; in fact, between folios 43v and 44r there is evidence that a folio is missing.

In his 2010 article, Pagano argues plausibly that the writing on the two folios is by the same hand.[31] Although it looks different with regard to spacing and abbreviations, the shape of the characters is essentially identical, and various idiosyncrasies of

[29] Pagano 2010, 156–168; Lämmel 1927.

[30] Dorn 2000.

[31] Pagano 2010, 169–172. The same conclusion has recently been advanced by other scholarly experts in graphology and paleography: Emma Condello, consulted by Frajese (2010, 57 n. 17, 103–106); and Isabella Truci, consulted by Fantoli (2010, 256–257).

handwriting are common to both parts of the document. Pagano does not merely report this, but also illustrates it visually by reproducing several photographic images of the writing. This graphological and paleographical evidence seems conclusive to me. Moreover, Pagano (following Francesco Beretta) adds that the writer can be identified as Andrea Pettini, who was the Inquisition notary at the time (1616).[32]

These two arguments will have to suffice here as examples of how Pagano supports and defends the authenticity of the February 26 document. They are perhaps the two most important examples regarding this particular issue. However, as mentioned above, there are other issues, regarding factual accuracy and juridical legitimacy, and they involve different kinds of evidence and argument. Let us now look at some of Pagano's arguments involving the issue of legitimacy.

Recall (from above) that my own case for the illegitimacy of Seghizzi's injunction depends, not on the alleged inauthenticity of the February 26 document, but on the indisputable fact that the precept deviates from Pope Paul's orders and also conflicts with Cardinal Bellarmine's testimony. Let us now ask whether this is really indisputable, whether there are objections or counter-arguments to the illegitimacy thesis, and whether Pagano has anything to say about this issue.

One weakness of Pagano's account is that he seems to completely ignore the discrepancy between Seghizzi's February 26 precept and Pope Paul's February 25 orders. This is a very serious weakness, indeed fatal (I am inclined to believe).

Leaving that aside, the most relevant argument advanced by Pagano in this regard is based on his interpretation of Bellarmine's silence about the special injunction in his statements of March 3 and May 26. Recall that on March 3, Bellarmine reported to the Inquisition that he had carried out the pope's orders: he had given Galileo the friendly warning, and Galileo had acquiesced. And in the May 26 certificate, Bellarmine wrote that the only order which Galileo had been issued was a warning that heliocentrism could not be held or defended as true or as biblically compatible, but merely as an hypothesis to save the appearances.

Pagano claims that Bellarmine had no obligation to reveal the detail of Seghizzi's precept either to the pope and cardinal-inquisitors at the meeting of March 3, or in the May 26 certificate written on Galileo's behalf for public consumption. On March 3, "the cardinal was synthetic … in relating the essential point of the action: warning to Galileo and his promise of obedience."[33] And on May 26, "the cardinal … clarified in a generic manner the purpose of the scientist being called to his house on February 26: to notify him of the Index decree that would be published in a few days and to warn him to comply with its prescriptions."[34]

We can agree on the desirability and indeed necessity of being "synthetic" and "generic" in situations like these. Clearly one cannot and should not report every detail of what happened, no matter how insignificant. However, even synthesis and generality require the inclusion of significant, important, and essential details. The prohibition to hold or defend heliocentrism in any way was significant enough to be

[32] Pagano 2010, 181; Beretta 1999, 475–476.

[33] Pagano 2010, 192.

[34] Pagano 2010, 194.

included in Pope Paul's orders. Hence, it would have been a significant detail worth reporting, if it had actually happened or if it was legally relevant or justified. Thus its omission from Bellarmine's report shows that it did not occur and/or was not legitimate.

In other words, Pagano's interpretation seems committed to regarding Bellarmine as a liar; that is, as someone who is not telling the whole truth. However, truthfulness requires that one tell the truth, the whole truth, and nothing but the truth. Such a consequence is a kind of *reductio ad absurdum* of Pagano's interpretation. The absurdity is to treat Bellarmine as a liar.

At this point, it could be objected that, given that Bellarmine was being truthful, and given (as shown by Pagano) that the February 26 document is authentic, then either one or both of two consequences follow. The first is that the February 26 document is factually inaccurate, and Commissary Seghizzi and/or notary Pettini were liars. The second is that the special injunction was illegitimate, and Seghizzi acted improperly or illegally (in light of the pope's orders and of the cardinal's judgment).

I would answer this objection by arguing that, although such consequences do indeed follow, they are not at all absurd. Bureaucrats and officials do not always tell the truth or act in accordance with the law. Here we have an instance when Seghizzi did not. The contrast is with attributing a major and needless lie to Bellarmine, who after all was a saint (being canonized by Pope Pius XI in 1930).

17.5 Conclusion

This essay has been a critical appreciation of Mons. Sergio Pagano's 2010 article "Cardinal Bellarmine's Precept to Galileo: A Forgery?", and the two related volumes which he edited in 1984 and 2009, respectively: *The Documents of the Trial of Galileo Galilei* and *The Vatican Documents of the Trial of Galileo Galilei (1611–1741).* Pagano's article deals with what is perhaps the most important and controversial document and action in the Inquisition proceedings against Galileo in 1615–1633, as well as in the subsequent controversy that continues to our own day; that is, the special precept issued to Galileo on February 26, 1616, by the Inquisition Commissary Michelangelo Seghizzi, prohibiting Galileo to believe, support, defend, or teach in any way the Copernican doctrine of the earth's motion. Pagano argues convincingly and conclusively that the February 26 document is authentic and not a forgery; this is an important accomplishment because it settles a controversy that has raged since the 1860's, with wide-ranging cultural repercussions.

However, the documentary authenticity of the precept does not prove its factual accuracy, and still less its legal validity. Indeed, I believe it can be shown that it was illegitimate, because it deviates from the orders decided upon the previous day by the Inquisition and Pope Paul V, and because it conflicts with the testimony of Cardinal-inquisitor Robert Bellarmine given on two occasions (March 3 and May 25, 1616). On the other hand, I regard it as an open question whether Seghizzi's precept

as described in the document actually happened. Pagano has a tendency to conflate and to equivocate among these three concepts: documentary authenticity, factual correctness, and legal validity. Moreover, Pagano tends to conflate the conceptual contents of four distinct Inquisition orders to Galileo regarding Copernicanism: Pope Paul V's intended order not to believe; his intended order not to discuss; Cardinal-inquisitor Bellarmine's friendly warning not to hold or defend as true or as biblically compatible; and Commissary Seghizzi's injunction not to hold or teach in any way.

In short, Pagano's contributions to Galilean scholarship help us settle one important problem in the Galileo affair: the document of February 26, 1616, is indeed authentic and not a forgery. However, this does not prove that the document is factually accurate or juridically legitimate. On the contrary, this thesis helps us be clear and understand better that and why the document is illegitimate. The Galileo "case" is not "closed," but remains controversial.

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Part IV
Galilean Scholarship

Chapter 18
Shapere's *Galileo*: Philosophy vs. History vs. Erudition

Abstract In an attempt to understand and to evaluate Dudley Shapere's *Galileo: A Philosophical Study,* a number of philosophical problems, ideas, and opportunities concerning the study of Galileo are discussed. As a basis for discussion, a summary of the book is first given. Then, to avoid misunderstanding criticism, some of its scholarly and historiographical deficiencies are identified, thus determining what the book is *not* and also illustrating the differences among scholarship, historiography, and philosophy. After interpreting the work as a much-needed philosophical contribution to the theory of scientific rationality, and after an appreciation of the specific philosophical problems to which the book addresses itself, it is argued that the book is beset by a serious inconclusiveness. Finally, in a major section of the essay, it is argued that this inconclusiveness can be remedied by appropriately modifying Shapere's basic approach; for when Galileo is studied with such a modified approach, his work becomes the basis of conclusions about scientific method and rationality, and the testing ground for the application of logical concepts such as deductive and inductive validity and for ideas about the nature of philosophy and its proper relation to science.

18.1 Summary of Shapere's Book

Shapere's (1974) *Galileo* begins with a chapter entitled "Galileo and the Interpretation of Science," which consists of two parts: a statement of the aim and method of the book, and a sketch of Galileo's life. The stated aim is to study some of Galileo's ideas and certain aspects of his method in order to understand the rise of modern science, Galileo's role in it, and thus the nature of scientific development in general, and ultimately the nature of the scientific enterprise itself. The method he will follow, Shapere tells us, is to examine some of Galileo's statements and arguments in the light of a number of interpretations, which will thereby be critically examined. The major interpretations he describes are: the one propounded by Ernst Mach, and to which physics textbooks typically pay lip service, claims that Galileo was an empiricist and a revolutionary innovator; the one developed by Pierre Duhem claims that

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Galileo was the continuator and culminator of a medieval tradition of Aristotelian critics and commentators; the one elaborated by Alexandre Koyré claims that Galileo was an aprioristic intellectualist (a 'rationalist' is Shapere's word) and a Platonist.

In the second chapter, entitled "The Intellectual Background," the author lays the groundwork for his examination of Galileo's originality, which is a feature dealt with in each of the three prevailing interpretations. Shapere does this by summarizing what he thinks are the relevant views of Plato, Aristotle, the fourteenth-century impetus theorists, and previous astronomical thinkers.

The third chapter is entitled "The Early Development of Galileo's Thought" and includes a section on his Pisan period and one on his Paduan period. The "Pisan Period" is a clear exposition of the main ideas in Galileo's *On Motion* and a plausible interpretation of it as being partly Aristotelian, partly Archimedean, and partly impetus-theoretic in intellectual content. The "Paduan Period" notes that Galileo's ideas at that time corresponded basically to those found in *Two New Sciences,* which raises the question of how he arrived at these from his Pisan theory; Shapere then concentrates on the problem of how Galileo originally arrived at the law of squares, discusses various available interpretations, and concludes that the historical evidence is inconclusive.

Chapter four, entitled "Galileo and the Principle of Inertia," discusses the evidence from the *Dialogue,* that from the *Discourse,* and some problems about determining to what extent Galileo approached the modern conception of inertia. As regards the *Dialogue,* Shapere argues that Galileo tries to establish the following five propositions: (1) "anything but rest or circular motion is contrary to the orderliness of the universe" (p. 87); (2) "natural circular motion is uniform and perpetual" (p. 89); (3) "the only purpose that might possibly be attributed to straight-line motion is that it could be used to create or restore orderliness" (p. 91); (4) "if non-circular order-restoring motion does indeed exist, it would be accelerated and rectilinear" (p. 93); (5) "really, what appears to be natural accelerated straight-line motion is uniform and circular after all' (p. 98). Concerning the *Discourse,* Shapere has the following argument: although Galileo's analysis of the parabolic trajectory of projectiles uses the idea of a rectilinear horizontal natural motion, in his reflections on the problem he claims that the rectilinearity of the horizontal component is only an approximation, since he says that such a component follows the curvature of the earth; hence, argues Shapere, Galileo's inertial component is really circular, from which he concludes, interestingly, that Galileo's commitment to Copernicanism is primary even in the *Discourse.* Shapere's final conclusion is that, although Galileo's conception of natural motion has something in common with "inertia," it is far from approaching the latter but is rather the culmination of traditional ideas (p. 87), and that the Drake-Koyré dispute about whether Galileo understood the essence of "inertia" is semantical (p. 125).

The last chapter, "Reason and Experience in Galileo's Thought," is basically a criticism of Koyré's Platonic, "rationalistic" (i.e. aprioristic) view of science as it is based on Galileo's work. Shapere argues (rightly) that his use of the Socratic method does not imply epistemological innatism or apriorism since Galileo claims to be reminding us of experience we may have had already but forgotten. As regards

Galileo's mathematical approach, Shapere argues that he was somewhat ambiguous about its power to give a full understanding of nature; for Shapere it is more important that such a mathematical approach is no less anti-Aristotelian than anti-Platonic, since both Plato and Aristotle agreed that mathematics cannot give an adequate account of the physical world, and disagreed only on their reasons for this view. He concludes that Galileo's practice was not and could not have been aprioristic ("rationalistic"); that it was partly original and partly traditional (Medieval); that it was mathematical as well as empirical, not in the simplistic Machian sense, but in an hypothetico-deductive manner.

18.2 Scholarly Problems

Galileo scholars can and will find fault with this book for reasons such as the following. In his discussion of Galileo and inertia, Shapere claims that one proposition that the *Dialogue* tries to establish is that "if non-circular order restoring motion does indeed exist, it would be accelerated and rectilinear" (p. 93). The following passage is then presented as containing one of Galileo's supporting arguments:

> Every body constituted in a state of rest but naturally capable of motion will move when set at liberty only if it has a natural tendency toward some particular place; for if it were indifferent to all places it would remain at rest, having no more cause to move one way rather than another. Having such a tendency, it naturally follows that in its motion it will be continually accelerating. Beginning with the slowest motion, it will never acquire any degree of speed without first having passed through all the gradations of lesser speed—or should I say of greater slowness? For, leaving a state of rest, which is the infinite degree of slowness, there is no way whatever for it to enter into a definite degree of speed before having entered into a lesser, and another still less before that. It seems much more reasonable for it to pass first through those degrees nearest to that from which it set out, and from this to those farther on. [pp. 93, 94][1]

In analyzing this passage Shapere's main point is that "the argument does *not* prove that a body moving in natural straight-line motion will continually accelerate; all it shows (assuming the Principle of Continuity, of course) is that, *if* the body is to arrive at any particular finite non-zero velocity, it will have to pass successively through all lesser velocities; it does not show that when any given speed is reached, the body will continue to increase (rather than maintain or decrease) its speed beyond that until it arrives at the goal" (p. 94). However, Galileo is in fact inferring *only* what the argument allegedly shows: he is *not* drawing the consequence that allegedly does not follow. This can be seen from Galileo's original Italian text, where the conclusion reads that the body "*nel suo moto si anderà continuamente accelerando*."[2] Shapere apparently was misled by Stillman Drake's translation of *'continuamente accelerando'* as 'continually accelerating' and interpreted 'continually' to have a meaning distinct from 'non-discontinuous', which it sometimes has. In other words,

[1] Quoted by Shapere from Galilei 1953, 20.

[2] Galilei 1890–1909, 7: 44.

Shapere is distinguishing two kinds of accelerations: non-discontinuous and non-stopping. He thinks that Galileo is in this passage talking about both, whereas Galileo is only talking about the former.

However, such knit-picking would be basically misconceived. For though it is part of Shapere's aim to be as scholarly as possible (p. x), and though scholarly accuracy is a general methodological duty, nevertheless scholarship is not the animating force behind Shapere's work. Moreover, in attempting a synthesis of recent Galileo scholarship, the book does something valuable which too many specialist-scholars too often fail to do, preferring instead the comforting safety of exploring small and fine points. Perhaps scholars who have not produced such syntheses lack the right to make any serious objection to those who have; and to say this is not cheap *ad hominem* reasoning, but rather an attempt to make a distinction between atomistic, analytical scholarship and wholistic, synthetic scholarship, and to suggest that different criteria of evaluation apply.

18.3 Historiographical Problems

It is at this point that general historians (of science) may enter the scene; for they can and will object to the book for reasons like the following. In his discussion of Galileo's intellectual development, Shapere points out, following Duhem, that there were only three steps left to arrive at an inertial physics from Buridan's impetus theory: "first, the conception of the possibility of motion in a void or vacuum; second, the abandonment of the idea that there is circular impetus as well as rectilinear; and finally, the abandonment of the idea of impetus as an internal force distinct from the body and maintaining it at constant speed" (pp. 50–51). Hence, he concludes, "if this is a true historical picture, the Impetus Theory would thus stand as a transitional phase between these two traditions" (p. 51). Now, at this point a historian would try to determine whether, or to what extent, "this is a true historical picture." Instead, Shapere proceeds to ask, "If the Impetus Theorists were in this respect so close to modern physics … why did the transition—if it did, as a matter of historical fact, take place via the three steps mentioned—take so long?" (p. 52). The parenthetical expression in this quotation may be an indication of the attitude that questions of historical fact are themselves "parenthetical"; it may also be a natural rhetorical device to express ambiguity as to which of two distinct questions are being answered: (1) Why did the transition along these three steps take so long? (2) Why did *not* the transition take place along these three steps? What Shapere proceeds to do is to discuss the intellectual obstacles to the transition in question (pp. 52–56). The expressed ambiguity hides the fact that the question that has really been answered is: Why did not the transition take place along these three steps, or *if* it did, why did it take so long? In other words, the intellectual obstacles presented by Shapere are indeed both plausible and likely historical causes, but they are either dilatory or preventive causes, and he gives virtually no evidence or even an argument to exclude the preventive cause alternative. It is not, of course, *my* task here to show that the

transition did not take place, since my point is that Shapere has not shown that it did take place; worse, he has not even given any evidence for it; he has only given the above-mentioned ambiguous argument, and hence not really argued, but rather insinuated that modern physics originated from the impetus theorists.

A second example is the anti-Koyré argument where Shapere tries to show that Galileo's practice was not "rational" because it *could not* have been (pp. 132–133). I do not intend to give the impression that this is Shapere's only argument in this context, for he also gives evidence (rightly, I believe) that Galileo's explicit attitude toward Platonism was ambiguous and evasive and that his actual practice was not in fact Platonistic-rationalist (pp. 131–132). However, the apparently "philosophical" argument mentioned above will offend historians' sensibilities, though it is doubtful whether they will be able to give a reasoned criticism of it and show that it is self-inconsistent or circular, or both. In fact, Shapere's premise that Galileo's practice could not have been rationalistic is to be justified either a priori ("rationalistically") or a posteriori (empirically). He hints at an aprioristic justification when he italicizes the 'could' and thus indicates that he is referring to a logical or conceptual possibility; and he hints at an empirical justification in saying that "the history of science and philosophy has not dealt kindly with" (p. 144) rationalism in the sense under discussion. At any rate, the aprioristic justification would itself be rationalistic, and thus it would be doing or trying to do what Shapere's conclusions claims to be impossible; whereas the empirical justification would have to be based on historical case studies showing, for example, that Galileo's practice was not in fact rationalistic, which is precisely what Shapere's present argument tries to show.

Once again, however, such fault finding would be basically misconceived. Shapere's book may be about historical events, but it is not a historical work; its animating principle is not the historical sensibility, and thus any violence to it detracts little from the book's intrinsic value.

18.4 Philosophical Problems

Philosophy ought not to be equated with either scholarship or history, however much one may love both. A philosophical work ought to be judged by philosophical standards, and Shapere's book is "a philosophical study." Now, some philosophers may ask themselves what is the relevance of such a study. The answer is that philosophy at its best is not theoretical and abstract but applied and concrete. The present work is one in the philosophy of science. The philosophy of science may be conceived either as the philosophical study of contemporary scientific knowledge or as the philosophical study of past scientific achievements. The former centers around contemporary science-textbooks, the latter around past scientists. There is certainly no good reason for regarding the former approach as superior to the latter: the latter need not be a duplicate history of science, any more than the former need be popular science; nor need the second approach degenerate into psychology, any more than the first need degenerate into linguistics, library science, communication theory, or pedagogics.

What seems undeniable is that philosophical reflection is an activity that can be exercised irrespective of the objects of study. In a sense, the two approaches are distinct in a complementary way. For contemporary textbooks are paradigm examples of scientific knowledge, whereas past great scientists are paradigm examples of scientific rationality. So what we have is the study of scientific knowledge and the study of scientific rationality. Now, scientific knowledge and scientific rationality are *prima facie* two more or less equally important elements of science; hence a comprehensive philosophy of science cannot limit itself to either one. Nevertheless, one cannot talk or think about everything at once, and so, for some purposes, one may limit himself to the study of scientific rationality. Shapere's book is then a work in the theory of scientific rationality. As such it is to be welcomed since it helps to correct the prevailing over-emphasis of work in the theory of scientific knowledge.

In studying scientific rationality as we find it in Galileo, Shapere realizes that it is likely to contain a number of distinct elements, rather than being a monolithic unit. So he selects three distinct aspects of Galileo's work: the laws of falling bodies, his ideas on "inertial" motion, and his "scientific method." These three elements are well chosen since they are traditionally and still widely regarded as Galileo's great contributions to science. If we accepted these three contributions as great, epoch-making, and paradigmatic scientific achievements, then we could study Galileo at work in these three areas, try to determine whether or how he is aprioristic, or empiricist, or mathematicist, or Platonist, and thereby learn what makes a good scientist and understand the nature of scientific rationality. Shapere follows this approach for the case of the laws of falling bodies (pp. 78–86). However, he reaches no conclusion here about the nature of scientific attitude because he thinks that the evidence is insufficient to enable one to determine how Galileo originally arrived at the law that the distance fallen varies as the square of the time.

Unfortunately, Shapere's study of Galileo's ideas on inertia seems no more conclusive. In fact, though he is willing to say rather clearly and explicitly that and how Galileo arrived at his principle of natural circular motion, Shapere also points out that such a principle not only never became a part of modern science but misses the main point of modern physics. According to Shapere, Galileo tried to deduce natural circular motion from the conception of the orderliness and perfection of the universe in an attempt to seek a deeper and more consistent cosmology than Aristotle's. Presumably then, in this case, we know the method used to arrive at a scientifically worthless idea. But this seems to inform us about scientific irrationality rather than rationality. This is especially true since Shapere argues that virtually all of Galileo's justifications of his circularity ideas are logically invalid, so that in this aspect of his work Galileo would be a paradigm example of irrationality, in a double sense, namely that his conclusion as well as his arguments are wrong.[3]

[3] This problem cannot be dismissed by saying that all that Shapere is trying to do is "to find out the facts" about some of Galileo's thinking, and that philosophers can thereby reach any conclusion they want. For to say this would be to regard the book as a scholarly or historical work, which interpretation would be neither accurate nor generous.

The problem with Shapere's examination of the third important aspect of Galileo's work is of a still different nature. It is to be found in the chapter entitled "Reason and Experience in Galileo's Thought," which is a discussion of the latter's explicit ideas and reflections on scientific method, as distinct from Galileo's method per se. In other words, it is an examination of Galileo's so-called philosophy of science, as distinct from the method inherent or implicit in his science. Unfortunately when Shapere tries to extract a philosophy of science from Galileo, it evaporates: allegedly he does not have one, if we mean a systematically and consistently worked out theory of scientific rationality. For Shapere argues that Platonistic rationalism is not an element of his philosophy (pp. 132–133), and "concerning the limits (if any) of mathematical treatment of nature, it appears that Galileo's position was not clearly formulated, and probably he was not even fully cognizant of the issue" (p. 138). So we cannot put our hands on a Galilean "philosophy of science." But even if we could, Shapere recognizes and in part argues that its relevance would have to be justified by the extent to which that philosophical theory corresponded to his scientific practice. Hence this part of Shapere's book is as inconclusive as the previous two regarding the nature of scientific rationality. What it does seem to amount to is an argument that it is of little or no value to study Galileo's philosophy of science. And someone who seriously proposed such an argument would have to face the (possibly rhetorical) question of whether there does not exist a better argument for the same conclusion, namely silence.

18.5 Toward a Theory of Scientific Rationality

I do not think, however, that the situation is as desperate as it has appeared so far. In fact, suppose we agree with Shapere that there is no way of establishing from the available evidence how Galileo first arrived at the law of squares. It turns out that this lacuna is largely inconsequential and that the evidence is not lacking for what is really needed. What we need is an explanation of Galileo's sources concerning the law of squares. In fact, Shapere is right in saying that "the importance of the controversy between these three alternative interpretations of the sources of Galileo's correct law of falling bodies must be recognized: for each of the three possibilities lends credence to a different interpretation of the rise of modern science" (p. 86). But if this argument is to be valid, the source of Galileo's law of falling bodies cannot be his method of first arrival at the law. For the fact that a particular scientist (however important) arrived at a particular law (however important) in a certain way would give little support to the generalization that modern science as a whole originated that way. On the other hand, this generalization would be *significantly* supported if we could assert that a particular important scientist "arrived at" a particular important law *because* he proceeded in a certain way. 'Arrived at' here would mean 'was able to arrive at' or 'was successful in arriving at' or 'worked successfully with'. Now, the kind of evidence needed to explain Galileo's success with the law is evidence relating to what he did with it *after* he became convinced that it was true, of which there is an

abundance. Shapere himself refers to some of it and describes it as "searching for a proof of the relationship" (p. 81). This description is correct at a phenomenological level, but the search for a proof can be given a deeper interpretation by showing it to constitute, as I have done elsewhere,[4] a search for an explanation.

Turning to "inertia," can the methodological inconclusiveness of Shapere's account be remedied? I believe it can if we modify his approach slightly. His approach seems to have the following elements: (1) concentration on the context of justification, as distinct from the context of discovery; (2) comparison of the historical material with present-day textbooks; and (3) logical analysis, that is, reconstruction and evaluation of arguments.

Now, there is nothing wrong with emphasizing the context of justification as long as one is aware that it is *conceptually* distinct from the context of discovery and as long as the conclusions one reaches are not inappropriate but rather pertain to the context one has examined. Moreover, the conceptual distinction between the two contexts does not mean that they are always separate in reality and in practice; in fact, Galileo's justification of natural circular motion in the *Dialogue* is a beautiful example of such coincidence: his beliefs did in fact originate from the reasons contained in arguments like the ones examined by Shapere.

Second, to compare Galileo's ideas about natural motion with later ideas or present-day knowledge is unobjectionable if one makes the comparison in order to have some contrasts to understand better the content of those ideas. What would be problematic is to conclude from this contrast that these ideas were unscientific or that Galileo was in this instance a bad scientist; but Shapere draws no such conclusions. Instead, I believe he would agree that such conclusions are to be supported by an analysis of Galileo's own evidence and the justifications he put forth; this would explain, in part, why Shapere tries to reconstruct and evaluate Galileo's arguments—the third element of Shapere's approach.

Logical analysis would seem to be ideally suited in a case like the present one. For here the ideas did not (at least allegedly) become part of modern science and thus one cannot ground one's philosophical conclusions on a method-explanation of success, as in the case (however problematical) of Galileo's law of squares; moreover, the origin of ideas here happens to coincide with their justification. Therefore, those who are dissatisfied with the methodological inconclusiveness of Shapere's account here, cannot easily blame it on the method of logical analysis; instead they can try to modify it.

One modification concerns the application of the concept of logical validity in the sense of inconceivability of premises being true and conclusion false. If Galileo's arguments are to be evaluated in terms of such deductive validity, then greater care should be taken in their reconstruction. For example, they should be reconstructed from the original text, not from a translation. The fairness of this requirement is supported by the problem mentioned in another context earlier, pertaining to Galileo's argument for the continuity of acceleration in rectilinear order-restoring motion. But this requirement ought to sound intrinsically plausible to philosophers nowadays

[4] Finocchiaro 1973. [See also Chapter 3 of this book.]

since it is just another way of stating the interconnection between logic and philosophy of language. My point is not that deductive validity is a language-dependent concept, but rather that since it is such a rigorous criterion of evaluation, the deductive validity of an *actual* argument is likely to be dependent on how exactly it is stated.

Another modification would be to require that the conclusion of a given argument be identified with as much preciseness as the concept of deductive validity demands for the connection between the premises and the conclusion of an argument. So it is not at all a peripheral issue what exactly is the conclusion of Galileo's argument about the actual path of a falling body: whether it is Shapere's Proposition 5, namely that "really, what appears to be natural accelerated straight-line motion is uniform and circular after all" (p. 98); or whether it is, as I believe the text more strongly indicates, that: *if* the earth rotates, *then* what appears to be natural accelerated straight-line motion is really uniform and circular, *as a first approximation.*

When care is exercised in the reconstruction of an argument, which is a requirement deriving from the rigorous character of deductive validity, it soon emerges that the application of this concept is severely limited. For one discovers that the connection between premises and conclusion stated in the text is one of probability, or plausibility, or mere relevant evidence; in other words, inductive validity becomes the more relevant concept, as long as one understands the latter in a general-intuitive way rather than a specific, technical-formal theoretical elaboration.

A fourth feature of careful logical analysis would be to pay attention to a fuller context. One might say that logical analysis comes into being when, given a proposition, one asks where it came from in the sense of the reasons being offered to justify it. Now, one is doing a half-way job if one stops here and does not ask where the proposition is going, in the sense of to what use it is being put, that is, what further conclusions are being drawn from it. Of the five propositions on "inertia" attributed by Shapere to Galileo, the first four occur in the context of Galileo's critique of the Aristotelian a priori argument for the terrestrial-celestial dichotomy, the argument from the presence or absence of contrary motions. When this is taken into account, the merit of Galileo's arguments would be different since they would be judged relative to the Aristotelian arguments supporting the two (or three) types of natural motion; such relative assessment (which might be an important part of inductive evaluation) would also yield a different evaluation of Galileo's commitment to "circular inertia." As for the fifth proposition, careful logical analysis would note that it is justified in the context of Galileo's refutation of the anti-Copernican argument from falling bodies and is explicitly labelled and implicitly treated as a speculative digression.

Thus I conclude that the method of logical analysis, used by Shapere, can be a very useful tool in the theory of scientific rationality, if it is properly carried out. Such a modified use would presumably reveal a different Galileo in his work on "inertia."

In regard to Galileo's philosophy of science, I would agree with Shapere that it is important to be cognizant of the theory–practice distinction, and realize that Galileo's philosophy of science and his scientific practice may not correspond. This means that it is inappropriate to reach conclusions about Galileo's actual scientific procedure on the basis of evidence from his reflections about science. Hence, if Galileo does not have a philosophy of science, this creates no problem for the theory

of scientific rationality, whose primary concern is the study of scientific practice. All this is correct as a first approximation but needs to be qualified as follows. If Galileo does not have a philosophy of science, then since he is a paradigmatic scientist, that lack must be significant; perhaps the significance is that scientists ought *not* to have a philosophy of science, if for example that lack accounts partly for Galileo's success; or if Galileo succeeded in spite of the lack, then perhaps his lack would not justify a norm. In other words, a complete theory of scientific rationality must say something about the proper relationship between philosophical and scientific activity, for the simple reason that we find a mixture of both in many paradigmatic scientists.

I do not have the answer to this problem, but I wish to suggest one possibility that ought not to be overlooked in searching for a solution: it may be that to *found* modern science required philosophical genius, though the continuation of it does not; this would mean that Galileo need not be paradigmatic for scientists in regard to his philosophical sophistication, even if he does have a philosophy of science which was instrumental for his scientific achievement. In this case the possibility would arise, however, that he might be exemplary for philosophers, in that his example suggests that it might be an element of *philosophic* rationality that one has to be a "scientist," at least in the sense of a student of a specific field. But these matters need not be pursued any further; suffice it to say that the question of the nature and role of Galileo's philosophy of science is not to be dismissed simply because such philosophy does not have a direct connection with scientific rationality, since the connection may be indirect.

I believe that Shapere is right in saying that Galileo does not have a philosophy of science, in the sense of a systematically formulated theory of science. However, since philosophy ought not to be equated with a systematically formulated theory, it would be improper to say without qualification that he did not have a philosophy. This would be as improper as to say (unqualifiedly) that Socrates was not a moral philosopher (in that he did not have a systematically formulated and fully consistent ethical theory). In fact, Galileo no less than Socrates is a philosopher because both are thoughtful doers and practical, action-minded theorists; in both, thought enlightens action, thinking enriches life. Socrates's domain of concern was morality and the good life, Galileo's was natural phenomena, science, and the well-informed life; but such differences are material, not formal. Galileo's way of living an examined life was to pursue science as he did; it would have been no more appropriate (philosophical) for him to appropriate Socrates's own concerns, than for Socrates to have remained in the grip of natural philosophy, or for us to forget our own problems and appropriate those of either one. It is beyond the scope of the present essay to explore and articulate this idea any further; instead I wish to mention another related fact. It is an overwhelming feature of Galileo's works that he has a methodological awareness; that is, he is always engaged, whenever the need arises, in formulating and justifying principles which underlie the particular ideas he expresses and procedures he follows. It is as true that his methodological discussions always arise out of concrete problem situations as it is that they never reach metaphysical depths; indeed Galileo was not a metaphysician. But to lack any significant metaphysical awareness or interest prevents one from being a philosopher only in the mind of those who equate metaphysics and philosophy.

Here too, since Socrates would also be excluded, Galileo would be again in good (philosophical) company.

In conclusion it may be said that if we accept Shapere's negative results (about the origin of Galileo's law of squares, about the historical and logical worth of his "inertia," and about the existence of a Galilean philosophy of science), they need not be a cause for despair, because it is possible to use them for a better and more philosophical appreciation of Galileo. Specifically we see that the theory of scientific rationality need not rely on quasi-psychological origins of discoveries but can devise and utilize methodological origins, by means of method-explanations of scientific achievements. Second, logical analysts are given a hitherto unexplored field of opportunities, the domain of scientific justification, if they are willing to exercise the proper care. Finally, philosophers, historians of philosophers, and philosophers of science are given some work to do to explore the philosophical dimension of the work of the Philosopher to the Grand Duke of Tuscany.

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Chapter 19
Koyré's *Études galiléennes*: Critical Reasoning vs. A Priori Rationalism

Abstract This essay is an attempt to determine the adequacy of the technique of error analysis characteristic of Alexander Koyré's historiography, and more specifically to determine the soundness of his interpretation of Galileo; thus, the essay focuses on the part of Koyré's *Études galiléennes* that criticizes the basic logic underlying Galileo's *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*. The essay finds little value in the technique of error analysis as such, but it also illustrates the importance of applied logic and of logical analysis in the history of science, since inadequate logic turns out to be Koyré's own central flaw. Furthermore, new insight is gained into the true philosophical significance of Galileo's work, since it turns out that Galileo's skills in logical analysis can be appreciated simultaneously with the disclosure of Koyré's confusions; these involve primarily the failure to distinguish between rationalism in the sense of reasoning or argumentation and rationalism in the sense of apriorism or speculation.

19.1 Koyré's Influence

Few scholars in the twentieth century have had as great an impact on our understanding of science as Alexandre Koyré. It is perhaps no accident that his pioneering work on the subject is one dealing with Galileo. Entitled *Études galiléennes*, the book has become both a model and a source of inspiration for historians and philosophers alike. The influence on contemporary historians of science is both explicitly acknowledged[1] and bordering on the classical, in the sense that his ideas and his approaches can now be freely borrowed and adapted and have become internalized in their professional sensibilities.

The influence on philosophers of science is less explicit, but significant. For example, the view propounded in Thomas S. Kuhn's *Structure of Scientific Revolutions* is the one which in the last decade or two has caught the imagination, if not

[1] Professors Marshall Clagett and I. Bernard Cohen have dedicated to him their major works, and Professor Charles C. Gillispie (1960, 523) states that "he is the master of us all."

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_19

the intellect, of most scholars, scientists, and laymen alike.[2] It would perhaps be an exaggeration to repeat the judgment I once heard from a philosopher, namely that, in Kuhn's book, what is new is not true, and what is true is not new, vis-à-vis Koyré's. Though exaggerated the judgment is not unfounded, and it does reflect the extent of Koyré's influence. At any rate one other philosopher, Joseph Agassi in *Towards an Historiography of Science*, has focused on one of Koyré's central techniques—error analysis, has articulated it, synthesized it with Karl Popper's philosophy of science, provided it with novel content and illustrations, and advocated it as the most fruitful approach to the philosophy of science.[3] Furthermore, some of Paul Feyerabend's own critiques, remind one of Koyré's technique of error analysis, though of course, Feyerabend's central concern is to explore the limitations of reason by studying the limitations of method and the power of rhetoric, whereas Koyré's concern was to extol the power of reason by exploring the rationalistic character of science, rationalist in the sense of a priori rationalism. This is all the more curious since they both use similar approaches and evidence. In my opinion, Koyré's rationalist apriorism is no more adequate than what might be labeled Feyerabend's pseudo-irrationalistic anarchism.[4]

I might pay my own homage to Koyré by interpreting his introduction of methods of intellectual history into the historiography of science as a move toward the technique of logical analysis, which is one whose powers I am interested in exploring. However, to do this would be insincere lip service on my part, for though it is true that Koyré's work is uniquely valuable as an introduction to the use of logical analysis in the interpretation of science, yet it needs such serious corrections, as it is shown below, that in their absence I feel it is bound to lead to abuse and to aprioristic and rationalistic excesses.

At any rate, now that Koyré "is the master of us all," to use words of historian Charles C. Gillispie,[5] the methodologically aware scholar, and most of all one interested in Galileo, ought to ask himself whether uncritical acceptance is any more justified here and now than it was at the time of Galileo vis-à-vis that supreme "master of those who know," Aristotle. To combat one authority with others, one might say that, though Koyré may be acknowledged as a founder of professional history of science in its present form, yet a discipline which hesitates to forget its founders is lost, a dictum coined by Alfred North Whitehead and popularized by sociologist Robert K. Merton.[6]

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[2] See Kuhn 1962; cf. Finocchiaro 1973, 188–196.

[3] See Agassi 1963; cf. Finocchiaro 1973, 131–157.

[4] See Feyerabend 1975. [Cf. Chapter 20 below.]

[5] Gillispie 1960, 523.

[6] Quoted from Alfred North Whitehead's *The Organization of Thought*, by Merton (1967, 1), as the epigraph to Chapter 1.

19.2 Critics of Koyré

To be sure, there have been critics of Koyré's work. For example, Koyré's erudition has been questioned by Eugenio Garin in 1957.[7] It concerns the character of Koyré's discussion of Galileo's alleged Platonism. Garin argues that Koyré's characterization of Platonism is abstract and unhistorical, and hence when the problem is discussed in such terms it is insoluble. He exhibits the inadequacies of Koyré's discussion of the actual historical situation of Platonism (that is, of Platonism as a historical entity) by pointing out a number of errors and confusions about relevant texts and persons. Garin argues that Koyré's distinction between Platonism as mathematicism and Platonism as mystical, magical numerology is a figment of Koyré's imagination in the sense that it has no historical basis in texts such as Ficino's works or even in Clavius's commentary to Euclid.[8] Another example given by Garin is Koyré's error in identifying the 'Lud. Buccaf.' mentioned in Bonamici's *De Motu* as a certain Lodovico Buccafiga[9] instead of Ludovico Boccadiferro; the latter is a nonnegligible figure, was a professor in various Italian universities including Bologna, and wrote several commentaries to Aristotle full of Platonic as well as Aristotelian doctrines[10]; moreover, he is mentioned in Galileo's own *Iuvenilia* (under the Latin name Buccaferrus).[11] Another example given by Garin is a reference to Crescas in Koyré's discussion of Descartes's notion (in *Le Monde*) of quantity of rest,[12] when, as Garin states, an educated person at the beginning of the seventeenth century could easily read Crescas's theses in the widely known *Examen vanitatis doctrinae gentium* by Francesco Pico.[13]

If Koyré's erudition can be so questioned, at the other end of the scholarly spectrum, the logic of his reasoning has been faulted in a significant way. It has been argued in my *History of Science as Explanation*[14] that an analysis of Koyré's central volume of *Études galiléennes* shows that the conclusions he himself draws are not supported by the evidence he himself gives.

Somewhere in between the extremes of these critiques lie two other equally serious problems. One is Koyré's persistent confusion of the context of scientific discovery and the context of scientific justification. There is nothing wrong with focusing on one or the other of these contexts, as long as one's conclusions are supported by evidence from the appropriate context. The error is to reach conclusions about one context on the basis of evidence from the other context. Koyré's study of Galileo's law of falling bodies examines evidence from the context of justification since he examines the various attempts found in Galileo's (published) writings to give a theoretical

[7] Garin 1957.

[8] Garin 1957, 406–407.

[9] Koyré 1966, 44.

[10] Garin 1957, 408.

[11] Galilei 1890–1909, 1: 134, 167, 172.

[12] Koyré 1966, 330 n. 3.

[13] Garin 1957, 408.

[14] Finocchiaro 1973, 86–116.

demonstration of it.[15] On the other hand, Koyré's conclusions[16] are about the method which enabled Galileo to succeed, where success can only be interpreted to pertain to his discovery of the law, since Koyré is at pains to point out the inadequacies of Galileo's various proofs of the law (and hence presumably there was no success in the context of justification), and since Koyré is contrasting that success to Descartes's failure, which was clearly a failure to arrive at the correct law of fall.[17]

It is interesting to point out that this type of criticism of Koyré has emanated from two very different quarters. On the one hand, it has been made in the terms just used in my work on historical method whose explicit and primary aim is to work out a philosophy of the historiography of science.[18] On the other hand, a similar criticism is made in the domain of pure Galileo scholarship, in Drake's attempt to emphasize "Galileo Studies" as contrasted to the Koyré-type of "Galilean Studies."[19] Drake's distinction between the biographical context and the history-of-ideas context is similar to that between discovery and justification, and his judicious work in the former areas has led to some epoch-making results about Galileo's discovery of the law of fall.[20]

To this problem in Koyré's work which one may categorize as methodological or historiographical, we may add one that pertains to scholarship as such. Perhaps the single most striking feature that is apparent to the reader of Koyré's works, or at least of the book for which he is most famous, is the use of very long and very frequent quotations, interspersed with commentary. I am somewhat embarrassed to report my having discovered that in the translation, citation, and arrangement of such quotations Koyré takes a number of impermissible liberties. It is almost as if these texts were being quoted so as to enable him to insinuate by distortion what could not be suggested by explicit argument.

For example, on p. 278 of *Études galiléennes* Koyré has a quotation for which he gives the reference "*Dialogo*, II, p. 423". The quoted passage occurs neither on p. 423 (of the National Edition), nor in Day II, which at any rate does not include that page; since it is Day III that includes that page, one may look for the passage in Day III, but in vain. In fact, the passage is not from the *Dialogue* at all, but from Jacopo Mazzoni, as the interested scholar can discover by studying the rest of Koyré's footnote, which reads in part "cf. Jacobi Mazzonii, …, *In Universam Platonis et Aristotelis Philosophiam* …, p. 187 sq. …" Thus one concludes that Koyré's footnote should have read "Jacobi Mazzonii, *In Universam* …, p. 187 sq.; cf. *Dialogo*, III, p. 423" rather than the other way around "*Dialogo*, p. 423; cf. Jacobi

[15] Koyré 1966, 83–107, 136–155.

[16] Koyré 1966, 155–158.

[17] Koyré 1966, 107–136.

[18] Finocchiaro 1973, especially pp. 234–238.

[19] Drake 1970, 14.

[20] Drake 1973.

Mazzonii …" As it is, most readers will get the impression that Koyré is quoting from the *Dialogue*, when in fact he is not.[21]

19.3 Koyré on the Argumentation in the *Dialogue*

However, the influence of Koyré's work persists in spite of such demonstrated inadequacies in erudition, logic (reasoning), methodology (historiography), and scholarship. So perhaps his appeal does not derive from the attraction of his approach but from the attraction of his substantive thesis, namely his apriorist-rationalist interpretation of science. For this reason, as well as because Garin's above criticism refers primarily to the first volume of Koyré's book, and because Drake's critique and mine above refer primarily to the second volume, it will be good to examine in detail the third which contains a statement of that thesis classic in its clarity and simplicity. It will emerge that Koyré confuses the activity of reasoning with the attitude of apriorist rationalism, and that his evidence supports an interpretation of science from the point of view of the former rather than one from the point of view of the latter.

The third volume deals with the law of inertia and contains a chapter entitled "The *Dialogue on the Two Chief World Systems* and the Anti-Aristotelian Polemic" (K205–238).[22] It begins with an introductory discussion (K205–211) which emphasizes the connection between physics and cosmology in Galileo's work and in the rise of modern science in general; then it contains a brief discussion (K212–215) of the many aspects of the *Dialogue*: polemical, pedagogical, philosophical, and autobiographical. This is followed by an analysis of what Koyré takes to be the central core of the book, namely the physical arguments against the earth's motion. His analysis consists of three elements: a quotation of the main arguments (K215–219), an assessment of Galileo's criticism of these arguments (K219–220), and a justification of this assessment (K220–238).

The passage quoted by Koyré is that in which, first, Simplicio quotes from *De Caelo* Aristotle's original four arguments, and, then, Salviati gives a statement of several contemporary arguments inspired by the Aristotelian ones (F150–153).[23] The careful reader of Galileo's *Dialogue* will be struck by the last three lines of Koyré's quotation (K219) which read: "Moreover, the same thing would happen in all cases where one would shoot a cannon: the ball would pass above or below the mark according as one would shoot toward the east or toward the west …" These

[21] This reference would later be corrected in the English translation of Koyré's *Études galiléennes*; cf. Koyré 1978, 233 n. 285.

[22] Hereafter references to Koyré 1966 will be made in the text, by prefixing page numbers with 'K', short for 'Koyré'.

[23] Galilei 1890–1909, 7: 150–153. Hereafter, references to this volume of Galileo's *Opere* (1890–1909), which contains the *Dialogue*, will be made in the text by prefixing page numbers with 'F', short for 'Favaro', the main editor of the "National Edition" of the *Opere*.

lines constitute an excessively free translation[24] for the *initial segment* of the *last portion* of Salviati's speech on pp. 151–153 of the *Dialogue*. This last portion reads:

> And not only the shots along the meridians, but also those toward the east or toward the west would not result right, the eastward ones resulting high, and the westward ones low whenever the shooting were point-blank; for, since the path of the ball in both shots would be along the tangent, namely along a line parallel to the horizon, and since if the diurnal motion belongs to the earth, the horizon would be constantly falling in the east and rising in the west (that's why eastern stars appear to be rising and western ones to be falling), therefore the eastern target would be falling under the shot, so that the shot would result high, and the rising of the western target would render the westward shot low. In this way one could not shoot right in any direction; and since our experience is different, we are forced to say that the earth is motionless. [F153, my literal translation]

The liberties that Koyré takes are inadmissible because he thereby fails to see or to inform his readers that Galileo is here reporting a distinct problem with the earth's rotation, deriving specifically and exclusively from the point-blank nature of these gunshots. He is not merely repeating the east–west gunshot objection, stated earlier in Salviati's speech; in fact, the two problems require different solutions and these solutions are given by Galileo in two different passages (F193–197, and F205–209 respectively). Nor is Galileo merely summarizing the arguments he has just stated, which Koyré suggests by emphasizing the word 'all'. To excise this passage the way Koyré does, and to translate its beginning as he does, may be in accordance with his perception of the repetitive nature of the *Dialogue*; he argues that this repetitiveness has the important pedagogical and rhetorical function of familiarizing and accustoming its readers to the new concept of motion (K234, 237). However, if the alleged repetitiveness of the *Dialogue* is grounded on evidence like the present one, then it is an oversimplification at best, or perhaps an invention.

Another reason why it is improper for Koyré to quote the passage the way he does is that the point-blank objection and reply contain discussions of rectilinear motion along the tangent to the point of firing. The context of Koyré's discussion of the *Dialogue* is that of "Galileo and the Law of Inertia", namely, to what extent Galileo's conservation of motion involves rectilinear motion. Therefore, the point-blank objection would have been relevant, regardless of whether or not it would disconfirm Koyré's interpretation.

Let us now examine his assessment of Galileo's critiques. Here it is impossible to do Koyré any greater injustice than the one his own words do him. So let us quote this brief passage in full:

> Let's now pass to the criticism. It is at once very profound and very simple. Galileo tells us that the arguments of the Aristotelians are nothing but paralogisms. They presuppose what must be shown. And, no doubt, it is true. But an Aristotelian could very well not accept the criticism, [which is] a consequence of the objection that Copernicus had already addressed to him: Aristotle does not reason, as he pretends, by starting from the facts, but on the contrary by starting from a theory. To this the Aristotelian could respond with good reason:

[24] The English translation of Koyré's *Études galiléennes* would not read this way for this passage since, instead of translating Koyré's words, it quotes from Drake's translation of the *Dialogue*; cf. Koyré 1978, 162.

> (a) that it is impossible to reason otherwise;
>
> (b) that Galileo does the same.
>
> In fact, the Aristotelian reasoning presupposes a theory, or if you prefer, a particular concept of motion, namely that of a process which affects the moving thing. It also presupposes that sense perception permits us to apprehend directly physical reality, that it is even the only means of apprehending it, and that, consequently, a physical theory can never cast doubt upon the immediate data of perception.
>
> Now, Galileo expressly denies this. He starts from directly opposite assumptions:
>
> (a) that physical reality is not given to the senses, but on the contrary apprehended by reason;
>
> (b) that motion does not affect the moving thing, which remains indifferent to all motion that animates it, and that motion affects only the relations between a moving thing and one which does not move.
>
> A paralogism from Galileo's point of view, the Aristotelian reasoning is in itself unobjectionable. Nevertheless, dialectically speaking, Galileo no doubt has the right, at least within the *Dialogue*, to designate the Aristotelian reasoning as a paralogism. For, before having stated the physical and mechanical proofs of the earth's immobility, Galileo has already laid down the double principle of the optical as well as mechanical relativity of motion. [K219–220]

19.4 Criticism of Koyré's Assessment

This is at best an oversimplification, and probably a disservice to the very rationalism that Koyré's account is meant to support. Let us see why.

First, it is simply not true that "Galileo tells us that the arguments of the Aristotelians are nothing but paralogisms." In Koyré's own quotation, we have Aristotle's four original arguments (from violent motion, from double circular motion, from natural motion, and from vertical fall) plus five modern arguments (from the ship experiment, from vertical gunshots, from east–west gunshots, from north–south gunshots, and from point-blank gunshots). Of these nine arguments, Galileo claims that only three are paralogisms, namely the argument from violent motion (F159–162), the argument from double circular motion (F162–164), and the argument from vertical fall (F164–167). The problems with the other arguments, according to Galileo, are as follows. The argument from natural motion is the one to which most of the First Day is devoted (FI64); it presupposes an untenable concept of natural motion, namely that straight and circular motion are two distinct instances of simple motion, whereas Galileo argues that they are two different stages of natural motion: straight motion can be acquired naturally but cannot naturally continue forever, whereas circular motion can naturally continue forever but cannot be acquired naturally without straight motion (F38–62). The ship experiment argument is simply based on a false premise, namely that on a moving ship a rock dropped from the top of the mast lands away from its foot (F169–175). The east–west gunshot argument involves a failure of hypothetical reasoning, namely a failure to take seriously the motion of the earth, even when examining the consequences of such motion (F193–197). The vertical gunshots argument, though in one place it is incidentally described as a paralogism just like the vertical fall argument (F200), in effect is shown

to suffer from a failure to take into account both the relativity and the composition of motion, for this is what is actually discussed in the criticism (F197–203). The north–south gunshot argument is criticized as failing to take into account the conservation of motion (F203–205). Finally, the point-blank gun-shot argument, which Koyré confuses with the one from east–west gunshots, is criticized as based on a phenomenon of such a small magnitude that it could not be detected even if it existed (F205–209), namely that on a moving earth the deviation from the horizontal would be of the order of a fraction of an inch.

Second, of the arguments that Galileo claims are paralogisms, it is not true that the paralogism is always that "they presupposed what must be shown." Only the vertical fall argument begs the question, the other two are fallacies of equivocation. The argument from violent motion misuses the ambiguity of the proposition "the parts of the earth would also move circularly," which can mean either that these parts would move around their own centers or that they would move around the earth's center (F159–162); Aristotle's second argument allegedly commits the fallacy of equivocation because its conclusion could mean either that the earth lacks the diurnal motion or that it lacks the annual motion (F162–164).

Koyré's next error is to confuse begging the question (reasoning that presupposes what must be shown) with apriorism (reasoning based on theories rather than facts). For the reason that he gives why it must be admitted that the Aristotelian reasoning begs the question is that it starts from a theory rather than from facts. Koyré's confusion is a serious error. An argument presupposes what must be shown when one of its premises depends on its conclusion. Such reasoning is fallacious because an argument is an attempt to show that since you accept the premises you must accept the conclusion; if one of the premises depends on the conclusion, in the sense that it implicitly assumes the conclusion, then the argument is assuming what it is trying to prove, which is surely wrong. Nor would Simplicio or Aristotle deny this. Nowhere in the *Dialogue* does Simplicio take lightly the accusation of begging the question, which after all belongs to Aristotle's own list of fallacies.[25] On the other hand, to reason from a theory may or may not be correct, depending on the adequacy of the theory. But even if the presupposed theory is false or inadequate there is no *logical* error, no paralogism, unless the theory from which you are reasoning is the same as the theory (or alleged facts) *to* which you are reasoning; in this case you are involved in circular reasoning and are begging the question. In other words, not every instance of reasoning from a theory is an instance of presupposing what is being shown. Nor is every instance of begging the question an instance of a priori reasoning: it may be that what you are trying to prove is a fact; and it may be that you are proving it on the basis of other facts; then there would be no apriorism, but if one of the facts on the basis of which you are proving the conclusion, *depends on* the conclusion, then you are still begging the question. Circular reasoning does not become legitimate merely because it goes on within a domain of facts. In conclusion, then, apriorism is neither a sufficient nor a necessary condition for circular reasoning.

[25] *Topics*, 162b34; *Prior Analytics*, 65a10, 64b33. Cf. Hamblin 1970, 50–58.

Let us illustrate such abstractions with discussions from the *Dialogue*. The Aristotelian argument claimed by Galileo to be a paralogism in the sense of begging the question is the following (F164–166):

> The earth cannot rotate because if it did bodies could not fall vertically; but they do since they can be seen to graze the edge of a tower when dropped from it. [My reconstruction]

The final portion of this argument grounds the impossibility of the earth's rotation on the vertical fall of bodies, so that one premise of that final step is the proposition that "bodies fall vertically." Another portion of the argument bases this vertical fall of bodies on the apparent grazing; hence it is being assumed that this apparent grazing implies vertical fall. Now, Galileo argues, this implication can be questioned because it would not hold on a rotating earth (since if the earth rotated and bodies were seen to fall grazing the tower, then their actual path would not be vertical but slanted). Since the implication can be questioned, it is fair to ask for a justification of it: how do you know that the apparent grazing implies vertical fall? One abstractly possible justification would be the following: if the earth stands still then the implication holds; the earth does stand still; therefore the implication holds. There being no other means whereby an Aristotelian could justify the implication, he would have to use this abstractly possible argument. In so doing, though it is indeed true that the implication holds if the earth stands still, he is also assuming that the earth stands still, which is the final conclusion he wants to reach. Thus the Aristotelian argument from vertical fall begs the question because its premise that bodies fall vertically presupposes (i.e., would have to be justified by the proposition) that the earth stands still.

Feeling the force of Salviati's criticism, Simplicio with Sagredo's help states a new argument which can serve here as a good example of reasoning starting from a theory (F166–167):

> If the earth rotated then bodies could not be seen to fall grazing the edge of a tower, since on a rotating earth this apparent grazing would imply that bodies would have two natural motions, toward and around the center, which is impossible. But bodies are seen to fall grazing the edge of a tower. Therefore, the earth can't rotate. [My reconstruction]

The final portion of this argument grounds the impossibility of the earth's rotation directly on the apparent grazing, which is self-evident. But it also grounds the impossibility of rotation on the claim that if the earth rotated the apparent grazing would not occur. In another portion of the argument, this claim is grounded on two premises, a self-evident one and a theory. It is self-evident that on a rotating earth apparent grazing would entail simultaneous motions toward and around the center of the earth. But it is merely a "theory" to say that it is impossible for material bodies to have two spontaneous motions toward and around the center. This proposition may be merely a theory, but it is not the same as the conclusion of the argument. Hence this argument does not beg the question, though it is an instance of what Koyré would call a priori reasoning, reasoning from a theory.

Now, since Koyré is conflating the paralogism of presupposing what is being shown with the problem of a priori reasoning or reasoning from a theory, and since

he claims that all the Aristotelian arguments are paralogisms, it follows that what he *means* is probably that these arguments are all reasoning from a theory. Let us ask whether he is right in so claiming. Unfortunately not, if for no other reason than because the ship experiment argument, which is quoted by him, is clearly not a piece of a priori reasoning. Koyré thinks that it is probably because he thinks that Galileo's criticism of it is a priori; however, even if this were so, it would not make the original ship experiment argument an a priori one. The simple truth is that someone may, on the basis of a theory, criticize an argument which is not itself based on a theory. Another reason Koyré might give for regarding the ship argument as a priori is to say that the experiment had never been made (K225). However, this may be a piece of a priori reasoning by Koyré himself; in fact Chiaramonti claims in his 1633 book answering Galileo's *Dialogue* that the experiment had been made by a certain Giovanni Cotunio of the University of Padua.[26] A final reason Koyré might give is his belief that "it is impossible to reason otherwise" than from a theory; however, he gives no justification for this allegation, unless it be the claim that the Aristotelians as well as Galileo did *in fact* reason from a theory; hence such an argument would beg the question.

Let us continue to place qualifications on Koyré's claims to see if we can find some truth in them. Though not all the Aristotelian arguments involve reasoning from a theory, some of them do. Concerning these, can we agree with Koyré that they are unobjectionable because it is impossible to reason otherwise and because Galileo does the same? It is true that if it were impossible to reason otherwise than from a theory, then an argument could not be faulted for doing so. However, it is obvious that reasoning from a theory is merely one type, and Koyré's claim that it is impossible to reason otherwise is an extravagant exaggeration at best. In fact, he gives no justification of this claim. Or perhaps his analysis of the Aristotelian and of Galileo's reasoning could be interpreted as a supporting argument, namely, that it is impossible to reason otherwise than from a theory because both the Aristotelians and Galileo did so. It would not be a very serious criticism of this argument to object that a generalization is being reached from two cases; its real problem is that these cases are not typical. In fact, the relevant Aristotelian reasoning consists of those arguments against the earth's motion which *happen* to involve reasoning from a theory; hence, these arguments would be prejudicially chosen to ground Koyré's generalization. On the other hand, the Galilean reasoning under consideration is that whereby he criticizes these and other Aristotelian arguments. Now, arguments criticizing a priori arguments are not typical since, whatever a priori element they might contain, it could be the result of the a priori arguments being criticized. Regarding Galileo's arguments that criticize Aristotelian reasoning that does not start from a theory, even if they did presuppose a priori elements, as Koyré tries to show, this very feature would make them atypical, since they would then be a priori criticism of empirical arguments. Since the question here is whether Koyré's alleged impossibility of reasoning other than from a theory has been reached by generalizing typical cases, we may dismiss Koyré's generalization.

[26] Chiaramonti 1633, 339; cf. Barenghi 1638, 183.

Let us now examine Koyré's argument that the Aristotelian arguments which start from a theory are unobjectionable because Galileo is doing the same. Here it is important to see that, even if Galileo were reasoning from a theory, even if Koyré's account had shown this, his conclusion still would not follow: the Aristotelians could not justify their position with such a *tu quoque*. There are two reasons for this. First, as Koyré himself points out, an important element of the theory *from* which the Aristotelians argued was their emphasis on empiricism: that sense experience is the only means of apprehending reality; hence the Aristotelians could not consistently have admitted that their arguments against the motion of the earth were really reasoning from a theory. Second, much of Galileo's criticism consists of an analysis of the a priori elements of the Aristotelian reasoning, together with the argument that if one replaces these a priori elements with other, more plausible theories one cannot then draw the conclusion that the earth must stand still. Of course, Koyré denies that these other theories, from which Galileo reasons, were more plausible than those from which the Aristotelians started; but even if Koyré is right about the equal plausibility of the respective presupposed theories, the situation is *not* otherwise symmetric. For, whereas the Aristotelians were trying to prove the impossibility of the earth's motion, Galileo in his criticism of these arguments is merely trying to prove that the Aristotelian reasoning is incorrect, not that the earth moves. In his criticism, Galileo never argues that, given his new concept of motion, it follows that the earth moves, but rather that, given the new concept, it follows the motionlessness of the earth is not proved by the Aristotelian arguments. In other words, the crucial difference is that Galileo's conclusion is that the Aristotelian reasoning is incorrect, whereas the Aristotelian conclusion is that the earth stands still; the Aristotelian point is that the earth stands still, Galileo's is that the Aristotelians haven't proved their point. That is why it is wrong for the Aristotelians to argue from a theory, but not for Galileo. Galileo argues from a theory in the context of showing that from a different theory one could not reach the conclusion the Aristotelians reach in their argument; since their argument presupposes a theory, they haven't proved their conclusion. For example, consider the above mentioned argument given as a good example of Aristotelian reasoning from a theory, the argument that based the impossibility of the earth's motion on the falling body's apparent grazing of the edge of a building, and ultimately, on denying the possibility of natural motion both toward and around the center. Given a theory that allows this possibility, one could not conclude the motionlessness of the earth from the apparent grazing. Galileo reminds us very frequently that his criticism is merely disproving the Aristotelian arguments, not proving the earth's motion, so much so that it would be tedious to collect the references.

Though Galileo's alleged reasoning based on a theory cannot justify the Aristotelian procedure, Koyré's claim is intrinsically interesting, and it is worth examining. In the passage quoted above, Koyré claims that Galileo's reasoning presupposes the principle that physical reality is apprehended by reason, not by the senses, and the principles of relativity and of conservation of motion. In the rest of his account (K220–238) Koyré then supports these claims by analyzing the passages where Galileo answers the Aristotelian arguments; special attention is given to the answer to the ship experiment argument which is the only passage used by Koyré

as showing Galileo's presupposition of aprioristic rationalism. It should be noted first that, even if Koyré is otherwise correct in these interpretations, he would not be justified in claiming that the way in which Galileo presupposes the principle of apriorism is the same as that in which he presupposes the principles of relativity and of conservation, namely that all these principles are theories from which Galileo starts in his reasoning, so that his reasoning is no better grounded than that of the Aristotelians, who start from different theories. The fact that the principle of apriorism is an epistemological one concerning the nature of knowledge, and that the other principles are physical ones concerning the nature of motion, is perhaps the least of their differences. The more significant difference is that the physical principles are or could be used as *premises* in certain parts of Galileo's counter-arguments, whereas the epistemological principle is not so used. For example, in the answer to the ship experiment it is clear that Galileo is or could be arguing as follows: given the principle of conservation, the rock will retain its horizontal motion even after it is dropped; and given the principle of relativity, the simultaneous downward motion will not constitute an interfering disturbance to this horizontal motion; therefore, on the moving ship the rock will end up at the foot of the mast. On the other hand, how would Galileo be committed to apriorism, assuming that he were? If Galileo can argue, by reasoning of the type just sketched, that the rock will land at the foot of the mast even on a moving ship, then it follows that one has to accept such a result on a moving ship unless one can show that there is something wrong with Galileo's argument. Now, to find something wrong with this argument one has to fault either some steps in reasoning or some premises being assumed in it; since presumably there is nothing wrong with the steps in reasoning, the only thing one could question is the assumed premises; but, if Koyré is right, these assumed premises are the principles of relativity and of conservation which cannot be faulted empirically but only by opposing to them another concept of motion such as the Aristotelian one; it follows that the only objection one could have to Galileo's reasoning is to produce the Aristotelian *argument* concluding that the rock must fall behind on a moving ship. Now, this may be in accordance with Koyré's desire of showing that both Galileo and the Aristotelians are reasoning from a theory, but it is not in accordance with his claim that Galileo is assuming apriorism as a principle *from* which he reasons to arrive at his physical conclusion, for it also follows from the above considerations that the apriorist principle is *implied by* Galileo's allegedly a priori argument. In other words, the apriorist principle is a *consequence* rather than an assumed premise of Galileo's ship experiment counter-argument; the principle follows from the argument, rather than the other way around; Galileo does not need the epistemological principle to justify his physical argument, but rather his physical argument (the fact that such an argument can be given) justifies the epistemological principle. In short, *if* Galileo can answer the ship experiment argument in the a priori way that Koyré thinks, *then* his answer is supporting, *not assuming*, apriorism. Moreover, and conclusively for the present issue, if it is a fact that the Galilean and Aristotelian arguments are a priori, then apriorism is itself a fact and not a theory, for it would then be the consequence of a fact.

To explain further this difference between an assumption and an implication, and thus to reinforce this criticism of Koyré, I will call attention to an Aristotelian argument whose answer does involve Galileo in an epistemological assumption about the relationship between sense experience and reason similar (though not identical) with the apriorist rationalism here attributed to him by Koyré. The argument is that from the deception of the senses; it is explicitly discussed by Galileo in the Second Day, at the beginning of his critique of Chiaramonti's book on the three new stars (F272–281); it is also implicitly discussed at the beginning of the First Day (F56–62) and in the discussion of the gunshot arguments (F197–200). However, it is not one of the arguments quoted or mentioned by Koyré, though it would have provided him with a proper illustration of how an argument against the earth's motion and Galileo's answer to it assume an epistemological principle. The argument is that if the earth rotated then our senses would be deceived insofar as (a) we do not *feel* any motion, and (b) falling bodies would be appearing to move vertically but would in reality move in a slanted path. Obviously, in order to be able to conclude from this (by *modus tollens*) that the earth does not rotate, one needs another premise which would be a denial of the 'then' clause of the conditional premise; we could say that our senses cannot be deceived, or to use Koyré's words, that "sense perception permits us to apprehend physical reality directly" (K220). Galileo answers (F57–59, F166, F272–280) that our senses tell us merely that bodies *appear* to fall vertically (vision) and that the earth *feels* to be at rest (internal sense); to think from this that bodies really fall vertically and that the earth is really at rest is to draw a conclusion that involves reasoning; this reasoning happens to be incorrect since we cannot say without qualification that apparent vertical fall implies actual vertical fall (it does so only on a motionless earth), and since it is not true that we can feel all motion (we can only feel *changes* of motion); therefore, if the earth rotated it would not be our senses that would be deceived, but rather our reason, or at least the reason of those who would draw the unwarranted conclusion from the sense data; therefore the argument from the deception of the senses is incorrect. But in any case, it is by reasoning, not by the mere senses, that we can apprehend reality, or in Koyré's words, "physical reality is not given to the senses, but on the contrary apprehended by reason" (K220). From this "theory" Galileo is refuting the Aristotelian argument, which was based on the opposite theory. However, I believe that, besides providing a good illustration for Koyré's point, this argument provides a good illustration of something he does not want to accept, namely that the two "theories" are *not* on a par; at the very least Galileo's is the more plausible one; actually, in the sense in which they relate to the present argument, Galileo's "theory" is true, whereas the Aristotelian one is false. Hence, though Galileo's critique of this argument illustrates Koyré's point, it does not support it; though it confirms the letter of his thesis, it does not confirm its spirit, since he attaches an apriorist meaning to Galileo's epistemological assumption, whereas in the present context it has a critical meaning. Galileo is being both a *critical* empiricist and a reasoning-oriented rationalist.

The same type of non-apriorist rationalism is presupposed by Galileo, though by way of implication rather than assumption, in the answer to the ship experiment argument examined by Koyré. Galileo is not a rationalist in the sense of an apriorist,

but in the sense that he likes to use reasoning and arguments as much as possible, we might say a rationalist in the sense of a logician, a logician-in-action. The difference is that the logician will not limit himself to a priori reasoning, reasoning from a theory; some reasoning is reasoning based on facts, where by starting from facts one attempts to justify a conclusion; if the conclusion is a theory then we would have reasoning *to* a theory; or the conclusion may be itself factual, if only facts are used as premises and if all the inferences are strong. To be more specific, the thesis implied by the present passage (F169–180) could be formulated as follows: experiments are sometimes unnecessary to ascertain the results of a test, for sometimes it can be argued on the basis of known or more easily ascertainable facts, what these results must be. In fact, the passage could be reconstructed as follows.

Consider dropping a rock from the mast of a moving ship; the Aristotelians believe that the rock will be left behind, and from this, by analogy, they argue that the vertical fall of a rock dropped from a tower implies that the earth stands still. It is doubtful whether they ever made the experiment; but at any rate, it can be shown that on the moving ship the rock will fall at the foot of the mast. Here, the more easily ascertainable facts are that *(a)* the undisturbed downward motion of bodies on an inclined plane is accelerated, *(b)* their undisturbed motion up an inclined plane is decelerated (F171–133), and *(c)* the cause of the motion of projectiles is not the motion of the air surrounding them (F175–180). From *(a)* and *(b)* one may conclude that (1) the motion of bodies on an horizontal plane is conserved if undisturbed, and consequently that (2) the horizontal motion of the rock on the moving ship, even after being dropped, continues if undisturbed. Now from *(c)* one can conclude that (3) the cause of the motion of projectiles is the "virtue" impressed on them by the projector, and consequently that (4) the cause of the horizontal motion of the rock, after it has been dropped, would be the horizontal "virtue" impressed on it by the hand holding it before dropping. Since there is no way in which this horizontal impressed virtue could be disturbed by the vertically downward tendency due to weight, it follows that (5) the horizontal motion of the dropped rock is undisturbed, and hence by (2), that (6) the horizontal motion of the dropped rock will continue, and therefore that (7) the rock will end up at the foot of the mast.

In his analysis Koyré neglects to take into account the very important fact that, though Galileo is justifying the conclusion of his own counter-argument by reference to the principle of conservation, this is not being merely assumed but empirical evidence is given in its support. The principle of conservation corresponds to proposition (1) in the above reconstruction, which is justified by means of *(a)* and *(b)*. Of course Koyré may say that the reasoning from *(a)* and *(b)* to (1) is not completely valid from a logical point of view, or perhaps that other principles are being assumed in this subargument. However, to the former it could be replied that it is one thing for a proposition to be incompletely supported and it is another for it to be completely unsupported; only the latter would be a "theory" in Koyré's sense. To the second alternative one could reply that the other assumed principles would probably themselves be supportable when questioned, and though other principles might be assumed in these other arguments, perhaps they too could be supported if required, or if not

perhaps one would be dealing with highly abstract, general, and universal metaphysical principles, whose assumption would present no practical or physical problem. Thus the impact of Koyré's point would end up being something to the effect that whenever one is reasoning one is justifying certain propositions on the basis of others which for the time being are not questioned; that is, in reasoning one can't prove everything simultaneously. But these statements about reasoning, far from presenting problems, constitute very elementary facts about the nature of reasoning; they amount to saying that reasoning is a step-by-step process. The only real question one could raise is whether such an activity is effective. To this the facts of life and of history provide the obvious answer.

Concerning the principle of relativity, the situation is more complicated. Koyré would argue as follows: though this principle is not directly present in the argument, the argument is assuming that "there is no way in which the horizontal impressed virtue could be disturbed by the vertically downward tendency due to weight", which is a form of the principle of composition; now, the composition of motion is a consequence of its relativity (K222–223). So presumably Galileo's answer is based on the principle of relativity in the sense that it would have to be used to justify one of the premises in his argument. However, Koyré's interpretation is not a faithful reconstruction of the argument actually given by Galileo. It is indeed true that if one assumes the principle of relativity of motion, and *if* one interprets it in such a way as to imply the composition of motion, *then* one could arrive at Galileo's result for the moving ship (having also used the principle of conservation). However, in the passage where Galileo gives his answer (F169–180), the relevant step (F175) is much more concrete: the vertical fall does not represent a disturbance to the horizontal motion because the body is indifferent to horizontal motion, and because the cause of the horizontal motion (the impressed 'virtue') is distinct from the cause of the vertical fall (gravity). Of course, Koyré could question the soundness of these inferences, though they are not questioned by Simplicio, who merely questions the truth of the two premises, that the body is indifferent to horizontal motion, and that the cause of this motion is an impressed "virtue." But even if the soundness of the inferences is questionable, Koyré's substitute argument is not unquestionable either, for though it may contain no problem of the soundness of the inferences, it contains the problem that the crucial premises are just being assumed. I am inclined to believe that the inferences in Galileo's actual answer are contextually sound. It follows that this answer is not assuming, but proving (supporting) the principle of relativity. This can be seen even more clearly from Galileo's answers to the vertical fall argument (F164–166) and to the east–west gunshot argument (F193–177), as we will see below.

Koyré's evidence that the principle of relativity is being presupposed in the sense of assumed consists of (1) the fact that Galileo obviously holds the principle, (2) the fact that by *postulating* the principle one *could* answer the objections to the earth's motion being considered by Koyré, and (3) the alleged fact that Galileo gives several statements of the principle before he gives his answers to the objections (K220, n. 3; K221, n. 1; K222, n. 1; K237). Now, (1) is indeed true, but by itself it does not support Koyré's conclusion, since it might be, as I think is the case, that Galileo holds the principle because it is supported by his critiques, rather than because he needs to

assume it in order to make those critiques. (2) is irrelevant, even if true, for the fact that the logic of the situation could be rearranged differently, does not change the facts of the *actual* logic of the situation, as we have just seen for the case of the ship experiment.

As for Galileo's alleged previous references, they are misinterpreted by Koyré (K220, n. 3: F57, F101, F139, F 141; K221, n. 1: F139 ff.; K237). The first reference alleged by Koyré occurs at the beginning of the First Day where Simplicio objects to Salviati's principle of circular motion, by saying that it conflicts with the clear evidence of the senses that bodies are seen to fall in a straight line (F57). Salviati answers, among other things, that it is questionable, as he promises to show later, whether falling bodies really follow a straight path. However, there is no statement of the principle of relativity; there is only a reference to a future discussion, hence even if that discussion were the proper one, the present passage could not be regarded as evidence of any antecedent commitment to relativity. At any rate the subsequent passages being referred to can only be those containing the critique of the argument from vertical fall (F164–166) and the one containing the suggestion that the real path of a falling body is circular (F188–193); and in these passages the principle of relativity is not in sight.

Koyré's second reference is to p. 101, where I find the general topic to be the roughness of the lunar surface. More specifically, on that page we find the ending of the discussion of the experiment comparing reflections onto a wall from a flat and from a spherical mirror, and the beginning of the discussion where Salviati attempts to explain the just-observed experimental facts in terms of eye irradiation and of the microscopic irregularities of the reflecting surface. Perhaps, Koyré meant a different page.

Koyré's other references (F139, F141, F139 ff., and F141 ff.) are to the beginning of the Second Day, after the preliminary discussion criticizing the slavish followers of Aristotle's authority. The passage (F139–143) begins with Salviati saying:

> Then let the beginning of our reflections be the consideration that whatever motion comes to be attributed to the earth must necessarily remain imperceptible to us and as if nonexistent, so long as we look only at terrestrial objects; for as inhabitants of the earth, we consequently participate in the same motion. But on the other hand it is indeed just as necessary that it display itself very generally in all other visible bodies and objects which, being separated from the earth, do not take part in this movement. So the true method of investigating whether any motion can be attributed to the earth, and if so what it may be, is to observe and consider whether bodies separated from the earth exhibit some appearance of motion which belongs equally to all.[27]

Here Koyré not only takes this passage out of context, but he manages to perpetrate an equivocation with the Italian word *principio*. This can mean either 'beginning' or 'principle'. The text suggests 'beginning' as the meaning, and both Drake and Salusbury-Santillana[28] translate it this way. Koyré *interprets* the meaning as being, "Let the principle of our reflections be …" More importantly, the context of the

[27] Galilei 1967, 114; cf. K139–140.

[28] Santillana 1953, 127.

present passage is as follows. The discussion mentioned by Koyré is part of a passage containing several arguments favorable to the earth's rotation (F139–150); this is followed by another passage containing statements of the arguments against (F150–159); and then come the critiques of these arguments (F159–244). The favorable arguments are explicitly labeled by Galileo as being merely probable (because based on the principle of simplicity); as requiring the removal of the apparently conclusive counter-evidence, since a single conclusive objection would overcome all probable arguments (F148); and as being based on a principle of relativity of motion which both parties agree is good old Peripatetic doctrine, and perhaps even older than Aristotle (F142). Therefore, the principle being explicitly laid down here must be that of optical, not mechanical, relativity. The reasons for this are the presentation of it as Aristotelian doctrine, just mentioned, and the explicit requirement that it remains to be shown that there is no conclusive counter-evidence (F148–150). For Galileo is very clear that on the basis of the kind of relativity mentioned here, because of the principle of simplicity, it only follows that, *other things being equal*, the earth's rotation is more probable. At this stage Simplicio believes that the other things are *not* equal, since he thinks there are conclusive arguments against the earth's motion; also at this stage Salviati is confident that he will be able to show that the other things *are* equal, though it is obvious that he has not yet done so. The other things that have to be equal are all the terrestrial phenomena usually mentioned as proof of the earth's rest: these phenomena have to be equally explicable by the motion and by the rest of the earth. So Salviati knows that his task is to show this; but this is what is usually called the principle of mechanical relativity, which is very different from that of optical relativity, and which everyone including Koyré agrees to be one of Galileo's great innovations. But this mechanical relativity is not the principle already stated in this passage but the one that remains to be shown. In Salviati's words:

> All inconveniences will be removed as you propound them. Up to this point, only the first and most general reasons have been mentioned which render it not entirely improbable that the daily rotation belongs to the earth rather than to the rest of the universe. Nor do I set these forth to you as inviolable laws, but merely as plausible reasons. For I understand very well that one single experiment or conclusive proof to the contrary would suffice to overthrow both these and a great many other probable arguments. So there is no need to stop here; rather let us proceed ahead and hear what Simplicio answers, and what greater probabilities or firmer arguments he adduces on the other side …
>
> Finding out whether both positions satisfy us equally well will be included in the detailed examination of the appearances which they have to satisfy. For we have argued *ex hypothesi* up to now, and will continue to argue so, assuming that both positions are equally adapted to the fulfillment of all the appearances.[29]

In fact, when the time comes to answer the objections, Galileo does not answer them in the way portrayed by Koyré, namely by appealing to the principle of mechanical relativity, on the basis of which the answers would be trivial, and then by trying to familiarize and accustom Simplicio to this new concept (K222–223, K231–236). Instead Galileo engages in a detailed critical analysis showing that the evidence adduced by the objections cannot be used to decide whether or not we are on a

[29] Galilei 1967, 122–124.

moving earth. Let us take the vertical fall argument (F164–166). Of course, given the principle of mechanical relativity, "if the rock and the tower share the same motion of the earth, then this motion will be for them as nonexistent, and everything will happen as if it did not really exist, that is, as if the earth were motionless," as Koyré puts it (K223). And, of course, "that's exactly what the Aristotelian cannot admit," as Koyré also says (K223). But that is why Galileo attempts to prove his point without previously assuming the principle of mechanical relativity. As we saw above, Galileo argues, by very concrete and basically correct considerations, that the argument from vertical fall begs the question, because we can't know that bodies really fall vertically unless we know that the earth stands still. Though Galileo here does not argue explicitly for the principle of mechanical relativity, it is obvious what the next few steps of the argument would be. Given Galileo's detailed critique of the objection from vertical fall, it follows that the phenomenon of vertical fall, that is fall which is really perpendicular to the earth's surface, cannot be used to prove the motionlessness of the earth. But since we can't know that vertical fall is a fact unless we know that the earth stands still, it follows that we can't know that vertical fall is a fact unless we know that the earth moves; and from the latter it follows that vertical fall can't be used to prove the motion of the earth either. Therefore, vertical fall cannot be used to prove either that the earth moves or that it doesn't. So here we have one crucial phenomenon that has been shown to obey the principle of mechanical relativity.

However, the fact that this is true for one phenomenon doesn't mean that it will be true for other phenomena; and so Galileo criticizes the east–west gunshot argument and ends up showing that the range of east–west gunshots also obeys the principle. Thus Galileo is not being repetitious for the sake of familiarizing Simplicio with a new concept by recourse to experience, as Koyré seems to think. Moreover, there is no real need for him to make it look as if it were Simplicio who was requiring the recourse to experience. Koyré so claims when he says that "the reader contemporary to Galileo … through Simplicio's mouth, once again asks for a recourse to experience: 'I should like, he tells us, to find some means of making an experiment concerning these projectiles …'" (K234). A footnote refers us to p. 194 of the *Dialogue*. When this is consulted one finds that the speaker is Salviati, not Simplicio![30] Finally, the specific reasons, not based on a previous assumption of mechanical relativity, why the range would be the same on a moving earth, involve a comparison to shooting arrows from a moving cart (F193–197), and a comparison between this and shooting them by throwing them with different speeds from a motionless cart. Galileo argues that the arrows can be made to move through correspondingly different distances either by shooting with equal force from a moving cart or by imparting them different initial speeds from a motionless one, for the equal forces from a moving cart generate more speed in the arrow in the direction of the cart and less in the opposite direction than the same forces do on a motionless cart; the different traveled distances, in turn, generate equal ranges from a moving cart. Galileo's argument hinges on the analogy between the earth and the cart, and on the difference in effective force produced by

[30] The English translation of Koyré's book would later correct this mistake; cf. Koyré 1978, 172, 226 n. 181.

applying the same force in different directions on a moving cart. It follows, that the range of east–west gunshots is a phenomenon that would occur in the same way whether the earth moves or not. To that extent the principle of mechanical relativity is thereby justified.

19.5 Conclusion

In conclusion, we may say that Koyré's account of the logic of the Aristotelian objections to the earth's motion and of Galileo's counter-arguments is mistaken both in its fundamentals and in its details. The basic problems seem to be superficiality in logical analysis, oversimplification, injudicious exaggerations, and questionable manipulation of the text by means of excessive quotations, of taking passages out of context, and of not infrequent scholarly carelessness. Nevertheless, Koyré does deserve the credit for having called attention to the logical structure and validity of Galileo's arguments and to his rationalism, even though he misunderstands the former as circular and misinterprets the latter as apriorism.

Finally, it would be unhistorical to deny that the study of the history of science made great progress with Koyré; to turn the clock backwards is simply unthinkable. Nevertheless, even historical sensibility is not an absolute methodological requirement; indeed, a purely historical understanding of the Koyré case would presently be rather injudicious. For at a time when the technique of error analysis which he pioneered is undergoing a "cancerous growth,"[31] and when the apriorist rationalism which he defended is acquiring the status of a dogma, the proper thing to do is to be critical of Koyré. The fact that the logical analysis advocated here may be seen to stem from his technique of error analysis, and that our emphasis on reasoning may be taken to resemble his emphasis on rationalism, ought to serve as a warning not to lower one's standards of conceptual clarity.[32]

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[31] To give an ironical twist to this phrase which Agassi used to characterize the pre-Koyré period in the historiography of science; see Agassi 1963, 33–40.

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Chapter 20
Feyerabend's *Against Method*: Rationalism vs. Pseudo-irrationalism

Abstract This is a critical examination of Paul Feyerabend's *Against Method* (originally published in 1975). I argue that, although Feyerabend's book may superficially appear as primarily destructive, he is really practicing a relatively novel and essentially sound approach to the analysis of scientific rationality; Feyerabend's approach is a concrete, empirical, historical, and rhetorical one, and his account of Galileo's methodology is meant to be a case study. I also argue that Feyerabend's account of Galileo's methodology is not really irrationalistic, but pseudo-irrationalistic; in reality, Galileo proceeds rationally for Feyerabend, as long as the concept of scientific rationality is expanded to allow for rhetorical factors that are a-logical rather than anti-logical, and for epistemological practices neglected by orthodox scholars but ultimately reducible to reasoning and argumentation. Finally, I elaborate, along such lines, a critical appreciation of Feyerabend's discussion of Galileo's critique of the vertical-fall argument against the earth's motion.

20.1 Feyerabend's Approach to Scientific Rationality

Few recent philosophers have been as involved in solving, or at least dissolving, the problem of scientific rationality as Paul Feyerabend. Thus, since his views are relatively well-known and accessible in several languages,[1] I shall formulate mine in the course of a critical examination of his recent book *Against Method.*

One of the most attractive features of Feyerabend's approach is what I shall call its *concreteness*. This is not his word, but it is obviously what he has in mind when, for example, he criticizes Carnap's excessively abstract approach (p. 183, n. 7).[2] Moreover, Feyerabend's use of historical evidence is an expression of this concreteness, for he is not interested in history as such, but insofar as it puts the philosopher in better contact with scientific practice.

[1] Feyerabend 1971, 1973, 1974a, b, 1975, 1976.

[2] Hereafter references to *Against Method* (Feyerabend 1975) will be made in parenthesis in the text.

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Second, I agree with the *anthropological* aspect of Feyerabend's approach (Ch. 17, esp. pp. 249–260, and p. 252). This partly corresponds to what some would call the phenomenological method (in the sense of existential phenomenology), and partly to the historical method, in one sense of 'historical', according to which one suspends one's own point of view and takes the point of view of the historical agents. To be sure, Feyerabend does not always practice this anthropological method that he preaches. For example, he portrays Galileo as a counter-inductivist, which is behavior in accordance with the rule that "advises us to introduce and elaborate hypotheses which are inconsistent with well-established theories and/or well-established facts" (p. 29). However, none of this evidence, even if it were otherwise acceptable, tends to show that Galileo introduces hypotheses which *he believes* are inconsistent with well-established theories and/or facts; for example, Feyerabend gives no evidence that Galileo agrees with him and with the Aristotelians in thinking that the Ptolemaic system was well established. For someone following the anthropological method, it is Galileo's actual thought and beliefs that should be reconstructed. However, in spite of such lapses, Feyerabend's "anthropological" orientation is to be welcomed.

A third acceptable feature of Feyerabend's approach is his normative, critical orientation. He wants to find out not only what scientific rationality is, but also whether it is good or bad, and he wants to reform it insofar as it is bad. This may seem to conflict with the anthropological approach insofar as the latter could be interpreted as emphasizing description, rather than prescription. There is no conflict, however, because Feyerabend is critical primarily of contemporary science anthropologically understood, and he grounds this criticism on past science also anthropologically understood. In other words, he uses certain parts (historical stages) of science against other parts. To be sure, Feyerabend also tends to exaggerate his criticism. For example, noting that a streak of intolerance has developed within modern science, he bemoans the fact "while an American can now choose the religion he likes, he is still not permitted to demand that his children learn magic rather than science at school" (p. 299). However, there is no good reason to follow him in going that far. Moreover, some of his criticism is grounded on extrascientific factors, for example that parts of modern scientific education "cannot be reconciled with a humanitarian attitude" (p. 20); but such criticism, however intrinsically plausible, derives its relevance from the other grounded on the history of science. Thus, Feyerabend's destructive, antiscientific pronouncements should not be confused with the healthy, reformist, critical stance.

Fourth, I agree with Feyerabend's argument that scientific rationality does not consist of normative principles stated in terms of the theory/observation distinction. His argument would be that as long as we use such concepts, then we are bound to value theories which are well-grounded on facts, and hence to be intolerant toward theories that conflict with facts or other well-established theories. But such intolerance will make it difficult or impossible for better theories to be formulated or new facts discovered because a principle which Feyerabend labels "counter-induction" can be shown to be a very effective method. Feyerabend supports the counter-inductive rule with an historical and a philosophical argument. The historical argument consists of evidence that most of the great discoveries in the history of

science have had this feature of conflicting with previously well-established theories and/or facts. His philosophical argument has two parts, one relating to theoretical counter-induction, the other to factual counter-induction. First, given a very general theory, some facts cannot be discovered except with the help of a conflicting alternative theory; hence if we value new facts, and if we want to be critical toward a well-established theory, we must consider alternatives; in short, the improvement of a general theory, no matter how well-established, is best done by the external criticism of contrast to other theories, rather than by internal criticism of comparison with experience. As for factual counter-induction, historical investigation shows that there is not a single interesting theory that agrees with all known facts in its domain; and epistemological analysis shows that every observational report presupposes some theoretical assumption; therefore it is a mistake to take "facts" too seriously, so seriously as to prevent the consideration of theories that conflict with them. From such arguments Feyerabend does *not* conclude that counter-induction ought to be the new rule for the conduct of scientific research, but rather that even such an obvious rule as the inductive principle, which counter-induction opposes, is no guarantee of success (pp. 32–33). However, he does conclude that there can be no rules of any kind, whereas the only thing that follows is that there are no rules of the kind considered by him, namely formulated in terms of the theoretical/observational distinction. In other words, Feyerabend's arguments "against method," as he puts it, are really arguments that we ought to do away with the theory/observation distinction (cf., e.g., p. 168). However, as he himself argues in his book, theories are not rejected until there is an alternative to replace them; applying this to the theory of the "theory/observation" distinction, we note that he has not provided an alternative way of studying scientific rationality, which would replace the philosophy of science based on the theory/observation distinction. The present investigation is in part an attempt to provide such an alternative by using the premise/conclusion distinction of logicians, that is by using the concepts of argument and reasoning of elementary logic.

This brings us to a consideration of Feyerabend's discussion of the role of reason in science, for after all his book is full of the rhetoric of irrationalism. The first point to be made here is that even if he were otherwise right, his irrationalism would be wrong. That is, even if it were true that "*essential ingredients of modern science survived only because reason was frequently overruled in their past*" (p. 145, ital. in original; cf. p. 155), this would only show that it is sometimes reasonable to overrule "reason," which can therefore be only what was thought to be, but is not really, reason. Thus, in this type of argument, far from it being the case that Feyerabend can't lose (as some critics have pointed out in other contexts),[3] he can't win.

Moreover, Feyerabend frequently (e.g., pp. 153–154) fails to distinguish the nonrational from the irrational, and just because a certain procedure is not in accordance with reason, he concludes that it is in violation of reason. Just as frequently, Feyerabend concludes that something is irrational just because it goes against what

[3] Gellner 1975.

his opponents (be they empiricists, inductivists, or Popperians) would call "rationality" (cf., e.g., pp. 179–180). The best example of this is his analysis of Galileo, which will be criticized in detail below. Here I merely wish to point out that Feyerabend is at pains to point out that *he* thinks that Galileo's procedure was highly desirable, though it supposedly goes against widely accepted methodological principles, so that *those who accept them* would have to regard it as irrational.

Let us look at some of Feyerabend's specific points against "reason." His considerations can be divided into three arguments, which we may call the insufficiency of reason argument, the argument from incommensurability, and the historical argument. In the first, Feyerabend argues as follows. Since the teaching of small children is not exclusively a matter of argument, and since there are processes like the mastery of a language which look like the result of reason but are due partly to indoctrination and partly to *natural* processes of growth, it follows that nonrational growth is possible both in adults and in institutions. Therefore "even the most puritanical rationalist will then be forced to stop reasoning and use *propaganda* and *coercion*" (p. 25) whenever conditions are such that "forms of argumentation turn out to be too weak" (p. 25) to accomplish his ends. Feyerabend's argument may be accepted with the following qualifications. There is nothing irrational in sometimes stopping reasoning; there would be only if one assumed that reasoning is the only proper activity human beings can engage in, an assumption no rationalist needs to make. Moreover, there is no need to resort to propaganda and coercion, for propaganda is a perversion of rhetoric, while coercion is a misuse of noncognitive causes, and such perversions and misuses may or may not be used by the rationalist; the only thing that follows is that rhetoric and noncognitive causes may have to be used; but such things are nonrational rather than irrational, and they are susceptible of being handled properly as well as improperly. So the conclusion to be reached is that reasons and arguments are not the only causes that affect human actions and thought; there are also rhetoric and noncognitive causes that operate.

Now at this point, Feyerabend would like to add that therefore it would be very strange if arguments were the only thing operating in scientific rationality, so strange that even if that *were* the case, then one should rebel against such unnatural restrictions and help to create a more human science in which the other factors are not excised. To this we may reply by asking why should every human activity be a microcosm of the entire human life? Why can't there be some activity, e.g., science, where the only rules of the game involve argumentation? As long as we remember that there are other things in life, why should this activity of reasoning which we have labeled "science" be mixed with the others? I believe Feyerabend's only plausible answer could be, because as a matter of (historical) fact what we label "science" does contain a mixture of elements. Here we would leave Feyerabend's general considerations and go to his historical argument, which therefore is more crucial to his position than he makes it seem.

Before examining Feyerabend's historical argument, however, let us look at the one from incommensurability, which he thinks "creates problems for all theories of rationality" (p. 214). Feyerabend calls two theories incommensurable (pp. 223, 228–229, 269) when the subject matter to which they refer differs so radically that

(1) the existence of the entities and processes presupposed by one theory implies the nonexistence of the entities and processes presupposed by the other theory, and (2) it makes no sense to say that one theory is a generalization of the other, or that they overlap. The main step in the argument consists in trying to establish that there are scientific changes where the theories involved are incommensurable, e.g., Aristotelian and Galilean physics, classical and quantum physics, and Newtonian and relativistic mechanics (pp. 224–225, 271, 276–277); given this incommensurability, Feyerabend concludes that one cannot say that the new theory which replaces the old is epistemologically better. However, if we distinguish progress from rationality, then we realize that Feyerabend's conclusion does not imply that the agents involved in the transition did not act rationally. In fact, while answering a number of philosophical objections against his incommensurability claims (pp. 277–285), Feyerabend himself argues cogently that, in spite of incommensurability, it is still possible to behave rationally; for example, a self-inconsistent observation report would disconfirm the corresponding theory (p. 278); one could learn the meaning of the new theory the way anthropologists learn the language of newly-discovered tribes (pp. 278–282); and crucial experiments are still possible (pp. 282–283). So Feyerabend has not shown that incommensurability creates problems for theories of rationality, but rather that it creates problems for theories of progress. Moreover, since, besides being distinct, rationality and progress are related, the possibility of rationality opens up a minimal possibility of progress, namely that changes involving incommensurable theories are changes for the better in the sense that they result from the behavior of rational agents.

But how shall we test for the actuality of these possibilities still left open after Feyerabend's arguments? The answer, whose suggestion can be found in Feyerabend himself, is that the test is to be conducted by anthropological field work: "Let us commence field work in this domain also, and let us study the language of new theories not in the definition-factories of the double language model, but in the company of those metaphysicians, physicists, playwrights, courtesans, who have constructed new world views!" (p. 282). When the event under consideration is relatively far into the past, like the emergence of modern science in the 17th century, the only kind of field work possible is the analysis of historical records. However, these historical records must be sufficiently rich, and the analysis must be conducted with an appropriate "anthropological" spirit. For the emergence of modern science a sufficiently rich record can be found in Galileo's *Dialogue*, parts of which have therefore been examined by Feyerabend, though, alas, without a sufficiently "anthropological" attitude, as I shall soon show.

This brings us to his historical argument against reason. Here he tries to show that "*essential ingredients of modem science survived only because reason was frequently overruled in the past*" (p. 145). When we look at the details of the argument (e.g., pp. 145–161, 179–180, 196–200), we discover, as mentioned earlier, that the reason which was overruled was usually what empiricists, inductivists, Popperian critical rationalists, or Imre Lakatos *think* is reason. In other cases, there was no *overriding* of reason but rather a combination of it with nonrational factors; nor does Feyerabend show that such nonrational factors were improper from their own point of view. All

of the strengths and all of the limitations of Feyerabend's position are present in his analysis of Galileo, and so it will be valuable to examine this in detail.

20.2 Feyerabend on Galileo's Methodology

The longest case study on which Feyerabend grounds many of his philosophical conclusions concerns Galileo. The account consists of four elements: an interpretation of Galileo's work in terms of a definite procedure which he allegedly follows; a description of this procedure in terms of such concepts as theory, observation, propaganda, appeals to emotion and prejudices, etc.; a historical explanation of the *fact* of Galileo's success as resulting from the procedures he used; and a favorable evaluation of Galileo insofar as the procedure he follows is "*perfectly reasonable*" (p. 143).

One need not have mentioned the first item in this list were it not for the fact that all of Feyerabend's talk against method may easily be interpreted as showing that the concept of method is a useless one in the philosophy of science. It is clear, however, from Feyerabend's "actual procedure" *(his* method, as it were) that he is only arguing against "the idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science" (p. 23), i.e., "the idea that science can, and should be run according to fixed and universal rules" (p. 295). In a few places (e.g., pp. 145, 163) he even uses the forbidden word "method" to refer to Galileo's procedure. So Feyerabend's point is really that no general theory of method is defensible, which says that all scientists in all situations use a certain definite method; instead different scientists use different methods on different occasions. It is important to note, however, that Feyerabend's account presupposes that the concept of method is a very useful one in understanding what a scientist does; a method becomes the effective cause (in a non-deterministic sense) in the historical explanation of a given scientific success. I do not think that such method-explanations of success are objectionable on general grounds; I think that such accounts provide historical understanding and can even be used to ground philosophical theses.[4] However, in this case the following questions must be raised.

First, it may be that Feyerabend is not being radical enough in ascribing a certain definite method to Galileo; that is, it may be that not even the same scientist follows the same method to any significant extent in his activities. Perhaps there is "anarchy" within the work of a given scientist. What I am saying is that, for Feyerabend, science is an "anarchical" enterprise, but individual scientists are not.[5] There is no

[4] Cf. Finocchiaro 1973, 223–228.

[5] Feyerabend has objected in private correspondence that he is not ascribing a single definite method to Galileo since "he does different things in the case of dynamics and in the case of optics (in the first case he changes the grammar of dynamical terms, in the second case he changes *sensations* by substituting the telescope for the eye)." Though it is true that Feyerabend examines Galileo's work in these two contexts, and that in the sense just described different procedures are operative, it is also true that both are subsumed under Feyerabend's concept of counter-induction, the counter-inductive

logical inconsistency here; but there is a tension within Feyerabend's account since *he* tends to transfer the characteristics from science-the-institution to the individual scientist. I believe there is textual evidence for this tendency in Feyerabend, but let me illustrate the problem as follows. Feyerabend presupposes that one could not understand Galileo's work unless one found *some* order in it, enough to speak of a method or procedure. But if one does this with an individual scientist, why shouldn't one do the same with science as a whole? Of course, one is *not* logically bound to conclude that there is at least as much order in science as in a scientist, but if one chooses to order a given scientist's activities by reference to a definite method, why should one not do enough work so as to order science as a whole into a method? Feyerabend might answer, because this *can* be done in the case of a given scientist, e.g., Galileo, but not for the whole of science. What follows from this is that one has to be very careful about whether or not there is method within a scientist's activities. To some extent this problem reduces to that of describing the method in a sufficiently complex and sophisticated way. However, if this complexity was too great then we wouldn't have a method. In these investigations I am partly testing this assumption that Feyerabend seems to make.

Second, once we realize that Feyerabend depicts an anarchical science consisting of methodical scientists, the possibility arises for a methodical science consisting of anarchical scientists. Given these two abstract alternatives, it is not clear which Feyerabend would choose. Perhaps he would opt for a third possibility of an anarchical science of anarchical scientists; though this may be his wish, it would go against his actual account in *Against Method.*

Third, Feyerabend's method-explanation of Galileo's success never takes seriously the question of the *connection* between the procedures and the success. That is, even if Feyerabend is right about the methods he attributes to Galileo, there is no argument that it was *because* of these methods that he succeeded. There is no question that Feyerabend is committed to such a causal claim (e.g., p. 112), which therefore requires careful examination. One way to conduct this examination is to ask whether the situations in which Galileo proceeded à la Feyerabend involved activities which would be regarded as successful. For example, he examines mostly Galileo's attempts to prove the motion of the earth; however, few people, be they 17th century contemporaries of Galileo, or present-day scholars, would agree that Galileo *succeeded* in his attempts. Feyerabend would be the first to claim that Galileo failed here. But if he failed when he was proceeding as Feyerabend claims, then this is, if anything, evidence that such Galilean procedures led to failure, not that they led to success.

Prescinding now from such problems, let us see how Galileo's method is described. Feyerabend deserves great credit for enlarging Galileo's method beyond the epistemological factors usually considered and for including rhetorical and artistic components. The epistemological factors considered by Feyerabend are the following.

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idea in the former case being the earth's motion, in the latter, the celestial reliability of the telescope. Hence, Galileo's activities are being ordered in a non-anarchical fashion.

(1) Galileo introduces and elaborates hypotheses (e.g., motion of the earth, relativity of motion, celestial reliability of telescopes) which are inconsistent with well-established theories (e.g., geocentrism, operative character of motion, optics) and with well-established facts (e.g., vertical fall, apparent size of Venus and Mars); this procedure is labeled 'counter-induction' (pp. 29, 77, 99–101). (2) Galileo lets inadequate views mutually support each other, e.g., (a) "an inadequate view, the Copernican theory, is supported by another inadequate view, the idea of the non-operative character of shared motion" (p. 89); (b) he lets one refuted view—Copernicanism—support another refuted view—the idea that telescopic phenomena are faithful images of the sky (pp. 141–142); and (c) "Galileo changed his view about the 'neutral' motions—he made them permanent and 'natural'—in order to make them compatible with the rotation of the earth" (p. 96); Feyerabend gives no special name to this aspect of Galileo's procedure, which I shall label "dialectical," to follow some terminology suggested elsewhere.[6] (3) *Ad hocness*: Galileo uncritically accepts any and all ideas and observations which support Copernicanism (e.g., pp. 90, 93–98). (4) Galileo drastically reduced the *content* of dynamics by replacing the Aristotelian comprehensive theory of change, including locomotion, qualitative change, and generation and corruption by a theory dealing merely with the locomotion of matter (pp. 90–100, 160–161); this is what Feyerabend labels the "backward step" (pp. 113, 153, 176).

The rhetorical aspects of Galileo's method are allegedly the following: deceptive tactics (pp. 70, 81, 87, 160); utterances which are arguments in appearances only (p. 81); propaganda (pp. 81, 90, 157, 160); psychological tricks (pp. 81, 88, 154); persuasion by confusion (p. 84); clever techniques of persuasion (pp. 141, 143); misleading insinuations (p. 160); distortion (p. 160); appeal to emotion and prejudice (p. 154); jokes (p. 154); non sequiturs (p. 154).

The artistic, aesthetic, literary factors include "style, elegance of expression, simplicity of expression, tension of plot and narrative" (p. 157), "a sense of humor, an elasticity … and an awareness of the valuable weaknesses of human thinking" (p. 161).

The epistemological factors emphasized by Feyerabend are ones that go against almost every principle held by orthodox philosophers of science; the rhetorical factors seem to violate the ideas of the basic honesty and decency of science, widely held by scientists and laymen alike; the aesthetic factors contradict the alleged gap between the arts and the sciences. Indeed Feyerabend delights in being contrary. His contrariness reaches its highest pitch when he argues that such Galilean procedure is "perfectly reasonable" in itself (pp. 143, 145–161) and fruitful in other fields (pp. 163–164). In other words, Feyerabend's *evaluation* of Galileo's method is the orthodox one, it is his descriptive interpretation of the features of that method that is unorthodox; and since his evaluation is partly grounded on his interpretation, that means that his *reasons* for his evaluation are also unorthodox. Feyerabend does not always *effectively* distinguish his description and his evaluation, though he often seems to or tries (pp. 143, 156). Thus we often find him arguing that because it can

[6] Funkenstein 1975; Finocchiaro 1975.

be shown (by Feyerabend himself) that a certain view was refuted or inadequate, and because therefore Galileo *should* have seen that it was inadequate, therefore it follows that Galileo regarded it as inadequate, and hence that he was acting counter-inductively; or that because it can be shown (by Feyerabend himself) that a certain argument is incorrect, and because therefore Galileo *should* have known that it was incorrect, therefore it follows that he knew it to be incorrect, and hence that his using it amounted to deceptive trickery. We might say that Feyerabend's alleged descriptive interpretations are really evaluations, and hence his whole case is historically unfounded; and since, as we argued earlier, his two other arguments (from the insufficiency of reason, and from incommensurability) depended on the historical argument, we might conclude that Feyerabend's views do not merit any further consideration.

This, however, would be a mistake. We need not deny our inclination to think that Feyerabend is doubly in the wrong, namely that his historical interpretations are descriptively wrong, and that *if* they were right, then that would show that *Galileo* was wrong (irrational). The fact is, or rather my suspicion is, that Feyerabend turns out to be *right in spite of himself*. That is, his account of Galileo is not really irrationalistic, but pseudo-irrationalistic, and in reality Galileo proceeds rationally for Feyerabend; however, our idea of scientific rationality must be expanded first to allow aesthetic and rhetorical factors, and second epistemological practices proscribed by orthodox philosophies of science; moreover the aesthetic and rhetorical factors are by themselves merely *alogical*, and they must be judged by their own criteria; finally, whatever unorthodox epistemological practices Galileo may engage in, the real test of their rationality or propriety is their correspondence to basic and elementary forms of reasoning and argumentation, rather than to philosophically articulated *theories* of scientific rationality, most of which presuppose the theory/observation distinction, for example. Keeping this in mind, I now proceed to test my suspicion. I shall do so by examining Feyerabend's account of the tower argument.

20.3 The Tower Argument

The tower argument was one of the classical objections to the earth's rotation. Galileo states and criticizes it in the following passage:

> *SALV*: … Aristotle says, then, that a most certain proof of the earth's being motionless is that things projected perpendicularly upward are seen to return by the same line to the same place from which they were thrown, even though the movement is extremely high. This, he argues, could not happen if the earth moved, since in the time during which the projectile is moving upward and then downward it is separated from the earth, and the place from which the projectile began its motion would go a long way toward the east, thanks to the revolving of the earth, and the falling projectile would strike the earth that distance away from the place in question. Thus we can accommodate here the argument of the cannon ball as well as the other argument, used by Aristotle and Ptolemy, of seeing heavy bodies falling from great heights along a straight line perpendicular to the surface of the

earth. Now, in order to begin to untie these knots, I ask Simplicio by what means he would prove that freely falling bodies go along straight and perpendicular lines directed toward the center, should anyone refuse to grant this to Aristotle and Ptolemy.

SIMP: By means of the senses, which assure us that the tower is straight and perpendicular, and which show us that a falling stone goes along grazing it, without deviating a hairsbreadth to one side or the other, and strikes at the foot of the tower exactly under the place from which it was dropped.

SALV: But if it happened that the earth rotated, and consequently carried along the tower, and if the falling stone were seen to graze the side of the tower just the same, what would its motion then have to be?

SIMP: In that case one would have to say "its motions," for there would be one with which it went from top to bottom, and another one needed for following the path of the tower.

SALV: The motion would then be a compound of two motions; the one with which it measures the tower, and the other with which it follows it. From this compounding it would follow that the rock would no longer describe that simple straight perpendicular line, but a slanting one, and perhaps not straight.

SIMP: I don't know about its not being straight, but I understand well enough that it would have to be slanting, and different from the straight perpendicular line it would describe with the earth motionless.

SALV: Hence just from seeing the falling stone graze the tower, you could not say for sure that it described a straight and perpendicular line, unless you first assumed the earth to stand still.

SIMP: Exactly so; for if the earth were moving, the motion of the stone would be slanting and not perpendicular.

SALV: Then here, clear and evident, is the paralogism of Aristotle and of Ptolemy, discovered by you yourself. They take as known that which is intended to be proved.

SIMP: In what way? It looks to me like a syllogism in proper form, and not a *petitio principii.*

SALV: In this way: Does he not, in his proof, take the conclusion as unknown?

SIMP: Unknown, for otherwise it would be superfluous to prove it.

SALV: And the middle term; does he not require that to be known?

SIMP: Of course; otherwise it would be an attempt to prove *ignotum per aeque ignotum.*

SALV: Our conclusion, which is unknown and is to be proved; is this not the motionlessness of the earth?

SIMP: That is what it is.

SALV: Is not the middle term, which must be known, the straight and perpendicular fall of the stone?

SIMP: That is the middle term.

SALV: But wasn't it concluded a little while ago that we could not have any knowledge of this fall being straight and perpendicular unless it was first known that the earth stood still? Therefore in your syllogism, the certainty of the middle term is drawn from the uncertainty of the conclusion. Thus you see how, and how badly, it is a paralogism.[7]

[7] Galilei 1967, 139–140.

The argument which according to Galileo's spokesman, Salviati, begs the question is the one contained in Salviati's first speech in this passage; it may be reconstructed as follows:

(1) Bodies fall vertically.
(2) If the earth rotated, bodies could not fall vertically,

because

(3) If the earth rotated, then while a body was falling the place on the earth directly below it would be carried along toward the east and the body would land to the west of where it was originally dropped.

Therefore,

(4) The earth does not rotate.

Galileo has no objection to the (formal) validity of the last step in this argument (the one from (1) and (2) to (4)); however, he questions its soundness in terms of whether premise (1) is justified. The Aristotelian spokesman, Simplicio, gives the following justification:

(1) Bodies fall vertically

because

(5) Bodies *are seen* to fall vertically.

Now, with respect to this argument Galileo accepts the premise but questions its (formal) validity. This validity depends on whether or not apparent vertical fall implies actual vertical fall. This implication needs justification because it would *not* hold on a rotating earth. In fact, if the earth *were* rotating and bodies were *seen* to fall vertically, then in actuality they would be following a path slanted to the earth's surface; that is, on a rotating earth, apparent vertical fall would *not* imply actual vertical fall. Now, how could one possibly justify the implication? The only relevant argument would seem to be the following one:

(6) If the earth does not rotate, then apparent vertical fall implies actual vertical fall.
(7) The earth does not rotate.

Therefore,

(8) Apparent vertical fall implies actual vertical fall.

Premise (6) is indeed true, and the argument is (formally) valid. Unfortunately, premise (7) is identical with conclusion (4) of the original argument, which is being examined. In short, the vertical fall argument, from (1) and (2) to (4), is such that one of its premises (1) is being justified on the basis of the same proposition (7) [=(4)] it has for conclusion.

The important issues can be put into focus in terms of the following argument, which combines the three separate segments just discussed, and where some obvious symbolic abbreviations have been made and the proposition numbers correspond:

(6) If not-R, then S implies V.
(7) Not-R.
(8) Therefore, S implies V.
(5) S.
(1) Therefore, V.
(2) If R, then not-V.
(4) Therefore, Not-R.

Logically speaking, this is a very interesting deduction; in particular, its three steps are valid. Rhetorically speaking, however, the identity of propositions (7) and (4) makes it worthless: someone who does not adhere to (4), will not be any more favorably inclined toward it after this argument, which requires such adherence already at the very beginning.

In Feyerabend's account four main theses are involved. First (pp. 70–75), the Aristotelian tower argument presupposes naïve realism with respect to motion (p. 75); and it is important to notice that this Aristotelian presupposition takes the form of what Feyerabend calls a "natural interpretation"; that is, "one does not first distinguish the apparent motion from the real motion and then connect the two by a correspondence rule. One rather describes, perceives, acts towards motion as if it were already the real thing" (p. 75). Second (pp. 75–78), Galileo discovered this Aristotelian natural interpretation counter-inductively. Third (pp. 78–81), Galileo tests and examines the Aristotelian identification of real and apparent motion by introducing a different interpretation which "restores the senses to their position as instruments of exploration, *but only with respect to the reality of relative motion.* Motion 'among things which share it in common' is 'non-operative', that is, 'it remains insensible, imperceptible, and without any effect whatever'" (p. 78). Fourth (pp. 81–92), Galileo uses, and needs to use, propaganda, psychological tricks, and deception in elaborating this new concept of the relativity of motion.

It is a great insight for Feyerabend to have arrived at the first of these conclusions, though it must be qualified to restrict it to vertical fall, so as to say that the tower argument presupposes, as a "natural interpretation," the identification of real and apparent *vertical fall.* Feyerabend himself goes through the motions of making the qualification (p. 75), but he then forgets its importance in the rest of his discussion. At any rate, it is possible to give a textual and logical proof of the qualified conclusion.[8]

However, that same analysis also shows that Feyerabend is completely wrong in his second thesis; Galileo is simply not aware of this Aristotelian presupposition of the tower argument, so he neither discovers it counter-inductively, nor does he discover it at all. *We* can discover it by logical manipulations, but these are merely suggested by the text, nowhere contained in it.[9] Feyerabend formulates his second

[8] Cf. Finocchiaro 1974.

[9] In my reconstruction, twenty five steps are needed to arrive at a conclusion of this form: R if and only if not-(S if and only if V). Feyerabend's insight is to realize that this proposition is implied by Galileo's critique, but his error also occurs at this point. Being under the wrong impression that Galileo is *here* not merely trying to refute the tower argument, but also trying to prove the earth's motion, and confusing the tower and the deception of senses arguments, Feyerabend has

thesis because he approaches the philosophy of science with the spectacles of the theory/observation distinction; so he wants to reconstruct some theory which the Aristotelians held, and the one that Galileo introduced. If Feyerabend had approached the tower argument as an *argument*, which is the way Galileo does, then he would have been inclined to interpret Galileo's discovery as one about an argument, namely that the tower argument is a *petitio principii.* Though Galileo could have proceeded to inquire about general theories and natural interpretations, the fact is that he does not do so; and he does not do so because he seems to be interested in *arguments.*

Thirdly, Galileo introduces his concept of the relativity of motion *not* to test a presupposition of the tower argument, but to answer the objection to the earth's motion from the deception of the senses. Feyerabend thinks as he does because he fails to distinguish two different arguments against the earth's motion, namely the one from vertical fall and the one from the deception of the senses. The logic of the two arguments is different. Galileo correctly criticizes the first as being circular, the second as being groundless,[10] in the sense that the earth's motion would *not* involve any deception of the senses; and there would be no deception of the senses because of the relativity of motion. In other words, it's not a case where one does not see something one ought to see (the real slanted path on a rotating earth); there is no reason why one ought to see such a slanted path because our eyes can only detect motion relative to them; that our eyes are so built can be proved by experiences on moving systems such as ships. Such being the logic of the argument from the deception of the senses, and Galileo's use therein of the relativity of motion, Galileo needs neither propaganda, nor tricks, nor deceptions, which are figments of Feyerabend's imagination, as we will see below. He so imagines because he thinks wrongly that Galileo is using the relativity of motion in discussing the tower argument. Feyerabend does not realize that Galileo's analysis of this argument is not designed to produce a positive, substantive conclusion about phenomena, but rather a negative destructive criticism of the Aristotelian argument. For this purpose Galileo does not *need* the relativity of motion. *If* Galileo *were* using the relativity of motion in connection with the tower argument, and *if* he *were* thereby trying to prove the earth's motion rather than merely refuting the argument, *then* Galileo would have to resort to propaganda, tricks, and deception, since that task cannot be accomplished by legitimate means. However, such a conditional is counter-factual.

Galileo support the right side of this biconditional, namely not-(S if only if V), on the basis of the relativity principle. Feyerabend's reconstruction is the following: apparent and actual vertical fall are distinct on moving systems like a ship, since only relative motion is visible in such systems; hence apparent and actual fall *with respect to the earth* are distinct. Feyerabend correctly points out that this amounts to subsuming vertical motion under 'Paradigm II: Motion of objects in boats, coaches, and other moving systems' (p. 87) instead of under 'Paradigm I: Motion of compact objects in stable surroundings of great spatial extension—deer observed by the hunter' (p. 87). He is also right in saying that the gap between these two paradigms is unbridgeable, in the sense that there is no noncircular way of choosing the former for the purpose of applying it to vertical fall. He is wrong, however, in attributing such an argument to Galileo, who wisely avoided it.

[10] For statements and critiques of the argument on the deception of the senses see Galilei 1890–1909, 7: 197–200, 272–281; 1967, 167–71, 233–247.

At this point the question arises, whether Galileo is doing anything wrong in the context of his discussion of the deception of the senses argument, where he admittedly uses the relativity principle. In that context, Galileo refutes the objection by arguing that there would be no deception of the senses if we experienced apparent vertical fall on a rotating earth, because only motion relative to the observer is perceivable. In this critique Galileo is certainly subsuming vertical fall under what Feyerabend calls 'Paradigm II' (p. 87), namely the paradigm of motion in a moving system, like a boat. Is he therefore being deceptive, etc. in so doing? Not at all, because this is the same paradigm used in the statement of the objection, which is that *on a rotating earth* our senses would be deceived in experiencing the appearance of vertical fall while in actuality the fall was over a slanted path. In other words, this *different* argument against the motion of the earth cannot even be made without using Paradigm II. Hence, in answering it, it is quite proper to use the same paradigm.

And this brings us to Feyerabend's fourth thesis. He is certainly right that Galileo uses rhetorical alogical considerations which are interesting to examine and which play an important function. However, their propriety is a very complicated affair. In order to begin to understand this matter, it is essential to have in front of us the text of the dialogue immediately following the above quotation:

SAGR: On behalf of Simplicio I should like, if possible, to defend Aristotle, or at least to be better persuaded as to the force of your deduction. You say that seeing the stone graze the tower is not enough to assure us that the motion of the rock is perpendicular (and this is the middle term of the syllogism) unless one assumes the earth to stand still (which is the conclusion to be proved). For if the tower moved along with the earth and the rock grazed it, the motion of the rock would be slanting, and not perpendicular. But I reply that if the tower were moving, it would be impossible for the rock to fall grazing it; therefore, from the scraping fall is inferred the stability of the earth.

SIMP: So it is. For to expect the rock to go grazing the tower if that were carried along by the earth would be requiring the rock to have two natural motions; that is, a straight one toward the center, and a circular one about the center, which is impossible.

SALV: So Aristotle's defense consists in its being impossible, or at least in his having considered it impossible, that the rock might move with a motion mixed of straight and circular. For if he had not held it to be impossible that the stone might move both toward and around the center at the same time, he would have understood how it could happen that the falling rock might go grazing the tower whether that was moving or was standing still, and consequently he would have been able to perceive that this grazing could imply nothing as to the motion or rest of the earth.

Nevertheless this does not excuse Aristotle, not only because if he did have this idea he ought to have said so, it being such an important point in the argument, but also, and more so, because it cannot be said either that such an effect is impossible or that Aristotle considered it impossible. The former cannot be said because, as I shall shortly prove to you, this is not only possible, but necessary; and the latter cannot be said either, because Aristotle himself admits that fire moves naturally upward in a straight line and also turns in the diurnal motion which is imparted by the sky to all the element of fire and to the greater part of the air. Therefore if he saw no impossibility in the mixing of straight-upward with circular motion, as communicated to fire and to the air up as far as the moon's orbit, no more should he deem this impossible with regard to the rock's straight-downward motion and

the circular motion natural to the entire globe of the earth, of which the rock is a part.

SIMP: It does not look that way to me at all. If the element of fire goes around together with the air, this is a very easy and even a necessary thing for a particle of fire, which, rising high from the earth, receives that very motion in passing through the moving air, being so tenuous and light a body and so easily moved. But it is quite incredible that a very heavy rock or a cannon ball which is dropping without restraint should let itself be budged by the air or by anything else. Besides which, there is the very appropriate experiment of the stone dropped from the top of the mast of a ship, which falls to the foot of the mast when the ship is standing still, but falls as far from the same point when the ship is sailing as the ship is perceived to have advanced during the time of the fall, this being several yards when the ship's course is rapid.

SALV: There is a considerable difference between the matter of the ship and that of the earth under the assumption that the diurnal motion belongs to the terrestrial globe. For …[11]

It was certainly a clever move for Galileo to begin his critique of the tower argument (in the earlier quotation) by interpreting it to refer to *actual* vertical fall. Feyerabend is right in saying that the Aristotelian argument simply interchanges apparent and actual fall without distinguishing them; so Galileo could have begun with either version of the argument. The fault of the argument from *apparent* vertical fall (Sagredo's first speech in the passage just quoted) is that it depends on a premise which is as much in need of proof as the conclusion at issue. This fault is less serious than the circularity of the argument from actual vertical fall. It is indeed more effective to start with the more serious criticism, and so Galileo's first rhetorical move is a judicious one.

Having focused on the actual vertical fall version, Galileo had in front of him a formally valid instance of denying the consequent (*modus tollens*). Of the two premises of this argument, propositions (1) and (2) above, Galileo could have questioned either one. If he had questioned the conditional proposition, the circularity could not have been exhibited as easily since the justification of this conditional premise is circular only insofar as it depends ultimately on the non-conditional premise of the original *modus tollens*, namely the proposition (1) that bodies really fall vertically.[12] It is certainly rhetorically effective, but certainly not improper, to exhibit the failure of an argument *in the easiest possible way.*

Third, when Galileo comes around to discuss the apparent vertical fall version of the argument, this is made to look as a *revised* version of the original argument, and hence to some extent as *ad hoc*. This seems to be a merely rhetorical fault, rather than a logical one. However, the argument is also shown to be dependent on a premise which is as much in need of proof as its conclusion. This feature, though not purely logical, is not purely rhetorical either. For though questions of the comparative knowability of propositions are context-dependent, the context is an epistemological one, rather than one dependent merely on who is speaking to whom.

[11] Galilei 1967, 140–142.

[12] See Finocchiaro 1974, 144–145.

Fourth, the way that the discussion changes from apparent vertical fall to the ship experiment involves rhetorical considerations. Someone might say, following Feyerabend's evaluation of Galileo's rhetoric, that besides introducing the argument from apparent vertical fall in a prejudicial manner, he drops the topic in such a way as to make it look as if Simplicio was giving up on the argument and relying on a new one.[13] This rhetorical evaluation would depend on interpreting Galileo's criticism of the ship/earth analogy,[14] which follows the passage just quoted, as part of his criticism of the ship experiment argument against the earth's motion.[15] This interpretation in turn would involve attributing to Galileo another deception, namely that he would be trying to show that a rock dropped from the top of the mast on a moving ship would land at the foot of the mast, and similarly on a rotating earth, while he does not believe in the analogy. However, it is itself a prejudicial interpretation to regard the passage where Galileo criticizes the ship/earth analogy as a logical part of his discussion of the ship experiment argument. No sound reason can be given for this interpretation, whereas evidence can be given to show that Galileo wants his criticism of the analogy to be part of his criticism of the apparent vertical fall argument. The evidence consists, first, of the logical fact that such a connection exists; for the alleged results of the ship experiment can be cited as evidence that bodies refuse to move simultaneously toward and around the center, and hence as support for the corresponding premise in the apparent vertical fall argument, namely the proposition that if the earth rotated, bodies could not be seen to fall vertically. Moreover, there is textual evidence that this is what Galileo has in mind. For example, the first time that the ship experiment is mentioned[16] is in the context of an argument where the impossibility of mixed motion is given as the explanation for the alleged deflection on the moving ship. Finally, the way the switch is made from the criticism of the analogy to the criticism of the alleged experimental results[17] suggests the same thing: Salviati agrees that "up to this point" the ship experiment as an argument purely from analogy has not been considered. In other words, in this passage Galileo is saying that though the ship experiment has just been criticized insofar as it is the basis of an explanatory argument against mixed motion, it is still standing as a pure analogy argument against the earth's motion. I believe Galileo is right here, and that a pure analogy argument has *some* force even though the mechanism of the analogy is unknown, i.e., even though we do not know *why* the analogy holds, as long as we are prepared to claim *that* it holds. At any rate, the conclusion is inescapable that interpretations of the rhetorical situation must be grounded on interpretations of the logical situation; and since it emerged earlier that logical analysis was crucial for the understanding of scientific rationality per se, this means that a systematic logical analysis of a crucial work like Galileo's *Dialogue* is very important for the philosophy of science.

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[13] Galilei 1890–1909, 7: 167; 1967, 141.

[14] Galilei 1890–1909, 7: 167–169; 1967, 141–143.

[15] Galilei 1890–1909, 7: 169–175; 1967, 143–149.

[16] Galilei 1890–1909, 7: 151–152; 1967, 126.

[17] Galilei 1890–1909, 7: 169; 1967, 143.

20.4 Conclusion

This essay has been a critical examination of Paul Feyerabend's account of scientific rationality in his book *Against Method*. His approach to scientific rationality can be easily misunderstood as being merely negative, but suggests two very important positive insights. First, the anarchism of "anything goes" (which ought to be distinguished from the specific counter-inductive rule) may be understood as a manner of speaking about judgment, and when so interpreted it receives the support of the evidence of what may be called Galileo's synthetic or "dialectical" methodology. Second, the propagandist-manipulative interpretation of scientific rationality may be taken as being itself a rhetorical exaggeration of the truth that rhetorical persuasion has an important role. Underlying both elements stands the logical dimension of science (logical in the sense of the theory and practice of reasoning), as a deeper level to which judgment is reducible, and on which rhetoric needs to be grounded.

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Chapter 21
Wallace's *Galileo and His Sources*: Suppositional vs. Hypothetical Reasoning

Abstract This is a critical examination of William Wallace's *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science* (1984). I argue that Wallace's book is a noteworthy contribution to the history of philosophy, the history of science, Medieval and Renaissance studies, and Galilean scholarship. In particular, Wallace establishes a significant connection between Galileo and the Jesuit Collegio Romano, thus suggesting an approach to the critical interpretation of the Galileo affair that is more judicious than the usual accounts. Moreover, Wallace emphasizes what he calls suppositional reasoning in both Galileo's epistemological writings and his scientific work, thus strengthening the emerging appreciation of Galileo as a logician-in-action. However, questions remain about whether all science-vs-religion conflict can be really eliminated from the Galileo affair, and about whether Wallace's concept of suppositional reasoning is really powerful enough to do away with other crucial forms such as demonstrative, hypothetical, and probable reasoning.

21.1 Introduction

Philosophers have traditionally appreciated the relevance of Galileo in a number of ways. First, since his scientific contributions made him the "father of modern science," or at least one of its founders, philosophers like Husserl, Ortega y Gasset, and Burtt[1] have studied his work to determine its epistemological implications and metaphysical foundations; this kind of approach is pursued more or less systematically by professional philosophers of science, for whom Galileo's work has become a standard test case for their theses and analyses, as we find in books by Paul Feyerabend, Thomas Kuhn, and Dudley Shapere.[2] Second, since Galileo was subjected to an Inquisition trial and condemnation that came to be labeled "the greatest scandal in Christendom," the interpretation and appraisal of the Galileo affair has become a cause célèbre, attracting the attention of cultural generalists (such as Arthur

[1] Husserl 1970, 23–59; Ortega y Gasset 1956, 1958; Burtt 1954.

[2] Feyerabend 1975; Kuhn 1957, 1970; Shapere 1974.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
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Koestler[3]) and resulting in works of socio-political criticism with an anti-clerical tendency (e.g., Santillana's *Crime of Galileo*[4]) and of theological apologetics (Langford's *Galileo, Science, and the Church*[5]). A third type of philosophical appreciation has consisted in exploring and establishing conceptual and empirical ties between Galileo and the history of philosophy as such, which can be done and has been done in regard to his philosophical contemporaries such as Bacon, Hobbes, Descartes, Bruno, and Campanella, as well as successors like Hume, and predecessors like Plato; the last connection, in particular, had recently acquired some popularity, after having been pioneered by Koyré's celebrated "Galileo and Plato" in the pages of the *Journal of the History of Ideas*.[6] More recently, the present reviewer has elaborated a fourth type of philosophical appreciation of Galileo, from the point of view of what is now generally called "informal logic."[7] Here Galileo's work becomes a model for the analysis and evaluation of given arguments and for the construction of new ones, as well as a data base for the elaboration of a theory of reasoning oriented toward practice, applications, and everyday use.

21.2 On Galileo's Early Notebooks

William A. Wallace's latest book[8] is an original and important contribution to philosophy in all four of these areas. To be sure, like his previous works on this and related topics, the present book can and will be appreciated also from the point of view of the history of science, Medieval and Renaissance studies, and Galilean scholarship. For example, Wallace constructs a very sophisticated and convincing case in favor of a novel version of the continuity between medieval and modern science, the link being the Jesuit Collegio Romano founded by Ignatius Loyola in 1551; this is bound to lead to a re-examination of whether or not the so-called "Scientific Revolution" is a misnomer, and it may spark a new debate about the historiographical validity of the method of continuity, which Joseph Agassi has criticized as being an approach superseded by more powerful techniques.[9] Concerning Galileo's biography, two illustrations will suffice. Wallace gives compelling evidence that it was during his first academic appointment at the University of Pisa (1589–1592) that Galileo compiled his early Latin notebooks, the "Logical Questions" dealing with epistemological topics such as the presuppositions of scientific inquiry and the nature of scientific proof, and the "Physical Questions" about the nature of heavenly bodies and of terrestrial elements and change; this means that these Galilean writings ought

[3] Koestler 1959.
[4] Santillana 1955.
[5] Langford 1966.
[6] Koyré 1943.
[7] Finocchiaro 1980.
[8] Wallace 1984.
[9] Agassi 1963.

not to be treated as lightly as they have been in the past. Moreover, because of the obviously philosophical character of these notebooks, they vindicate the essential accuracy of Galileo's justification for requesting the title of "Philosopher" in addition to that of "Mathematician" from the Grand Duke of Tuscany: "I may claim to have studied more years in philosophy than months in pure mathematics" (quoted by Wallace, p. 226); this famous remark had recently come to be seen (in the Koestler-Feyerabend view of Galileo) as a typical instance of his extravagance and tendency to exaggeration. And this brings us back to the question of philosophical relevance.

Wallace's major accomplishment is perhaps that of embedding Galileo into the tradition of progressive Aristotelianism, a surprising picture, in view of the explicit criticism of Aristotelian philosophers and ideas in almost all of his published works. To bring this about Wallace focuses on Galileo's early notebooks and reconstructs both the philosophical background from which they originated and the considerable traces they left on Galileo's later development. The analysis reveals that the Aristotelian content and scholastic structure of the notebooks were mostly copied, adapted, or otherwise compiled from unpublished lecture notes or published works of Jesuit professors at the Collegio Romano, which can be specifically identified; they were not meant to be original works, but they do show some reflective assimilation, and so they were probably Galileo's own lecture notes in preparation for teaching courses in logic and natural philosophy. After establishing this connection, Wallace gives a detailed account of the doctrines of science and of motion that are found in those same sources, or in the published or manuscript writings of their fellow professors, or students, or followers; the motivation for this undertaking is that Galileo's notebook on logic is incomplete and lacks an explicit discussion of the nature of science per se, his notebook on natural philosophy lacks an account of local motion, and Wallace takes Galileo's main achievement to be the formulation of a science of motion. These parts of the book (two-thirds), containing the textual analysis of Galileo's notebooks and the background logic and physics taught at the Collegio Romano, are not only flawless and masterful, but contain such a wealth of not easily accessible information that they will be valuable even to scholars who might want to put it to a different use.

21.3 On Galileo's Mature Work

In the last part Wallace discusses Galileo's career from his Pisan days to his death (1642), stressing both the continuity of his development and the constant presence of the epistemological and physical concerns, problems, orientations, and ideals of his early notebooks and of the Collegio Romano background. Besides being sensitive to the primary Galilean texts, this discussion is also an enlightening synthesis of recent scholarship. In the nature of the case, the account in this part is more questionable, partly because the relevance of the material included is not always apparent, partly because the Aristotelian connection is claimed even when Galileo speaks as if he were

ignorant of the Aristotelian precedents of an idea (e.g., pp. 239, 243, 317),[10] and partly because Wallace's interpretations are sometimes disputable. However, the net effect is that he succeeds in making a plausible case for both continuity of development and persistence of Aristotelianism. An example of each will be instructive.

First, the biggest break in Galileo's career came in 1609–1610 as a result of his telescopic discoveries. If this does not count as discontinuity, what would? Where is the continuity here? For Wallace the continuity lies in the fact that up to 1609, though Galileo had not published anything on the topic, he had done enough research, both theoretical and experimental, "that he had within his grasp a 'new science' of motion. Now, with the availability of unexpected evidence against which the *ippotesi* and *supposizioni* of astronomy could be checked, he was prepared to move into this field also and show how this could be put on the same basis as the science of mechanics. Instead of restricting itself to 'saving the appearances,' as he had characterized it in the *Trattato della Sfera*, the science of the heavens could henceforth be able to employ 'necessary demonstrations' and so establish with apodictic certitude the true system of the world" (p. 282; cf. 290). In other words, Galileo underwent no epistemological change, but only extended some epistemological possibilities into another field. Second, Galileo's readiness to apply mathematics to physical reality is one of his most persistent methodological attitudes. How can Wallace interpret this as evidence of Aristotelianism? The answer is that, though Aristotelians in Northern Italian universities like Pisa and Padua were indeed opposed to mathematics, there were others who interpreted Aristotle differently and, while regarding themselves as his followers, favored mathematics. They were people like Clavius, Blancanus, and Guevara, and it is they who inspired Galileo. We may conclude here by saying that Wallace has made a substantial case for placing Galileo in a tradition of progressive Aristotelianism, and that despite its complexity the argument is plausible enough so that it is fruitful to explore some further questions.

One is whether Galileo's being a progressive Aristotelian is incompatible with his belonging also to other traditions. For example, is this Aristotelian interpretation meant to deny that Galileo was also a type of Platonist? Wallace himself mentions this problem (pp. 347–348), and I agree with him that this remains a relatively open question. Another query would concern the meaning of "Aristotelianism." Given that we regard as Aristotelians both progressives like Clavius and Galileo, and conservatives such as Cesare Cremonini and Antonio Rocco, does the concept become too general and all-inclusive? Here I do not see any other promising direction besides a historicist approach, according to which Aristotelianism would not be an abstract entity definable in terms of an unchangeable set of theses or methods, but rather an evolving tradition having merely historical-empirical ties among its distinct developmental stages. (This might be a way of enhancing the importance of Wallace's discovery of the Collegio Romano sources of Galileo's early notebooks.) Third, even if ultimately

[10] Of course, it could be that on these occasions Galileo was not ignorant, but that he pretended to be ignorant for rhetorical reasons; or it could be that his knowledge of the Aristotelian tradition was limited to the one type of source established by Wallace, and so he was not adequately informed about the tradition.

one has to rely on historicist criteria, one may ask to know more about the epistemological and methodological content of Galileo's progressive Aristotelianism. This brings us to another one of the dimensions of philosophical relevance mentioned earlier.

In his analysis of Galileo's notebook on the "Logical Questions," Wallace shows that, despite its incompleteness, it provides what may be called a theory of hypothetical reasoning, or suppositional reasoning, as he likes to term it. And in his analysis of Galileo's scientific work in the following half a century, he interprets it as suppositional reasoning in action, primarily about the topic of (local) motion. So by the end of the book he is able to conclude, with some justice, that "it is in the domain of suppositional reasoning, therefore, that a good part of Galileo's contribution was made" (p. 342). Now, in Wallace's account, suppositional reasoning is a somewhat technical term of Aristotelianism, and so this epistemological thesis is supported, and in turn supports, the previous historical claim. However, in the present epistemological-theoretical context we need not elaborate such mutual supporting. Moreover, the details of Wallace's notion of suppositional reasoning are too complex to be reported here. Suffice it to say that in such suppositional reasoning both mathematization and experimentation play a crucial role, and that another crucial element of the process is what, in my own words, I would call the independent testing of the suppositions on which the conclusions of such reasoning are based. It should also be noted that in this book Wallace refrains from describing such Galilean suppositional reasoning by means of the label "reasoning *ex suppositione*," which is the one he had stressed in earlier works[11]; the reason is presumably that the latter expression is more problematic, and in fact he points out how it can mislead (pp. 342–343). In short, what Wallace has done is to give us a picture of Galileo as a theorist and practitioner of suppositional reasoning, and this strikes me as an interpretation of great originality and fruitfulness.

To be sure, there are secondary points which I find more or less questionable. For example, I remain unconvinced about the alleged apodicticity of such "suppositional" reasoning, both in Galileo's mind and as a matter of philosophical theory; just to mention one difficulty, since, as Wallace himself is at great pains to show, approximation plays a crucial role in suppositional reasoning, and since approximation is a pragmatic and contingent affair, I do not see how we can ever get necessity out of the whole process. Moreover, I am unclear about the relationship between suppositional reasoning and hypothetico-deductive methodology, which Wallace wants to keep sharply separate; but the difficulty here may be partly due to differences about conceptions of the latter. This second sort of difficulty leads to a third one, which is that someone may get the impression that epistemologically speaking Wallace's interpretation is primarily a redescription of Galileo in "progressive Aristotelian" terminology; for example, it might be possible to view it as a new version of the common hypothetico-deductivist interpretation of Galileo. I mention this difficulty partly because I would like to suggest a way of solving it. Of course, one can answer

[11] Wallace 1981, 129–159.

that, given Wallace's historical documentation and empirical evidence, his interpretation is definitely *not* a *mere* semantic innovation. But to say this would bring us back to the history-of-philosophy appreciation of his book, which I have already discussed. If we want to attribute it an additional value on the epistemological plane, that could be done by emphasizing the "reasoning" part of "suppositional reasoning." It seems to me that this is the fruitful direction to move toward. This is one reason why I stated earlier that Wallace's book was also a contribution to a third kind of philosophical study of Galileo. Obviously, the emphasis on suppositional reasoning has great affinity to the above-mentioned "informal logic" interpretation of Galileo as first and foremost a theorist and practitioner of reasoning.

But there is another reason that points in the same direction. Since the "informal logic" interpretation restricted itself to an analysis of the *Dialogue on the Two Chief World Systems* (1632), Wallace's work may be taken as providing a historical-biographical foundation for it in the notebook on the "Logical Questions," and also as extending the same type of analysis to all phases of Galileo's career.

21.4 On the Galileo Affair

Last, but not least, we come to the Galileo affair. How does Wallace's book contribute to its proper philosophical understanding? On this point, I should begin with some preliminary clarifications. First, unlike the history-of-philosophy thesis about Galileo's Aristotelianism (which is central), and unlike the epistemological interpretation in terms of suppositional reasoning (which, while less central, is very explicit), and unlike the implication for the "informal logic" account (which, while only implicit, is very direct), the relevance to the Galileo affair is only indirect. Second, though Wallace is explicit about the possibility of such implications (p. xii), nowhere does he discuss them in this book. Third, the potential implications I am talking about are *not* those that Wallace himself has drawn in some recent articles,[12] namely that it was possible for Galileo to abjure in the trial of 1633 without perjury, in view of the apodictic ideal of science which Wallace's account attributes to him, and in view of the merely probable force of the pro-Copernican arguments which my analysis of the *Dialogue* (1632) reveals.

Rather, the more promising implications for the Galileo affair seem to me to lie elsewhere. At a very general and programmatic level, the connection established by Wallace between Galileo and the Collegio Romano adds further counter-evidence to the traditional interpretation according to which the affair is an illustration of the warfare between science and religion. In fact, this connection represents a strengthening of the ties between Galileo and religious institutions, and this renders less plausible the idea of a conflict. I am not sure that out of the affair one can get an instance of the *harmony* between reason and faith, as recent Vatican pronouncements advocate, but that is another matter.

[12] Wallace 1983a, b.

Another feature of Wallace's account promises an even more fruitful reappraisal. We have seen that, in portraying Galileo as a progressive Aristotelian, Wallace does not deny the existence of a conservative wing of the Aristotelian tradition. Now, Wallace's emphasis on the Jesuit Collegio Romano is a constant reminder that *some* Catholic institutions were progressive-minded; it thus refutes the monolithic view that the Catholic Church as a whole was a regressive institution. On the other hand, as Pope John Paul II admitted in his 1979 speech at the Pontifical Academy of Sciences in commemoration of Einstein's centennial, it is obvious that Galileo "had to suffer a great deal—we cannot conceal the fact—at the hands of men and organisms of the Church."[13] Combining the two facts, we reach the conclusion that the Galileo affair may be the result, in part, of a struggle between a conservative and a progressive wing of the Catholic Church. Now, this in turn suggests that the truly fruitful struggle is not that between science and religion, but that between conservation and innovation, and that the latter cuts across many different traditions, not only Catholicism and Aristotelianism, but also science and philosophy themselves. The really challenging task then becomes the identification of the specific ways in which that never-ending and all-pervasive struggle occurred in the Galileo affair.

A third and more specific way of throwing light on the episode is as follows. As mentioned above, Wallace's view is that the telescopic discoveries of 1609–1610 led, not to a break in the continuity of his intellectual development, but rather to the realization that a science of the heavens was now possible, a science logically and epistemologically similar to the science of local motion which he had been slowly and carefully working out for the previous twenty years. The point I wish to emphasize is the *possibility* as distinct from the *actuality* of such a "scientific" astronomy. This emphasis, though not expressed by Wallace himself, is exactly in accordance both with the theses he formulates and with the evidence he gives. For example, even four years after the telescope, when he published the *Letters on Sunspots*, the situation is described by Wallace as such that "by this time Galileo was convinced that astronomy need no longer be a *scientia secundum quid* but could now, through the use of telescopic observations, achieve the status of a strict demonstrative science" (p. 290; cf. pp. 242 and 282). That is, astronomy had not yet achieved "scientific" status, "but could now achieve the status." The significance of this for the affair is that it implies that the cause to which Galileo became committed, and which led to trouble with the Inquisition, must have been the possibility rather than the actuality of a "scientific" astronomy, the demonstrability rather than the truth of (the main assumption) of the Copernican system, or better, the belief that the earth's motion was capable of being proved rather than the belief that it was already actually proved.

Now, this thesis, so understood, allows us to make methodological sense of the main texts that make up the documentary history of the affair from 1613 to 1633. The *Dialogue* of 1632 becomes an explicit, detailed, and logically successful attempt to show that the geokinetic hypothesis is *demonstrable*, a result independently established as an immediate consequence of my analysis of that fateful work. The "Letter to the Grand Duchess" becomes a correct argument designed to establish

[13] John Paul II 1979, section 6.

the methodological conclusion that physical theses which are *demonstrable* should not be condemned as heretical even if they contradict the Bible; this interpretation too has been independently supported by the textual analysis which I have already carried out. (Incidentally, this interpretation also solves what Wallace seems to regard as an open question when he asserts, "from the viewpoint of scientific methodology it is difficult to know what to make of this *Letter to Christina*" (p. 293). His puzzlement is understandable, given the usual accounts of the "Letter," but it should disappear in the light of my interpretation.)

Once the main (methodological-philosophical) issue in the affair is taken to be whether or not the earth's motion is *capable* of being proved true, this enables us to make *more* sense (if not *complete* sense) of his opponents. In fact, criteria of provability are admittedly more general and debatable matters. Here further research is needed, but some guesses are suggested by the preceding. Perhaps Bellarmine's objection (in his letter to Foscarini) was that he did not think that the Copernican hypothesis could *ever* be proved true, because of Biblical statements to the contrary; and perhaps he is saying that the only way of showing that a thesis is capable of proof is to produce the proof.[14] And perhaps Urban VIII's theological objection is that Copernicanism could never be proved true, because it is conceivable that God might have created a non-Copernican universe. The careful textual analysis of these documents is likely to reveal the tension between conservation and innovation as regards criteria of provability.

21.5 Conclusion

In summary, then, besides being a noteworthy contribution to the history of science, Medieval and Renaissance studies, and Galilean scholarship, this book can be recommended to philosophers for four interrelated reasons. Because it establishes a strong and significant connection between Galileo and the Jesuit Collegio Romano, it suggests a novel approach to the interpretation and evaluation of the Galileo affair which is more judicious than the usual accounts. Because of its emphasis on what it calls suppositional reasoning as a key concept in both Galileo's epistemological writings and his scientific work, it adds new evidence and original nuances to the emerging appreciation of Galileo as a logician in action. Because of the novelty of the epistemological and methodological import it attributes to Galileo's science, it suggests instructive and fruitful reflections about the nature of scientific knowledge, and the role of and interrelationships among demonstration, mathematization, experimentation, approximation, causal investigation, and hypothetical reasoning. And because of the historical ties it establishes between Galileo and the progressive Aristotelianism of his time, it will be of interest to historians of philosophy and other students of the *philosophia perennis.*

[14] Such questions could not be adequately resolved without taking into account recent work on Bellarmine, such as Baldini and Coyne 1984; and Baldini 1984.

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Chapter 22
Drake on Galileo: Science vs. Philosophy, Methodology vs. Metaphysics

Abstract This is a critical examination of two books by Stillman Drake, *Galileo Against the Philosophers* (1976) and *Galileo at Work* (1978). Drake argues that Galileo's science was deeply experimental and importantly anti-philosophical. I appreciate Drake's argument for the experimental nature of Galileo's science; but I criticize his anti-philosophical interpretation by pointing out that, although Galileo avoided metaphysical discussions, his scientific work is full of explicit argumentation and methodological analyses.

22.1 Introduction

Stillman Drake (1910–1993) was the most accomplished Galilean scholar of the twentieth century. Thus, it ought to come as no surprise that I studied and learned from many of his works, and that there were several occasions for me to write and publish critical examinations of some of them. For example, at the very beginning of this book, the two essays in Chapters 1 and 2 provide an interpretation, analysis, and evaluation of Galileo's refutation of the idea that the speed of falling bodies is proportional to the distance traversed; and it is obvious that those essays are in large part a critical examination of Drake's account of the same Galilean argument. On other occasions, my critical examination of Drake's works is even more explicit, since my own essays are critical reviews of Drake's books.[1]

Two such examples will be provided in this chapter. The first essay (in Sect. 22.2 below) provides a brief critical examination of Drake's most important and comprehensive book, *Galileo at Work: His Scientific Biography* (1978). The second essay (in Sect. 22.3 below) undertakes a critical examination of Drake's book *Galileo Against the Philosophers* (1976). What connects these two books, and what makes it valuable to discuss them here, is the fact that they illustrate what is probably Drake's main theme in all of his works: the experimental nature of Galileo's science and its allegedly anti-philosophical character. My discussion will attempt to clarify that although

[1] Finocchiaro 1977, 1979, 1980, 1982, 1991, 2002.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
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Galileo's science is anti-philosophical in the sense of being non-metaphysical, it is highly and explicitly philosophical in the sense of being full of explicit argumentation and methodological analyses.

22.2 Drake's *Galileo at Work*

Scientists have found Galileo Galilei most congenial, in the sense that they have been inclined to find their own "roots" in his work. The practice goes back at least to Isaac Newton, who in his *Mathematical Principles of Natural Philosophy* credited Galileo with knowledge or discovery of several concepts used in his own system of the world, specifically the law of inertia, the law of force, the principle of superposition of motion, the times-squared law of fall, and the parabolic path of projectiles. The list also includes Albert Einstein, who wrote a foreword to Stillman Drake's English translation of Galileo's *Dialogue Concerning the Two Chief World Systems* (1953) in which he summarized the main point of the book as demonstrating the uselessness of an abstract center of the universe for explaining the fall of heavy bodies, thus drawing an analogy between Galileo's approach and his own general-relativistic rejection of an inertial system to explain the inertial behavior of matter.

To this traditional scientific appreciation of Galileo contrast modern interpretations put forth by historians and philosophers of science. Early in the twentieth century Pierre Duhem, in *Studies on Leonardo da Vinci* (1913), argued that, historically, Galileo's important ideas derived from the medieval scholastics of Paris and that, philosophically, he was reactionary for upholding a realistic interpretation of scientific knowledge against what Duhem regarded as the more progressive positivist instrumentalist view allegedly held by the Catholic Church. Then in 1924 there was Edwin A. Burtt's *Metaphysical Foundations of Modern Physical Science*, arguing that there was no place for human values in the world of facts and figures created by Galileo's science. And in 1939 Alexandre Koyré's *Galilean Studies* argued that on the one hand Galileo's work shows that good physics is made a priori, and on the other hand his physics was not that good; for example, his conception of inertia was that of natural circular motion, not rectilinear motion (the latter conception being allegedly a contribution made by Descartes).

The trend toward such historical-philosophical interpretation has continued to the present, and in the last quarter-century it has been criticized by Stillman Drake, whose efforts began and have continued with the much-needed translation into English of all of Galileo's major works, and whose interpretations are incomparably superior to those of his predecessors in scientific understanding and biographical information. The present work[2] is Drake's most ambitious undertaking and most substantial accomplishment to date.

In a sense, the book is a well-documented vindication of the traditional scientific appreciation of Galileo. Moreover, the documentation is so extensive (including

[2] Drake 1978.

almost everything Galileo said or did scientifically) and includes so much new material (Galileo's unpublished and previously mostly unexamined manuscript notes on motion) that it may be some time before historians and philosophers can assimilate the information found in the book. In calling it a vindication I do not want to give the impression that the book is an explicit polemic. On the contrary, Drake's concern is with the systematic chronological arrangement and explanation of all documentary evidence relating to Galileo's scientific career; in this project Drake's approach could be described as a painstaking and unswerving adherence to two principles: to describe the facts as they really happened and to understand or explain, not judge or evaluate, what Galileo did. This approach is reflected not only in the content of Drake's accounts but in their arrangement: each of the book's 22 chapters bears for its title merely the years of Galileo's life under consideration and a list of scientific topics he studied in that period. For example, chapter 7 is headed "1606-8: The nova again; beam strength; hydrostatics; speed paradox resolved; inertial experiment and trajectory."

Perhaps the most important result of Drake's examination is his proof that virtually all of Galileo's work in kinematics, published in 1638 in Days III and IV of *Two New Sciences*,[3] had been completed by 1609, when his telescopic discoveries got him involved in astronomy, and that most of that part of the book had been written by the time the *Dialogue* of 1632 was published. The significance of this is that the latter book is thus shown to be Galileo's synthesis of physics and astronomy, and a defense of Copernicanism grounded on the second of his "two new sciences," rather than an incompletely thought-out espousal of Copernicanism. With respect to methodology, Drake documents at least two important qualities that augment the traditional view of Galileo as a keen observer, ingenious experimenter, and mathematical interpreter of nature: one is interest in and success with predictions, and the other is an engineer mentality. Galileo's predictive prowess showed itself primarily in connection with the positions of Jupiter's satellites and the appearance and disappearance of Saturn's rings, predictions concerning which were sent by letter to acquaintances and were usually confirmed. His engineering frame of mind becomes evident form Drake's discussion of Galileo's construction of such instruments as the calculating "sector," the microscope, and the thermoscope (besides, of course, the telescope) and of the reports he frequently was asked to make to his employer (the Grand Duke of Tuscany) concerning various projects and problems of civil engineering.

The most controversial issue the book is likely to raise concerns what I shall call the philosophical question. Drake explicitly asserts in the preface that he wants to avoid discussion of the "philosophical implications" of Galileo's scientific work. This he does partly for what I cannot refrain from calling philosophical reasons. He justifies his approach by reference to Galileo's view (with which he expresses agreement) that science and philosophy are distinct, and that hence one may engage in the former without engaging in the latter. The case, of course, depends on what is meant by these terms.

[3] Galilei 1974.

By "science" Drake usually means the study of questions that can be decided by "sense experiences and necessary demonstrations," and by "philosophy" the study of those questions that cannot be so decided. This view provides Drake's rationale for avoiding the philosophical question in favor of staying as close as possible to the texts, documents, and evidence of Galileo's scientific work and developing the theme of "Galileo at work" to which the book's title calls attention. Other times in Drake's usage "science" refers to the investigation of physical problems and "philosophy" to speculation on metaphysical topics. Here I think Drake is completely right when he emphasizes that Galileo did not engage in metaphysics.

A qualification is in order concerning the type of "philosophy" that is most likely to come to mind in a scientific context, namely philosophy of science, meaning considerations about the nature, aims, and methods of scientific knowledge. Drake's book documents that Galileo frequently engaged in such considerations, mostly in the process of explaining and justifying his scientific ideas in the face of opposition. Thus we have Galileo portrayed as both a scientist and a philosopher in this sense.

In summary, this is a timely book for scientists interested in their roots, an epoch-making book for the quality and thoroughness of the documentation, and a provocative one concerning the "philosophical implications" which Drake refrains from discussing but which others inevitably will.

22.3 Drake's *Galileo Against the Philosophers*

Though more recondite in subject matter, this book[4] is patterned after Drake's earlier, classic *Discoveries and Opinions of Galileo*[5]; that is, it contains translations of original sources interspersed with biographical and interpretative commentaries, the proportion here being about half and half. The original sources are two pseudonymous books[6] about the new star of 1604: the *Dialogo de la stella nuova*, by Cecco di Ronchitti, written in Paduan dialect and first published in Padua in 1605; and the *Considerazioni sopra alcuni luoghi del discorso di Lodovico delle Colombe intorno alla stella apparita 1604*, by Alimberto Mauri, written in Italian and first published in Florence in 1606. Galileo's authorship of the Cecco *Dialogue* was established by Favaro, and a text in Paduan dialect together with an Italian translation can be found in Galileo's *Opere*.[7]

As for Mauri's *Considerations*, the question of authorship is more problematic, so that it was not included by Favaro in the Edizione Nazionale; nevertheless, Drake is convinced that it is Galileo's, and a very valuable aspect of the present book is Drake's discussion of the available evidence, statement of his reasons, and criticism of the objections. Of the latter I am most impressed by the fact that in Mauri's

[4] Drake 1976.

[5] Drake 1957.

[6] Cecco 1605; Mauri 1606.

[7] Galilei 1890–1909, 2: 311–334.

Considerations one finds a favorable attitude toward astrology (which Colombe had criticized along with anti-Peripatetic interpretations of the new star), so that on the whole I was not convinced either by Drake's arguments or by my reading of the *Considerations* that Galileo was the author. (Nevertheless, I shall, for the sake of the argument in the rest of this review, assume that Galileo was the author.)

On the basis of the content of these two books, and of various facts relating to them, Drake concludes that the animating force for Galileo's work from beginning to end was not Copernican zeal but rather an anti-philosophical attitude. Drake stresses the fact that the Cecco *Dialogue* is essentially an argument supporting the idea that parallax theory is applicable to the 1604 nova regardless of its nature or substance; he shows that though this argument was ostensibly directed against Lorenzini's *Discourse* on the new star, it really was aimed at Cremonini, the senior professor of philosophy at Padua, who held a view characterized by Drake as "intelligent, defensible, and highly orthodox Aristotelianism."[8] Hence Drake concludes that the Cecco *Dialogue* is part of an historically actual and methodologically serious debate about the role of mathematics in physical inquiry. The pages[9] where this line of reasoning by Drake comes to a climax and where he makes the methodological distinctions necessary to appreciate the situation strike me as paradigms of analysis of a doubly philosophical sort; that is, Drake's account shows more subtlety and sensitivity than is common in many contemporary philosophical discussions, and it portrays Galileo as a methodologist-in-action. Thus Drake's own account here supports his central conclusion about Galileo's anti-philosophical attitude only if we take seriously the distinction (drawn by Drake himself later[10]) between philosophy and philosophers, and if we note that Galileo's attitude is one against philosophers (i.e., the philosophers of his acquaintance) and not against philosophy, concerning which he appears instead as a skillful practitioner of methodological analysis.

Next, regarding Galileo's attitude toward Copernicanism, Drake points out that though the Cecco *Dialogue* is not particularly concerned with it, there are two favorable references to it; however, he also points out that these were changed into unfavorable references in a second edition of the book printed in Verona a few months after the first. Since Drake argues plausibly that Galileo was responsible for these changes, it follows that by the summer of 1605 Galileo was not so favorably disposed toward Copernicanism. Moreover, Drake argues with incomparable knowledge of biographical details and of the scientific issues in question, that Galileo's motivation for these changes was the nova's failure to confirm the earth's annual motion by failing to show any annual parallax, which he had expected on the basis of his assumption that the nova was moving uniformly away from the earth (because of its apparent diminution). It follows, and here I find Drake's argument compelling, that we have thereby evidence that in astronomy Galileo's attitude was as careful, tentative, and open-minded as his attitude in mechanics can be demonstrated to be (by Drake's recent discoveries).

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[8] Drake 1976, 13.

[9] Drake 1976, 12–14.

[10] Drake 1976, 138.

Drake's argument about Mauri's *Considerations* is analogous. He stresses the fact that it contains an attack on Colombe's theory, according to which the new star was located in a previously unsuspected stellar sphere beyond the sphere of fixed stars and beyond another previously unsuspected crystalline sphere devoid of stars but containing differences in density and whose postulated rotation could create an eye-glass type of effect rendering visible the normally invisible stars further out. In taking criticism of such a theory as evidence of an anti-philosophical attitude, Drake is equating philosophy with speculation which is both ad hoc and unfalsifiable.[11] This type of thinking is indeed objectionable, but it is no more characteristic of so-called philosophy than of historians, scientists, and laymen; rather it is a type of maneuver characteristic of certain degenerating stages of intellectual development both at the individual and at the socio-cultural level, indicative of a conservative attitude and defensive with respect to alleged new evidence. Such theorizing is an important type of thought process because of its prevalence and because it is so difficult to avoid. It follows merely that Galileo was against ad hoc, unfalsifiable speculation.

Regarding Copernicanism, my reading of the Mauri book inclines me to agree with Drake that we get a new picture of Galileo's pre-telescopic astronomical views:

> There is no indication of concern, let alone of preoccupation, with the rival systems of Ptolemy, Copernicus, and Tycho Brahe. Mauri was confident that astronomy could be improved only by observations, and that observations already in hand could improve it. He showed also a very acute understanding of the history of astronomy as related to the observational data that had been available in earlier epochs and their role in successive theoretical interpretations of necessarily increasing complexity.[12]

In short, the important thesis which this book supports with thoughtful arguments and novel evidence amounts to saying that Galileo was at no time a pro-Copernican zealot but rather was at all times an anti-philosophical one. I believe Drake has adequately supported the negative side of this claim in the significant sense that, for example, the *Dialogue* of 1632 becomes the end result of a "patient study of observational data, first in physics and then in astronomy,"[13] and hence becomes Galileo's mature "synthesis of a new physics and the new astronomy."[14] On the other hand, the positive side of Drake's thesis is adequately supported only in the sense that Galileo was constantly opposed to, and opposed by, (1) "philosophers" like Lorenzini, Cremonini, and Colombe, and (2) "philosophizing" consisting of ad hoc explanations and unfalsifiable speculation; so that in spite of its anti-philosophical tone, Drake's book will offend only the sensibilities of those philosophers who are unable to see that beneath the surface of its anti-philosophical polemic lies an exemplary model of methodological analysis and an incomparably well-grounded portrayal of Galileo as a methodologist-in-action. We thus have a valuable book whose diffusion will be hampered only by its price and its limited edition of 500 copies.

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[11] Drake 1976, 58–59, 69, 70.

[12] Drake 1976, 70.

[13] Drake 1976, p. xi.

[14] Drake 1976, pp. xi, 144.

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Chapter 23
Camerota's *Galileo Galilei e la cultura scientifica*: Between Ptolemy and Copernicus?

Abstract This essay is a critical examination of Michele Camerota's *Galileo Galilei e la cultura scientifica nell'età della Controriforma* (2004). I argue that Camerota's biography of Galileo is welcome for its timeliness, useful for its breadth, valuable for its depth, usually well-documented, and often insightful. For example, and crucially important, Camerota seems to elaborate correctly the issue of the logical strength of Galileo's case for the earth's motion: that he lacked a conclusive proof, but did show that the pro-Copernican arguments were much stronger than the Tychonic as well as Ptolemaic alternatives. I also argue that other parts of Camerota's account are questionable. For example, he seems to exaggerate Galileo's readiness to accept Copernicanism; his commitment to the mathematization of natural phenomena; and the extent to which the Inquisition's condemnation was for heresy rather than disobedience.

23.1 Introduction

In the last half a century, there have been several major biographies or comprehensive accounts of Galileo's works. In 1957, Ludovico Geymonat published one in Italy, which was later translated into English with the subtitle "a biography and inquiry into his philosophy of science"[1]; Geymonat's account was useful, but much has happened since then in Galilean scholarship, as well as in the world at large, and such developments usually act as catalysts and motivations for new accounts. In 1964, there appeared in Italy a work by Pio Paschini that provided a comprehensive and detailed account of Galileo's life and works; although the scope of this work remains unsurpassed, and although it remains of some use even today, when the book was first published it was already about 20 years out of phase since it had been researched and written in 1941–1944, at which time it was not published due to circumstances that are both complex and controversial, so much so that they have given rise to one of the

[1] Geymonat 1957, 1965.

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latest episodes of the Galileo affair.[2] In 1966, Alexandre Koyré's *Études galiléennes*, which had been first published in 1939, was reprinted; this work was immensely influential and inspiring, and nobody can question its brilliance, insight, historical sensitivity, and analytical acuity.[3] Nevertheless, its focus was Galileo's thought and methodology, and it paid little attention to biographical developments or the Galileo affair; moreover, by the time Koyré's work was reprinted, it was being superseded with more recent developments[4]; at any rate, it is important, indeed essential, that the work of influential masters be assimilated in such a way that one is able to move beyond the letter of their teaching.[5] In 1968, Maurice Clavelin published in France a comprehensive account of the background, development, and significance of Galileo's scientific and epistemological contributions; although the breadth and depth of Clavelin's book remain in some ways unsurpassed, it was not and did not claim to be a biography.[6] In 1978, Stillman Drake published a "scientific biography" that represented a synthesis of his own epoch-making research and of other recent Galileo scholarship; it established beyond any reasonable doubt that Galileo was a skillful and indefatigable practitioner of the art of experiment. However, although one can appreciate Drake's book as such a milestone, one need not deny its weaknesses: its portrayal of Galileo as a-philosophical or anti-philosophical was both naïve and untenable, and its failure to include a significant account of the details of the trial suggests that Drake's monograph falls short of what one would expect even from a "scientific" biography.[7] Since 1978 there have been several important works on selected aspects of the topic, such as William Wallace's account of the historical and conceptual connections between Galileo and the progressive wing of Aristotelianism; Mario Biagioli's account of the development of Galileo's career from the point of view of the historically specific institution of patronage and social psychology of courtly etiquette; and Annibale Fantoli's account of the Galileo affair.[8] However, since then there have been no attempts[9] to give an account of Galileo's life and works aiming to include not only his scientific work in kinematics and astronomy, but also his practical and reflective contributions in methodology, and the vicissitudes of his trial and condemnation.

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[2] Paschini 1964, 1965; cf. Finocchiaro 2005, 318–337.

[3] Koyré 1939, 1966, 1976, 1978.

[4] Such as Settle 1961.

[5] Cf. Finocchiaro 1973b, 86–116; 1977; 1980, 202–223. [See also Chapter 19 of this book.]

[6] Clavelin 1968, 1974, 1996. Cf. Finocchiaro 1980, 246–253.

[7] Drake 1978; cf. Finocchiaro 1980, 237–245; 2002. [See also Chapter 22 of this book.]

[8] Wallace 1981, 1984, 1992a, b; Biagioli 1993; Fantoli 1993, 1994, 1996, 1997.

[9] I discount such successful popularizations as Sobel 1999 and such amateurish efforts as Reston 1994; that is, I am not taking them into account in the present discussions of scholarly contributions, although it would be a serious mistake to ignore them in a discussion of the popular-cultural significance of Galileo and the impact of historical scholarship; cf. Finocchiaro 2005, especially pp. 359–365.

Such a task has now been attempted by Michele Camerota,[10] and consequently his book may be welcome as a work for which the time was ripe. Besides such breadth, the book usually discusses such topics in enough detail that we may add depth as another merit. Moreover, since the book is, generally speaking, well documented on primary and secondary sources, it deserves the attention of every serious scholar of the subject. Of course, it is a different story to determine how successful Camerota's attempt is, how insightful the in-depth discussions are, and how cogent the documentation is; ultimately, readers will have to make this determination themselves. Some of the impressions of this reader will be elaborated presently. I shall begin with some favorable impressions.

23.2 Appreciation

It is well known that as early as 1604 Galileo was convinced of the truth of the law of squares, according to which the distance from rest traversed by a freely falling body increases in proportion to the square of the time elapsed; and it is well known that at that time he was trying to explain this fact about falling bodies by trying to derive the law of squares from the principle of space proportionality, according to which the velocity acquired by a falling body is directly proportional to the distance traversed.[11] This derivation is fallacious and this principle is false, and by the time he wrote the *Two New Sciences* Galileo had rejected them; instead he gave an argument to refute the principle, and he derived the law of squares from the principle of time proportionality, which he called the definition of uniform acceleration, according to which the velocity acquired by a freely falling body is directly proportional to the time elapsed.[12]

Thus, sometime between 1604 and 1638 Galileo rejected space proportionality in favor of time proportionality. When exactly did this happen? Did it happen in 1629–1631 as Drake suggested, or in 1609 as Koyré implied?[13] Camerota argues cogently that it must have happened before April 9, 1611 because this is the date of a letter to Galileo by his disciple Daniello Antonini in which the writer refers to an argument attributed to his teacher to the effect that a body whose velocity increases in proportion to its distance from rest would move instantaneously, and this is the key point of Galileo's refutation of space proportionality in *Two New Sciences*; although Camerota himself credits Mario Helbing and Ottavio Besomi for having recently made the point, Camerota deserves credit for having stressed and contextualized it.[14]

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[10] Camerota 2004. References to this work will be given in parenthesis in the text, unless the references are too many, or mixed with others, or accompanied by comments.

[11] Galilei 1890–1909, 8: 373–374, 10: 115; cf. Finocchiaro 1973b, 88–96.

[12] Galilei 1890–1909, 8: 197–210, especially pp. 203–204; 1974, 153–167, esp. pp. 159–160. Cf. Finocchiaro 1972; 1973a; 1975. [See also Chapters 1, 2, and 3 of this book.]

[13] Drake 1978, 314–319; Koyré 1966, 138 n. 2; 1978, 124 n. 137. Cf. Camerota 2004, 146.

[14] Antonini to Galileo, in Galilei 1890–1909, 11: 84–86, at p. 85; Camerota 2004, 146–147, 595 n. 268; cf. Besomi and Helbing 1998, 2: 546–547. It should be noted that here Camerota ends up

Next, it is also well known that a few months after the publication of Galileo's *Sidereal Messenger,* Kepler published a *Conversation with the Sidereal Messenger* in which he essentially endorsed Galileo's telescopic discoveries. Galileo's booklet had been published in March 1610, and Kepler's endorsement came in May, before having access to a telescope and observing the new phenomena first-hand. Eventually, in August, Kepler was able to use an instrument sent by Galileo to the Elector of Cologne and to make the confirming observations; and in the fall, Kepler published a report more explicitly favorable to Galileo.[15]

An important aspect of this part of Camerota's account is the following. He stresses that although Kepler's *Conversation* is in part a defense of Galileo's discoveries, it is also a defense of his own ideas and the ideas of other precursors. Thus for a while many people interpreted Kepler's *Conversation* as an anti-Galilean document exposing Galileo's pretensions and undercutting his originality. These people included Giovanni A. Magini (professor of mathematics at the University of Bologna), Martin Horky (who was about to publish the *Brevissima peregrinatio contra Nuncium Sidereum*), Michael Maestlin (former teacher of Kepler), the Neapolitan mathematician Giovanni Camillo Gloriosi, Martin Hasdale in Prague, Georg Fugger (the imperial ambassador to Venice), and even the Lincean Academician Francesco Stelluti. Again, as some of Camerota's own notes suggest, here he is adopting and utilizing the work of other scholars, in particular Massimo Bucciantini's work on Galileo and Kepler[16]; nevertheless Camerota is right to stress the relatively "mixed" character of Kepler's *Conversation* with Galileo and how it was exploited by more hostile critics.

One of the most insightful and best argued parts of Camerota's book is a chapter-length section entitled "Between Ptolemy and Copernicus" (333–354) in a longer chapter dealing with the period between the 1616 condemnation of Copernicanism and the 1623 election of pope Urban VIII. Camerota documents the relatively well known fact that that period witnessed a wide diffusion and acceptance (especially among Catholics) of Tycho Brahe's world system, according to which the earth stands still at the center of the universe, but the planets revolve around the sun while the sun revolves with diurnal and annual motion around the earth. After such an historical account, Camerota takes up the more evaluative and controversial questions whether the reason why Galileo was not impressed by the Tychonic system was Copernican bias and zealotry; whether his low opinion of the Tychonic system amounted to a failure to consider it at all; and whether his neglect or dismissal of that system invalidated or weakened his case in favor of Copernicanism. There is a common view

agreeing with Koyré and disagreeing with Drake; that this is not an isolated case; that indeed in both text and notes Camerota is constantly criticizing Drake's claims and favorably elaborating Koyré's views; so much so that one could describe the book as an attempt to debunk Drake and to update and revive Koyré by writing the kind of biography that Koyré might have written if he had ever taken up such a project. As someone who has shown both appreciation and criticism of both Drake and Koyré, I would not be bothered by such a situation if that were the position to which the documents, evidence, and arguments lead us; but that is a crucial and questionable "if."

[15] Camerota 2004, 160, 174–75, 184–185.

[16] Camerota 2004, 601 nn. 129, 131; cf. Bucciantini 2003, 180–184.

advanced in both scholarly and lay-popular circles that answers all such questions in the affirmative; and this view then leads to a common apologetic line of defense of the Inquisition to the effect that its condemnation of Galileo was essentially correct at least scientifically, methodologically, epistemologically, and logically, whatever its shortcomings may have been from a judicial, moral, or Christian-charity point of view. Camerota is at pains to refute this apologia and the historical interpretations and philosophical evaluations on which it is based. And I believe he does a very good job.

Some of his remarks are worth quoting:

> It is wrong, therefore, to accuse Galileo to have operated in a prejudicial manner in favor of the Copernican view, masking the alleged weakness of his own arguments … by obscuring the reasons of opponents, especially Tycho's followers. In reality, Galileo's conviction that the Copernican and Tychonic hypotheses were not explanatorily equivalent originated from the consideration that the heliocentric view possessed greater coherence and simplicity for the reduction of the complexities of planetary motions, and also was in accordance with the formulations of the new science of motion. [15–16]

Similarly,

> it has been often stressed that despite his 'realistic' claims, Galileo never provided a definitive 'proof' in support of the Copernican system. This is true as long as by 'proof' one means incontrovertible evidence such as the measurement of stellar parallax, that is, an accomplishment that was absolutely unattainable in light of the development of the instrumentation of that time. That said, one must admit that Galileo's battle for a new cosmology displayed such a quantity of 'sensory observations … confirming agreements and very strong arguments' as to exclude any suspicion that one is facing an ineffectual attempt by a fanatic Copernican 'activist'. [352]

On the contrary, the situation should be described by saying that "the demonstrations for the earth's motion are much stronger than those for the other side" (352); this description has the advantage of stressing that in regard to the question of the earth's motion the Tychonic and Ptolemaic systems are identical and both contrast to the Copernican theory. To understand why that description is correct,

> one should not forget what was the strength of the 'proofs' and reasons mentioned to support the alternative doctrines. Above all, one must remember the extra-scientific 'worries' (involving the defense of the world view contained in Sacred Scripture and/or in the texts of the philosophical tradition) that fed in large measure the anti-Copernican hostility and motivated the acceptance of the other (Ptolemaic and Tychonic) cosmological views. [354]

In this discussion, Camerota does not quote, cite, or acknowledge other scholars who have advanced similar views.[17] But we may agree that the point is important enough to deserve repetition; moreover, this apologetic line has a very long history,[18] and it keeps on being revived, and so it is useful for Camerota to focus on its most recent advocates.[19]

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[17] See, for example, Finocchiaro 1984; 1986; 1989, 8–10; 1997, 5, 53–55.

[18] As may be seen from Finocchiaro 2005, 79–85, 138–153, 218–221, 280–294, 306–326.

[19] Camerota mentions in passing Koestler 1959, but also quotes more recent examples: Zoffoli 1990, 106; D'Addio 1993, 19, 21; and Presenti 2001, 70.

The controversy over comets is another well-known episode. Three comets became visible for a few months in the latter part of 1618. In early 1619, an anonymous *Disputatio astronomica* was published in Rome, critical of the Aristotelian view that comets have a terrestrial origin and sublunary location, and favorable to the Tychonic view according to which the 1618 comets were located beyond the moon and followed circular orbits. It was soon learned that the author was Jesuit Orazio Grassi, professor of mathematics at the Roman College. In June 1619, there appeared in Florence a *Discourse on the Comets* that was a joint work of Galileo and his disciple Mario Guiducci, but displayed only Guiducci as the author. The *Discourse* was critical of Grassi's view and advanced a theory according to which comets had a terrestrial origin, but a superlunary location, and followed a rectilinear path away from and orthogonal to the earth. Grassi replied immediately, defending his views and going to the counterattack in the *Libra astronomica ac philosophica*, published the same year under the pseudonym of Lotario Sarsi. Four years later, Galileo responded with a detailed and wide-ranging rebuttal in *The Assayer* (1623), publishing it under his own name. However, Grassi did not acquiesce and three years later published a rebuttal in the *Ratio ponderum Librae et Simbellae* (1626).

This was a very complex dispute that became extremely bitter and polarized. The intellectual issues often appear trivial and the respective positions confused and confusing. Camerota's account contains an insightful discussion of a detail (370–372) which I think goes a long way toward explaining how and why such apparent non-issues could have generated so much animosity. He stresses that Grassi's *Libra* contained an argument to the effect that the Galilean *Discourse* was committed to Copernicanism and thus violated the anti-Copernican decree of 1616; for Galileo claimed that the 1618 comets followed a rectilinear path away from and orthogonal to the earth, and he knew that they exhibited an observable northward deviation, but he was also aware that this apparent deviation could be explained only in terms of the earth's own annual motion. I believe that the contortions of the Galilean view of comets were partly defensive moves and partly over-reactions to this venomous charge. In short, the controversy about comets was not just about comets, but was connected with both the cosmological debate over Copernicanism and the theological controversy over its religious propriety.

Another one of Camerota's insights involves the related topics of the divine-omnipotence argument favored by pope Urban VIII and the ending of Galileo's *Dialogue on the Two Chief World Systems* (1632). Again, much is relatively well known about this episode: the *Dialogue* ends with a *statement* of pope Urban VIII's favorite objection to Copernicanism; Galileo was required by ecclesiastic authorities to include this argument in his book; this was the argument that regardless of how much evidence there is for the geokinetic explanation of tides (and more generally, for the earth's motion), we can never be absolutely certain that this is so (that is, we can never say that this *must* be so), because God is omnipotent, and so He could have created a world in which the tides are caused not by the earth's motion but by something else (or more generally, a world in which the earth does not move), but to say that the tides *must* be caused by the earth's motion (or more generally, that the earth *must* move) limits God's power to do otherwise. On the other hand,

there is much that is controversial about this argument: whether or how strongly the argument in *endorsed* in the text of the book; whether or not the argument is valid, which in turn depends on whether one states the argument in a stronger or weaker version.

Now, an important and novel point here elaborated by Camerota is that

> according to a centuries old tradition, the phenomenon of the tides represented the most impenetrable among the secrets of nature, so much so that not infrequently the problem was regarded as unsolvable in principle, and that the roots of this unsolvability lay deep in the divine will to mortify the vainglory of human reason and to remind it or make it aware of its limitations. [455]

He documents this thesis with evidence from Diogenes Laërtius, Seneca, the pseudo-Augustinian *De mirabilibus Sacrae Scripturae*, Isidore of Seville, Giulio Cesare Scaligero, Leonard Lessius, Eustachio di S. Paolo, and (importantly) from the ending of Galileo's "Discourse on the Tides."[20] Although much more needs to be taken into account, this thesis helps to explain why Church authorities did not want the tides mentioned in the title of Galileo's *Dialogue* and why this book angered Urban: the point is not (as commonly claimed) that the Church did not want to put its stamp of approval on the conclusiveness of the tidal argument, or that Urban resented the fact that in the dialogue his favorite argument is advanced by the relatively unintelligent character Simplicio; rather they felt it was irreverent (toward divine omnipotence) to *seriously* consider *any* explanation of the tides (let alone a geokinetic explanation), and Urban resented the fact that Simplicio has been made to advance an insufficiently strong version of the argument (457–459).

There is at least one other discussion that impressed me favorably, namely Camerota's adaptation of Giorgio de Santillana's thesis that a crucial document in the Inquisition proceedings of Galileo's trial (the executive summary presented to the cardinal-inquisitors and the pope) is a biased account of the proceedings.[21]

23.3 Criticism

But I should not be carried away by favorable impressions, and must give some space to more critical remarks. For despite the just-mentioned and other insights and merits, the book contains a large number of discussions with which I would want to take issue, and many of them involve significant or general questions.

For example, I find the book ambivalent on the question of Galileo's attitude and degree of pursuit and belief toward Copernicanism: sometime Camerota claims (as we saw above) that the Galilean interest was gradual, nuanced, and moderate, rather than total and fanatical, but most of the time he speaks as if the reverse were true. Thus, he claims (98) that in the late 1590's Galileo explicitly accepted Copernicanism,

[20] For this last reference, see Galilei 1890–1909, 5: 395; Finocchiaro 1989, 133.

[21] Camerota 2004, 504–506. Cf. Santillana 1955, 277–283; Galilei 1890–1909, 19: 293–297; Finocchiaro 1989, 281–286.

basing this claim on Galileo's letters to Kepler and to Jacopo Mazzoni, which however need a more critical reading; Camerota claims that in the sunspot letters Galileo held the Copernican view "with complete clarity" (253) and "without hesitation" (259), as if the heliocentrity of Venus's orbit and the changeability of the heavenly region were the only two elements of the Copernican system; and he claims that by 1615 Galileo "believed that Copernicus's doctrine had been fully ascertained" (283), as if Galileo had forgotten about such unanswered problems as the extruding power of diurnal whirling or annual stellar parallax.[22]

Another over-arching and crucial issue is that of Galileo's attitude toward mathematization and the application of mathematics to nature. Here Camerota's account is not ambivalent, but rather attributes to Galileo a position that might be labeled apriorist mathematicism, reminiscent of Koyré.[23] Camerota's argument makes much of the Galilean statement from *The Assayer* that the book of nature is written in the mathematical language of triangles, circles, etc. The difficulty is that, like Koyré, Camerota's analysis of such remarks is excessively abstract insofar as he takes them out of context, both the context of the passages where they occur, as well as the context of Galileo's scientific practice. In any case, to portray Galileo as mathematicist and apriorist, Camerota seems to ignore what could be regarded as the epistemological climax of the *Two New Sciences*, where Galileo discusses the crucial issue of whether the particular mathematical analysis of uniform acceleration which he has just elaborated corresponds to the way heavy bodies really fall[24]; and similarly in one of the methodological highpoints of the *Dialogue*, Galileo discusses the question of whether his mathematical analysis of the problem of centrifugal extrusion on a rotating earth corresponds to physical reality.[25] The key point is that, as he makes clear, even if we are assured that nature is written in the mathematical language of triangles, circles, etc., which particular type of geometrical figure corresponds to which particular phenomenon is a question that can only be resolved by observation and experiment.[26]

Yet another kind of difficulty besets Camerota's discussion of a third important and general issue. It relates to the Galileo affair. One of the most highly debated issues has been whether in 1633 he was condemned for disobedience or for heresy, and the discussions are usually carried out as if these two alternatives were mutually exclusive. The disobedience would be his alleged violation of the restrictions developed in 1616, either the special injunction that forbade him to discuss the earth's motion in any way whatever, or cardinal Bellarmine's warning prohibiting him from defending the idea. The alleged heresy would be the Copernican doctrine, which had

[22] For a fuller documentation and elaboration of these necessarily condensed remarks, see Finocchiaro 1988.

[23] The most emblematic passage occurs in Camerota 2004, 559–560; but see also, for example, pp. 20–23, 386–387.

[24] Galilei 1890–1909, 8: 198–205; 1974, 154–161.

[25] Galilei 1890–1909, 7: 229–237; Finocchiaro 1997, 193–202.

[26] Again, for a fuller documentation and elaboration of these necessarily condensed remarks, see Finocchiaro 2003; 1997, 348–352; 1980, 62–102.

been condemned by the Index's decree of 5 March 1616. The disobedience interpretation makes the trial and condemnation of Galileo an instance of merely disciplinary proceedings, thus diminishing its general significance and in turn the seriousness of the presumed error by the Inquisition, and consequently offering a common apologetic line of defense. The heresy interpretation makes the issue a doctrinal one, thus aggravating the gravity of Galileo's alleged crime, but also the gravity of the Church's own misdeed, and perhaps even its claims to (doctrinal) infallibility. Camerota argues that "the sentence of June 1633 struck Galileo not for having disobeyed the 'injunction' of 1616, but for having entertained an opinion declared heretical by reason of its contrast with the scriptural text" (518); and his argument is very good, indeed perhaps the best I have seen in support of such a thesis, and it should give pause to anyone who holds the reverse thesis (that Galileo was condemned for disobedience and not heresy). Nevertheless, the cogency of Camerota's argument applies only to the positive part of his thesis; the negative part that denies the alternative interpretation is unjustified. The crux of the matter is that on this particular issue, both sides are wrong insofar as they deny the alternative, although they are both partly right for what they choose to affirm; in short, they both presuppose the false dilemma that Galileo was condemned for only one of the two alternatives (disobedience or heresy), but not both. In fact, I believe the most tenable position is that Galileo was condemned for both disobedience and heresy. A key part of my argument would be that the disobedience in question involves intellectual matters of what to hold or defend, and so it is not merely a disciplinary matter but becomes a doctrinal one; moreover, heresy ultimately is simply to believe what the Church commands not to believe, or not to believe what it commands us to believe, in short disobedience in matters of belief. So there is no contradiction between the two reasons, but they are rather different ways of looking at the situation.[27]

There is no space here to do anything more than mentioning, without elaboration, other, relatively minor difficulties. For example, I found Camerota's account of Galileo's tidal theory (354–363) superficial as a whole, despite his sound sensitivity to those aspects of it that may be scientifically correct; in particular, Camerota's explanation (358) of Galileo's basic diagram seems to interchange point *L* with *B* and *C* with *D*. I also believe Camerota does not document satisfactorily the claim (424) that in the spring of 1630 (while in Rome to have his manuscript of the *Dialogue* approved for publication) Galileo attended a meeting (something of a party) organized by the Vallombrosan monk Orazio Morandi; in fact, although we have Morandi's letter of invitation,[28] there is no documentation that Galileo accepted the invitation. And Camerota seems too quick to dismiss (440–441) the Platonic cosmogony discussed speculatively by Galileo in the *Dialogue* and mentioned again in the *Two New Sciences*[29]; for although there is no question that the Galilean speculation commits the scientific error of assuming that the law of squares is universally

[27] Such an interpretation, of course, needs further elaboration. I have made a start in Finocchiaro 2001, 114–132; 2005, 273–274.

[28] Morandi to Galileo, 24 May 1630, in Galilei 1890–1909, 14: 107.

[29] Galilei 1890–1909, 7: 53–54, 8: 284.

valid at interplanetary distances, it is not at all obvious that the consequences drawn from this false assumption are themselves false or invalidly derived.[30]

23.4 Conclusion

In summary,[31] Camerota's biography of Galileo is welcome for its timeliness, useful for its breadth, valuable for its depth, usually well-documented, and occasionally insightful; but on some key issues it is best taken as a good occasion to reaffirm or develop more tenable positions.

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[30] For an argument that they are not, see Finocchiaro 1980, 82–84; for an argument on the other side, see I. B. Cohen 1967.

[31] It may be useful to also note a few points bordering on typographical and editorial matters. Among quasi-typographical errors, the most important could be corrected by replacing 'sfera' by 'regione' (439); 'oriente' by 'occidente' (443); '11 gennaio' by '11 febbraio' (483); 'Pasquale Zaccaligo' by 'Zaccaria Pasqualigo' (495); and 'precedute' by 'seguite' (531). And among semi-editorial flaws, one should mention that of the works cited in the notes, some are, but some are not, listed in the bibliography, with no discernible criterion of inclusion; the user-friendliness of the copious and informative endnotes (569–671) is marred by the lack of running heads with page or even chapter numbers; that although the book has a useful name index, it has no subject index; and the potentially useful information about monetary matters is limited insofar as the exchange rate between *scudi*, florins, and *lire* is only *implicitly* given once (79) and that between them and ducats (116) is not given at all.

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Chapter 24
Crombie's *Galileo's Natural Philosophy*: Disputation vs. Demonstration vs. Argumentation

Abstract In the context of a critical examination of Alistair C. Crombie's unpublished typescript *Galileo's Natural Philosophy*, I explore several themes that are intrinsically important and widely present in Galileo's scientific practice and methodological reflections. Crombie's typescript is impressive for its erudition. Moreover, it is valuable for drawing attention to several documents that suggest some promising research projects. However, the typescript displays mostly what must be regarded as ill-digested syncretism. More importantly, its key interpretation of Galileo's scientific methodology is deeply flawed for failing to understand and appreciate several Galilean distinctions: critical vs. a-critical disputation, necessary vs. contingent truth, mathematics vs. physics, and necessary demonstration vs. sense experience.

24.1 Introduction

This essay undertakes a critical examination of Alistair C. Crombie's unpublished book manuscript entitled *Galileo's Natural Philosophy*. The manuscript was apparently written in the late 1960s and early 1970s, and it was discovered in the last several years by Filip Buyse. Crombie was a well-known historian of science who taught mostly at Oxford University and died in 1996. Buyse found the manuscript among Crombie's papers, and was sufficiently intrigued to bring it to the attention of a number of scholars. He made the manuscript available in various electronic versions; organized a conference, to which I was invited but which I could not attend; and got the support to edit and publish a volume on Crombie's manuscript. Some other preliminary clarifications are now in order.

To begin with, Crombie makes it clear that the book was researched and written "with the collaboration of A. Carugo." Adriano Carugo was an Italian scholar specializing primarily on Galileo. His most noteworthy contribution is perhaps the 1958 Italian edition of Galileo's *Two New Sciences*[1]; the copious annotations and the

[1] Galilei 1958.

M. A. Finocchiaro, *Science, Method, and Argument in Galileo*,
Argumentation Library 40,
https://doi.org/10.1007/978-3-030-77147-8_24

translation into Italian of Galileo's Latin passages make this is a monumental accomplishment. It was formally co-edited with the distinguished philosopher Ludovico Geymonat (1908–1991); but, in the book's Preface, Geymonat himself gives Carugo primary credit for the edition. However, with regard to the manuscript that is the subject of the present essay, it is clear that Crombie is the primary author, so much so that, for convenience of exposition, I shall speak of Crombie's manuscript and omit Carugo's name.

A similar qualification is needed with regard to the subject matter of Crombie's manuscript. I have already referred to it by the title *Galileo's Natural Philosophy*. However, Crombie himself sometimes refers to it by other titles, such as *Galileo and Mersenne: Science, Nature, and the Senses in the Sixteenth and Early Seventeenth Centuries*. This would suggest that Crombie is dealing with both Galileo's and Mersenne's natural philosophies. On the other hand, in the manuscript provided to me by Buyse, Mersenne's natural philosophy takes up only one out of eight chapters, or, to be more precise, 326 pages of text plus 100 pages of notes out of a total of 2172 pages; that is, approximately one-fifth of the manuscript.[2] Moreover, Crombie says that this chapter on Mersenne is part of a more ambitious work entitled *Galilei, Mersenne, and the Science of Music*; but the other parts of this work are not in the present manuscript. Thus, again, for ease of exposition, I shall refer to Crombie's manuscript as dealing with Galileo's natural philosophy.

Next, to be more specific, it is clear that, for Crombie, Galileo's natural philosophy includes both his philosophy of nature and his philosophy of science, that is, both his theories of natural phenomena and his theories of scientific investigation. The natural phenomena in question are primarily: motion in general, motion of terrestrial bodies (fall, projectiles, pendulum, and floating in water), motion of heavenly bodies (sun, moon, planets, and fixed stars), matter and its properties, heat, and light. The aspects of scientific investigation are primarily: sense perception, experimentation, measurement, instrumentation, demonstration, disputation, the relationship between mathematics and physics, the principle of simplicity, and the problem of complexity.

Finally, it will be useful to note a number of miscellaneous items, whose explicit mention should prove informative and will facilitate exposition. The manuscript I have in my possession, provided to me by Buyse, consists of typewritten pages which were converted into ten PDF Documents, named DOC000 to DOC009. In my notes, and occasionally in my main text, I will refer to the Document number when discussing Crombie's claims and passages.

Document DOC000 consists of 435 pages, which makes it the longest. It contains an introductory Chapter 1, which in part summarizes the whole book. There is also Chapter 2, on Galileo's early logic of science and philosophy of nature, in the period 1584–1604. We also have a Chapter 3, dealing with mathematics in sixteenth-century thought.

Document DOC001 consists of 314 pages and contains the single Chapter 4, dealing with Galileo's activities as a mathematician and philosopher in the period 1610–1615. It includes discussions of Galileo's telescopic discoveries announced in

[2] DOC006, 1–326; DOC007, 1–100.

The Sidereal Messenger; of the controversy on floating bodies reported by Galileo in his book *Discourse on Bodies in Water*; and of the discovery of and controversy about sunspots, discussed by Galileo in *History and Demonstrations Concerning Sunspots*.

Document DOC002 amounts to 228 pages, and its only Chapter 5 discusses the Copernican controversy over the earth's motion climaxing in 1616: its Neoplatonic background; its theological and hermeneutical background; Galileo's criticism of the scriptural arguments against Copernicanism; his tidal argument; and the main ecclesiastic proceedings, concluding with the Index's condemnation of the Copernican idea of the earth's motion as false and contrary to Scripture.

Document DOC003 is 131 pages long, and contains only notes, hundreds of them, referring to the previous five chapters in the previous three Documents.

Document DOC004 contains Chapter 6, with a total of 133 pages, 119 of text, and 24 of notes referring to this chapter. The chapter is entitled "The World Perceived and Known (1623–1639)," and has one section discussing Galileo's *Assayer*, and another section discussing the *Dialogue on the Two Chief World Systems* and its consequences. There no is discussion of the Inquisition trial, or rather a mere sentence of 4 lines on p. 110.

In Document DOC005, of 159 pages, we find Chapter 7, on methodological problems. It has four parts, dealing respectively with simplicity issues in the study of motion in general; problems of experimentation and apriorism vs. empiricism, involving the laws of motion discussed in the *Dialogue* and in the *Two New Sciences*; problems of complexity, in giving accounts of the difference between inanimate matter and living things, and of the strength of materials; and problems of complexity in giving accounts of sensation, heat, light, colors, and sound.

Document DOC006 contains the text of Chapter 8, which is the one dealing with Mersenne's natural philosophy, as part of the announced work on the science of music. The notes to this chapter are in Document DOC007, which consists of 100 pages.

The other two Documents (DOC008 of 227 pages, and DOC009 of 119 pages) are all notes to the previous seven chapters on Galileo's natural philosophy.

It should be added that Crombie's various tables of contents indicate that his book was supposed to have an Appendix containing relevant documents as well as English translations of many primary sources discussed in the book. Such an Appendix is not included in the PDF files provided by Buyse, but I get the impression that it would amount to several hundred pages (perhaps thousands). On the other hand, it should also be mentioned that Crombie's manuscript, in the version of the available and above mentioned PDF files, contains many unusually lengthy quotations from the primary sources. These are usually translated into English when the original passages are in Latin or Italian, but they are usually left untranslated when the originals are in French. For example, quoted and translated in full are Galileo's letter to Castelli of December 21, 1613 (7 pp., 2815 words) and Galileo's letter to Dini of March 23, 1615 (8.5 pp., 3564 words).[3]

[3] DOC002, 1–7, 8–16.

24.2 Ill-Digested Syncretism

Let us now go on to more interpretive and evaluative issues. The first critical interpretation I would like to advance is one which I adopt from one of Crombie's own discussions. In Chapter 3, on the sixteenth-century background, he has a lengthy account of Jacopo Mazzoni (1548–1598). At the time, Mazzoni was a celebrated philosopher who had a very ambitious agenda that included the combination of the ideas of Plato and Aristotle. He was a senior colleague of Galileo's during the time that the latter taught mathematics at the University of Pisa, and they were friendly enough to engage in many argumentative conversations that reverberated even later, when Galileo moved to the University of Padua.[4] At the beginning of his lengthy discussion of Mazzoni, Crombie introduces him by reporting a number of judgments by his contemporaries as well as by subsequent writers. After mentioning or quoting some favorable judgments about Mazzoni, Crombie reports the following: "The eighteenth-century historian of philosophy Jacob Brucker struck a more sceptical note in describing the first of his [Mazzoni's] two main philosophical books as a work of erudite but ill-digested syncretism."[5] This is the critical judgment which I want to appropriate and apply to Crombie's own manuscript: it is a work of erudite but ill-digested syncretism.

As an indication of such syncretism, but also as another general flaw of Crombie's book, I would mention that he has a tendency to devote too much space to Galileo's predecessors, contemporaries, and interlocutors as compared to Galileo himself, often an equal amount of space, and sometimes more to them than to Galileo. Needless to say, history and contextualization are important, but Crombie seems to exaggerate and magnify them beyond proportion and propriety.

For example, whereas about 100 pages are devoted to Galileo's early logic of science and philosophy of nature in Chapter 2, about 200 pages are allocated to the natural philosophy of other sixteenth-century thinkers in Chapter 3.[6] And when, in Chapter 5, Crombie comes around to discussing the Copernican controversy up to 1616, he devotes about 143 pages to such topics as the neo-Platonic background and aftermath and the theological background, and only about 85 pages to Galileo's own ideas and activities.[7]

Another aspect of the same tendency toward imbalance involves quotations from primary sources. I have already mentioned that Crombie's manuscript is full of very lengthy quotations; and, as I'll discuss in a moment, some of these are very valuable and constitute merits. However, it turns out that the longest quotations are not from Galileo's works, but from the works of others.

For example, one of the two longest quotations comes from Giulio Cesare Lagalla's "Disputation on Light and Illumination," which amounts to about 6500

[4] See Finocchiaro 2019, 70–71.

[5] DOC000, 367; Crombie does not present this as a quotation, but does refer to Brucker (1743/1767).

[6] Respectively, DOC000, 109–216; DOC000, 217–421.

[7] Respectively, DOC002, 21–150, 215–228; and DOC002, 1–20, 129–214.

words and takes up 29 double-spaced pages in Crombie's manuscript[8]; this occurs in Chapter 4, in the context of his discussion of Galileo's natural philosophy in 1610–1515. The other quotation of comparable length is found in Chapter 7, dealing with methodological issues in the period 1632–1639; there, near the end, Crombie quotes almost the entire letter by Benedetto Castelli to Galileo dated August 15, 1637. In this letter, Castelli discussed various phenomena involving light, heat, colors, and sensation from the point of view of the methods of resolution and composition. In the National Edition of Galileo's works, the full letter amounts to about 6500 words, and takes up about 500 lines, or 14 pages.[9] Crombie breaks up the letter into two main parts, which together take up about 10 single-spaced pages of his manuscript.[10]

Besides such disproportions in coverage of Galilean and non-Galilean works, and in quotations from Galilean and non-Galilean sources, Crombie's manuscript exhibits a similar imbalance in the amount of space devoted to Galileo's own ideas and accomplishments. That is, Crombie tends to focus more or excessively on Galileo's early or secondary works, as compared with his mature or primary ones.

For example, Crombie devotes the entire Chapter 2, consisting of 107 pages, to Galileo's early natural philosophy of 1584–1604, including his so-called *Juvenilia*, or lecture notes on Aristotle's logic of science and philosophy of nature.[11] However, on the mature *Dialogue* and *Two New Sciences*, we find only a comparably meager 123 pages, in the second part of Chapter 6 and the first half of Chapter 7.[12]

Similarly, on Galileo's inconclusive speculations about the nature of light, heat, matter, human vision, and human sensation, Crombie seems profuse: 33 pages on vision and the nature of light, in the context of Chapter 4, "Mathematician and Philosopher (1610–15)"; 13 pages on Galileo's letter to Dini of March 23, 1615, in the context of Chapter 5, on "The Cosmological Debate to 1616"; 75 pages on the discussion of such topics in *The Assayer*, in Chapter 6, which Crombie entitles "The World Perceived and Known (1623–39)"; and 75 pages in Chapter 7, apropos of the discussion of the methodological problems of complexity examined in its second half.[13] These add up to 196 pages.

However, a diligent search for Crombie's discussion of more central topics yields 288 pages: 14 pages in the section on "Galileo and Copernicus (1597–1606)" in Chapter 3; 65 pages in Chapter 4, in a section on Galileo's *Sidereal Messenger* and initial telescopic discoveries; 86 pages again in Chapter 4, in the section on sunspots; 58 pages in Chapter 6, in a section on the *Dialogue* and the motion of the earth; and 65

[8] DOC001, 200–228. Crombie cites Lagalla, "De 1uce et de lumine disputatio," published in Lagalla 1612, 57–72; reprinted in Galilei 1842–1856, 3: 334–359; only Chapter 1 included in Galilei 1890–1909, 3: 927–929.

[9] Galilei 1890–1909, 17: 156–169.

[10] DOC005, 114–123.

[11] DOC000, 109–216.

[12] DOC004, 76–133; DOC005, 2–66.

[13] Respectively, in DOC001, 127–159; DOC002, 8–20; DOC003, 1–76; and DOC005, 67–141.

pages on motion in general and falling bodies in the first two sections of Chapter 7.[14] Thus, admittedly, these more important topics are given more attention; but this also means that the secondary topics are getting 2/5 of the attention and the central issues are getting 3/5, and such a proportion strikes me as grossly problematic and unbalanced.

24.3 Appreciation

Before I go on to elaborate some more criticism regarding more substantive, substantial, and instructive issues, I want to say a few words of appreciation for Crombie's manuscript. For I must confess that reading it enabled me to learn of the existence and importance of several texts and related developments, which deserve further study and could become the subject of some promising future research projects.

One such memorable text is Tommaso Campanella's letter to Galileo dated January 13, 1611.[15] Campanella (1568–1639) is an important and interesting figure in his own right, author of many thoughtful and innovating works in metaphysics, natural philosophy, political philosophy, and theology. Moreover, partly because of such writings, and partly because of some of his practical involvements, he got in trouble with both the Inquisition and the civil authorities of the Kingdom of Naples in Southern Italy. And to complicate matters even further, Campanella was an ordained Dominican friar. Especially in a Galilean context, Campanella's most important work is clearly his *Apologia pro Galileo*, written in 1616, first published in Frankfurt in 1622, and promptly placed on the *Index of Prohibited Books* by the Catholic Church.[16]

The letter in question was Campanella's first letter to Galileo, written immediately after he had read the *Sidereal Messenger*. It is worth mentioning that Campanella was in prison in Naples at the time, but apparently he had the freedom to read and write. However, he did not know that by January 1611 Galileo was living in Florence and held the position of Philosopher and Chief Mathematician to the Grand Duke of Tuscany. Thus, the letter is addressed to Galileo in Padua, where he had been professor of mathematics at the university, from 1592 until mid-1610.

This is a long letter, consisting of 5 pages of small print. In it, Campanella is enthusiastically congratulating Galileo for his telescopic discoveries and their significance. In the process, he displays acquaintance with esoteric points and questions of astronomy; and he makes erudite references to astronomers, theologians, and Church Fathers. He is favorably inclined toward Copernican astronomy, but with an attitude of openness to novelties rather than of commitment to Copernicanism. He compares Galileo to Christopher Columbus with regard to the significance of their respective discoveries, and this is a comparison which Galileo would adopt. And, at one point,

[14] Respectively, in DOC000, 422–435; DOC001, 62–126; DOC001, 29–314; DOC004, 76–133; and DOC005, 2–66.

[15] DOC002, 133–138; Galilei 1890–1909, 11: 16–20.

[16] See Campanella 1622, 1992, 1994.

Campanella points out that theologians will probably object to such discoveries, but adds that the answer to their objections can be found in a proper reading of the theological tradition and of Holy Scripture; and this corresponds to one main line of argument which Campanella would pursue in his later *Apologia*.[17]

Thus, on the one hand, I am a little embarrassed that I had never read this letter before; on the other hand, I am not embarrassed to credit Crombie's manuscript for bringing it to my attention. Campanella's letter is certainly a significant reaction to Galileo's *Sidereal Messenger*, and it adds evidence to the idea of studying it further. The letter deserves a full and better translation into English, since Crombie's translation is partial and often sloppy. More generally, the letter adds evidence to the idea of a book-length study of Galileo and Campanella, comparing and contrasting their lives, their trials, and their works.[18]

Next, Crombie's manuscript also contains another interesting and important discussion, but one which is striking for very different reasons. In Chapter 7, we find an account of the correspondence, in 1633–1635, between Galileo and a French engineer named Antoine De Ville.[19] Now, again, I am not embarrassed to confess, not only that I had never read this correspondence, but also that I did not know of its existence and I had never heard of this person. Crombie tells us that, at the time, De Ville was employed by the Republic of Venice, and, as usual, he quotes extensively from this correspondence.

From Crombie's account and quotations, we learn that De Ville's first letter is dated January 1633 and was apparently a very enthusiastic, but detailed and thoughtful, reaction to Galileo's *Dialogue*, published about ten months earlier. Moreover, another letter by De Ville was dated March 1635 and was a reaction to the manuscript of the First Day of the *Two New Sciences*. In fact, Galileo had sent parts of his manuscript to his Venetian friend and supporter Fulgenzio Micanzio, to explore the possibility of publishing his book in Venice. Micanzio had shown the manuscript to De Ville, and the latter was contacting the author with detailed and thoughtful, but this time critical and respectful, comments. And Galileo took the trouble of replying to these comments in a letter to De Ville.

Some of what Crombie does not tell us is also important and interesting. The correspondence consists of only three letters, two by De Ville and one by Galileo.[20] That is, for example, Galileo did not reply to De Ville's first letter; but recall that in January 1633 Galileo left Florence to go to Rome and stand trial by the Inquisition, because of his publication of the *Dialogue*. Clearly, at that time, De Ville did not know anything about such developments, and Galileo was too overwhelmed with them to respond to normal correspondence. Moreover, De Ville's letters are relatively long, respectively 3440 words and 3562 words. His first letter is significant at least because

[17] Cf. Finocchiaro 2010, 89–93; 2019, 208–214.

[18] Stefano Gattei is pursuing research along these lines; see Gattei 2019, 297 n. 47.

[19] DOC005, 81–89.

[20] De Ville to Galileo, 4 January 1633, in Galilei 1890–1909, 15: 12–18; De Ville to Galileo, March 1635, in Galilei 1890–1909, 16: 221–228; and Galileo to De Ville, March 1635, in Galilei 1890–1909, 16: 242–244.

it says that he was favorably impressed by the strength of Galileo's arguments in the *Dialogue*, and thus it provides some relatively objective and external evidence of the reception of this book by competent readers at the time. And De Ville's second letter is important because it probably contributed to some revisions and improvements in Galileo's final manuscript of the *Two New Sciences.*

De Ville's letters are in Italian, which is understandable, given that he lived and worked in Venice, and given Galileo's well-known preference for the vernacular rather than Latin. Although they are clear and comprehensible, they are also full of mis-spellings, grammatical errors, linguistic solecisms, and other such oddities. However, the fact that De Ville wrote in Italian is noteworthy. With some exaggeration, but not much, one could say that this fact is as remarkable as the fact that the current (2019) President of the French Republic, Emmanuel Macron, uses the English language (not his native French) when speaking at international gatherings (such as his visit to the United States in April 2018 and the celebration of the 75th anniversary of D-Day on June 6, 2019).

Thus, again, here we may credit Crombie's manuscript for suggesting a future fruitful research project. However, the most memorable example of an item that cries out for further interpretation, evaluation, and analysis is the following.

In Chapter 7, on "Scientific Methods (1623–1639)," the last section deals with problems of complexity involving the nature of light, heat, and human sensation. The most sustained of these discussions is an account of three letters by Benedetto Castelli to Galileo, dated June 27, August 9, and August 15, 1637.[21] Here, Crombie's primary interest seems to be the content or substance of Castelli's views, such as the similarities and differences between light and heat, whether they consist of particles or of vibratory processes, and how the human senses manage to perceive them. The only methodological aspect of Crombie's discussion is to point out that this topic is much more complex than the topic of the nature of motion and the laws of falling bodies. In the process, as usual, Crombie also gives lengthy quotations from Castelli, especially from his third letter; indeed, as I mentioned earlier, Crombie's quotation of this letter is one of the two longest quotations in the whole manuscript.

Now, it should be clear that in this case I am not criticizing Crombie for devoting more space to the works and ideas of others than to Galileo. Such a criticism would be misconceived, to say the least, since it is well known that Castelli was a university student and professional follower of Galileo's and developed similar ideas; and on the present topic, it is likely that by and large Galileo would have agreed with Castelli, subject of course to different nuances and minor qualifications. Nor would I want to use this case to object that Crombie's discussion of Castelli's letters focuses on aspects that miss their main value; for although I think that Crombie does miss the main value of these letters, he reports and quotes enough of their content that the reader can easily perceive or intuit their significance; that is what happened to me, who had never read them before. What is the significance of these letters, especially

[21] DOC005, 110–124. See Castelli to Galileo, 27 June 1637, in Galilei 1890–1909, 17: 121–123 (#3509); 9 August 1637, in Galilei 1890–1909, 17: 150–155 (#3539); 15 August 1637, in Galilei 1890–1909, 17: 156–169 (#3541).

the third one, which is the longest one and is an expansion and elaboration of the previous two?

In my opinion, Castelli's (third) letter is a logical, methodological, and philosophical gem that deserves much more study than it has received so far. In fact, it is no accident, but rather revealing, that soon after his death (in 1643), this letter was published (in 1669) in a collection of philosophical essays by Castelli.[22] Let me explain.

Castelli begins his letter with the story of a conversation he had had with a young student about the following (easily ascertainable) fact: that a brick painted half black and half white on one of its sides gets hotter in the black part when exposed to sunlight. Castelli questions the young man with a series of helpful and suggestive questions until the youth arrives at an explanation of this fact. Then Castelli reflects on the process of finding such an explanation: he mentions that puzzlement has a role; that the process seems to involve the reduction of what is not understood to what is understood; that the process can be analyzed in terms of the method of resolution; and that it can also be analyzed in terms of the method of composition. Next, Castelli notes that one could also express puzzlement about two propositions used in the previous explanation: that light produces heat; and that white surfaces reflect more light than black surfaces. These are well known facts, but the explanation of them is a very difficult and complicated problem. Then Castelli proceeds to find explanations of these two facts, and of other facts ascertained in the process; and he continues to be explicit in distinguishing what is known from what is not known; what is understood from what is not understood; and what is known from what is explained.

The questioning of the young student to make him arrive at the explanation himself strikes me as a brilliant illustration of the Socratic method; this is as memorable as the classic example in Plato's *Meno*, where Socrates questions an ignorant slave boy to make him discover Pythagoras's theorem on his own. The explicit formulation and utilization of the distinction between a known fact and the explanation of the fact are a sign of impressive methodological sophistication; its importance is difficult to exaggerate, as one see from the discussion in the latest (June 2019) issue of the journal *Isis*.[23] The explicit formulation and analysis of various arguments in terms of the method of resolution and of the method of composition is a logical feat worth emulating and studying; it is reminiscent of some of Galileo's own logical accomplishments, such as his critique of the anti-Copernican argument from vertical fall in the *Dialogue*.[24]

There are two other noteworthy details I learned from Crombie's manuscript. One is a comment by philosopher Thomas Hobbes (1588–1679) in a letter to the Duke of Newcastle, dated February 5, 1634.[25] This is important in light of Hobbes's stature as

[22] Castelli (1669). I take this information from an annotation by Favaro in Galilei 1890–1909, 17: 156; and from DOC005, 155–156, n. 82 s.

[23] See especially Cohen 2019; "Focus," *Isis* 110: 290–359; and Finocchiaro 1973.

[24] See Finocchiaro 1980, 343–412; 2010, 124–129; 2014, 100–105; 2019, 228–235.

[25] DOC004, 110–111; also in Galilei 1890–1909, 20: 606–607; and in Camerota et al. 2015, 281–282.

a founder of modern philosophy, most famous as the author of the *Leviathan* (1651). In this letter, Hobbes reports that he has tried to buy a copy of Galileo's *Dialogue*, and has discovered than no one who has a copy is willing to part with it; but fortunately the book is being translated into English by someone named Joseph Webbe. Hobbes also states having heard that the book has been banned in Italy, and then suggests that this ban may be due to the "opposition … between their religion and naturall reason."[26]

Now, this explanation of Galileo's condemnation is worthy of further reflection, corroboration, and analysis. For now, it is just worth mentioning that Hobbes's focus on reason and the Catholic Church's opposition to it was also shared by another Englishman: Thomas Salusbury, who in 1661–1665 published a two-volume work with English translations of many Galilean works, including the *Dialogue*, *Two New Sciences*, and *Letter to Christina*.[27] Furthermore, this notion of an opposition between the Catholic Church and reason is not merely of historical interest; it is still viable and tenable, at least when the proper qualifications, nuances, and complications are taken into account, and when "reason" is understood to mean "critical reasoning" about natural phenomena.[28]

Finally, my appreciation for Crombie's manuscript involves an interaction between Marin Mersenne and Nicholas Claude Fabri de Peiresc (1580–1637), an enlightened French Catholic who was one of the leading intellectual and cultural politicians of the time. In the chapter (no. 8) on Mersenne, Crombie has a lengthy discussion of (and as usual many quotations from) some correspondence between Mersenne and Peiresc, dated mostly in 1635; the issue was Mersenne's critique of Galileo's work on falling bodies.[29]

In 1633, Mersenne had published a book entitled *Traitè des mouvements*, which included a critical examination of Galileo's claims on falling bodies advanced in the *Dialogue* (1632).[30] Mersenne agreed with Galileo on the correctness of the law of squares: that the distance traversed by a falling body is proportional to the square of the time elapsed. However, Mersenne expressed doubts about the numerical value given by Galileo for the acceleration: that a freely falling body traverses 100 cubits in 5 seconds; today, we know that this is incorrect, being about one half the true value. A related issue that worried Mersenne was the meaning (in French units) of the cubit (actually the Florentine *braccio*) that Galileo used as a unit of measurement here.

This was the main issue that started the 1635 correspondence: Mersenne was planning to include a revised version of his critique of Galileo in his forthcoming book *Harmonie universelle* (1636), and he wrote Peiresc if he could find out, perhaps from Galileo himself, the value of the Florentine *braccio*. In the process, Peiresc discovered that Mersenne was criticizing Galileo and felt that such criticism was

[26] DOC004, 111; Galilei 1890–1909, 20: 607; Camerota et al. 2015, 281–282.

[27] Salusbury 1661–1665, tome 1, part 1, Foreword to the Reader, unnumbered pages. For details, see Finocchiaro 2005b, 78–79.

[28] For an analysis along these lines, see Finocchiaro 2019.

[29] DOC006, 98–131.

[30] Specifically at Galilei 1890–1909, 7: 248–250; cf. Finocchiaro 2014, 139–145.

too cruel. Thus, Peiresc tried to convince Mersenne to make sure that his criticism was not too harsh; specifically, he suggested that if Mersenne's criticism was really warranted objectively, then he should not present it as an unfriendly or negative evaluation, but as a friendly improvement.

Peiresc's concern and attitude in this correspondence fit very well with the concern and attitude which we already knew he exhibited toward Galileo after the Inquisition trial and condemnation. The latter included Peiresc's plea for a pardon in some letters to Cardinal Francesco Barberini, member of the Roman Inquisition, Vatican secretary of state, and nephew of Pope Urban VIII.[31] However, now Peiresc's attitude seems more persistent and multi-faceted, and it certainly deserves further investigation.

24.4 Crombie's Key Interpretive Thesis

It is now time to examine some more central topics. The most significant of these is perhaps Galileo's attitude toward disputation on both sides of an issue, necessary demonstration, and mathematical proof. Crombie portrays Galileo as rejecting such disputation, and instead favoring such demonstration and proof.

Let us begin with a discussion that introduces some key terminology. In Chapter 4, dealing with the period 1610–1615, Crombie has a section on the controversy over bodies floating in water.[32] Recall that Galileo held that the primary cause of such floating is the fact that such bodies have a smaller density than water, and in the process he followed an Archimedean approach. His opponents claimed that the basic cause was the shape of such bodies, and followed an Aristotelian approach. The controversy produced several books: Galileo's own *Discourse on Bodies in Water* (1612); critiques by several authors; and Galilean criticism of his critics. These replies to Galileo's critics were published under the formal authorship of his former student Benedetto Castelli, although there is no doubt that they involved a joint collaboration with Galileo himself.

One of these critics was a professor at the University of Pisa, named Giorgio Coresio, and he published a book against Galileo's position. In turn, Castelli and Galileo produced a lengthy reply, which for various reasons was never published. However, the text of this reply has survived and is available in the collected works of Galileo. This edition also contains a series of notes in Galileo's own hand, which he wrote when re-reading the final manuscript formally ready for publication under Castelli's name.

One of the objections in this manuscript was to point out that Coresio "is mistaken in assuming that Signor Galileo had sent his *Discorso* for printing in order to provoke the learned and not because what he has expounded is his own opinion."[33] To this, Galileo's handwritten note added that "Galileo … being used to study in the book of

[31] For details, see Finocchiaro 2005b, 52–56.

[32] DOC001, 10–62.

[33] Quoted in DOC001, 36; cf. Galilei 1890–1909, 4: 248.

nature, where things are written in only one way, would not be able either to dispute any problem *ad utranque partem* or to maintain any conclusion not first believed or known as true."[34]

This Galilean rejection of disputation is something which Crombie does not mention only when (in Chapter 4) he is presenting a purely descriptive account of the controversy over bodies floating in water. Rather it seems to be his favorite quotation when he is elaborating his interpretation of Galileo's methodology and epistemology in general. For example, in the introductory Chapter 1, Crombie claims that for Galileo

> True rational science was not the same as a scholastic dispute conducted simply as a logical exercise, the construction of general systems that solved no particular physical problems, mere literary philosophical erudition with its eclectic search for concordance, … Galileo's ideal in natural philosophy, put in the strongest terms, was to find "necessary demonstrations" of "the true constitution of the universe. For such a constitution exists, and exists in only one, true, real way, that could not possibly be otherwise." This is the language of Aristotle's theory of truly scientific demonstration in the *Posterior Analytics*.[35]

And, between the two parts of this quotation, at one point Crombie quotes Galileo's rejection of disputation from the reply to Coresio. Moreover, here Crombie attributes to Galileo a version of the Aristotelian epistemology of necessary demonstration, with a quotation taken from Galileo's *Sunspots* book.[36]

There is at least another good example of Crombie's interpretation where he attributes to Galileo a rejection of disputation on both sides of an issue, as well as a commitment to necessary demonstration of natural truths, and he does so based on Galileo's remark in the floating-bodies controversy and his remark on the true constitution of the universe in the sunspots controversy. The pretext is provided by an assertion made by Campanella in a letter to Galileo in 1632, where the main topic was Campanella's attempt to get himself appointed to the commission investigating complaints about the *Dialogue* (which then led to the Inquisition trial). The attempt failed for many reasons, including the fact that Campanella had written, had published, and had had banned his *Apologia pro Galileo.* At one point in his letter of October 22, 1632, Campanella says that in the *Apology* he was engaged in disputation *ad utranque partem.* Crombie capitalizes on this remark to contrast Campanella and Galileo. In Crombie's own words:

> In two further letters Campanella went on: "… in it [the *Apology*] I do not take any decision but discuss *ad utranque partem.*" Campanella's conception of a new philosophy was as far as Telesio's from Galileo's search for "sensible experience and necessary demonstrations" of "the true constitution of the universe" such as "exists, and exists in only one, true, real way, that could not possibly be otherwise." For Galileo, "being used to study in the book of nature, where things are written in only one way," it had become a point of intellectual

[34] Quoted in DOC001, 36–37; cf. Galilei 1890–1909, 4: 248.

[35] DOC000, 83–86.

[36] Crombie quotes from Galilei 1890–1909, 5: 102; but he also refers to two passages in Galileo's *Letter to Christina*, in Galilei 1890–1909, 5: 316, 330, corresponding respectively to Finocchiaro 1989, 92–93, 404.

> commitment "not to be able to dispute any problem *ad utranque partem* or to maintain any conclusion not first believed or known to be true."[37]

Finally, it should be clear that Crombie's attribution of demonstrativism occurs in other contexts besides the one where he is also interested in claiming that Galileo rejected the practice of disputation. For example, in Chapter 5, in the context of a discussion of the methodology of the science of motion, the epistemological status of the laws of falling bodies gets some attention. Crombie examines primarily the account found in the *Dialogue* rather than the one in *Two New Sciences*. In the Second Day of the *Dialogue*, in the context of Galileo's critique of some mechanical objections to the earth's rotation, there is a brief discussion of the laws of fall, such as the uniformity of acceleration, the law of squares, the law of odd numbers, and the double-distance rule. Crombie uses this discussion to elaborate further his interpretation, quoting from the archaic language of the first English translation of the *Dialogue*, by Joseph Webbe and never published:

> The discussion went on to Salviati's statement of the relation "that the spaces passed by the movable parting from the quiet have among them selves a double proportion (*proporzione duplicata*) to that which the times have wherein such spaces are measured, or wee may say that the passed spaces are among themselves as the quadrats of the times [are]." Sagredo asked if there was a mathematical demonstration of this statement, to which Salviati replied: "Most pure mathematicall, and not only of this but of many other exceeding faire passions appertayning to naturall motions and to proiects also, all of them found and demonstrated by our friend." In the exchange that followed his demonstration of the law of free fall, Galileo once more insisted that by solving specific physical problems mathematics created not only a new and true natural philosophy, but the only available one.[38]

24.5 Criticism

Critical vs. A-critical Disputation What are we to make of these interpretive theses in Crombie's manuscript? Let us begin with Galileo's alleged rejection of disputation *ad utranque partem*. The key difficulty here is Crombie's failure to distinguish a-critical from critical disputation on both sides. By a-critical disputation I mean a presentation of the arguments for and against a given disputed thesis in a controversy, *without* an evaluation or assessment of the strength of such arguments. On the other hand, critical disputation means a presentation of the arguments on both sides, *together with* an evaluation of them; this would yield a judgment or conclusion about whether both sides are equally strong, or whether one thesis is stronger or more likely to be true, and if so how much stronger or more likely.

In the above quotations from Crombie, this distinction corresponds to Campanella's talk of a discussion *ad utranque partem* where one does "not take

[37] DOC002, 225–226; Crombie's quotation from Campanella's letter is from Galilei 1890–1909, 14: 415; his quotations from Galileo are, respectively, from Galilei 1890–1909, 5: 102 and 4: 248.

[38] DOC005, 9–10. Crombie's quotation from Webbe's translation of the *Dialogue* (Galilei 1635) corresponds to passages in Galilei (1890–1909, 7: 248; 2001, 257).

a decision," which obviously implies that sometimes in such discussions one makes, or can make, or can try to make, a decision about which side is true or more probable. Regarding Galileo's self-description apropos of the floating-bodies controversy, it should be obvious that he is referring to a-critical disputation when he states that he is unwilling and unable to "dispute any problem *ad utranque partem*." This should be obvious based on two categories of evidence, one pertaining to Galileo's scientific practice and the other to his methodological self-reflections.

For example, consider his *Dialogue on the Two Chief World Systems, Ptolemaic and Copernican*. In a sense, this book represents Galileo's synthesis of astronomy, physics, and methodology. In another sense, the book is a confirmation of the Copernican theory of the earth's motion—a strong and important confirmation, but neither a conclusive proof nor a necessary demonstration. From a still different point of view, the book is critical examination of all (non-theological) arguments for and against the earth's motion. And this is reflected in the book's structure, both in its surface structure and its deep structure: the surface structure is that of a dialogue among a Copernican (Salviati), an Aristotelian (Simplicio), and an intelligent layperson (Sagredo) willing to listen to, and engage, both sides; and the deep structure consists of the presentation, analysis, and evaluation of the scientific and philosophical arguments for and against the earth's motion, concluding that the pro-Copernican arguments are much stronger than the anti-Copernican ones.[39] In other words, to use the language of Crombie's quotations and interpretation, Galileo's major work is a critical disputation *ad utranque partem*. Or to use the language that I prefer, this work is an exercise in critical reasoning about the earth's motion, or more simply an exercise in argumentation, since the notion of argumentation includes not only the supporting of a conclusion with reasons, but also the evaluation of the reasons for and against one's conclusion.[40]

Furthermore, and importantly, Galileo was fully aware of what he was doing; that is, he had crystal clear in his mind the distinction between a-critical and critical disputation, which Crombie seems to ignore. The best evidence of Galileo's awareness comes from a methodological reflection at the beginning the Second Day of the *Dialogue*. To be more precise the passage in question occurs after Salviati has given a statement of the arguments in favor of the earth's daily axial rotation, and Simplicio (with Salviati's assistance) has given a statement of the arguments against it. Soon thereafter, Sagredo expresses joy and optimism that with such interlocutors and such arguments on both sides he will be able to arrive at the truth. However, Simplicio expresses pessimism and despair because his state is now one of confusion and of not knowing what to believe. There follows a Socratic dialogue in which through a series of questions Sagredo gradually convinces Simplicio that the search for truth is essentially dependent on an examination of the arguments and evidence

[39] For the details of this account, see Finocchiaro 1980, 1997, 2014.

[40] For more details on this conception of argumentation, see Finocchiaro 2005a, 292–326; 2013, 42–64.

on both sides, but that the key is the willingness and ability to evaluate arguments to determine whether or not they are correct and how correct or strong they are.[41]

Computational vs. Philosophical Astronomy Let us now go on to the demonstrativism or necessitarianism which Crombie attributes to Galileo. That is, Crombie makes it sound as if Galileo's position were the following: the aim of physics or natural science is to discover necessary truths about the universe, namely actual states of affairs "that could not be otherwise"; and the means to accomplish this aim are necessary demonstrations and mathematical proofs. Now, it is certainly true that Galileo valued and practiced necessary demonstrations and mathematical proofs, and that the just-quoted phrase appears in the referred passage of the *Sunspots* book. However, such a position is absurd and untenable, for at least three reasons: (1) truths about the physical universe are contingent rather than necessary; (2) mathematical proofs do produce necessary truth, but about abstract entities, not about the concrete ones existing in nature; and (3) necessary demonstrations are arguments such that if their premises are true then their conclusions must be true, and so the truth of their conclusions is not necessary but depends on the truth of the premises.

Indeed the position is so absurd that Crombie may be accused of violating the principle of charity in attributing it to Galileo; and the phrase quoted from the *Sunspots* book should have been regarded as a slip of the pen. This is especially true since the three points just made were known to, and elaborated by, Galileo in many places.

For example, the phrase just quoted occurs in the *Sunspots* book in a passage where Galileo is mentioning the distinction between pure computational astronomy and philosophical realistic astronomy. However, this distinction is elaborated at greater length and more explicitly in a famous passage in the Third Day of the *Dialogue*, where there is no trace of necessitarianism.[42] There, Galileo explains that pure or computational astronomy aims merely at saving the appearances, that is, to give the reasons for celestial phenomena in terms of devices from which these phenomena can be derived, without worrying about the physical reality of these devices. On the other hand, philosophical astronomy aims at describing and understanding physical reality, that is, to give the reasons for celestial appearances in terms of devices that can actually exist. He mentions Ptolemaic astronomy as an example of computational astronomy, because of devices such as epicycles and eccentrics. And he mentions Copernican astronomy as an example of philosophical astronomy, because its primary explanatory hypothesis is the earth's motion, which is taken as actually happening, although not necessarily so. Here, Galileo is distinguishing between an instrumentalist and a realist approach to astronomy, and making a plea for the latter, not for physical necessity.

Mathematics vs. Physics With regard to the role of mathematical proofs and their relationship to physical investigation and argumentation, the key discussion is the one in the Second Day of the *Dialogue*, in the middle of Galileo's criticism

[41] Galilei 1890–1909, 7: 155–157. For a more elaborate account of this passage, see Finocchiaro 1997, 142–155, 339–341; 2014, 259–264.

[42] Galilei 1890–1909, 7: 368–372; 2001, 396–400.

of the anti-Copernican argument based on the extruding power of whirling.[43] Part of his multi-faceted criticism argues that the extrusion objection is quantitatively invalid because of its neglect of some mathematical facts, and then Galileo (through Sagredo's mouth) ends this mathematical criticism by saying that "to want to treat physical questions without geometry is to attempt to do the impossible."[44] And this is the prelude to a memorable discussion of the relationship between physics and mathematics and the role of mathematical proof in natural science.

To make a long story short,[45] Galileo's key points are the following. First, mathematical truths are about abstract entities in the sense that they are statements about the necessary consequences of certain definitions and axioms. For example, consider the proposition that a sphere touches a plane at a single point; this claim is about abstract spheres and planes in the sense that it is a necessary consequence of the definition of a sphere and the axiom that a straight line is the shortest distance between two points. Second, mathematical truths are also about physical reality, although only conditionally; that is, a mathematical proposition is physically true if and only if the abstract entities to which it refers happen to exist as material entities in physical reality. For example, the proposition about spheres and planes is physically true in the sense that if there happen to be material spheres and planes then they touch in only one point. Third, mathematical truths are applicable to physical reality because and insofar as material entities instantiate or approach or approximate abstract ones, for when material entities do not approximate one type of abstract entity they may approximate another type. For example, if and to the extent that material spheres touch in more than one point, they instantiate abstract spheres and planes that are imperfect, and for these it is equally true in mathematics that they touch in more than one point. Finally, the real challenge is to find the proper type of abstract entity in terms of which to interpret physical entities and processes; although it is likely that the latter correspond to some type of abstract entity that can be treated mathematically, one cannot be sure of which one; for example, this may be a difficulty with the relevance of Galileo's own earlier mathematical criticism of the extrusion objection.

Necessary Demonstration vs. Sensory Observation Let us now examine the part of Crombie's interpretation which explicitly mentions necessary demonstration, and which seems to conflate necessary demonstrations and conclusive arguments (by ignoring the truth of the premises). Again, there happens to be striking evidence that Galileo did not share this confusion, but rather clearly understood and practiced the distinction. The evidence is found in a methodological reflection in the Third Day of *Two New Sciences*, after Galileo has given demonstrations of the most basic laws of falling bodies (the mean-speed theorem, law of squares, and law of odd numbers), based on the definition of uniform acceleration (as acquired velocity being proportional to time elapsed). The passage is memorable enough to deserve quotation:

> *Simp*: In truth, I find more pleasure in this simple and clear argument of Sagredo than in the Author's demonstration, which to me appears rather obscure; thus, I am

[43] Galilei 1890–1909, 7: 229–237; 1997, 193–202; 2001, 236–244.

[44] Galilei 1997, 193. Cf. Galilei 1890–1909, 7: 229; 2001, 236.

[45] For elaborations and details, see Finocchiaro 2010, 97–120; 2014, 274–280.

> convinced that matters are as described, once having accepted the definition of uniformly accelerated motion. But as to whether this acceleration is that which nature employs in the case of falling bodies, I am still doubtful. So it seems to me, not only for my own sake but also for all those who think as I do, that this would be the proper moment to introduce one of those experiments—and there are many of them, I understand—which correspond in several ways to the conclusions demonstrated.
>
> *Salv*: The request which you make, like a true scientist,[46] is a very reasonable one. For this is the custom—and properly so—in those sciences where mathematical demonstrations are applied to natural phenomena; this is seen in the case of perspective, astronomy, mechanics, music, and others, which by sense experience confirm the principles that become the foundations of the entire superstructure. I hope therefore it will not appear to be a waste of time if we discuss at considerable length this first and most fundamental question upon which hinge numerous consequences; of these we have in this book only a small number, placed there by the Author, who has done so much to open a pathway hitherto closed to minds of a speculative turn. As far as experiments go, they have not been neglected by the Author; and often, in his company, I have myself performed the tests to ascertain that the acceleration of naturally falling bodies is that above described.[47]

It should be clear that this criticism of Crombie's necessitarian thesis is not trying to fault him for completely ignoring the role of sense experience in Galileo's methodology. In fact, Crombie is aware of it, as suggested even by his incidental mention in the above quotation where he contrasts Galileo with Campanella. And more importantly, Crombie discusses the topic at length in Chapter 7, Section (ii), dealing with experimentation.[48] However, his general approach seems to be that experimentation, necessary demonstration, mathematical proof, etc. are separate methods for arriving at the truth in natural science; and such separation is problematic, to say the least. For, although these methods are distinct, they should not be separated: as Galileo understood, and as I have argued, necessary demonstration by itself cannot tell us what is true in the physical world, but only what follows from what, or how various truths are inter-related; and mathematical proof by itself cannot yield physical truths or truths about concrete entities, but only truths about abstract entities. And I would add, although I have not argued it here, sense experience or observation by itself cannot produce scientific truths either, since the latter require some degree of systematization, which can only be provided by such things as speculation, argumentation, necessary demonstration, and mathematical proof; whereas experimentation is a step in the direction of combining all these things.

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[46] Here the Italian text does indeed read *scienziato*.

[47] Galilei 2008, 149–150; cf. Galilei 1890–1909, 8: 212.

[48] DOC005, 20–48.

24.6 Mis-Understanding Argumentation

Besides criticizing Crombie for his failure to inter-relate such distinct elements of Galileo's scientific methodology, I have also argued that Crombie fails to appreciate a particular element (argumentation) which is especially important. Argumentation is important partly because it can combine many of the other methodological elements, and partly because of its inherent importance, as Galileo's own scientific practice and methodological reflections abundantly show. Thus, to conclude this essay, it will be instructive to discuss a particular example of Galilean argumentation which is especially rich and important, as well as egregiously misunderstood by Crombie.

The example is Galileo's tidal argument, which Crombie discusses on two occasions: first, in the part of Chapter 5 that examines the Copernican debate in 1610–1616, since Galileo first wrote down that argument in January 1616; and second, in the part of Chapter 7 dealing with Galileo's methodology as found in his mature works of 1632–1639, since an expanded version of that argument is included in the *Dialogue* of 1632.[49]

The tidal argument is an attempt to support the Copernican theory of the earth's motion based on the ability of the geokinetic hypothesis to explain why the tides occur and some other relatively well-known facts about them.[50] Galileo was clear that this was an explanatory argument, and as such the argument as a whole was neither a necessary demonstration nor a mathematical proof. However, he thought the argument was very good, indeed the strongest of all the pro-Copernican arguments. This judgment was based on his belief that there was no way to explain the existence of the tides in a geostatic framework, whereas for all other pro-Copernican arguments with an explanatory structure, he could always think of some alternative geostatic explanation, which however was more complex and so could be rejected based on the principle of simplicity.

Of course, we now know, after Isaac Newton, that there is a better explanation of the tides based on universal gravitation; this enables us to reject Galileo's tidal argument on the grounds that it presupposes a false premise. On the other hand, this criticism does not affect the plausibility of his reasoning, which must be judged on its own and in other ways.

Moreover, many scholars, including Crombie, also claim that Galileo's tidal argument was intended to be conclusive, necessary, and demonstrative, rather than probable and plausible. This enables them to criticize the argument in many other ways, whenever what he does or says is not completely correct, but merely plausible. However, such a deductivist interpretation of the tidal argument is textually untenable and a violation of the principle of charity.

With these logical and historiographical clarifications in mind, let us now go back to Crombie's manuscript, specifically his discussion of the tidal argument as presented in the Fourth Day of the *Dialogue*. Let us recall that this version of the

[49] Respectively, DOC002, 177–186; and DOC005, 54–66.

[50] For more details on my account sketched here and related issues, see Finocchiaro 1980, 74–79, 138–141; 1997, 282–308; 2010, 58–60; 2014, 215–240; 2019, 88–90, 144–149.

argument has two main parts, the first which explains the diurnal period of the tides, the second which explains the monthly and annual periods. Between these two main parts, Galileo discusses a possible objection to the explanation of the diurnal period. And it is here that we come to one of Crombie's most pertinent comments:

> Salviati's opening exposition was interrupted by Simplicio's suggestion that the argument was circular: "I do not perceive it can be denyed but that your discourse proceedeth arguing very probably, as wee say *ex suppositione*, that is, it being supposed that the Earth moveth by those two motions attributed to it by Copernicus; but if those motions be excluded all is vain and forcelesse; and then the exclusion of such hypotheses is by the same discourse of yours very manifestly show'd unto us. You with the supposition of the two terrestriall movings give a reason of the flux and reflux; and on the other side, discoursing circularly from the flux and reflux, you draw the signe and confirmation of the selfe moving …". Salviati ignored the logical point and continued with his heroic analysis … of the additions and subtractions that had to be made to the cause of the daily tides in order to account for the monthly and annual periods.[51]

Here, Crombie's logical and textual difficulties are as follows. It is not true that Simplicio is charging Salviati with having advanced a circular argument, which certainly would have been fallacious and invalid. Rather, the "circular" aspect of Salviati's argument is really a bi-directional feature, which Simplicio finds harmless, and goes on to exploit by himself engaging in the same manner of reasoning in order to arrive at a conclusion denying the earth's motion. Moreover, it is not true that Salviati ignores Simplicio's logical point. Rather, Salviati immediately goes on to refute Simplicio's logical objection, in a passage completely ignored by Crombie. Thirdly, part of the problem is Crombie's truncation of Simplicio's objection, which should have been quoted in full, since the missing part is about the same length as the part Crombie quotes.

When we study Simplicio's objection and Salviati's reply, we get the following.[52] Simplicio's objection is a kind of internal criticism. That is, he argues that, like water, air is a fluid and does not have to follow in every way all the motions which a moving earth would try to impart to it; thus if, as Salviati argues, the earth's motion causes tidal-like motions in the watery part of the terrestrial globe, then it would also cause similar motions in the earth's atmosphere; for example, in the case of air, one effect would be a constant wind from the east; but no such wind exists; it follows that the earth's motion is not the cause of the tides and does not exist.

Salviati replies that there are two things wrong with this objection. One is that although there are some similarities between water and air, there is one crucial difference: water can conserve the motion it acquires relatively easily, whereas air cannot do it as easily. This property of water is instrumental in the mechanism whereby the earth's two motions make seawater accelerate and decelerate, which in turn produces the tides. Hence, the geokinetic mechanism that produces the tides would not have to produce similar motions in the earth's atmosphere. This reply refutes one of the key premises of Simplicio's objection.

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[51] DOC005, 59–62. Again, Crombie quotes from Webbe's translation (Galilei 1635). Cf. Galilei 1890–1909, 7: 462; 2001, 406.

[52] See Finocchiaro 1980, 139; 2014, 227–232.

Additionally, Salviati also has a reply to the other premise regarding the lack of a constant wind from the east. He points out that this claim is false: as a matter of fact, there are prevailing winds from the east, especially in the equatorial regions. These are the so-called trade winds, which sailors know about, and which they exploit in the art of navigation.

Besides contributing to undermining Simplicio's internal criticism of the tidal argument, the existence of the trade winds then enables Galileo to formulate a new argument in favor of the earth's motion. This is an explanatory argument based on the fact that there are trade winds; on the geokinetic explanation of this fact; and on there being no other way of explaining this fact.

24.7 Conclusion

In this essay, I began by describing the unpublished book typescript by A.C. Crombie on *Galileo's Natural Philosophy*, dating from the late 1960s and early 1970s. There is no denying that the book looks impressive. For it deals with Galileo's philosophy of science as well as his philosophy of nature. It also discusses the natural philosophy of Marin Mersenne, with the goal of eventually elaborating (in a future work) the beginnings of the science of music. It promises to include (in the completed work) a lengthy Appendix with documents and original sources. And the available manuscript runs to about 2200 pages, about half of which are single-spaced and consist of lengthy quotations from Latin and Italian translated into English.

However, despite such superficial impression, Crombie's manuscript turns out to be a work of ill-digested syncretism. There are many clues leading to this judgment. For example, Crombie devotes about as much attention to Galileo's works as to the works of his predecessors and contemporaries (not counting the part of Mersenne). In the discussion of Galileo's own works, Crombie devotes about as much attention to his early and often unpublished writings as to his mature works. Regarding topics covered, he spends about as much time on secondary topics (such as the nature of matter, of light, of heat, and of sensation) as on primary ones (such as the epoch-making confirmation of Copernicanism and discovery of the laws of falling bodies). And regarding quotations, as the comment in the previous paragraph suggests, Crombie's manuscript consists for at least half and perhaps more of quotations as distinct from interpretation, evaluation, and analysis.

On the other hand, I had no hesitation in also crediting Crombie's manuscript for calling attention to several events and documents that are sufficiently interesting and important to deserve more investigation in the future, thus suggesting a number of promising research projects. One is the first and very long letter sent to Galileo in January 1611 by Tommaso Campanella, commenting on *The Sidereal Messenger* published nine months earlier. Another is the correspondence in 1633–1635 between Galileo and the French engineer Antoine De Ville, employed by the Republic of Venice; this is important in light of his reaction to Galileo's *Dialogue* and of his comments to the manuscript of the *Two New Sciences*. Third, there is Benedetto

Castelli's letter to Galileo of August 15, 1637, discussing the reflection and transmission of heat and light by a brick one side of which is painted half black and half white; I did not hesitate to label Castelli's letter a philosophical gem, containing brilliant discussions and illustrations of the Socratic method, logical analysis, the method of resolution, and the method of composition. A fourth item was Thomas Hobbes's comment on the Catholic Church's banning of Galileo's *Dialogue*, that it reflected an "opposition … between their religion and naturall reason"; and this judgment is demonstrably both historically important and philosophically viable. Finally, we had the 1635 correspondence between Mersenne and Nicholas Claude Fabri de Peiresc, regarding Mersenne's criticism of the value mentioned by Galileo for the acceleration of falling bodies; Peiresc tried to convince Mersenne to tone down and formulate constructively his critique.

In the main part of my essay, I discussed a more central topic. First, I tried to identify some key interpretive theses which Crombie advanced regarding Galileo's scientific methodology. Then I criticized Crombie's account.

According to Crombie, Galileo rejected the practice of disputation involving arguments on both sides of an issue. Crombie also attributes to Galileo a necessitarianism according to which the truths of nature are not merely contingently true but necessarily true, and necessary demonstrations are the means to establish them. And Crombie also attributes to Galileo a mathematicism according to which mathematical proofs are the norm and the ideal in natural science.

In my criticism, I argued that Crombie's interpretive theses do violence to Galileo's scientific practice and to his methodological reflections. Crombie fails to distinguish critical disputation from a-critical disputation, and fails to understand that Galileo rejected only a-critical disputation, but valued and practiced critical disputation. With regard to necessary truth in nature, Crombie uses uncharitably, injudiciously, and out of context a sentence from the *Sunspots* book, without realizing that Galileo is primarily making a distinction between computational and philosophical astronomy, and that for him the latter is realistic, not necessary. With regard to the role of mathematical proofs in physical science, Crombie ignores Galileo's nuanced elaboration of the distinction between abstract and concrete entities and the corresponding distinction between mathematics and physics. And regarding the role of necessary demonstration, Crombie overlooks Galileo's clear awareness that in natural science necessary demonstrations are limited to the relationship among physical propositions (for example, between premises and conclusion of an argument), and that sense experience is required to determine the truth of individual propositions.

Finally, I discussed the particular example of Galileo's tidal argument to illustrate the carelessness and untenability of Crombie's analysis. This contrasts to the refined sophistication and the clever simplicity of Galileo's discussion, which includes a presentation of the argument in favor of the earth's motion based on the tides, an analysis of its explanatory structure, an objection by a critic who exploits such a structure to try to refute it, and a reply to this criticism.

It is to be hoped that my criticism of Crombie's key interpretive theses will be found to be not merely negative and destructive, but also to have a positive and

constructive dimension. This dimension should be the appreciation of the nature and importance of argumentation, both Galileo's own and argumentation in general.

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