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Galileo's Logic of Discovery and Proof

The Background, Content, and Use of His Appropriated Treatises on Aristotle's *Posterior Analytics*

William A. Wallace

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PREFACE

This volume is presented as a companion study to my translation of Galileo's MS 27, *Galileo's Logical Treatises*, which contains Galileo's appropriated questions on Aristotle's *Posterior Analytics* – a work only recently transcribed from the Latin autograph. Its purpose is to acquaint an English-reading audience with the teaching in those treatises. This is basically a sixteenth-century logic of discovery and of proof about which little is known in the present day, yet one that arguably guided the most significant research program of the seventeenth century. Despite its historical and systematic importance, the teaching is difficult to explain to the modern reader. Part of the problem stems from the fragmentary nature of the manuscript in which it is preserved, part from the contents of the teaching itself, which requires a considerable propadeutic for its comprehension. A word of explanation is thus required to set out the structure of the volume and to detail the editorial decisions that underlie its organization.

Two major manuscript studies have advanced the cause of scholarship on Galileo within the past two decades. The first relates to Galileo's experimental activity at Padua prior to his discoveries with the telescope that led to the publication of his *Sidereus nuncius* in 1610. Much of this activity has been uncovered by Stillman Drake in analyses of manuscript fragments associated with the composition of Galileo's *Two New Sciences*, fragments now bound in a codex identified as MS 72 in the collection of Galileiana at the Biblioteca Nazionale Centrale in Florence. On the basis of these fragments Drake and subsequent investigators have argued convincingly that Galileo was not the Neoplatonist or Neopythagorean he has sometimes been seen, but was strongly empiricist in his thought, having already embarked on a systematic program of experimental research in the first decade of the seventeenth century that would eventually lead him to his "new science" of local motion, not published until 1638.

The second study relates to three earlier manuscripts, now generally agreed to have been written by Galileo while teaching, or preparing to teach, at the University of Pisa toward the end of the sixteenth century, in the years roughly between 1589 and 1591. The first of these, MS 27, contains the logical questions translated in *Galileo's Logical Treatises*; the second, MS 46, contains what have been referred to as "Physical Questions" based on Aristotle's *De caelo* and *De generatione*; and the third, MS 71, contains Galileo's earliest attempts to construct a science of local motion, usually labeled his *De motu antiquiora* to distinguish it from the mature science of motion published by him in 1638. The new discovery relating to these manuscripts has to do with their provenance and dating. Contrary to previous expectations, the first two manuscripts, MSS 27 and 46, have been shown to derive from lecture notes of Jesuits teaching at their university in Rome, the Collegio Romano, and the third, MS 71, to contain materials on motion that are in essential continuity with the contents of the first two manuscripts. The lecture notes on which the first two manuscripts are based can be dated, and thus, on the basis of the established derivation, they can serve to fix the earliest dates at which they could have been written by Galileo.

MS 27 is quite unusual in a very important respect. Despite the fact that it had been preserved along with Galileo's other manuscripts after his death, it was omitted from the National Edition of Galileo's works by its editor, Antonio Favaro. Thus it was not transcribed along with Galileo's other manuscripts, as were MSS 46 and 71, when the National Edition was being prepared in the years 1890–1909. This is somewhat strange considering the fact that the manuscript contains, in Galileo's own hand, a commentary on the *Posterior Analytics* of Aristotle – a work regarded at the outset of Galileo's career as the standard exposition of scientific methodology. The reason the manuscript was not transcribed and included in the National Edition was that its editor, Favaro, thought it was written while Galileo was a youth studying at the Monastery of Vallombrosa, before beginning serious studies at the University of Pisa. Favaro also thought it was copied from lecture notes of one of the monks at Vallombrosa, and thus was little more than a "trite scholastic exercise," the work of Galileo's hand but not of his head.

This appraisal of MS 27 has been challenged in my *Galileo and His Sources* (Princeton 1984), where I present extensive evidence to show that the "Logical Questions," like the "Physical Questions," were not juvenile exercises but rather were composed by Galileo in conjunction with his first teaching position at the University of Pisa (1589–1591). Rather than being records of a monk's teaching, moreover, I have been able to show, from a word-by-word comparison of the relevant texts, that they were derived from notes of philosophy lectures given at the Collegio Romano only a year or two previous to Galileo's writing. The "Logical Questions," in particular, were based on the logic course offered at the Collegio by the Italian Jesuit, Paulus Vallius, during the academic year 1587–1588. The manuscript of Vallius's lectures is no longer extant, but fortunately its contents have been preserved in two versions: one a plagiarized text, published by Ludovico Carbone at Venice in 1597 as *Additamenta* to the logic textbook of Francisco Toledo, a Jesuit professor who preceded Vallius at the Collegio; the other a more fully developed exposition, published in 1622 by Vallius himself at Lyons, in two folio volumes of over 700 pages each. A careful comparison of Galileo's manuscript with Carbone's plagiarized version (which here and elsewhere I attribute to Vallius-Carbone in view of the composite authorship) shows that Galileo's text reduced the content of Vallius's teaching by about 40 %, rearranging and abbreviating the material in so doing, without appreciably altering its conclusions. Thus the "Logical Questions," along with the "Physical Questions," offer *prima facie* of Galileo's activity as an aspiring young professor, appropriating from the lecture notes of another professor materials he would later put to use – either in his own teaching or in the scientific investigations he was about to pursue.

These discoveries relating to MS 27 give an unprecedented insight into Galileo's early intellectual formation, and particularly into the ways in which medieval and scholastic Aristotelianism, brought to the apex of its development in the Renaissance, contributed to the rise of modern science. The discoveries are all the more surprising in view of Galileo's attacks on Aristotle and the Peripatetics in his later writings, which might lead one to believe that only by his rejecting the traditional teachings of the schools could he make progress in uncovering the secrets of nature. Indeed, before the work of Paul Oskar Kristeller, Charles B. Schmitt, and Charles H. Lohr in opening up the riches of Renaissance Aristotelianism, Galileo's evaluations of Aristotle could easily be taken on face value by historians and philosophers of early modern science. The recently published *Cambridge History of Renaissance Philosophy* should help set the record straight on that score, but time will be needed for its contents to be assimilated within the intellectual community, and until it is, a general depreciation of Aristotelian science – and the purported lack of continuity between it and that of the early modern period – may continue to prevail.

In light of this situation it must be stressed that when Galileo was beginning his studies at the University of Pisa the Aristotelian corpus was not only the mainstay of academic learning but also the accepted repository of encyclopedic knowledge for laypeople as well. At that time Aristotle's collected works were available in numerous Greek editions; a new wave of translations into Latin, decidedly improved over medieval and humanist versions, had recently come on the scene; and some individual works had already appeared in the vernacular. Commentaries on the text were readily available in print: those of the great medieval commentators and expositors, especially Averroes and Aquinas; editions of Greek commentators such as Alexander of Aphrodisias, Themistius, Simplicius, and Philoponus; and new sixteenth-century commentaries, then coming in a steady stream from Italy, Spain, and Germany. Many of these, particularly the latter, contained questions interpolated into the text. Other expositions of Aristotelian teachings took the form of "questionaries," that is, independent series of questions that were aimed at resolving problems in Aristotle's text or at going beyond his teachings to incorporate discoveries from the intervening centuries. And then there were the monographs and manuals, textbooks in the modern sense, that made all this available to students at various educational levels. Clearly there was no lack of sources from which Galileo might have drawn for his knowledge of Aristotle.

The University of Pisa, like most universities of the period, exhibited in its faculty a variety of approaches to the Aristotelian corpus. During the period Galileo studied or taught there, for example, the better known professors of natural philosophy included Girolamo Borro, Francesco Buonamici, Andreas Cesalpino, and Jacopo Mazzoni. Borro was pronouncedly Averroist and Neoplatonist in his teaching; Buonamici urged a return to the Greek text and a redevelopment of Aristotle's thought on its basis; Cesalpino gave a reading that was naturalistic and more progressively scientific; and Mazzoni, in eclectic fashion, sought to reconcile Aristotle's basic ideas with those of his teacher, Plato. They thus represented an entire spectrum of views toward the *Physics* and its accompanying works. At the time the professors of logic, on the other hand, were less distinguished. Giovanni Talentoni, Orazio Mainetti, Ippolito Sestini, and Domenico Silvani lectured most frequently, but unlike their counterparts in natural philosophy they published nothing and it is difficult to determine precisely what they lectured about. The *rotulus* of professors at Pisa shows that each year from 1581 to 1591 the texts to be covered were the *Posterior Analytics* of Aristotle and the

Introductiones (i.e., the *Isagoge*) of Porphyry, but perforce the record is silent on how the different professors carried out that task.

In any event, from the circumstances of the composition of MS 27 it seems reasonable to assume that the instruction Galileo received in logic at Pisa was far from satisfactory, and thus that his appropriation of materials from Vallius's logic course was in part, at least, intended to fill a lacuna in his earlier education. The situation with regard to MSS 46 and 71 is somewhat similar, though it is more complex, as I explain in my introduction to *Galileo's Logical Treatises*. Suffice it to mention that in my earlier translation of MS 46, entitled *Galileo's Early Notebooks: The Physical Questions* (Notre Dame 1977), I have shown that these do not derive from Buonamici's lectures, as Favaro thought, but are likewise of Jesuit origin. Yet the translation of MS 27 has presented problems of an order of magnitude different from those faced in my translation of MS 46 or those of Drake and Drabkin in their translation of MS 71. The main problem arises from the technical terminology of MS 27, which presupposes much more for its understanding than is needed for grasping the materials in MSS 46 and 71. MS 46 is concerned mainly with the heavens and the elements, and MS 71 with local motion, topics about which the reader of general education should be reasonably well informed. MS 27, on the other hand, contains materials from a portion of a year-long course on Aristotelian logic, the part devoted to the first book of the *Posterior Analytics* and covered in about a month and a half toward the end of the course. Most of the instruction in logic, in fact, was devoted to building up the knowledge required for appreciating the matters treated in that book. Moreover, the concepts of logic elaborated in the course previous to the part Galileo appropriated and the concepts of science treated in the part following it are very different from those taught in the present day, and yet they are crucial for understanding Galileo's logical and scientific methodology.

The foregoing considerations dictate the general structure of this volume. The title, *Galileo's Logic of Discovery and Proof*, makes a twofold claim, first, that Galileo had a logic, and second, that this was not merely a logic of justification but that it was also a logic of invention, one that would prompt his later scientific discoveries. The warrant for the title is simply Galileo's statement in his letter to Fortunio Liceti, written only sixteen months before his death, to the effect that in his logic he has been an Aristotelian all his life. Now MS 27 is the only unambiguous exposition, in Galileo's own hand, of a logic to which that statement could possibly apply. Again, Aristotelian teaching on demonstration, when coupled with that on the demonstrative *regressus*, provides a clue to the reasoning processes Galileo employed throughout his life, particularly in his studies of astronomy and local motion. Other influences are of course detectable in Galileo's thought, but these are not the concern of this volume. Rather it purports to show that Galileo's methodology was already spelled out in the treatises he appropriated from the Collegio Romano, and that by and large these treatises are sufficient for grasping what he meant in his last testimonial to Liceti.

To implement this objective the first chapter addresses the questions of whether and how Galileo can be said to have had a logical methodology. It sets the stage for what follows by explaining what was meant by methodology or *methodus* in his time, how previous investigators have attributed a methodology to him, and how in a general way Galileo himself saw the teachings of the *Posterior Analytics* to be relevant to the sciences he set out to elaborate, first on the basis of his discoveries with the telescope and later on the basis of his experiments with bodies in motion.

The remainder of the volume is divided into parts entitled *Logica Docens* and *Logica Utens* respectively. The burden of *Logica Docens* ("Logic Doctrine" or "Logic Teaching") is twofold: first, to provide a summary of the materials Galileo did not appropriate from the logic course from which he worked but are necessary for understanding those he did; and second, to present a more systematic ordering of the materials he appropriated than is found in his exemplar. The first goal I aim for in the following two chapters, one outlining the understanding of logic implicit in MS 27 (Chap. 2), the other explaining the concepts of science and opinion it presupposes throughout (Chap. 3). But here the problem arose of how to present all this material, which turns out to be very extensive, namely, merely to summarize it or actually to translate large portions of the Latin text Galileo had in hand but never did copy. In the interests of accuracy I finally decided for the second alternative, though this involved me in considerably more translating than I had envisaged for the project. The second objective, a systematic presentation of the teaching on demonstration contained in MS 27, was more readily achieved, and this is covered in Chap. 4. It follows mainly the order of Galileo's presentation, treating the requirements of foreknowledge in relation to principles, suppositions, subjects, etc.; the nature and kinds of demonstration; the demands that apodictic reasoning make on the premises it uses; and the problem of circularity in reasoning and how this can be circumvented through use of the demonstrative regress. I also interpose brief sections on causality and induction so as to clarify how Galileo's understanding of these topics differs from that of the present day.

The next part, entitled *Logica Utens* ("Logic in Use" or "Logic Applied"), then addresses the issue touched on in the opening chapter, namely, whether the teaching contained in MS 27 was employed by Galileo in his scientific work or was not and so is only of antiquarian interest. Here I advance the case in support of the first alternative. I first stated that case in *Galileo and His Sources*, where I traced chronologically the use of key concepts in MS 27 throughout Galileo's writings, from his earliest notebooks to the last letters he wrote. Now, on the basis of a lesson learned from that effort, namely, that many of Galileo's ideas changed little throughout his life, I use a different approach and divide his lifelong work into two areas of investigation. The first is his search for a new science of the heavens, which he personally never brought to successful completion (Chap. 5); the second, his more substantial accomplishments in founding the two new sciences of mechanics and local motion, which laid foundations on which others would later erect a science of celestial mechanics (Chap. 6). Both efforts, I argue, were directed essentially by the logical canons found in MS 27.

For purposes of cross-reference and to facilitate access to the materials in the volume, I have divided its chapters into sections. I designate each of the chapters by a numeral and each section by a numeral also; I then divide the latter into subsections, designated by lower-case letters. The sectioning of the volume can thus be numbered, with the first number being that of the chapter in which it occurs and the remaining designations those of the section and subsection referred to. For example, Sec. 3.2b refers to Chapter 3, Section 2, Subsection b.

Since much of the documentation for the analysis presented herein is contained in the companion volume, *Galileo's Logical Treatises*, references are made throughout the text to that work. In place of the line numbers used in the Latin Edition, however, I have numbered the paragraphs of the English translation successively; these numbers are what I use here for purposes of cross-reference. MS 27 contains two treatises, the first dealing with the foreknowledge required for demonstration (designated F), the second dealing with demonstration itself (designated D). Each treatise contains three disputations, each disputation is divided into questions, and each question is divided into paragraphs. A sequence of numbers can thus be used to designate unambiguously each paragraph in the manuscript. Thus F3.2.4 refers to the treatise on foreknowledge, third disputation, second question, paragraph 4; D2.6.9 to the treatise on demonstration, second disputation, sixth question, paragraph 9. For the benefit of the reader who wishes to consult the Latin Edition, I provide a concordance of the page and line numbers of the latter, the paragraph numbers of the English translation, and the folio and line numbers of the original manuscript at the end of the volume.

The notes to the text, unlike those to the translation, have been kept to a minimum. A large number of these simply reference the sources from which translations or paraphrases of Galileo's source materials have been made. These usually are page or folio citations inserted directly into the text, with an acronym or other abbreviation employed to identify the source from which they are taken. A list of all such abbreviations is given following the Preface. Notes not inserted into the text serve either to identify less frequently cited works, all of which are listed in the bibliography, or to record my occasional, mostly philosophical, animadversions.

In view of the fact that the thesis of the volume is largely dependent on translation, for consistency I have made all translations herein in a style conformable to that of *Galileo's Early Notebooks: The Physical Questions* and *Galileo's Logical Treatises*, either adapting them from existing translations or making them myself from the original text.

I conclude the volume with a brief epilogue reflecting on the utility of recent research on Galileo's Pisan manuscripts for a revisionist history of his scientific contributions, and eventually for a reappraisal of elements of continuity and discontinuity in the so-called "Scientific Revolution" of the seventeenth century.

I should like to thank the National Endowment for the Humanities, an independent agency of the U.S. government, for the financial support that made this endeavor possible. I also wish to acknowledge the invaluable assistance of William F. Edwards, whose painstaking and accurate transcription of MS Gal. 27 was the basis for the Latin Edition. Other scholars to whom I am indebted include the late Charles B. Schmitt for encouraging Edwards and me to begin the original project, though he did not live to see its completion; the faculty of the Centro per la Storia della Tradizione Aristotelica nel Veneto of the University of Padua, especially Ezio Riondato, Enrico Berti, Antonino Poppi, and Luigi

Olivieri, for their encouragement and collaboration at all stages of the project; and my friend and colleague at The Catholic University of America, Jean Dietz Moss, who has been a benefactor to me in countless ways throughout the entire enterprise. Finally, I owe special thanks to Professor Robert S. Cohen of Boston University, editor of the Boston Studies in the Philosophy of Science, for publishing the results of my work in his distinguished series.

College Park, Maryland W.A.W.

ABBREVIATIONS

SOURCES: SHORT TITLES

CA	Carbone, *Additamenta*, Venice 1597
CP	Carbone, *Introductio in philosophiam*, Venice 1599
CL	Carbone, *Introductio in logicam*, Venice 1597
CT	Carbone, *Introductio in dialecticam Toleti*, Venice 1588
GG	Galileo, *Le opere di Galileo Galilei*, ed. A. Favaro, 20 vols. in 21, Florence 1890–1909, repr. 1968
GM	Galileo, *Le meccaniche*, ed. S. Drake, *Osiris* 13 (1958), 262–290
LL	Lorinus, *Logica*, Cologne 1620
VL1	Vallius, *Logica*, Vol. 1, Lyons 1622
VL2	Vallius, *Logica*, Vol. 2, Lyons 1622
ZL	Zabarella, *Logica*, Cologne 1597, Frankfurt 1608

VARIA

D3.2.1	*Galileo's Logical Treatises*, Treatise on demonstration, Disputation 3, Question 2, Paragraph 1
F3.3.2	*Galileo's Logical Treatises*, Treatise on Foreknowledge and Foreknowns, Disputation 3, Question 3, Paragraph 2
Lat. Ed.	Galileo, *Tractatio de praecognitionibus et praecognitis and Tractatio de demonstratione*, eds. W.F. Edwards and W.A. Wallace, Padua: Editrice Antenore, 1988
Sec. 4.3d	Section in *Galileo's Logic of Discovery and Proof*, Chap. 4, Sec. 3, Subsection d
Vallius-Carbone	Attribution of dual authorship for lecture notes of Vallius, subsequently plagiarized and published by Carbone

LIST OF SCHEMATIC ANALYSES OF GALILEO'S ARGUMENTS

CHAPTER 1

GALILEO'S LOGICAL METHODOLOGY

The title of this chapter implies that Galileo had a logical methodology and that his use of such methodology helped him in some way to develop the new science he is generally credited with founding as the "Father of Modern Science." Such an implication appears uncontroversial, and yet, as intimated in the preface, it could raise concern among historians and philosophers working in the field of Galileo studies. On this account it seems well to address at the outset such questions as whether Galileo had a logical methodology; if he did, whether he used it in his science; and again, if he did, whether it can be equated in any way with scientific methodology as understood in the present day. This opening chapter proposes to respond to these queries in a general way; its aim is to give an overview of Galileo's views on logic as explained in this volume and thus introduce the reader to the more detailed discussions to be found in the chapters to follow.

1. METHOD, LOGIC, AND SCIENCE

The answers to such questions obviously depend on the meanings given to the terms method, logic, and science. All three terms have a long history, and one may expect that they would have different meanings for a late sixteenth-century thinker such as Galileo and for a reader in the late twentieth century. Yet the historical range of the questions spans considerably more than these five centuries. As noted in the preface, the focus of this study, Galileo's MS 27, is an appropriated commentary on Aristotle's *Posterior Analytics*, a highly influential work written in the fourth century B.C., and thereafter studied and commented on by scholars for over two millenia. Such being the case, one can gain a better sense of the historiographical problem by moving back into history and considering two analogous questions about Aristotle and his science: Did Aristotle have a logical methodology, and, if so, did he actually employ it in the development of his science, for which he is usually credited with being the "Father of Science in the West?"

Within the Aristotelian tradition up to the seventeenth century both these questions would have been answered unambiguously in the affirmative. They are still answered in this way by most Aristotelian scholars, although discordant notes have occasionally been sounded. The problem that arose in the seventeenth century had to do with epistemology generally, and particularly with the possibility of achieving certitude in the study of nature using Aristotle's canons. Concern with this problem led to the wholesale revisions in the Aristotelian concept of science that serve to characterize most modern philosophies. These began to develop in the seventeenth century and continue down to our own: first came rationalism, then empiricism, then idealism, and finally positivism, to mention only the systems most relevant to our concerns. Without entering into historical detail, one may say that partisans of each movement invariably attributed a methodology to Aristotle but disagreed on how it was to be evaluated. Some hesitated to call it a logical methodology on the ground that it was too metaphysical, too enmeshed in ontological suppositions to conform to their views of logical rigor. A greater number hesitated to identify it as a scientific methodology on the ground that it failed to exhibit the concern for experimentation and mathematical reasoning that had become increasingly identified with modern science. However one may wish to interpret these developments in the long run, they make clear that there are definite interrelationships between conceptions of logic, science, and method. Particular views of science condition corresponding views of method, perhaps to an even greater degree than do related views of logic.

It must be emphasized that none of the development from the seventeenth century onward could have had an impact on Galileo, certainly not when he was writing out the contents of MS 27 around 1589, and very probably not in his later years as well. In his day Aristotle's *Posterior Analytics* still set the ideals for science and laid out the logical methods by which it was to be achieved. There is no indication that Galileo had doubts about Aristotle's having had a logical methodology that was guiding the development of his science. What Galileo did doubt was the validity of the results Aristotle claimed as scientific, particularly those relating to the heavens and to local motion. Much of his concern came from empirical evidence that he knew was not available to Aristotle and that he felt would substantially alter those results had they been available. A related concern for him was Aristotle's conception of mathematics as a science and how it could function in providing new information about nature. Both of these concerns are reflected in

Galileo's many statements about Aristotle and method that have attracted the attention of scholars to date. These are most important as clues for ascertaining the degree to which Galileo appropriated an Aristotelian methodology in his early years and then possibly modified it in his later, considerations to be addressed in subsequent chapters of this volume.

It is in connection with such statements that the materials contained in MS 27 assume their greatest importance. Not infrequently, when working on Galileo's citations of Aristotle, historians of science turn to a vernacular translation of the Greek text and attempt to puzzle out the meanings of expressions there relating to demonstration and resolutive method. They do so, understandably enough, for ease of reference, since this offers them simple access to Aristotle's thought. But the fact of the matter is that Galileo was not living in the fourth century B.C., and even a present-day commentary on the Greek text may have little or no relevance to specific points being made by him. Obviously Galileo has to be inserted into the historical continuum at his proper place in time, and, with respect to the many schools of Aristotelian commentary that had developed over the centuries, into the particular school that best enables us to interpret his statements properly. Although much work has already been done in identifying possibilities – and noteworthy here is that of John Herman Randall, Jr.[1] – no one has succeeded in that task to date. Only with the identification of the sources from which MS 27 was appropriated have materials adequate to it become available.

As sketched in the preface, Galileo's understanding of the *Posterior Analytics* must be located within the type of Aristotelianism taught by Jesuits at their university in Rome, the Collegio Romano, in the closing decades of the sixteenth century. This was a scholastic-humanist version based on the Greek text and on Greek commentaries newly recovered in the Renaissance, further informed by Arab commentaries, particularly that of Averroes, and yet partial to the tradition of Latin commentaries, usually those of Thomists, Scotists, and Albertists, less frequently those of nominalists and terminists.[2] The views of method, logic, and science thus taught at the Collegio Romano are of singular importance. They are invaluable no less for unraveling Galileo's sometimes cryptic notations in MS 27 than for understanding his later references to the materials it contains or are presupposed in its contents.

In what immediately follows in this chapter the main emphasis is on method, since special chapters are later devoted to logic and to science. To illustrate how views different from those of Jesuit Aristotelianism have influenced interpretations of Galileo's methodology we first provide an overview of recent studies of that subject and then go into fuller detail on *methodos* and *methodus* as these had earlier developed in the Greek and Latin traditions respectively. Following this we explain the concept of method implicit in MS 27, along with related concepts such as those of order and of resolution and composition. We conclude the chapter with a discussion of the difference between having a logical method and putting it to use in a particular scientific investigation – known among the Latins as the difference between *logica docens* and *logica utens* – which provides the general framework for our subsequent analysis of Galileo's methodology.

2. ASSESSMENTS OF GALILEO'S METHODOLOGY

Pioneer historians of science such as William Whewell and Ernst Mach tended to locate Galileo in the empiricist tradition and saw him essentially as a collector of facts who had eschewed Aristotle's search for causes and instead had sought laws such as that of falling bodies by simple inductive methods.[3] With the rise of logical positivism as the leading philosophy of science in the U.S. this view became popular and Galileo was commonly cited in scientific textbooks as a prime example of empiricist methodology. In the years prior to World War II the methodology seen in his work gradually assumed canonical form in what is now referred to as hypothetico-deductive (HD) method. Because of the importance it has assumed in Galileo studies a few words may be devoted here to its characterization.

a. *HD Method.* In HD method the investigator formulates an hypothesis that is capable of empirical test, and then designs an experimental procedure for verifying or falsifying consequences deducible from that hypothesis; the hypothesis (H) and the deduction (D) following from it explain why it is called HD method. After repeated instances of having confirmed or disconfirmed empirical consequences deduced from the hypothesis, the researcher is in a position to judge on its validity. The more confirming instances he has, the more his hypothesis is verified; the more disconfirming instances, the more it is falsified or seen to be in need of revision. Since the hypothesis itself can be generalized beyond a hitherto unknown fact into a law or a theory, and since consequences can be tested by a broad range of fact-finding techniques, ranging from experiments employing precise measurements to polls and questionnaires, the method is easily extended into all fields of inquiry. Additionally, since its testing procedures are repeatable by other investigators, it has come to be regarded as the most potent way of publicly verifying the knowledge claims of science. In the years following World War II it has generally been accorded the label "scientific method" and has become paradigmatic for all investigative research in the natural sciences, the behavioral sciences, and the social and political sciences as well. Indeed, science itself is now defined by many as justified true belief, the terms justified and true referring to the verification afforded by HD method.

Paralleling the gradual acceptance of HD method as synonymous with scientific research, the history of science was slowly developing as an academic discipline in its own right. Galileo was one of the first subjects to be studied in detail, and historians began to examine his writings to check out popular stories about the Leaning Tower of Pisa and various experiments he was alleged to have performed. The inclined plane experiment described on the Third Day of the *Two New Sciences* at first seemed to support the attribution to him of HD methodology, for Galileo himself affirmed that he was arguing hypothetically and cited that particular experiment as confirmation of what has come to be known as the "times-squared" law. But other of his statements left room for doubt. These, plus the realization that his early experiments from the Leaning Tower, if ever actually performed, could not possibly have established the result attributed to them, namely, the law of uniform acceleration in free fall, led to a wholesale reevaluation of his methodology. This was not now conducted by scientists like Mach with an avocation for the history of their discipline, but by academic historians who were prepared to examine even his manuscripts if need be to uncover the relevant details.

b. *Koyré.* Among the first to question seriously the empiricist account of Galilean method was the famous French historian of science, Alexandre Koyré. Himself a Plato scholar, in 1939 Koyré took up a theme that had earlier been advanced on the basis of Galileo's references to mathematics and characterized him not as an empiricist but as a rationalist. While acknowledging Galileo's acquaintance with Greek thought, he saw the Pisan scientist as less indebted to Aristotle than to Plato and Pythagoras. Koyré was a good textual scholar who wrote with grace and persuasion; his careful examination of the results claimed by Galileo for experiments with pendulums and inclined planes convinced him, and many who read him, that Galileo's science was not empirically based but derived essentially from his own intellectual insights. The experiments mentioned in his writings were performed either poorly or not at all, for the results alleged of them could not be verified, as Koyré clearly pointed out. In his view they were thought experiments, described by Galileo to induce assent by those with less mathematical acumen than he, but not the foundation of his science. It should be mentioned here that Koyré had little appreciation for medieval science, and questioned the efforts of his compatriot, Pierre Duhem, to trace the origins of Galileo's science to scholastic sources such as those mentioned in the manuscripts described in subsequent chapters of this volume.[4] In this respect he was similar to the editor of the National Edition of Galileo's works mentioned in the preface, Antonio Favaro, who likewise rejected Duhem's thesis.[5]

Aided by the circumstance that many historians of science in the U.S. were trained in the years following World War II and became acquainted with, and enthralled by, Koyré's thesis, the post-war years saw a decided shift in methodological evaluations of Galileo. Whereas before the war he was seen as an empiricist, after it he had become a rationalist. A dissenting voice came from Thomas Settle, who began duplicating Galileo's experiments and arguing that they were not thought experiments at all but actual tests that, when properly performed, yielded the results claimed for them. But by and large the rationalists held the day until the early 1970's, when Stillman Drake began the manuscript studies described briefly in the preface and more fully in Sec. 6.4 below. Not only was Drake able to advance Settle's argument, but he uncovered considerable evidence of additional experiments performed by Galileo and yet never reported by him. These experiments have subsequently been duplicated by Drake and others, and at this writing knowledge of them has changed once again the consensus on Galileo's methodology. It is now generally conceded that his science was experimentally based after all, and thus he was not the Platonist or Pythagorean Koyré had made him out to be. Galileo probably was not a positivist either, as he had earlier been seen in the empirical tradition, for by now additional resources were available to characterize him. In the intervening years the history of science movement had produced scholars well versed in medieval, scholastic, and Renaissance thought, and these were able to redirect a more nuanced attention to the Aristotelian tradition as the possible source of Galileo's methodology.

At present, despite the consensus concerning experimental activity, there is little agreement on how to reconcile the conflicting claims for empiricism and rationalism – the first as seen in Galileo's detailed experimentation and the second, in his mathematicist view of nature as a whole – when assessing his overall methodology. To give some idea of the spectrum of opinion on the matter we summarize here representative views from the Blacksburg Workshop of 1975, where historians and philosophers of science convened to reexamine Galileo's methodology in the aftermath of Drake's discoveries. Of the many excellent papers published from the workshop in 1978, we select only those of Winifred Wisan, Robert Butts, Peter Machamer, and Ernan McMullin as the more directly related to the theme of this volume.[6]

c. *Wisan.* Earlier having published a well researched dissertation on Galileo's *De motu locali*, Wisan proposes to give an historical overview of his methodological statements from the beginning to the end of his career.[7] While acknowledging the fragmentary nature and the inconsistency of such statements, she discerns variations in methodology associated with his chronology, which she divides into the following periods: his early writings on motion and cosmography at Pisa (1589–1591) and Padua (1592–1610); his work on hydrostatics and astronomy at Florence (1610–1623); his *Two World Systems* of 1632; and his *Two New Sciences* of 1638. Other variations she sees in the subjects on which Galileo wrote: when treating mechanics his standards and methods are for her different from those he employed in treating astronomy. Generally the former follow the method of deduction from true and evident principles employed by mathematicians, the latter a type of HD method combined with persuasive argumentation that leaves his conclusions somewhat in doubt. Weaving together these variations of time and subject matter, Wisan constructs a picture of Galileo pursuing a somewhat erratic course. He begins with mixed allegiances to Aristotle and Archimedes, but in his early writings on motion and mechanics the latter wins out; in hydrostatics he combines the mathematical model with new empirical methods; in the astronomy of sunspots he moves from rationalism to empiricism to mitigated skepticism; in the *Two World Systems* he attempts to merge mathematical with empirical reasoning to discover the true cause of the tides; but in the *Two New Sciences*, sensing the need to base his new science of motion on intuitively evident principles, he is unable, or unwilling, to make the compromises associated with hypothetico-deductive techniques and reverts finally to rationalism.

Wisan's essay is a fitting introduction to Galileo's methodology, for she manages in the course of it to focus attention on most of the statements in his various works that have been pointed to by previous authors as indications of a methodological commitment on his part. She detects Aristotelian influences on his work, but for the most part interprets Galileo's knowledge of Aristotle as restricting him to the search for first principles that are directly and intuitively evident on their own terms. Thus she discounts the possibility of his discovering principles and causes through *a posteriori* reasoning. Whether she does so on the basis of the Aristotelian text or from her own view of logic is not clear; she does imply, however, that there is no logical way to argue from effects to causes.[8] She is aware of claims made for the regress method, for resolution and composition, and for suppositional necessity, but sees none of these as signs of Galileo's having employed an Aristotelian method in his work. On the subject of resolution, she rejects the Paduan understanding and, following Nicholas Jardine,[9] sees Galileo employing Pappus's resolutive method as this had become known to Renaissance mathematicians.

d. *Butts.* The next contributor to consider is Robert Butts, a philosopher of science who has written extensively on Whewell and who is less sympathetic to Galileo.[10] He agrees in some ways with Paul Feyerabend in viewing the Pisan scientist as a propagandist who lacked an integrated philosophy of his own, and who possibly had no method behind his new science. Butts admits that in some respects Galileo was an Aristotelian, but sees him as failing in his attempt to reduce experience of the physical world to experience that can be expressed in mathematical terms alone. Rather than propose a global analysis, as does Wisan, Butts concentrates on passages in Galileo's letters on sunspots, in *The Assayer*, in the *Two World Systems*, and in the *Two New Sciences* to point out inconsistencies in their ontological claims. He presents a careful appraisal of Galileo's arguments for the subjectivity of sensory qualities, showing, for example, the epistemological difficulties one encounters when attempting to equate the ontological status of heat in water to that of a tickle. It may be true, he points out, that the tickle is not in the feather but in the one perceiving it and that the motion of the feather is what causes the tickle, but this does allow one to say that heat is not in the water but only in the one who senses it. After all, a thermometer measures something, and even if motion is in some way the cause of heat, this of itself does not permit one to say that heat, humanly unperceived, exists in no way in boiling water. Butts also analyzes the discussion of a sphere touching a plane at a single point in the *Two World Systems*, with its invocation of suppositions, impediments, and counterfactual conditionals, and the similarly qualified discussion of the laws of falling bodies in the *Two New Sciences*. In both cases, Butts argues, Galileo's mathematical realism was flawed, involving as it did a leap of faith that had to await a philosophical justification he himself never was able to provide.[11]

Just what that philosophical justification might be is not clear from Butts's essay, but from his statements one may surmise that it would later come from Immanuel Kant. In his view Kant's account of the presuppositions for all possible experience was directed in large part to showing how and why scientific experiences must be mathematical. The difference, and Butts points it out, is that the Kantian solution is essentially psychological,[12] and so relinquishes the very ontological claims Galileo was most determined to make.

e. *Machamer*. A yet different view of Galileo's methods is provided by Peter Machamer, who explores more sympathetically than either Wisan or Butts the Aristotelian foundations of Galileo's thought.[13] Focusing on Galileo's use of causal argument, Machamer advances a theme taken up in different ways by James Lennox, James Weisheipl, and the author, to the effect that Galileo's work must be understood in the light of late sixteenth- and early seventeenth-century methodological traditions. When one attempts to locate it there, the tradition of the mixed sciences emerges as the most likely candidate. In stating this Machamer allows, as does Butts, that Galileo "was not in many ways a philosopher" and that he was not given to methodological reflection in any cohesive or systematic fashion.[14] This for him serves to explain the inconsistent and sometimes contradictory passages in his writings.

Machamer's reason for situating Galileo in the mixed science tradition is that this tradition successfully merges the elements of what Galilean scholars continually refer to as the empiricist-rationalist, experimental-mathematical, and Aristotelian-Platonic dichotomies in Galileo's thought. A mixed science considers mathematical objects *qua* physical (in contrast to mathematics, which considers physical objects *qua* mathematical), and thus has special appeal to one who wishes to start with geometry when solving physical problems in the tradition of Archimedes. Such an orientation renders plausible Galileo's identifying himself as an Archimedean, allying himself with Plato and the Pythagoreans, and in general exalting the power of mathematics. It also proves useful in explaining the somewhat anomalous way in which he uses causal argument in his writings. Machamer provides an interesting analysis of optics and mechanics as mixed sciences, showing how their arguments (or *rationes*) are invariably presented in causal form. What is generally overlooked today is that Galileo's Aristotelian background would induce him to take cause in a fourfold sense – formal, material, efficient, and final – and not merely as an efficient agent as it might be taken at present. Now it is characteristic of mixed sciences, Machamer argues, to concentrate on the formal, material, and final causes of phenomena and rarely to consider efficient causes, whose operation they simply presuppose. Ultimately God is the efficient cause of everything, and so can be taken for granted; otherwise nature can be invoked, as in Galileo's explanation of the fall of heavy bodies. In this connection Machamer observes that for Galileo talk about formal and material causes tends to collapse into talk about natures.

Passing over Galileo's early writings, where, as we have shown elsewhere,[15] causal terminology most abounds, Machamer concentrates on the *Two New Sciences* to illustrate his point. The choice is felicitous because it undercuts Stillman Drake's claim that it was Galileo's rejection of causes that was pivotal to his founding the new science of local motion – "causes" for Drake being efficient causes in the modern sense. Machamer argues that in the demonstrative portions of that work Galileo uses the techniques of a mixed science, implicitly invoking formal, material, and final causes in his explanations; only when he moves into the realm of opinion or *fantasia* does he speak of efficient causes. Most usually, of course, his proofs are based on formal considerations (wherein *ratio* replaces *causa* or *cagione*) as most appropriate in a treatise on mathematical physics.

f. *McMullin.* The final author we shall consider here is Ernan McMullin, whose views are more synthetic than the foregoing and so can serve to round out our sampling of current opinion on Galileo's methodology.[16] Like Butts and Machamer McMullin is a philosopher and historian of science; he is acquainted with the text of Aristotle as seen through the eyes of modern commentators but is less familiar with the Greek, medieval, and Renaissance traditions. In his view Galileo's science is a diverse enterprise, pursued in many different contexts, following methods that altered over the years. Galileo inherited a strict notion of science as demonstration, but his heritage was ambiguous on the relationships between physics and mathematics; unable to resolve the ambiguity, he unwittingly ended up with two conceptions of science. For McMullin these are the demonstrative ideal and its search for causal explanations, which Galileo never abandoned though it led him into the gravest difficulties, and the retroductive ideal, which he employed whenever the causes he was seeking were remote, enigmatic, or invisible. The latter ideal McMullin refuses to see as embodying the HD method described above, substituting instead the notion of retroduction, which he appropriates (anachronistically, one might observe) from the writings of C. S. Peirce.[17]

Significantly, for McMullin the ambiguities that plagued Galileo are more Aristotle's than Galileo's, and these are already found in the *Posterior Analytics* in its discussion of how planets are known to be near from their non-twinkling. Here McMullin turns commentator on Aristotle, effectively questions the latter's distinction between demonstrations *oti* and *dioti*, rejects the first and its use of *a posteriori* reasoning on grounds similar to Wisan's, and argues on this account that *apodeixis* must be largely ineffective in the natural sciences. Though noting Renaissance teaching on the demonstrative regress, McMullin also follows Jardine in rejecting that as a viable interpretation of Galileo's method. These difficulties notwithstanding he is painstaking in pointing out Galileo's commitment to causality and to the demonstrative ideal of necessary truth: for him, contrary to Drake and Maurice Clavelin, Galileo never abandoned his search for causes.[18] Galileo's problems came when he tried to realize that ideal in his new science of mechanics. Here McMullin considers the possibility that he did so in the tradition of the mixed sciences, as advanced by Machamer, and finds difficulties with that too. Four considerations weigh against it: physical principles lack the intrinsic intelligibility of mathematical principles; the *impedimenta* to which Galileo refers frequently block access to a strict science of physics; the technique of reasoning *ex suppositione* is ineffective in getting around such impediments; and finally, Galileo's science of local motion is not a dynamics but rather a kinematics, and for the limited conclusions one reaches in kinematics, as contrasted with dynamics, he did not have to invoke causes anyway.[19]

Turning finally to cosmology, to Galileo's work on the very large (sunspots, comets, and the earth's motion) and his related investigations into the very small (atoms and interstitial voids), McMullin points out that in these areas one could not have an intuitive knowledge of causes, and thus one had to resort to hypotheses. This put Galileo in a bind on the Copernican issue, for he did not want to leave the earth's motion a simple hypothesis. His alternative was to use a causal argument based on the tides; this was hypothetico-deductive in form, but since its illation was based on a causal connection, McMullin prefers to use Peirce's term and consider it as retroductive. Actually, in his eyes all of Galileo's attempts to realize the demonstrative ideal of science failed in the end, and we should salute him not so much for giving us a new *scientia* of either mechanics or astronomy as for being so dogged in his vain attempt to do so.

This survey of just a few assessments of Galileo's methodology show how variegated these assessments can be depending on the assessor's own philosophy and the view of logic or of science implicit in it. We need not pass judgment at this point on which view may be right and which wrong.[20] More noteworthy is the fact that not one was made with detailed knowledge of the contents of Galileo's MS 27 or the sources from which they derived.[21] This is important because only in this manuscript and its sources can one find a definitive statement of the concepts of logic and of science behind Galileo's writings. We must therefore turn to the early Galileo himself if we wish to ascertain how he first used the term *methodus* and how that usage may have impacted on his later works. Before doing so, however, we supply a brief review of the history of the concept of method in the Greek and Latin traditions respectively, so as to provide a proper background against which to situate Galileo's understanding.

3. METHOD IN THE GREEK AND LATIN TRADITIONS

The English word "method" comes directly from the postclassical Latin transliteration of the Greek *methodos*, a term that does not even occur in the *Posterior Analytics* although it is found in Aristotle's other writings.[22] Derived from *meta*, meaning "after" or "following," and *hodos*, meaning "way," the Greek compound originally was taken to mean the way or order to be followed in rational inquiry. In this meaning it implied the rules or norms according to which inquiry was to be conducted, and in such usage logic was said to be a method. From the idea of norm the term was transferred to a discussion or questioning that proceeded along a logical path, and this sense is conveyed by the expression "Socratic method." Finally the word came to mean any doctrine attained as the result of such inquiry, and thus the term *methodoi* came to designate various schools or philosophies; this usage is found in Galen and in early ecclesiastical writers.

a. *Greek Teaching.* The basic Greek teaching on method derives from the medical writer Hippocrates, as described in a section of Plato's *Phaedrus* wherein Socrates is opposing the Sophists and their conception of rhetoric. After describing the processes of dividing (*diairesis*) and gathering (*synagogē*) used by dialecticians, Socrates compares the manner (*tropos*) of healing with that of persuading and says that these are similar in that they involve the analysis of a nature (*phusis*). The physician must know the nature of the body and the rhetorician must know the nature of the soul; to know either one requires the *methodos* used by Hippocrates.[23] The procedure, as outlined by Socrates, is one of setting up the problem of the art, whether of medicine or of rhetoric, to ascertain its goal and explicate the means of attaining it. The first step is to study the nature dealt with by the art to see if it is simple or multiform; the second is to describe the action and reaction of the parts discovered; and the third is to classify the parts and determine the causes of their actions and reactions. Socrates illustrates the procedure by the art of rhetoric, but obviously it can be applied to any useful art or *technē*.

From this beginning, wherein methods of knowing are derived from methods of doing or acting, came the entire Greek methodological tradition as seen in the writings of Plato and his pupils, especially Aristotle, and the Stoics. Among Platonists "dialectical method" was seen as actually a composite of four different methods: the analytical, the definitive, the divisive, and the apodictic. But in the Platonic tradition *methodos* always remained closely associated with *technē*, with division and analysis taking on a certain primacy, the *technē* setting out the end of the art and division and analysis determining different parts and functions to evaluate their merits for achieving it.

To Aristotle we owe the greatest development of Plato's teaching, for he not only outlined a general method to be used in all rational inquiry but also wrote on special methods appropriate to various disciplines. In the two parts of the *Analytics*, *Prior* and *Posterior*, he elaborated his general methods of analysis and definition for all the sciences, and then, in the introductions to his various works, the *Parts of Animals* being a good example, he gave additional prescriptions for investigating particular subject matters. Aristotle also appropriated the Platonic method of dialectics, but in doing so construed it less as suited to apodictic reasoning than to arguing on contingent matters where only opinion or probability could be attained. Norms for this type of reasoning he set out in the *Topics*. It may be noted here that when method is taken broadly enough to be coextensive with logic all of his writings in the *Organon* may be seen as treatises on methodology.

With regard to Aristotle's own use of the term *methodos*, he never employs it in a generic sense but always applies it to a precise way, i.e., a reasoned way. The *Index Aristotelicus* lists only two basic meanings used by him, one a mode of inquiry (*via ac ratio inquirendi*), the other a disputation (*disputatio ac disquisitio*); to these it adds a third possible meaning, that of a teaching (*disciplina ac doctrina*). These are the three meanings we have already pointed out when explaining the etymology of the term at the beginning of this section.

A somewhat different development of Socratic method, but one still focused on *technē*, is seen in Stoic teachings on art. Here method was seen as a grasping (*katalepsis*) of sense impressions that would be strong enough to guide one's way of life. Cicero translated the Greek term into Latin as perception (*perceptio*), but subsequent medieval elaborations transformed it into prescription (*praeceptio*), thereby adding the connotation of providing rules or precepts. This conception of method also exerted considerable later influence, particularly among the humanists of the Renaissance.

By far the greatest influence, however, derived from the Greek medical writer, Galen, whose ideas were basically Aristotelian but who also drew much from the work of Hippocrates and Plato. His major work on method was lost sometime after the sixth century, and most attempts to reconstruct it are based on references to methodology in his other writings. From these one can gather that he focused first on analysis, then on synthesis; in association with these he also spoke of definition and division. His references to *methodos* are usually in the plural, as when he writes of logical methods and scientific methods, but sometimes they are in the singular, as when he mentions the demonstrative method (*apodeiktikē methodos*).

The great Greek astronomer, Ptolemy, subscribed to much the same methodological doctrines as did Galen, also adopting some Stoic elements within a framework that was generally Aristotelian. Pappus, the mathematician of Alexandria, likewise wrote on methods, though he used the term *hodoi* rather than *methodoi* and confined his attention to geometry; the fact that he attributed his views to Euclid and Apollonius of

Perga suggests that he might have compiled them from earlier sources. His discussion of analysis and synthesis is important for later discussions of Galileo's methodology, although there is little indication that it was influential in any way before his *Collectiones*, translated into Latin by Federico Commandino, were published at Pesaro in 1588 and at Venice in 1589. By the time the work could have reached Galileo he probably was already appropriating the contents of MS 27. Pappus defines analysis or *resolutio* as a way (*via*) wherein one considers the solution sought as a fact and then investigates the premises from which it derives, and then their antecedents in turn, and so on, until one arrives at a principle already known; for him synthesis or *compositio*, on the other hand, proceeds in the reverse order, starting at the result arrived at from the resolution and deducing from it successive consequents until one returns to the point from which one started.

Greek commentaries on Aristotle are the final source for teachings on method in antiquity, of singular importance because of their becoming available in the Renaissance and being the major source of renewed interest in method among Renaissance Aristotelians. Not only were the commentators eclectic in their philosophies, like Galen and Ptolemy, but many were also Platonists and so found it expedient to correlate the four dialectical methods of Plato – division, definition, demonstration, and analysis – with Aristotle's various logical teachings. One of the questions that greatly interested them, a question we will see considered in detail in Galileo's D1.2, is whether definition is subservient to demonstration, or vice versa. The ordering of the various works of the *Organon* was another topic of debate, as was the title *Analytics* for its two major treatises. Questions naturally arose as to what was being analyzed in those treatises, and why they were not accompanied by an expected complement entitled the *Synthetics*.

b. *Latin Teaching.* The Latin methodological tradition does not focus on the transliterated term *methodus* to the extent that *methodos* figures in the Greek tradition. The reason for this is that much of the philosophical terminology in classical Latin derives from Cicero, and he apparently avoided the term and never gave it in the Greek, though he quoted other Greek terms. Usually in classical writers one finds *via* or *ratio* in place of *methodus*. Boethius is the first to employ the term *methodus*, doing so in his translation of Aristotle's *Topics*. Added to this sparsity from classical sources is the further complication that Latin translations of Averroes's many commentaries on Aristotle do not contain the term *methodus* either. Somewhere along the line, whether in intermediary Syrian translations or in Averroes's own Arabic, it appears that the distinction between *hodos* and *methodos* became blurred and the latter term dropped out of use. Only when Latin translations of Greek texts and commentaries began to appear in great numbers in the Renaissance did *methodus* emerge as a common philosophical term, and by that time it had taken on so many connotations it is difficult to determine its precise meaning.

Among medieval Latin authors, apart from Boethius, John of Salisbury mentions *methodon* and takes it in the sense of the Latin *compendium*, a term that incorporates the ideas of gathering scattered items together and of shortening or saving time. Following Boethius, in his commentary on the *Topics* Albert the Great compares method with art and says that *methodus* is a short way (*brevis via*), the way of a compendium, popularly called a summary (*summa*). He also makes the statement that art is a rectification of operation, that science is a rectification of thought, and that *methodus* is "a demonstration of the way" (*demonstratio viae*) in both.

Another term for method, and the one favored by Albert's student, Thomas Aquinas, is mode or *modus*. In its original imposition *modus* meant a measure or a norm according to which something is measured; in this sense it implied a standard of measurement for qualities as well as for quantities.[24] Later the term was taken passively to mean the determination within a thing imposed by an extrinsic measure; in this derived sense it implied a limit, a restriction imposed by some standard, as one speaks of a mode or manner of life. These two meanings of mode correspond roughly to the first two meanings of *methodus* pointed out at the beginning of this section, the first referring to logic as the norm for proper inquiry, the second to an inquiry that has been conducted according to that norm and thus has the modality of being disciplined or logical. In this connection Aquinas refers to resolution and composition as modes, as when he associates the resolutive mode (*modus resolutivus*) with the speculative sciences and the compositive mode (*modus compositivus*) with the practical disciplines. But he also speaks of resolution as a resolutory process (*processus resolutorius*).

A final Latin term that shares some of the connotations of mode and process is order (*ordo*), as when one speaks of the order of resolution (*ordo resolutionis*) and the order of composition (*ordo compositionis*). The word order is frequently used in educational contexts simply to designate how one should proceed in a particular subject matter; in this usage it is practically synonymous with other neutral words such as *via* and *ratio* (as in *ratio studiorum*, meaning course of studies), and with *modus*, *methodus*, and *processus* in the senses just explained. But Jacopo Zabarella, the famous Paduan logician, gave order a more precise meaning by relating it directly to method and thereby stimulated discussion among Renaissance Aristotelians. In his *De methodis* he defines method in the broad sense as a logical habit assisting one in acquiring knowledge; he then divides this broad sense into two special senses, one of order, the other of method in the proper sense. Order for him means simply that one thing should be learned before another, whereas method adds to this the further connotation that what is known first will lead to or produce scientific knowledge of the second [ZL139].[25] It is perhaps noteworthy that one of Galileo's teachers at the University of Pisa, Girolamo Borro, published in 1584 "a defense of the peripatetic method of teaching and learning" wherein he argues that order is presupposed to all method and is a necessary condition for it. For him method is a more precise term, signifying a short way whereby one ascends as quickly as possible to a particular knowledge or skill.[26] Yet another of Galileo's teachers, Francesco Buonamici, is important for his return to Greek sources and their terminological usage. He in fact placed great stress on *methodus* and constructed his thousand folio-paged *De motu* in such a way as to illustrate how, through its use, one can proceed progressively from things known to man to those that are more knowable by nature.[27] This process, for him, involves two stages, one resolutive and the other compositive; when both of these are completed, this closes a circle that is commonly referred to as the demonstrative *regressus*.[28] And Galileo's friend and colleague when he himself was teaching at Pisa, Jacopo Mazzoni, invokes a distinction between order and method similar to Zabarella's: order for him implies that one thing is learned *after* another, whereas method implies that one thing is learned *from* another and so specifically connotes a demonstrative process.[29]

4. THE SETTING FOR GALILEO'S METHODOLOGICAL TERMINOLOGY

The foregoing exposition of method and related concepts in the long period leading up to the writing of MS 27 provides all that is needed for an appreciation of Galileo's methodological terminology in that work and indeed in most of his other writings. Despite the fact that the term *methodus* occurs only a few times in the manuscript, these few occurrences can lead us to the detailed treatment of methodological terms in Vallius's logic course, and thus enable us to reconstruct the setting in which such terms were appropriated by Galileo. The reconstruction itself is somewhat complex and on that account will be postponed to the next chapter. Here it may suffice to observe that Vallius was a good scholar, that he was well acquainted with both the Greek and the Latin methodological traditions, and that he incorporated most of the terms explained in the previous section into his own synthesis. Thus, if we assume that Galileo was acquainted not only with the parts of Vallius's course he actually wrote out but also, at least in a general way, with the portions that preceded it, we are in a favorable position to comment on how Galileo understood *methodus* in his early period and how this usage probably carried over to his later compositions.

a. *Variety of Sources.* The first thing to remark, on the basis of the details to be supplied in Chapter 2, is the richness of the traditions on which Vallius drew in preparing his logic course. Although Aristotle's text remained the focus throughout, Vallius was appreciative of the thought of Aristotle's predecessors and also of the Greek commentators who expounded his works; in the portions of the course appropriated by Galileo alone, Alexander of Aphrodisias, Simplicius, Themistius, and Philoponus are all cited, the last two the most frequently. Vallius draws likewise on the medieval methodological tradition: among the Arabs, Averroes to the greatest extent, but also Avicenna, Alfarabi, Algazel and others; among the Latins, Thomas Aquinas and his school frequently, but also Scotus and his followers, Robert Grosseteste, Albert the Great, and Giles of Rome, to mention but a few. He also culls Renaissance commentators for possible aids in understanding the text: Zabarella receives most careful attention, but so do Balduinus, Zimara, and many lesser known figures. All of these sources Vallius melds into a synthesis that conflates their terminology and their thought so as to offer a consistent view of what Aristotle was attempting in his logic generally, and especially in the various books of the *Analytics*.

This richness of traditions spawns a variety of expressions that can be used to characterize the method or methods Aristotle employed. Thus Vallius, and Galileo as a person learning from him, was not locked into any one term to describe what the scientific investigator is or should be doing when studying the world of nature. In general, logic would supply him with instruments of scientific knowing, and such instruments could be taken in a sense broad enough to include method and order or they might be restricted to the more precise meanings of definition and demonstration. In some contexts method and order could mean the same thing; sometimes a method is any way (*via*) or course (*ratio*) or mode (*modus*) of conducting an inquiry; sometimes it is a brief and compendious way. Again, an order in some usages is a process (*processus*), in others it is a progression (*progressio* or *progressus*), in still others it is a reduction (*reductio*) or a regress (*regressus*). Similar observations may be made about the expressions for resolution and composition, for both of these designate methods or orders or modes or processes. Inevitably, whenever one is caught up with the search for scientific knowledge, or is seeking principles or causes or elements that can validate claims for true and necessary conclusions, one is following analytical procedures, that is to say resolutive procedures, whether one uses the term resolution (and its inflected forms) or not. The resulting procedure is then both a logical methodology and a scientific methodology in the senses in which both these expressions were understood in Galileo's own day.

In Galileo's early writings one might expect that his Latin expressions would more readily reflect scholastic ways of thinking, but even there, considering the source of his logic, he had remarkable linguistic resources to draw upon. In the more polemical compositions of his later years, of course, he could take greater advantage of this diversity to add nuances and persuasive force to his argumentation. This makes it quite difficult for anyone counting the number of times Galileo might use any particular expression such as *metodo risolutivo* in a particular work to judge, in the end, whether or not he is there employing an Aristotelian methodology. The entire thrust of his effort to arrive at *scienza* as true and certain but not-evident knowledge is so obviously the goal of the *Posterior Analytics* that it is practically impossible to see any other method behind his work.

b. *Previous Assessments.* These considerations obviously have direct bearing on the assessments of Galileo's summarized above in Section 2. Early scholars were certainly justified in seeing an empirical strain in his work, but Galileo's empiricism was not of a kind with that of Locke and Hume, who had an entirely different agenda in mind. Galileo's empiricism took its starting point from sense experience, but that sense experience had to be resolved to its intelligible content before it would reveal the causes behind nature's operations. With regard to HD method, it is true that Galileo argued frequently from hypotheses or suppositions, and even claimed at times that he was demonstrating *ex suppositione*, but such claims have to be evaluated carefully in their appropriate contexts. In general, within the logical system laid out by Vallius, HD method would have to be seen as a dialectical method whose canons pertain to the *Topics*. As explained in Chapter 3, arguing from the *topos* of antecedents and consequents can yield only probability or opinion, not science in the Aristotelian sense; yet it can be helpful in uncovering the principles on which such a science can be based, as will be seen in the sequel.

The spectrum of views about Galileo's methods propounded in the Blacksburg Conference again shows how differences of commitment to logical or philosophical systems influence what is purported to characterize his work. Apart from the problem of the validity of *a posteriori* demonstration, to be taken up in Chapter 4, much of the difficulty comes from contemporary philosophy of science. The concepts employed in this discipline are very different from Galileo's and are not particularly helpful for categorizing his results and the procedures he used to attain them. Working in a twentieth-century thought context, addressing intricate problems raised by quantum and relativity theories, philosophers of science rarely feel at home when reading Aristotle.[30] And, as already remarked, they can easily be thrown off by modern English translations of his works. The difficulties attending Aristotelian exegesis, particularly those bearing on the *Posterior Analytics*, presented themselves very differently in the sixteenth century from the way they do today. Much better, therefore, to see Galileo through Vallius's eyes rather than through those of a twentieth-century scholar working directly from the Greek text.

With regard to the problem of resolution as used in the mathematical sciences and the extent to which Galileo's use of the term derives from Pappus rather than from Aristotle, this problem as stated poses a false dichotomy. In one sense, following Vallius's usage as explained in Sec. 2.7a, mathematics supplies a paradigm for the way in which resolution is carried out in all speculative sciences. In another sense, not detailed by Vallius but known to the mathematicians of the Collegio Romano, resolution and composition can have a use in mathematics different from what they have in the other speculative sciences. The second sense is that explained by Pappus, but in a text not yet available to Galileo when appropriating his notes on the *Posterior Analytics*, as noted in the previous section. As will be seen later [Sec. 2.7c], in some of his writings Galileo mentions resolution in ways consonant with Pappus's understanding and possibly conflates it improperly with resolution as employed in the physical sciences. That possibility need not be interpreted as vitiating the more basic understanding of resolution as common to both the physical and the mathematical sciences.

A final observation relates to the tendency of some authors to consider resolution and composition as a dual methodology, and thus to express concern when Galileo speaks of the resolutive mode alone without coupling it explicitly to the compositive. Here it is important to appreciate the special way in which the notion of resolution is tied to the books of the *Analytics* whereas that of composition is not. As will be explained in Sec. 2.7a, for Vallius (and for Galileo, as learning from him) resolution is what the *Analytics*, by its very title, is all about. Once one knows how to resolve conclusions to their proper principles using the procedures described in that work, composition becomes a simple procedure and so can be taken for granted. The one case that requires special treatment is the demonstrative *regressus*, but Vallius was aware of that, as was Galileo in his appropriation of Vallius's treatment in his question D3.3, explained below in Chapter 4. Otherwise his more frequent references to resolution are quite consonant with Aristotle's own usage and confirm, rather than disconfirm, his appropriation of Aristotelian resolutory terminology.

5. *LOGICA DOCENS* AND *LOGICA UTENS*

We come now to a topic that has bearing on one of the questions raised at the beginning of this chapter, namely, whether one who knows or writes about a logical methodology necessarily uses that methodology in his scientific work. The question, as already noted, may be asked not only of Galileo but also of Aristotle himself. It is not discussed explicitly by Vallius, but materials that suggest his likely answer are found in his discussion of the necessity of logic in his *Logica* of 1622 and also in his references in the same work to the scholastic distinction between logic as a doctrine, *logica docens*, and logic in use, *logica utens*. The Latin expressions were known to Thomas Aquinas and occur with some frequency in medieval and Renaissance works on logic. The context is usually the consideration of whether logic is a science or an art, and if so, whether these attributions are made more properly to logic as it is a *logica* *docens* or a *logica utens*. Since this distinction structures our volume, in that we set out Galileo's teaching on logic (*logica docens*) in Chapters 2 through 4 and then document his use of it (*logica utens*) in Chapters 5 and 6, it will be well to explain the meaning of the terms and their relevance to the thesis being developed in what follows.

a. *The Necessity of Logic.* To inquire about logic's necessity one must presuppose its existence, and this consideration opens up Vallius's treatment of the problem in his *Logica*. He first makes a distinction between natural logic (*logica naturalis*[31]), the innate capacity of the human mind to define, to distinguish, and to reason correctly, and artificial logic (*logica artificialis*), contained in various treatises on the subject. The first obviously existed before the second, and yet no one denies that the second exists also; even those who reject all the sciences and deny the necessity of logic, such as Sextus Empiricus, admit its existence. Vallius lists the early treatises in which artificial logics were first proposed, pointing out that they were developed, though in imperfect form, before Plato and Aristotle. He also states that various parts of logic were discovered by different people, that Plato, without providing detailed rules, nonetheless provided a complete system, as discerned in his writings by Alcinous and Porphyry, but that Aristotle can properly be called its chief inventor, for he developed it systematically and provided a method and rules whereby it could be used by others. He then explains various compendia and elaborations of different parts of Aristotle's logic made by the Stoics, Cicero, Porphyry, Boethius, Gilbert Porretanus, and various commentators, concluding with the structure of the logic course as it was being taught by the end of the sixteenth century [VL1: 62–63].[32]

Three positions have been held, according to Vallius, on the necessity of artificial logic, the only type under dispute and henceforth used here without the qualifier, since no one would deny the necessity of natural logic. The first is that logic is not necessary for acquiring a science, that it is not even useful, indeed that it is harmful; this he attributes to Cyrenaics, Epicureans, and various skeptics and sophists. The second is that logic is useful for acquiring a science but it is not absolutely necessary; he associates John of Jandun and Zabarella with that position. The third opinion, which he says is the common teaching of philosophers and physicians in the Peripatetic tradition, is that logic is necessary for perfectly acquiring all other sciences [VL1: 63–64].

To present his own view, Vallius first defines the notions of logic and necessity. Logic for him is an acquired intellectual habit wherewith we are taught how to define, distinguish, and argue, along with rules that will assure our doing so infallibly and without error if we observe them properly. Necessity or the necessary, on the other hand, is what cannot be otherwise, and this has two types: absolute, which is applicable to eternal truths, and suppositional (*ex suppositione*), which holds under a condition of some kind. Since conditions can be required in two ways, one for simple existence, the other for perfect existence, there are two subdivisions of suppositional necessity [VL1: 64–65].

On the basis of these definitions Vallius defends the third position noted above: logic is necessary, not absolutely but suppositionally, on the supposition, namely, that one wishes to acquire other sciences in a perfect way. Whether one learns logic from another or works it out for oneself is immaterial. The key expression is acquiring sciences "in a perfect way." This means not only to know a particular subject matter, but to know that one knows it, and so to have certitude about one's conclusions. Natural logic is unable to give such assurance, and that is why a logic such as Aristotle's is necessary for perfectly acquiring other sciences. Apart from its necessity, Vallius goes on to enumerate various utilities that derive from thus knowing logic: it enables one to grasp the quiddities or essential characteristics of things, to reason well, to unravel sophistries, to put order among the sciences, and most importantly, to know what one knows and what one does not. Because of its utility and necessity, moreover, logic should be learned before other sciences. Aristotle himself makes this point rather clear, emphasizing that it is impossible to acquire knowledge and to acquire, at the very same time, the method to be used in acquiring it [VL1: 65–69].

After an extensive explanation and justification of the foregoing statements, Vallius concludes that all of this is to be understood of *logica docens*, that is, logic as it is separated from things (*avulsa a rebus*), which is logic in an unqualified sense (*simpliciter*), and not of *logica utens*, that is, logic as put to use or applied to things (*applicata rebus*). This second, he writes, is properly not logic at all but is the science to whose subject matter logic is being applied [VL1: 69]. This, to our knowledge, is Vallius's first mention of the difference between the two logics in his *Logica*, although he invokes the distinction frequently in what follows.

b. *Logic and the Sciences.* The main discussion of this distinction is in Vallius's lengthy treatment of his combined question whether logic is a science and whether it is speculative or practical [VL1: 102–122]. Much of this material can be deferred to the next chapter; here our interest is in how the distinction may be applied to Galileo and his work. After the statements summarized above, wherein Vallius argues that logic is necessary for acquiring perfectly "all *other* sciences," one would think that he would list logic among the sciences. In point of fact he does not. Logic for him is, strictly speaking, neither an art nor a science nor a faculty; rather it is a special habit of mind which he labels simply instrumental. To establish this conclusion he reviews the opinions of those who hold that logic is a science in a strict and proper sense, among whom he lists Aquinas and the Thomists, Scotus and his followers, and additionally Soto, Toletus, and Fonseca. (The last two were Jesuits; although Soto was a Dominican, he is probably listed along with them because he had taught Toletus before the latter entered the Jesuit Order.) In this context, Vallius observes, the distinction between *logica docens* and *logica utens* assumes importance, for it has bearing on whether logic is a science or not. To be more specific, St. Thomas and Thomists generally, along with the Jesuits cited, teach that *logica docens* is a science and that *logica utens* is not, whereas the Greeks, as referenced by Zabarella in his *De natura logicae*, hold the opposite, namely, that *logica utens* is a science and that *logica docens* is not [VL1: 105–107]. Apparently convinced by Zabarella, Vallius adopts as his own the position of the Greeks as more consonant with Aristotle's own teaching, though he does not relinquish the Thomistic teaching entirely. If science is taken in a sense broad enough to include certain, evident, and necessary knowledge based on definitions and demonstrations, in this understanding logic can be said to be a science distinct from other sciences. But Vallius himself would prefer to add an additional stricter requirement: science must also be concerned with real beings and their causes. When this is added, logic cannot be said to be a science in the strict sense, since its object is not real being but rational being (*ens rationis*), that is, mind-dependent being that has no independent existence in the real world [VL1: 107–115].

Vallius's concern on this score is reflected in Galileo's statements in F3.1.11 and D2.1.8, in the first of which he mentions "rational sciences" and adds the qualification "if there be such, since many regard logic as of this kind," and in the second of which he asserts outright that "there cannot be a science of rational being." More important for our purposes, these statements provide a clue to Galileo's own use of the term science and the way in which he saw it as related to the logical teaching contained in the *Posterior Analytics*. They also may have some bearing on the expressions "scientific methodology" and "logical methodology" as these are used in the present day.

In the position developed by Vallius, which also happens to be St. Thomas's view, all of the requirements for strict scientific knowledge are worked out in rigorous fashion in the *Posterior Analytics*. A person who is expert in reasoning about those requirements, who understands the meanings of principle and cause, definition and demonstration, etc., can properly be called a "logician." He may not be a "scientist," in Vallius's stricter sense, but that is not the point at issue. The crucial point is that as a logician he is doing *logica docens*, whether engaged in teaching logic or not. A person, on the other hand, who is studying a particular subject matter and is using the canons of the *Posterior Analytics* to investigate it, has left the realm of *logica docens* and has shifted over to *logica utens*. Here the position is more nuanced. If he succeeds in demonstrating in that subject matter, then he has attained scientific knowledge of it and has become a "scientist" in the stricter sense. He is not a logician except in the sense that he knows a logical treatise; what has happened is that his successful use of the teaching contained in that treatise, his *logica utens*, has made him into a mathematician, or, to use the modern equivalents of the natural philosopher of his day, a physicist, an astronomer, a chemist, etc. If, on the other hand, he does not succeed in attaining demonstrative knowledge but has only opinions about his subject matter, he is in a sort of no man's land between logic and the real sciences. Actually he is a dialectician and, in the Aristotelian view, has to employ the canons of the *Topics* until he can extricate himself from probable reasoning and make claims for truth and certitude. Only when he can do this does he truly "know," in the sense of having scientific knowledge of his subject matter. (The difference between "having an opinion" and "knowing" in this sense is discussed more fully in Chapter 3.)

c. *Galileo: Logician or Scientist?* When Galileo is situated against this background it becomes relatively easy to answer the questions whether he had a logical methodology and whether he used that methodology in the development of his science. With regard to the distinction between *logica docens* and *logica utens*, he certainly was aware of it and uses it effectively in arguing against the Peripatetics of his day [GG7: 76; cf. Sec. 5.6a]. Moreover, there can be little doubt about Galileo's specific aspirations to being a "logician" or a "scientist" in the senses just described. Logic was never a subject that interested him in its own right. His life's ambition, on the other hand, was to be a "mathematician" and a "philosopher," meaning by the latter a natural philosopher, the Renaissance equivalent of a scientist in our own day. To achieve that status it was imperative that he know, and know well, the canons of the *Posterior Analytics*. His attempts to understand that work, and the many clues he gives to his knowledge of it, mainly through his terminology, assure us that he had a logical methodology, a *logica docens*; it was this, as he himself acknowledged, that guided his investigations to the end of his life. How skilled he was in its discipline may be open to question, but in the author's view he had a remarkably good command of the Aristotelian canons.

More problematic was Galileo's success in achieving a *logica utens* through the use of such canons in the difficult subjects he committed himself to investigate, basically local motion (or mechanics) and the structure of the universe (or astronomy). It will be argued in the pages that follow that, in the end, he was reasonably successful in establishing a science of the first but had less success in establishing a science of the second. In Aristotelian terms, he had a "scientific methodology" for mechanics but ultimately lacked one for astronomy. In neither case did his results come easily. His arguments on this account are rarely perfect demonstrations, being mostly "of the fact" and employing a combination of mathematical and physical reasoning. Again, they are almost always prepared for, and intermingled with, dialectics, and, especially in the case of the Copernican issue, also with rhetoric. His resorting to dialectics in a number of instances creates the impression that he is employing the HD method of modern science, and that is what leads some scholars to attribute to him a "scientific methodology" akin to that used in the present day. This is not the essence of his achievement, however, nor should it be used to characterize the logic that gave form and substance to his life's work.

NOTES

[1] Especially in his "The Development of Scientific Method in the School of Padua," *Journal of the History of Ideas* 1 (1940), 177–206, later expanded into a monograph, *The School of Padua and the Emergence of Modern Science*, Padua: Editrice Antenore, 1961.

[2] For fuller details, see our *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*, Dordrecht-Boston: D. Reidel Publishing Company, 1981,

and *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science*, Princeton: Princeton University Press,1984.

[3] Whewell's appraisal is contained in his *Philosophy of the Inductive Sciences*, 2 vols., London: J.W. Parker, 1847 (repr. 1967), 2: 216–220, and Mach's in his *The Science of Mechanics: A Critical and Historical Account of Its Development*, 6th ed., Chicago: The Open Court Publishing Company, 1960, 151–191. Earlier, in his biography of Galileo reprinted in the National Edition of Galileo's works (*Le opere di Galileo Galilei*, ed. A. Favaro, Florence: 1890–1909, Vol. 19, 597–632, henceforth abbreviated as GG19: 597–632), Vincenzio Viviani had likewise portrayed his teacher as an empiricist; see Michael Segre, "Viviani's Life of Galileo," *Isis* 80 (1989), 207–231.

[4] Typical of Koyré's evaluation of medieval science is his rejection of Domingo de Soto's having exerted any influence on Galileo's thought; see our "Duhem and Koyré on Domingo de Soto," *Synthese* 83 (1990), 239–260.

[5] For Favaro's position see our "Galileo Galilei and the *Doctores Parisienses*," in *New Perspectives on Galileo*, eds. R.E. Butts and J.C. Pitt, Dordrecht-Boston: D. Reidel Publishing Company, 1978, 87–138, reprinted and enlarged in *Prelude to Galileo*, 192–252.

[6] These are collected in Butts and Pitt, *New Perspectives on Galileo*, cited in the previous note.

[7] "Galileo's Scientific Method: A Reexamination," *New Perspectives on Galileo*, 1–57.

[8] "Today, of course, everyone knows that one cannot argue rigorously from effects to causes...," *New Perspectives on Galileo*, 47 n. 3.

[9] In Jardine's essay, "Galileo's Road to Truth and the Demonstrative Regress," *Studies in History and Philosophy of Science* 7 (1976), 277–318.

[10] "Some Tactics in Galileo's Propaganda for the Mathematization of Scientific Experience," *New Perspectives on Galileo*, 59–85.

[11] *New Perspectives on Galileo*, 81.

[12] Butts would say "epistemological" rather than "psychological," as in his statement: "To help in understanding Galileo's problem, we might consider that a couple of centuries later Kant addressed himself to the same problem, but in epistemological rather than ontological terms," *New Perspectives on Galileo*, 63. In the context of the theory of knowledge implicit in Galileo's MS 27, as detailed below in Sec. 2.1, one would tend to see Kant's solution more as a psychological projection on reality than as an epistemology in the Aristotelian sense.

[13] "Galileo and the Causes," *New Perspectives on Galileo*, 161–180.

[14] *New Perspectives on Galileo*, 161.

[15] In "The Problem of Causality in Galileo's Science," *Review of Metaphysics* 36 (1983), 607–632.

[16] "The Conception of Science in Galileo's Work," *New Perspectives on Galileo*, 209–257. Some of the points McMullin makes in this essay are further elaborated in his review of our *Prelude to Galileo* in *Philosophy of Science* 50(1983), 171–173; the reply to these will be found in our "Galileo and the Continuity Thesis," *Philosophy of Science* 51 (1984), 504–510.

[17] McMullin cites Peirce, 227, but does not indicate his source. The English retroduction in fact conveys pretty much the sense of the Latin *regressus*, and one wonders if this is not yet another case of reinventing the wheel, either on Peirce's part or McMullin's.

[18] Citing Drake's essay, "Galileo's New Science of Motion," in *Reason, Experiment, and Mysticism in the Scientific Revolution*, eds. M.L. Righini Bonelli and W.R. Shea (New

York: Science History Publications, 1975), 153–154, and Clavelin's *The Natural Philosophy of Galileo* Cambridge, Mass.: M.I.T.Press, 1974), 390, in *New Perspectives on Galileo*, 237 and 223–224 respectively. A more extensive examination of Drake's views on Galileo'spurported rejection of causal explanation will be found in our "The Problem of Causality in Galileo's Science."

[19] *New Perspectives on Galileo*, 228–240. The first three of these considerations, as will be seen in what follows, derive from McMullin's fragmentary knowledge of Galileo's logic in MS 27 and the ways in which it was used in his subsequent work. The fourth consideration is revealing in that it shows the restrictive meaning McMullin wishes to attach to the word cause, taking it in the sense of efficient cause alone. This is not Galileo's usage, as Machamer correctly discerns in his essay. Most of Galileo's demonstrations, schematized below in Chaps. 5 and 6, pertain to the middle sciences of astronomy and mechanics, and as such invoke types of formal causality that already presuppose the action of efficient causes.

[20] Our evaluation will become clear in the exposition that follows. In the main we would say that Mach, Koyré, and Butts are unduly influenced by the respective empiricist, Platonic, and Kantian strains in their personal philosophies, whereas Wisan and McMullin are similarly constrained by a logic they see as canonical for recent science but that bears little relationship to the logic employed by Galileo. Only Machamer, owing to his appreciation of medieval and Renaissance philosophy, has been able to penetrate to the kernel of Galileo's thought.

[21] Some of the essays in *New Perspectives on Galileo* show an awareness of MS 27's existence through the brief summary of its contents reported by A.C. Crombie, "Sources of Galileo's Early Natural Philosophy," in *Reason, Experiment, and Mysticism*, 157–175, 303–305, but manifest no knowledge of its teachings or its derivation from Jesuit source materials.

[22] On this, see H. Bonitz, *Index Aristotelicus* (Berlin: 1870), 449–450, cited by J.A. Weisheipl, *Nature and Motion in the Middle Ages*, ed. W.E.Carroll, Washington, D.C.: The Catholic University of America Press, 1985, 240. Our main sources for what follows are Weisheipl's essay in this collection entitled "The Evolution of Scientific Method" and N.W. Gilbert's pioneering study, *Renaissance Concepts of Method*, New York and London: Columbia University Press, 1960.

[23] See Gilbert, *Renaissance Concepts*, 3, citing *Phaedrus*, 270A–271C.

[24] Weisheipl, *Nature and Motion*, 241.

[25] Here and hereafter, in view of the large number of citations, we cite Zabarella's work directly in the text using the abbreviation ZL followed by the column number(s). The reference is to Iacobus Zabarella, *Opera logica*, 3d ed., Frankfurt 1608, photoreproduced Frankfurt: Minerva, 1966, col. 139, which has the same column enumeration as the 3d ed., Cologne 1597, photo reproduced Hildesheim: Georg Olms, 1966.

[26] Gilbert, *Renaissance Concepts*, 188, citing Borro's *De peripatetica docendi atque addiscendi methodo* (Florence: 1584).

[27] M.O. Helbing, *La Filosofia di Francesco Buonamici, professore di Galileo a Pisa*, Pisa: Nistri-Lischi Editori, 1989, 73, referencing *De motu* (Florence: 1591), 3. This work is henceforth cited as *Buonamici*.

[28] Helbing, *Buonamici*, 31.

[29] Gilbert, *Renaissance Concepts*, 177, citing *In universam Platonis et Aristotelis philosophiam praeludia* (Venice: 1597), 165–166.

[30] McMullin illustrates this discomfort in his essay, for he faults Aristotle rather than

Galileo for what he regards as methodological errors, consistently seeing ambiguities and ambivalences in the former's thought, *New Perspectives on Galileo*, 211–217 and 220–221. Apparently it does not occur to him that these ambiguities and ambivalences might simply be overlays on methodological canons radically different from his own.

[31] This expression is used by Galileo in *The Assayer* in its Italian form, *logica naturale*, GG6: 333.7, but is mistranslated by Stillman Drake in his *Discoveries and Opinions of Galileo* (New York: 1957), 268, as "physical logic," which is meaningless in the context.

[32] This abbreviation is used throughout for Vallius's *Logica* (Lyons: 1622), with VL1 designating the first volume and VL2 the second.

LOGICA DOCENS

CHAPTER 2

THE UNDERSTANDING OF LOGIC IMPLICIT IN MS 27

Logic as a discipline has a long history, and this must be taken into account when attempting to reconstruct the logic that was functional in Galileo's early writings. In the present day logic is generally divorced from natural philosophy or psychology and thought of as a formal system that can be used independently of the knowledge content to which it is applied. Not infrequently it is spoken of as a formal logic, or alternatively as a symbolic logic, since content can be replaced by symbols, or again as a mathematical logic, since symbols can be manipulated in much the same way as the numbers and figures of mathematics. While this way of viewing logic has elements in common with that implicit in Galileo's MS 27, it leaves out of consideration much of what would be important for understanding the logical teaching contained in the lecture notes on which that manuscript is now known to be based.

Vallius's logic, as should be clear from the previous chapter, is an Aristotelian logic of the late sixteenth century. At that time the scope of Aristotelian logic was seen to be roughly coextensive with the whole of the *Organon*, which means that it included, at a minimum, the content of Aristotle's *Categories*, *On Interpretation*, the *Prior* and *Posterior Analytics*, the *Topics*, and the *Sophistical Refutations*, together with systematic elaborations of themes in those works such as Porphyry's *Isagoge* and Peter of Spain's *Summulae*. Of all this matter, only the portions contained in *On Interpretation*, the *Prior Analytics*, and the *Summulae* tradition have extensive counterparts in formal logic. On this account it is difficult to characterize the remaining portions in terms intelligible to a reader instructed only in modern logic. One might speak of those portions as constituting a non-formal or informal logic, or an intuitive or natural logic. Such usage has the advantage of differentiating it from its modern counterpart, but also the disadvantage of conveying the impression that, being informal or intuitive, it is not as rigorous or precise as the logic to which it is being juxtaposed. This is somewhat paradoxical, because for the Aristotelian logician the non-formal parts are in a sense more rigorous than the formal parts, and indeed are necessary to certify the type of reasoning he regards as most rigorous, namely, scientific reasoning.

An alternative way of characterizing a non-formal logic is to use the correlative of form, namely, matter, and speak of it as a material logic. The latter expression is the one commonly used in the scholastic tradition. In this usage the sense of material is that of content. A content logic can also be termed an intentional logic, for, as will be seen in what follows, the matter or object being considered can be subsumed under the Latin term *intentio* as easily as can the form. Some modern writers indeed use the term intention to differentiate Aristotelian logic from modern formal logic on the grounds that the former is an intentional logic and the latter an extensional logic.[1] Here the use of extension and intension as correlatives further suggests changing the second "t" in "intentional" to an "s," thus making it an intensional logic; this adds the connotation that it is concerned with the intension or meaning of concepts rather than with their extension or the number of objects to which they can be applied. While not exactly the same as the scholastic usage, the change is helpful for pointing to another factor that must be taken into account when attempting to understand Galileo's logic, namely, its close connection with a theory of knowledge.

Aristotelian logic as contained in the *Organon* is but a part of an entire philosophy that, when compared with modern philosophies, makes very strong knowledge claims. As not only pre-critical but pre-modern as well, in its late sixteenth-century form this philosophy has few skeptical overtones. Its basic supposition is that it is possible for the human intellect to know material objects as they are in themselves, and in this sense to grasp real or mind-independent being, designated by the Latin *ens reale*. Apart from this the intellect can reflect on its knowledge of the real world and generate another type of being that is mind-dependent, that is, created by the mind in its attempt to put order in its knowledge of what is real, and thus spoken of as a being of reason, an *ens rationis*.[2] Generally speaking, apart from the books of the *Organon* all of the works in the Aristotelian corpus are concerned with real being or *ens reale*. The *Organon*, on the other hand, has its primary focus not on *ens reale* but on *ens rationis*, the type of being elaborated by the mind in its attempt to define, judge, and reason correctly about the real world. The Greek term *organon*, which translates into Latin as *instrumentum*, thus designates those books as an instrument or tool that guides one's mental operations and so is helpful for elaborating (and understanding) the remainder of the books in the Aristotelian corpus. These notions thus serve to define the scope of logic in its most general understanding as the term is employed in MS 27.

The foregoing mentions of mind and intellect direct attention to another supposition that lies behind Galileo's terminology in his logical treatises. This is the close connection seen by sixteenth-century commentators on Aristotle between the *Organon* and the *De anima*, the part of natural philosophy concerned with the soul and whose modern counterpart would be psychology. Aristotelian psychology is very different from the empiricist thought to which the term psychology is generally applied in the present day. It considers the human being a composite of body and soul, both of which are articulated into various parts or components. The parts of the body, its organs, are studied in biology or medicine, whereas the parts of the soul, its powers, are studied in psychology. These powers include the intellect and the will, the various senses and the appetites or emotions associated with them, motive powers, and so on. Each power is known and characterized through the operations it initiates, and thus one can speak of the operations of the mind, of the will, of the senses, of the appetites, etc. In this context the operations of the intellect take on a dual character and thus fall under two disciplines, psychology and logic. They fall under psychology insofar as they themselves are real activities of a thinking human being, and they fall under logic insofar as they involve a type of reflective activity associated with the beings of reason described above. For this reason the logical terminology one finds in Galileo's writings is frequently intermingled with psychological terminology, and both components must be understood at least in a general way for one to grasp the import of his thought.

On this account reconstructing the background to Galileo's logic is at best a difficult undertaking; it would be impossible if the details of Galileo's appropriation of MS 27 as noted in the preface were unknown. Fortunately the clues given by Vallius and Carbone are quite helpful, for Vallius explains the logical aspects in great detail in his *Logica* of 1622, and Carbone, with his concern for pedagogy, provides sufficient information to fill out the psychological aspects needed to understand Vallius's more mature work. By culling materials from Carbone's plagiarized versions of various treatises in Vallius's lectures of 1588 and from Vallius's 1622 development of the materials contained in them, we can reconstruct with fair accuracy the way logic was conceived by Galileo when composing his early Latin manuscripts.

One of two plausible assumptions may be made for this task. The first is that Galileo possessed a complete set of notes for the course Vallius taught at the Collegio Romano in 1587–1588, perhaps bound in a codex similar in its physical aspects to the codices whose contents have been outlined in the Introduction to *Galileo's Logical Treatises*, for this was the normal way of "publishing" notes in those days. This supposition is obviously connected with how Galileo gained access to Vallius's materials. Among the various possibilities it seems more likely that Galileo had access to Vallius's entire course rather than to the folios containing his treatises on foreknowledge and demonstration alone. If such were the case, one may assume that Galileo acquainted himself generally with its contents before appropriating the questions contained in MS 27. The selections summarized below from Vallius-Carbone's *Introductio in logicam* and *Additamenta* would thus have been basically present in those notes, though without Carbone's pedagogical emendations, as also explained in the introduction to the translation. Selections from Vallius's *Logica* of 1622, on the other hand, while not fully developed as in the mature work, would also have been present, at least in seminal form.

The alternative assumption is that Galileo had in hand only the treatises on foreknowledge and demonstration, which we know he appropriated, and probably also that on science, since he indicates in the manuscript his intention to appropriate the treatise on science as well. Even in that event it seems unlikely that Galileo would have copied extensively from Vallius's notes without some knowledge of their basic orientation and contents, especially in view of the consistency of his terminology in MS 27 and the fact that he makes frequent and intelligent use of notions found throughout Vallius's course. Considering the dearth of materials available heretofore to assess Galileo's knowledge of Aristotelian logic – a logic he claimed to have known all his life in a letter of 14 September 1640 [GG18: 248] – either supposition permits us to make a considerable advance over previous estimates of his logical capabilities.[3]

In this chapter the major portion of the exposition is based on Carbone's *Introductio in logicam*, the introduction to logic Vallius claims was plagiarized from his original teaching notes, and thus attributed to Vallius-Carbone as heretofore.[4] Additional portions are taken from the *Additamenta*, of similar origin, from Vallius's *Logica* of 1622, and from Carbone's preludes to Toletus's *Introductio in dialecticam*, published in 1588.[5] In many instances it has seemed desirable to translate the teachings as they appear in these sources; in others, to save space and eliminate the more tedious passages, their contents are simply paraphrased.[6] The topics on which we focus, in light of these preliminary remarks, are how individuals come to know things, the operations of the human intellect, the term intention and its various kinds, the nature of logic as determined from its object, various instruments of scientific knowing, the concepts of method and order, and the notion of resolution and how it is understood in a variety of logical settings.

1. HOW INDIVIDUALS COME TO KNOW THINGS

We begin with the intersection between psychology and logic mentioned above so as to provide background on the mind's operations as explained by Vallius-Carbone. A convenient starting point is Carbone's preludes to Toletus's *Introductio*, written in the very year Vallius concluded his logic course and obviously inspired by Vallius, in view of their being incorporated again in slightly different form in the *Introductio in logicam* of 1597. After explaining why introductions are necessary and useful for all disciplines, Carbone discourses briefly on the various impediments that students have to overcome if they are to make progress in the sciences. He enumerates these as three in number: their inadequate understanding of how people come to know things; the complexity and difficulty of the subject matter they will be investigating; and errors that can occur in reasoning and their lack of a method to assure that they reason correctly. His first impediment may be paraphrased as follows:

> The first is that these young investigators cannot understand how the human mind comes to a knowledge of what they are investigating, or how it is that objects in the external world can enter into their intellects, or how men use their various powers to know things, or finally how one cognitive power is differentiated from another. As a result they are unable to distinguish sense from intellect and so come to think that they can know only what falls under the senses, or even that nothing exists apart from what might be perceived by a sense power. This is a source of many and serious errors [CT3r].

The warning here against a radical empiricism elicits from Carbone a brief description of the powers of the human soul, on the basis of which he then sketches an abbreviated theory of knowledge. The theory is aimed at showing how it is possible to grasp the natures of things, thus overcoming the second impediment, and how one can learn a logical method for doing so, thereby overcoming the third. In Carbone's program, therefore, the starting point is psychology, and this grounds his epistemology, which in turn grounds his logic.

a. *The Soul and Its Powers.* A similar beginning is found in Carbone's plagiarized *Introductio in logicam*, which provides explanations that are fuller than those in the earlier preludes and so will be used in what follows. In this *Introductio*, which we continue to refer to as that of Vallius-Carbone, the authors note the same three obstacles to knowing and say that they hope to remove them in their treatment. They propose to focus on the operations of the intellect, since logic is concerned with directing these operations. To discuss these, however, they must first explain how the intellect comes to a knowledge of external reality and how things that are known enter the mind.

This requires some knowledge of the soul and its powers:

> To begin simply, we note that man is a composite of body and soul, and that he has certain powers or faculties, some on the part of his body and others on the part of his soul, which enable him to perform operations that are distinctively human [CL9].

This preliminary statement elicits from them a concise explanation of the three kinds of soul explained in commentaries on Aristotle's *De anima*: the vegetative, found in plants and trees; the sensitive, found in brutes; and the intellective, found only in man.

In their account the vegetative soul has three principal powers: the reproductive, which produces offspring; the augmentative, which accounts for growth and development; and the nutritive, which converts food into nourishment for both. None of these powers is cognitive, for their functions are limited to generating organisms and to conserving organisms already in existence, thus supplying the basic requirements for life [CL9]. The powers of the sensitive soul, as opposed to this, include some that are cognitive; they are referred to as senses and are of two types, external and internal.

The external senses they list as the usual five: sight, whose organ is the eye and whose object is the colored or bright object; hearing, whose organ is the ear and whose object is sound; smell, whose organ is the nose and whose object is odors; taste, whose organ is the tongue and whose object is flavors; and touch, whose organ is skin and nerves and whose object is hot and cold, wet and dry. An animal perceives with these powers by sensing things outside itself, and they become the avenues through which objects enter the soul [CL10]. How they do so Vallius-Carbone explain as follows:

> To produce an act of sensation, for example vision, three things are required: the power of sight in the organ, as the power of seeing in the eye; an object to be perceived, as a colored object; and the union of the object with the power, since there cannot be action if the agent and the thing acted upon are not conjoined... This union is effected by a certain species, a

> similitude or representation of the thing seen, which is transmitted from the object to the eye; when the eye receives this species and is affected by it, it sees [CL10].

They then note that what was said of the eye and sight applies similarly to the other external senses.

Taking account then of the remaining sense powers, they describe the internal senses as three in number: the central or common sense, the imagination, and the memory, and state that all of these have their seats in different parts of the brain. Their functions are the following:

> The central sense receives and perceives the species that come from all the external senses, on which account it is called the common sense; the species it perceives it unifies and transmits to the imagination. From this species the imagination forms an image, which it retains and whereby it knows things that are absent from it; it also associates a notion of good or harmful with the percepts of things perceived, and stores these in the memory. The memory, finally, has the power of conserving all species transmitted by the imagination, and on this account becomes a type of storehouse of information [CL11].[7]

Apart from these internal senses, Vallius-Carbone continue, animals also have appetitive powers. These are of two types: an impulse or concupiscible power, concerned with sensible goods that have no difficulty associated with them, and an aggressive or irascible power, concerned with difficult sensible goods. From these appetitive powers arise various emotions, such as love, hope, desire, fear, desperation, etc. Animals also commonly possess a motive power, enabling them to move from one place to another [CL11–12].

Within this context, Vallius-Carbone explain, man's soul differs from that of brutes in that it is rational; it is an immaterial and incorruptible form, more perfect than the other two types of soul, and containing within itself all the powers of the others. Thus it gives a human being the powers of vegetating, of sensing, and of reasoning. The powers that are properly its own Vallius-Carbone enumerate as three: the will, whose object is things under the aspect of their goodness and so enables man to seek the good; the intellect, whose object is truth and so enables man to know and understand the natures of things; and the intellective memory, whose function is to conserve a record of things past.

For their purposes, Vallius-Carbone continue, it suffices to consider only the intellect, because its operations alone are directed by logic. They describe its basic structure as follows:

> The intellect, as is commonly taught, is twofold, an agent intellect and a receptive or passive or possible intellect. The agent intellect, acting on the percepts formed by the imagination,

> produces by its own natural light (*lumen naturale*) immaterial species of things whereby they can be known under their universal aspects and without the singular conditions found in sensible species. These intelligible species are impressed on the receptive intellect and there produce, or give birth to, the concept, which is a terminus of the activity of intellective knowing [CL12].

Here they observe that the agent and the receptive intellects are not really two in number; they are one and the same intellect, but are called by different names because of the different activities attributed to them. Note also their reference to the natural light of the intellect, the *lumen naturale*, to be discussed more fully in what follows.

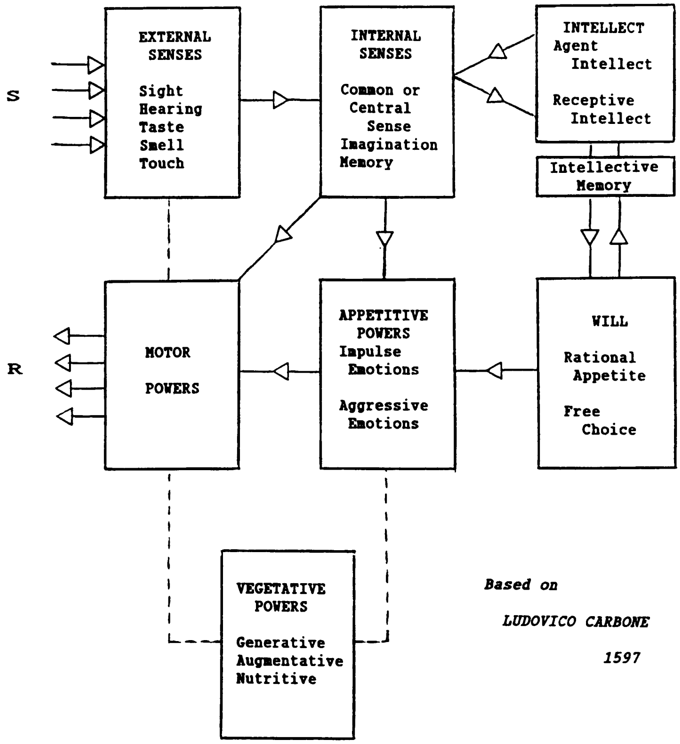
With the intellect's basic operation thus described, Vallius-Carbone quickly sketch its other operations, giving all three in sequence and identifying them with the names subsequently used to designate them:

> When the receptive intellect receives a simple intelligible species and produces a single concept, its operation is referred to as simple apprehension. When it receives two species and forms two concepts, it can join the two together and attribute one to the other, by composing or affirming, as it might with the concepts of man and animal (in the proposition "Man is an animal"); or it can divide the two and deny one of the other, as it might with the concepts of man and stone (in the proposition "Man is not a stone"); this process is the second operation of the intellect, also referred to as judgment. Finally, when the intellect joins one proposition to another and produces from it yet a third proposition, this is the third operation of the intellect, called discourse or reasoning (e.g., in the syllogism "An animal is sentient; man is an animal; therefore man is sentient"). These therefore are the three operations of the intellect – apprehension, composition, and reasoning – and logic directs these operations when it teaches how to perform all of these operations correctly...[CL13]

b. *A Life-Powers Model.* Before we examine each of these operations in detail, it will be helpful at this point to recapitulate Vallius-Carbone's account of the powers from which they proceed. These are shown schematically in Figure 1, elaborated somewhat to provide a fuller cognitive model of the life-powers type. The idea underlying it, basically Aristotle's, is this: just as the human body has quantitative or integral parts, its various organs, so the human soul has "power parts," its powers of informing or enlivening those organs to perform their proper functions. The basic life functions are those of the vegetative powers, shown at the bottom of the diagram; above them, to the left, is shown the motor power found in higher animals, enabling them to move locally; arranged around the motive power are the various powers of sense knowledge and appetition that activate it.

In view of the fact that the animal soul includes also the powers of the

POWERS OF THE HUMAN SOUL



Based on

LUDOVICO CARBONE

1597

Figure 1

plant soul, the five powers to the left of the diagram account for all animal activity. The schema can even be interpreted along the lines of a stimulus-response model, as intimated by the letters "S" and "R" shown at its left. When sense impressions from an object stimulate an animal's sense organs, they transmit species or impulses to the internal senses, where they register an image or percept. The image perceived may initiate a motor response directly, to perform what modern psychologists call an autonomic function, in which case the animal reacts by moving spontaneously. Alternatively, the percept may arouse a response in the animal's appetitive powers, in which case it provokes an emotional reaction that can in turn stimulate movements of various kinds.

All of the aforementioned powers are prerequisite to understanding human activity, which takes place at a higher plane than animal activity in view of man's unique status as a rational animal endowed with free will. Vallius-Carbone do not discuss the human will and its operations, but the will is shown on the right of the diagram to complete the S-R circuit for a distinctively human act. Their major interest is the intellect, shown directly above the will; this is the highest cognitive power in man and is served by the two preparatory sets of cognitive powers, the external and the internal senses. Whereas the external senses send their species directly to the internal senses, however, the internal senses do not affect the intellect directly. In their theory of knowledge a marked difference thus separates the percept generated in the internal senses from the concept formed in the intellect. The percept is a concrete and singular image that corresponds to the individual object perceived, whereas the concept is abstract and universal and as such is capable of being applied to any and all similar objects in sense experience. The universalizing process whereby the concept is produced is triggered by a natural light that illuminates the percept, as it were, and abstracts from it its intelligible content. This immaterial species, as Vallius-Carbone call it, acts on the receptive or possible intellect, which thereupon gives birth to the concept. All of this takes place naturally, in their view, just as does breathing and sensing and imagining. And once conceptualization of this type has taken place, the mind spontaneously goes about combining and separating its concepts first to form judgments and then to engage in discursive reasoning so as to attains the truth about the external world.

c. *Impediments to Knowledge.* A final digression before taking up the intellect's operations is required to say a word about the second and third impediments students have to overcome if they are to make progress in the sciences, as noted by Carbone in his preludes to Toletus's logic. The second of these Carbone explains as follows:

> The second impediment is the great number and general obscurity of things, from which it results that human mind cannot comprehend all their variety in simple and clear fashion and is unable to penetrate into their hidden natures. For we see not only the greatest variety of species of rocks, trees, animals, etc., but we also find many and diverse natures in one and the same thing. Thus it was that gradually, and with repeated errors, investigators began to distinguish, on the basis of sense knowledge, the different natures that could be discerned in any one object [CT14v].

By "natures" in the last two sentences of this citation Carbone means categories or predicaments (*praedicamenta*), for this observation leads him into a discussion of Aristotle's *Categories*, first differentiating the various types of accident one from another, namely, quality from quantity, action from passion or reception, change of position from location in time, etc., and then explaining the category of relation; he concludes with that of substance as the substrate that underlies all of these accidental modifications. Equipped with this knowledge, he says, one should be able to put order into the diversity found in individual objects [CT14v–18r].

But, Carbone continues, that still leaves the problem of how to grasp the natures of things that are hidden and are not so discernible to the senses, now meaning by "natures" the essences or definitions of sensible objects. He approaches the new difficulty by examining the categories again and showing how each type of being can be further subdivided into various genera and species, using the category of substance as an example. From this it is an easy step to explaining the Aristotelian (and later, Porphyrian) exposition of the predicables (*praedicabilia*), namely, genus, differentia, species, property, and accident, which he then does. He concludes by summarizing Aristotle's techniques, as laid out in the *Topics*, for finding the definitions of things. Since we commonly agree that the essence or nature of anything is that whereby it intrinsically differs from other things, we must first locate its genus, i.e, what its nature has in common with others, then search for its differentiae, then its properties, etc. In this way we will ultimately be able to define it, that is, separate it off from other things, and so come to know its nature. As a simple example illustrating this procedure Carbone applies it to finding the generally accepted definition of human nature, that is, man is a rational animal [CT18v–21v].

The third impediment to acquiring knowledge, in Carbone's account, is that students do not possess any art or science that will prevent them from falling into error in their efforts at reasoning. They tend to confuse the knowledge being sought with the reasoning process whereby they hope to attain it, and frequently end up in frustration. Aristotle saw this difficulty in the Socratic method taught by Plato. He recognized how absurd it is to do two things at once, namely, learn a science and at the same time learn the method to be used in acquiring it. It was this insight that led him to differentiate various arts and sciences from each other and explain their proper domains. All of these disciplines, in Aristotle's view, are concerned with perfecting human operations, whether such operations are those of knowing, of acting humanly, or of doing or making things well. This teaching Carbone then uses as a point of entry into a brief division of the arts and the sciences. He is particularly concerned with the disciplines concerned with human knowing, for a consideration of them enables him to separate logic off from the others as being concerned essentially with the mind's operations, with the beings of reason mentioned at the beginning of this chapter. For him, quite obviously, logic is the indispensable starting point for anyone seriously interested in learning about the world of nature [CT22r–24v].

2. THE OPERATIONS OF THE INTELLECT

With this we return to a more detailed discussion of the operations of the intellect modeled above in Figure 1. The exposition on which that model was based is found in the first book of the *Introductio in logicam*, where Vallius-Carbone provide a general overview of all of logic. They return again to the operations of the intellect in the sixth and last book of that work, where they take up epistemological issues relating to the cognitive process. Their intention here is to clarify the nature of logical entities and explain how these enable one to order one's concepts, form proper judgments, and reason correctly. In this context they again enumerate the mind's operations as three in number: conceptualization, or simple apprehension; judgment, known either as composition and division or as affirmation and negation; and reasoning, that is, discourse or ratiocination. A sign of this three-fold differentiation, they now say, is the way we speak:

> The various operations of the intellect can be gathered from human speech, since words are notes or signs of concepts that exist in the soul. Sometimes we pronounce one word, sometimes several. Sometimes we simply use separate terms, as when we say "man," "animal"; at other times we connect them, as when we affirm or negate one thing of another, as in the statements "Man is an animal," "Man is not a plant." Finally we draw an inference from one expression to another, as when we say "Man is an animal, therefore he senses." The first way of speaking is a sign of the first operation, that is, of simple apprehension; the second is a sign of composition; the third, of argumentation [CL243].

More detailed explanations of the three operations serve to clarify these notions. For example, the first operation is called simple conception not simply because it enables us to apprehend simple things alone, for by it we also apprehend composed things, as white man, rational animal; rather it is called such because its product lacks composition in the sense of affirmation or negation, and on this account it is not said to be true or false. The second operation is an act whereby we judge that one thing goes with, or does not go with, another by affirming or negating, on which account it is called composition or division: composition affirms, division denies, because the former attributes something to a subject, the latter separates something from it. The third operation is the deduction of one thing from another, or a judgment that one thing can truly follow from another; for example, if we posit that a plant grows, we deduce from this that it must be alive [CL245].

They then explain the connection between these operations and sense knowledge in terms of the abstractive theory of knowledge modeled in Figure 1. Their account may be paraphrased as follows:

> The operations of the intellect and all intellectual knowledge resulting from them take their origin from the senses. Hence the adage: "Nothing in the intellect that was not first in the senses." Sensitive knowledge is the first type of knowing in man, and intellective knowledge comes next. For the senses are affected by external things, and these become principles of human knowledge through the species they impress on the senses. These species arrive in the imagination, and from them phantasms are formed. From phantasms, in turn, the agent intellect elicits other species that are immaterial. When affected by these the possible intellect produces the concept in its first operation, and after it the others [CL245].

This is essentially a Thomistic account of the process whereby intellectual knowledge is acquired, mentioning as it does the impressed species involved in sensation, the phantasm or percept that is produced by the imagination, the action of the agent intellect on the phantasm to produce an immaterial species (called by Aquinas a *species intelligibilis* or intelligible species), and finally the formation of the concept in the possible intellect to complete the process.[8]

Vallius-Carbone then supply further particulars that accentuate not only the empirical cast to the knowledge process as a whole, but also the role of illumination by the intellect that effects the transition from sensible species or appearances to the various intellectual operations. They do so by way of explaining how the intellect forms its ideas:

> As to why and how the intellect can exercise its first operation, two reasons can be assigned. The first is the functioning of the senses, for just as the senses provide simple species, so they also show that one thing is found in another or not; thus they provide the occasion for the intellect to affirm or deny one thing of another by its process of judgment. The second is the natural light of the intellect (*lumen naturale intellectus*), which enables us not

> only to conceptualize the thing but also to recognize that a predicate goes with it or not. For example, in conceiving a whole the intellect apprehends it as composed of parts, and so by its own light it understands that the whole is greater than each part; on this basis it affirms that the whole is greater than a part. From this one can see how it also produces the third operation, which consists in reasoning, because this is a power of the human mind by which, from one thing known and understood, it reasons to something else not explicitly known; as, when it knows a thing to be good, it immediately infers that it is desirable; or, when it knows that a person is running, it immediately gathers that he is in motion [CL245–246].

Of key importance here is the mention of the natural light of the intellect, already referred to in the previous section. This expression occurs three times in Galileo's MS 27, twice when enumerating instruments of scientific knowing (D1.2.2 and D1.2.4), and once in D3.1.17 when stating that it is with the aid of this light that one recognizes a connection between subject and predicate to be necessary. The light also serves to explain the process whereby, for Galileo in F2.1.4 and again in F2.2.5, we come to the knowledge of primary and most universal principles; in the first of these instances he gives precisely the example cited by Vallius-Carbone, "The whole is greater than its part." Almost fifty years later, in the *Two New Sciences*, Galileo has Sagredo invoke the same light (now in the Italian, *il lume naturale*[9]), to induce his assent to the one postulate on which his theorems relating to naturally accelerated motion are based [GG8: 205.31]. This is a striking instance of the terminology of MS 27 carrying over into Galileo's most mature scientific work.

a. *The Need for Logic.* Having laid the groundwork in psychology and epistemology, Vallius-Carbone now make the transition to logic by explaining the necessity of some habit or art in the intellect to direct its own operations – thus rejoining a topic touched on in Sec. 1.5a. They do so by drawing a parallel between the art of living well and the art of thinking well: just as there are virtues and vices that affect man's way of life, so there are virtues and vices that affect his thinking. Their characterization reads as follows:

> The three operations of the intellect, by their very nature, can be exercized well or poorly; whence it happens that they acquire certain virtues and defects, and on this account need some art whereby possible vices can be corrected.
>
> The virtues and vices of the operations derive from two sources: from the objects with which they are concerned, and from their manner of acting. Virtue arises from the object in any operation when the whole and integral object is perceived, vice when the whole is not perceived or one thing is taken for another. This first vice is called ignorance. From the

> manner of proceeding the intellect acts well when it is concerned with its object in proper and orderly fashion, and since this may occur in various ways there are other virtues and vices that arise on this score. The virtues are clarity, rectitude, and truth; the vices opposed are obscurity, obliquity, and falsity.
>
> Ignorance can be found in all operations of the intellect. That this is so in the second and third operations is quite certain, but of the first there can be some doubt. Yet there can be ignorance even in the first operation: for example, when individuals who are uninformed do not have proper concepts of things, such as unusual animals, plants, and inanimate objects. Thus there can be ignorance in the first operation with respect to anything that is improperly conceptualized or conceived.
>
> The second vice, obliquity or lack of rectitude, is present when we apprehend a thing but not correctly, as when we take its genus for a differentia or a superior for an inferior. The third vice, falsity, cannot be explained properly if we do not understand what truth is, and so we must first say something about truth [CL247–248].

At this point, although they had mentioned truth and falsity earlier, Vallius-Carbone provide a brief overview of Thomistic epistemology, first making a distinction between ontological truth and epistemological truth so as to eliminate the former from discussion. Ontological truth, for them, is a common property of all beings and on this account is said to be the truth of things; this would be, for the metaphysician, the true (*verum*) that is convertible with being (*ens*) and so would be found in everything. As opposed to this, another type of truth is found only in cognitive powers, and particularly in the intellect; this is the conformity of the intellect with the thing known. Among philosophers this type is sometimes known as psychological truth, but the preferable characterization for purposes here is epistemological truth [Sec. 2.1]. It is this second type of truth that they wish to consider, first to make a distinction between active truth and passive truth, and then to show how each of these, and their opposite, falsity, can be found in the three operations of the intellect.

b. *Active and Passive Truth.* Vallius-Carbone begin their treatment with an explanation of the active-passive distinction:

> The truth that is in the intellect is twofold, active and passive. Active truth is what follows the judgment of the intellect and so depends on the intellect; thus, if the intellect were to judge that an animal is alive, this would be active truth because it follows from the judgment of an affirming intellect. Passive truth is the conformity between the intellect and the thing not only as it is in the intellect but also as it exists outside the intellect and apart from the intellect's judgment; thus, if a man were presented to me and I were to conceptualize "rational animal," the conformity or agreement between the concept in my mind and the man would be passive truth. This type of truth can also exist in the senses, which are said to attain truth and to be true if they receive a proper species.

> To both types of truth there is opposed its falsity. Whence active falsity is a lack of conformity between the intellect and the thing known that follows on the judgment of the intellect. Passive falsity, on the other hand, is the lack of conformity between the thing and the intellect that does not arise from the judgment of the intellect [CL249].

It should be noted here that, in scholastic logic, what Vallius-Carbone label active truth is more commonly known as formal truth and what they label passive truth, material truth. They apparently depart from the accepted terminology so as not to overwork the formal-material distinction, which, as we shall see, is pervasive in Thomistic theories of knowledge.

Having explained the differences between these types of truth and falsity, Vallius-Carbone then apply them to the three operations of the intellect to show how they are found in each:

> Passive truth and falsity are found only in the first operation of the intellect, because this operation alone is exercised without the effort of reason, since the intellect is related only passively to such apprehension.
>
> Active truth and falsity occur in the second and third operation of the intellect, but not in the same way, so the difference requires some discrimination. The basic reason is that when producing these two operations the intellect concurs with its own judgment and assent. The need for discrimination arises from the fact that there is only one truth and falsity in the second operation, whereas there are two types of both in the third [CL249–250].

They then enter into a fuller explanation of the reasoning process as found in the third operation so as to prepare the way for the distinction between the formal logic treated in Aristotle's *Prior Analytics* and the material logic treated in his *Posterior Analytics*:

> In the third operation there is one truth in the proposition, another in the form of the argument; the first is taken from the matter, the second from the form of reasoning. In the following syllogism both types of truth are found: "Every animal senses; every man is an animal; therefore every man senses." The reason for this is that both premises are true and the form or the illation is proper. Actually the truth discerned in the illation or the form is more properly said to be good and the falsity opposed to it bad. Thus we speak of good or bad form or consequence, not true or false.
>
> To the foregoing twofold truth is opposed a twofold falsity, again one on the part of the proposition and another on the part of the form, as seen in the following syllogism: "All learned individuals are good; all logicians are good; therefore all logicians are learned." Here there is falsity on the part of the matter, because it is not true that all learned individuals and all logicians are good, and also on the part of the form, since the reasoning is in the second figure with two affirmative premises, which yields a bad consequence.
>
> In both of the syllogisms cited above, however, the truths and falsities are said to be active because the intellect makes them, does so with its own judgment, and does this either correctly or not [CL250].

From this summary account Vallius-Carbone draw the corollary that it is the work of the logician to provide precepts in terms of which one can discern truth from falsity so as to retain the one and reject the other [CL250].

The foregoing exposition should make clear that the logician's main function, in their estimation, is to eliminate errors in the areas of knowledge where active truth and falsity are involved, that is, in the second and third operations of the intellect, for it is in judging and reasoning that the knower is most liable to make mistakes. The first operation of the intellect, that of conceptualization, is for them practically error-free, assuming that the knower avoids the defects of ignorance and obliquity, that is, that he has adequate sense experience and is able to define correctly the concepts that arise out of that experience by the natural light of the intellect. Perhaps it is awareness of this difference among the three operations that serves to explain why they argue strongly for definition being superior to demonstration as instruments of scientific knowing, a position taken over by Galileo in D2.2, as we shall see below.

3. THE TERM INTENTION AND ITS VARIOUS KINDS

Having thus explained the operations of the intellect, Vallius-Carbone now direct attention to the logical entities whereby these operations are directed, that is, the beings of reason referred to at the beginning of this chapter. These are also known as second intentions (*secundae intentiones*), an expression they accord practically the same meaning as beings of reason. Acknowledging that the term "intention" as applied to logical entities is ambiguous and obscure, they prefix their treatment with an explanation of its derivation and generally accepted meaning [CL251].

> Intention is a term composed of "in" and "tention," the latter part of which is no longer a word; it takes its origin, however, from tending. Thus, if we look to its signification, intention designates a tending toward, or a tendency to, something. Since each power by which we know or desire a thing tends towards its object, the word intention has come to be used to explain the operations of both the intellect and the will. As applied to the will, intention sometimes means the act of the will by which it tends to the good, and then it is the same as willing it or grasping it; at other times it means the object toward which the will tends, as when we say that the intention of the teacher is to make the pupil learn. Thus the word refers properly to the act, improperly, by metonymy, to the thing intended. The second kind of intention as applied to the will is divided into good and bad, and it is this kind that concerns moral philosophers and theologians [CL251].

This primary use of intention as applied to the will becomes more complex when applied to the intellect, for in the latter usage the intellect is seen as also tending toward its object and thus giving rise to new types of intentions:

> When the term is transferred to the intellect it takes on a variety of meanings. First it properly designates the knowledge or the act of knowing whereby the intellect is brought to bear on the thing known. Secondly and improperly it is used for the thing known; and because a thing is known through a species that is a kind of similitude of the object known, such species are also called intentions. Finally intention is taken to mean a type of relation that comes to the thing known precisely as it terminates the process of intellection.
>
> This last type of relation is of two kinds: either it is a relation to the intellect that understands what is known, or it is a relation to another thing that follows on the knowledge attained by the intellect, such as the relation of genus to species. For example, from the fact that the intellect considers human nature in its universal aspects, there arises in human nature the first kind of relation to the intellect whereby it is said to be known or understood, and also the second kind of relation to the individual human beings from which the universal is abstracted, on which account human nature is said to be a species, a universal, a predicate, and so on [CL251–252].

Vallius-Carbone then note that in logic the term intention is taken only in its third meaning, for to treat its other meanings pertains to psychology, ethics, and other treatises. So the word intention is henceforth taken by them to mean certain relations or extrinsic denominations that come to things either as they are understood or as they exist objectively in the intellect, as, for example, genus, species, predicate, subject, antecedent, and like terms.

a. *First and Second Intentions.* At this point they enter into the more difficult matter of classifying intentions into first and second intentions, and approach it in the following way:

> Note here that intention, when taken for the object on which the intellect bears, includes two things, one quasi material and the other quasi formal. The quasi material aspect is the real nature as the matter understood; the relationship of that nature to something extrinsic to it, the knowing intellect, denominates it as known, when, for example, human nature is said to be known or understood. The quasi formal aspect, on the other hand, is the relationship whereby the thing known takes on an additional denomination, such as the relationship of human nature to individual human beings, on which account human nature is said not only to be known but also to be a species. Take for example a concrete object, such as something white, where the expression "something white" refers materially to a body, formally to whiteness. In a similar way species refers materially to the real nature, formally to the added relation. In another way of speaking, the relation is referred to as a second intention and the real nature as known as a first intention. But the second intention may also be called formal and the first material, as will now be explained [CL252–253].

With this as a preliminary, a yet more complex division wherein first and second are further applied to formal and material intentions is here introduced:

> Intention generally taken is divided into formal and material; the formal intention is the act that tends towards the thing; the material, also called the objective intention, is the object towards which it tends. Each again is twofold, first and second. The first formal intention is the act in which the intellect bears directly on the thing, that is, in which the thing is first known as an object. The second formal intention is the additional or reflex act in which the intellect knows objective second intentions, namely, those relations that follow on things as first known. The first material or objective intention is the thing itself, directly known, as it exists on the part of the thing grasped by the intellect, such as man, lion, heavens, etc.; and on this account it is called first. The second material or objective intention is the relation that comes to the thing known precisely as it is in the intellect, and on which the intellect bears secondarily; on this account it is called second, as something intended and known in the second place. Objects therefore considered in themselves and as first known are said to be first intentions; relations that are attributed to things and that follow after them are called second intentions, as things known in the second place. The first can be called things known primarily; the second, things known secondarily. Both kinds of intention have in common that neither is said to be an intention except insofar as it is known and terminates an act of intelligence in such a way that it implies a relationship to a knowing intellect [CL253–254].

Having made the basic distinction, Vallius-Carbone explain more fully the differentiations that may be made between first and second intentions. Among these the following is of special interest:

> The two differ because the first intention also exists on the part of the thing and in fact is the real thing itself, either a substance or an accident, and so either subsists by itself or inheres in another. A second intention, on the other hand, does not exist on the part of the thing, nor does it subsist by itself or inhere in another as in a subject; it exists objectively in the intellect alone. Thus, when considering it the intellect bears directly on it, and when the intellect is not considering it, it has no existence whatsoever. On this account things that are said to exist objectively in the intellect are two in kind: one type are things that exist outside the intellect and independently of it – as man or lion, which when known are said to exist objectively in the intellect because its act terminates in them, just as a thing when seen is said to exist in the eye, because the object seen terminates the act of vision. The other type are things that do not exist apart from the intellect's operation, and thus they are said to be because they are known, and of this kind are second intentions and beings of reason [CL254].

Another point they wish to make bears on how first intentions can sometimes be confused with beings of reasons. To explain this they now provide a classification of first intentions, as follows:

> There are three kinds of first intentions. In the first category are things that really exist without any relation to the intellect, as man, lion, heavens. In the second are things that do

> not exist in nature but have existence only through the intellect; yet, because they are signified by the same terms as first intentions and as if they did exist in reality, they are said to be first intentions; examples are a chimera, a gold mountain, and other entities that the intellect fashions on the basis of what it discerns in other things. The third category consists of things that are not themselves real but that designate the negation or the privation of something real, such as blindness and darkness, which include some type of privation in their very meaning. A man is not said to be blind because the intellect grasps blindness in him; yet the blind man is really existent. His blindness is not something, but rather the lack or privation of something, namely, the ability to see [CL255].

Vallius-Carbone then note that some authors classify first intentions of the second and third types as second intentions. While recognizing the basis for this usage, they argue that it is not proper and that they would prefer to regard them as first intentions:

> Granted that some call fictitious entities and privations second intentions and beings of reason, this is not proper terminology. Since they are known as if they did exist in the thing and are not the consequence of something previously known by the intellect, they cannot be mere relations of reason. On this account they are classified more properly as first intentions. The basis for saying that things in the second category, fictitious entities, are beings of reason is that they are conceived by the reason and do not actually exist in reality. Those in the third category, privations, though having no positive existence outside the mind, are said to exist because reason apprehends them in this way and thus they are known as if they had actual existence [CL255–256].

Vallius-Carbone's preference in this matter, particularly that relating to the fictitious entity, may actually have bearing on Galileo's later discoveries. Only two decades after Galileo appropriated the contents of MS 27, the mountains on the moon and the sunspots he had discovered with the telescope would be alleged to be merely fictitious. Like Vallius-Carbone, Galileo was not disposed to regard them only as beings of reason, but rather thought of them as real, and thus as first intentions. The problem then was one that occurs at the interface between sense knowledge and intellectual knowledge, between the discernment of a sense appearance through an imperfect optical instrument and the formation of a concept enabling one to grasp the nature behind that appearance. How Galileo worked with that problem and attempted to solve it is a good example of logic in use, of *logica utens*, to be discussed in fuller detail in Chapter 5 below.

Vallius-Carbone sum up their discussion to this point by enumerating the four kinds of intention involved in intellectual knowing as follows:

> From what has already been said one can gather that there are four kinds of intentions: two are material or objective, and these are called objective concepts, and two are formal; both kinds are divided into first and second. Material or objective intentions are the things that

> are known, whereas formal intentions are the acts or operations of the mind whereby they are known; the former exist only objectively in the intellect, the latter exist there subjectively also [CL258].

Note here that intentions in the intellect are here identified by Vallius-Carbone as concepts, the term more commonly employed in scholastic logic, and used by them probably to avoid the ambiguity involved in the term intention. Also a material concept is spoken of as an objective concept, which henceforth becomes the preferred terminology.

b. *Epistemological Implications.* In light of these distinctions and the theory of knowledge shown in Figure 1 we now propose to piece together the various parts of the account Vallius-Carbone give of first and second intentions. Our objective in so doing is twofold: first to supply needed background for understanding Galileo's logic, and second to show how it supports a realist epistemology, indeed one quite different from that implicit in empiricist philosophies of the present day. Whether or not Galileo knew or understood all the intricacies of the foregoing account is not the point at issue; what is important is that he absorbed the orientation of the system in general, and on its basis made strong and novel knowledge claims, particularly relating to local motion and the structure of the universe. How he could make such knowledge claims has puzzled modern readers, but in fact it is not puzzling in light of the logical notions sketched above and the epistemology on which they are based.

In this epistemology the human intellect has the basic ability to know and understand objects of sense experience. These objects are real and have natures, and as known their natures become first intentions. The natures are real and they exist in the objects whose natures they are; as simply existent they are not intentions, but as known they are objective first intentions. A lion has a nature, and this is whatever it is that makes it be what it is; the objective first intention whereby it is known is a concept, the concept of lion, and this is its nature as known. Because the lion's nature is real, the concept whereby it is grasped may be called a real concept. But here one has to be careful, for the concept may be looked at in two ways, either as the act of conceptualizing (the formal concept) or as what is conceptualized (the objective concept). The formal concept is real only in the sense that the psychological act of conceptualizing is a real act in the one knowing; the objective concept is real in another sense, for as a first intention it is in the lion also, since it *is* the lion as known. In virtue of its being the lion, the knower can say that he *knows* the lion as a real, extramental, or mind-independent being. Strictly speaking he does not know the concept of lion; rather, *through* the concept, he knows the lion, and this is what ultimately grounds his strong ontological claim.[10]

Apart from such first intentions or real concepts there are also second intentions, called rational concepts or logical concepts to differentiate them from the real, in the sense real is being used here. Having the real concepts of lion and animal one may make the judgment that a lion is an animal. That judgment can prompt further acts of the intellect wherein animal, already grasped as a real concept or first intention, gives rise to additional concepts such as predicate and genus. On their basis additional judgments can be formed, such as that "animal" is a predicate (in the proposition "A lion is an animal," where "lion" is the subject), or that animal is a genus (in relation to lions and men, as species contained under it). The concepts of predicate and genus are second intentions. In this order too it is possible to differentiate between formal and objective intentions: there is the formal second intention, the act of conceptualizing predicate or genus, and the objective second intention, what is conceptualized in that act, namely, the type of being that is denominated a predicate or a genus. Logic is the discipline that works with beings of this type. Unlike real beings they exist only in the mind; in this sense they are mind-dependent, whereas real beings are mind-independent. Despite their mind-dependent character, and in fact because of it, they can be extremely helpful for putting order into real concepts, and particularly for making the best possible judgments relating to the truth and certainty of such concepts in investigating the world of nature.

By way of summarizing the materials in this Section, we present in Figure 2 a diagram that builds on the content of Figure 1 to show how first-second and formal-material intentions can be related to the life-powers model of Sec. 2.1b. Of the various powers of the soul only the intellect and the will are shown here, the other powers being blocked out. The contents of the intellect box are now those of the portion of the intellect box in Figure 1 designated the receptive intellect, for this is where concepts are generated through the action of the agent intellect on the percepts of sense experience. In view of the fact that intentions can be seen as acts of the will more readily than as acts of the intellect, the lower part of the diagram, showing the will and its object, should be considered first. The diagram assumes that a person is intent on going to a zoo to see a lion: if so, his intention has both a formal and an objective aspect. The formal

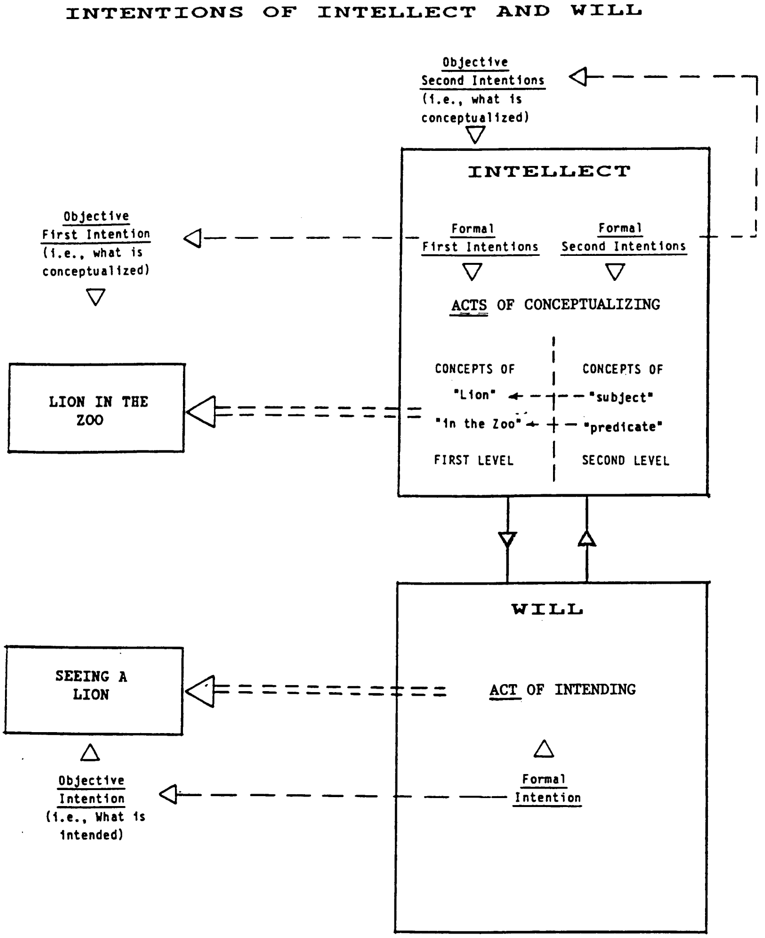


Figure 2

intention is the act of intending in his will, the objective intention is simply what he intends, namely, seeing a lion. Now these formal and objective aspects have parallels in the operation of his intellect whereby he knows the lion he comes to see, as follows.

Assuming that the person senses a lion at the zoo through a percept, he grasps it intellectually through a concept, which in turn he signifies by a word or term, "lion." (One way of understanding intention as applied to a concept is to see it as what the person "intends" when he uses the word that is the sign of the concept.) Consider then the content in the left side of the intellect box, that showing the level of first intentions. In knowing the lion in the zoo as presented to him by his sense powers, the person knowing it conceptualizes both the lion and its location, represented there by the words "lion" and "in the zoo." His corresponding concepts are formal first intentions; their correlate, what is conceptualized, are objective first intentions. These correspond to the formal and objective intentions in the will shown directly below them in the diagram. On their basis he can formulate the proposition "The lion is in the zoo." Although he forms the proposition in his intellect, it is a statement about the real world. The lion, its location, and the joining of the two are all mind-independent entities.

Consider, as opposed to this, the content in the right side of the intellect box, that showing the level of second intentions. After forming his first intentions of the lion and its location, the knower can form additional concepts, represented in the box by the words "subject" and "predicate." His acts of conceptualizing subject and predicate, similarly represented there by the words "subject" and "predicate," are formal second intentions; their correlate, what is conceptualized, are objective second intentions, namely, lion seen as a subject and its location seen as a predicate in the proposition, "The lion is in the zoo." Now he can form the additional propositions, "'Lion' is a subject" and "'in the zoo' is a predicate." These are not statements about the real world; rather they are statements about a statement and so involve entities that are mind-dependent or simply beings of reason. Such second-level concepts are logical entities, and these become the objects of consideration in Aristotelian logic.

4. THE NATURE OF LOGIC

On the basis of these general notions about the operations of the intellect and the types of intention to which they give rise – permitting an ontologically based logic wherein one differentiates real beings from beings of reason, or real concepts from logical concepts – we may now see how Vallius uses them to characterize logic itself as an intellectual discipline. He did so at the beginning of his logic course at the Collegio in 1587–1588, and then again in much greater detail in the introductory part of Volume 1 of his *Logica* of 1622. Fortunately his first introduction was plagiarized by Carbone in the *Introductio in logicam* described in the Introduction to *Galileo's Logical Treatises*. Since this statement is fairly brief we begin with that, and then supplement it with fuller particulars from the longer treatment.

> Logic is a type of habit existing in the intellect that helps us know things; and every habit of this kind is either an art, which is a correct plan for doing something, or a science, which is certain knowledge of the truth, either practical, which is concerned with action, or speculative, which consists in knowledge alone; now we can state briefly that logic is more a science than an art, and more speculative than practical; indeed it does not do any work in the proper sense, nor does it produce anything, nor is it ordered to action.
>
> But from the two causes of logic [i.e., its matter and form], and from what has been said, it is possible to gather what it is, and it may be described as follows. Logic is a habit that directs the operations of the mind; or, it is a science of beings of reason as they are directive of the intellect's operations. Or it is a faculty that treats of the method by which matters that are obscure are manifested by definition, those that are confused are discerned by division, and truths are confirmed and errors refuted by argumentation [CL6].

a. *Logic's Four Causes.* Since in the foregoing passage Vallius-Carbone here define logic only in terms of two causes, its matter (beings of reason) and its form (an intellectual habit dealing with them) it is interesting to see how Vallius expands this analysis in his *Logica* to offer a fuller definition. The passage is rather lengthy, but it serves to sum up his exposition of the definition of logic in that work in terms of all four causes:

> The material cause of logic is twofold. The first is the subject in which it is found, and since it is an intentional kind of being this cause is our intellect, which is also the cause in this way of all intellectual habits. The second is that with which it is concerned, and this is properly called its matter and subject, or the object of the science or of the habit; it is in virtue of this that one habit is differentiated from another, for all intellectual habits have the same subject of inherence. Thus from their object they receive their essence and unity, and for logic this is beings of reason as directive of the operations of the intellect, precisely as such, or an instrument or mode of knowing scientifically, precisely as an instrument or mode of knowing scientifically.
>
> Second, the final cause, and this is not demonstration alone, because definition is not reducible to demonstration but is treated as an instrument completely distinct; nor is it definition alone, because neither demonstration nor other syllogisms are contained under it; nor is it to discourse with probability, because this is the end of the *Topics*; nor is it to distinguish the true from the false, because there is neither truth nor falsity in the first operation of the intellect, which also is to be directed; but it is to treat the instruments of knowing and the science or scientific knowledge produced by them, or it is the direction of the operations of the intellect by means of such instruments of knowing.
>
> Third, the efficient cause is our intellect. For since logic is a natural habit of our intellect,

> not supernatural, and is produced by frequent acts as are other habits, its efficient cause must necessarily be our intellect, just as this is the cause of other intellectual and natural habits.
>
> Fourth, the formal cause of logic is its essence, which is explained by its definition. From the foregoing we can propose this as the definition of logic: *it is an instrumental habit treating of beings of reason as directive of operations of the intellect precisely as intellectual operations* [VL1: 120].[11]

Vallius further explains that in the foregoing definition instrumental habit is the proximate genus, and the remainder is a kind of differentia contracting the genus to the particular species that is logic [VL1: 120–121]. He then makes an interesting comment that ties his view of logic with that of Zabarella, while expressing a slight difference between the two:

> Similar to this is the definition given by Zabarella in his first book on the nature of logic (*De natura logicae*), last chapter, at the end, which reads: Logic is an intellectual instrumental habit, or a discipline instrumental to philosophers, generated from the habit of philosophy, which forms and fabricates second notions on the concepts of things, that these might be instruments whereby the truth is known in all matters and discerned from the false [cf. ZL52]. This definition differs from the preceding in what is put in place of the differentia, because Zabarella differs from us in assigning the object of logic, whence is taken its differentia and formal ratio [VL1: 121].

This comment is cryptic, and it is difficult to gather from it precisely what the point of difference is between Vallius and the Paduan philosopher. Apparently the argument is over the precise final cause that should be assigned to logic, for as can be seen in Vallius's exposition of its four causes paraphrased above, in explaining the second or final cause he makes the object of logic simply the direction of the operations of the intellect by means of beings of reason that are instruments of knowing. Zabarella, on the other hand, goes further than this in the definition just cited and makes its object the discernment of the true from the false in all matters – a position explicitly rejected by Vallius in his account of its four causes. Thus they agree on the proximate genus, that logic is an instrumental habit, but differ on its precise differentia, Vallius holding that this is simply the direction of intellectual operations by beings of reason, Zabarella that it is the discernment of truth from falsity.

b. *The Object of Logic.* A further difference between Vallius and Zabarella is touched on in Vallius's statement of logic's final cause, and this is whether the principal object of logic is demonstration alone, a position maintained by Zabarella, or definition as well as demonstration, that maintained by Vallius. In view of the importance of Zabarella in formulating what are generally regarded as the canons of logical methodology associated with the School of Padua, it will be worthwhile to delve further into this second difference. We can do so by turning back to Vallius's fuller treatment of it earlier in his *Logica.* Furthermore, considering that in Aristotelian thought the object of a discipline is the main determinant of its nature or definition, the following passages offer a convenient recapitulation of Vallius's views on the nature of logic:

> The adequate and total object of logic is instruments or modes of knowing scientifically, precisely as they are instruments or modes of knowing in this way, or alternatively, beings of reason that direct the operations of the intellect precisely as directive of them, and which include definition and demonstration.
>
> Practically all the sciences and arts treat beings of reason to the extent that they have their own terms for the second intentions they consider. Logic therefore is not alone in treating beings of reason, although it alone has them for its object. This cannot be said of any other science or art, for each of them has additionally something real in which it considers its beings of reason. Logic, on the other hand, has not, but it considers beings of reason not according to their own nature but precisely as they are directive of the operations of the intellect in the way already explained.
>
> The principal subject in logic is definition, because both demonstration and definition are instruments of knowing scientifically, and definition is much better and more perfect than demonstration. On this account it is the more principal object, even though many are in doubt whether Aristotle treats definition in his logic on its own merits, or does so only for the sake of demonstration, as we will explain in the *Posterior Analytics.*
>
> Finally, it is apparent that all matters that are assigned by others as logical objects are treated in the discipline. For words are treated insofar as beings of reason are explained with words put in their place. Real things are treated insofar as they lead to the knowledge of beings of reason, and through them things are sometimes explained that would be difficult to understand without application to a particular matter. Operations of the intellect are treated insofar as they are the end of logic, for directive beings of reason are ordered to directing operations of the intellect. Syllogism and argumentation are treated insofar as demonstration could not be understood without them. Finally many matters are treated that are either principles of instruments of knowing scientifically, or their parts or species or goals, or are in any way conducive to their better understanding [VL1: 101–102].

The third paragraph in this citation is the additional matter on which Vallius diverges from Zabarella. Indeed, in a special chapter devoted to Zabarella's views on the object of logic [VL1: 80–81], Vallius explains that Zabarella thought that demonstration alone is the object of logic [cf. ZL45–46], whereas he himself is convinced that both definition and demonstration are its objects, and indeed that definition is more principal than demonstration. This has bearing on the ultimate differentia to be assigned to logic as an instrumental habit, as seen in the previous subsection. It is noteworthy that Galileo appropriated Vallius's solution to the problem in his D1.2, and in this matter at least his view of logic is not completely consonant with Zabarella's.

c. *The Content of Logic.* To round out Vallius's account of the nature of logic, it will be helpful to lay out the entire scope of that discipline as he covered it in his *Logica* of 1622. The general content of the logic course at the Collegio Romano is shown in Figure 1 of the Introduction to *Galileo's Logical Treatises*, where the main tracts of Rugerius's lectures, given between November 3, 1589 and August 24, 1590, are indicated. Vallius's coverage is similar to Rugerius's, though developed in far greater detail. A summary of its entire contents is provided in Table 1. As can be seen there, Vallius covers the whole of Aristotle's *Organon* in two volumes, reserving his second volume for the *Analytics*, both *Prior* and *Posterior*, and treating everything necessary for understanding them in his first volume. There his introduction and prolegomena to the study of logic are remarkably similar to the materials in the *Introductio in logicam* being explained in this chapter. On the basis of this preparation Vallius then launches into detailed examinations of the predicables and the categories – knowledge that Carbone, in his *Praeludia* to Toletus's logic, pointed to as prerequisite for categorizing real beings and penetrating into their otherwise obscure natures. Vallius's exposition of the predicables is based on Porphyry's *Isagoge*, but this in turn incorporates most of the materials in Books 2 through 6 of Aristotle's *Topics*. Following that Vallius presents an exhaustive development of the predicaments as sketched in Aristotle's *Categories*, and then does the same for the content of his *On Interpretation*. All of this, in Vallius's view, is ordered toward understanding the instruments of knowing that serve to direct the first and second operations of the intellect and so perfect them in their attainment of truth.

This leaves only the third operation of the intellect to be treated in the second volume. Here the *Prior Analytics* is treated first, and it exposes the syllogism as the basic instrument of discursive reasoning. Following this Vallius devotes most of the volume to an exposition of the *Posterior Analytics*, dividing it into four major treatises, namely, those on foreknowledge, demonstration, definition, and science, basing the first two on Book 1 and the second two on Book 2 of that work. Of these, demonstration and definition are for Vallius the principal instruments of

Table 1

THE CONTENTS OF VALLIUS'S LOGIC COURSE

VOLUME 1: THE OLD LOGIC	
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The three operations of the intellect	1:3
First operation: the term and its definition	1:3
Second operation: the proposition and its various types	1:14
Third operation: the syllogism, including the topical and the sophistical	1:26
Prolegomena to the study of logic	1:44
The *ens rationis*: first and second intentions	1:46
The nature of logic, its necessity, object, and status as a science	1:61
The *Predicables* (i.e, *Isagoge*) of Porphyry	1:123
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What the predicables have in common, and wherein they differ	1:253
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The object of the work and its relation to logic	1:259
The antepredicaments	1:267
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What is placed in the predicaments, and their number and differentiation	1:289
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The *De interpretatione* (i.e., *Perihermenias*) of Aristotle	1:608
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VOLUME 2: THE NEW LOGIC	
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The kinds of demonstration	2:299
Circularity in argument and the demonstrative regress	2:340
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Summary exposition	2:376
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The nature of definition	2:380
The properties of definition as an instrument of knowing	2:397
Disputation on science	2:409
Habits in general	2:410
The essence or quiddity of science	2:527
The properties of science	2:561
The divisions of sciences and their subalternation	2:607
The comparison of science with opinion and other intellectual habits	2:662
Finis	2:696

scientific knowing; they, along with the habit of science they produce, receive a lion's share of the treatment.

Before considering these instruments in fuller detail, a remark may be made about the formal logic covered in Vallius's course, since this feature differentiates his work from much of the logic taught in the present day. Most of his analysis of logical form is found in the hundred-odd pages of his second volume devoted to the modes and figures of the syllogism explained in the *Prior Analytics*. The remainder is located in the first fifty-odd pages of the first volume, which essentially summarize the logical exercises of the *Summulae* tradition. All of this seems to be regarded by him as elementary material that is more or less presupposed to his course and so requires little attention for those studying logic at its advanced level.

5. INSTRUMENTS OF SCIENTIFIC KNOWING

Since instruments of scientific knowing (*instrumenta sciendi*) figure so prominently in Vallius's definition of logic, and since they are referred to several times in Galileo's logical questions, a fuller exposition of the expression is now indicated. This is particularly important in view of the matters covered in our first chapter on Galileo's logical methodology, for both method and resolution as a particular type of method are treated within the logical system he appropriated under the rubric of their being instruments of knowing.

At the outset a note should be made about the Latin expression itself. The term *instrumentum sciendi* is somewhat ambiguous: in some contexts it can be translated simply as "instruments of knowing," in others it has a stricter connotation and should be translated as "instruments of scientific knowing," that is, instruments that are capable of producing the rigorous type of knowing characterizing science in the sense of *scientia*. In most of Galileo's usages the stricter connotation is implied, and the translation here and in the text of his manuscript then renders it as "instruments of scientific knowing"; when it is not, "instruments of knowing" is used.

a. *Galileo's Division*. Aristotle's *Organon*, as the title indicates, is itself an instrument, and it is not surprising that the various logical operations it describes have in turn been labeled instruments or tools that assist the human mind in attaining knowledge and truth. Many such instruments can be identified, some designating broader classes than others and so including them as subspecies. In D1.2.2 Galileo mentions an initial broad division into natural instruments and adventitious instruments and gives a general idea of the types of instrument included under each. Then in D1.2.3 he offers a division of a special type of adventitious instrument that serves perfectly to direct the operation of the intellect in some way, and enumerates in this category six such instruments, namely, definition, demonstration, division, proposition, argumentation, and method. These are all obviously logical entities, or beings of reason, as explained in the previous section. Of these, Galileo states that division and method assist the mind only mediately, that argumentation assists it imperfectly, and that demonstration and definition assist it immediately and perfectly [D1.2.3–6]. He does not define any of these, apart from indicating that argumentation is for him a general term that includes under it the probable syllogism, induction, and the enthymeme. He also gives a few indications as to how the various instruments might be ranked in the order of their importance [D1.2.6], but in general his remarks are fragmentary and far from systematic.

As in the previous section, where it was possible to explain the nature of logic on the basis of Carbone's plagiarized notes in the *Introductio in logicam* and then more fully from the *Logica*, so here it is possible to fill out Galileo's teaching on instruments of knowing by providing a brief overview from the *Introductio* and then following it with a fuller exposition. In this case the fuller exposition is not from the *Logica* but from the treatise on instruments of knowing that was plagiarized by Carbone and included in his *Additamenta* of 1597. Both expositions are thus based on Vallius's lecture notes of 1587–1588, as explained in the previous chapter, and on that account may be presumed to be closer to the materials contained in Galileo's exemplar than the fuller explanation of the nature of logic in the previous section, which drew heavily on the *Logica* of 1622.

The brief description of instruments of knowing found in the *Introductio*, attributed as heretofore to Vallius-Carbone, may be paraphrased as follows:

> The instruments that serve the operations of the intellect are five in number: definition, division, proposition, argumentation, and method. Definition is an expression that manifests the nature of a thing, as rational animal manifests the nature of man, and it directs the first operation. Division is an expression wherein a whole is separated into its parts, as animal is divided into rational and irrational, man into body and soul; it serves mainly the second operation, though it assists others as well. Proposition is an expression wherein one thing is affirmed or denied of another, as risibility of man, and this is what directs the second operation. Argumentation, on the other hand, is an expression wherein one thing is noted as consequent on another, as the sun has risen and so it is day, and it directs the third operation. Finally, method stands apart from the other four instruments, for it is a proper arrangement (*dispositio*) for treating any matter and so is necessary for directing all of the foregoing and indeed for operations of any kind [CL14–15].

Note here that Vallius-Carbone list only five instruments, rather than the six given by Galileo, and indeed leave demonstration out of their enumeration entirely. The reason for this is rather simple: they give argumentation as a basic category, which in their usage includes demonstration and the other types of argumentation listed by Galileo under what he, in his enumeration, calls "argumentation in general."

b. *Defining the Instruments.* When we turn to the *Additamenta* and to the fuller treatise on instruments of knowing, we find there that Vallius-Carbone list the same six instruments as does Galileo in D1.2. In fact Galileo's materials have extensive counterparts in the second question of their treatise, a good indication that that question and Galileo's D1.2 derive from the same source. Then, in their third question, Vallius-Carbone proceed to define each of the instruments in much more detail than is given in the *Introductio* cited above. Their fuller definitions of definition and demonstration in the *Additamenta* are as follows:

> *Definition*: Definition is an expression whereby the nature of a thing is clearly manifested,

> or, as Aristotle says, one that shows its "what it was to be" (*quod quid erat esse*). It has two principal functions. The first: that we may perfectly apprehend and understand the thing's essence, since Aristotle teaches *passim* that 'what it is' is known by the definition, and Cicero rightly referred to it as 'a laying bare of things covered up'. The second: that through it we may prove the properties and accidents of a thing, and this function comes to it insofar as it is placed in a demonstration, that is, as it serves as a middle term for demonstrating the properties of the thing.
>
> *Demonstration*: Demonstration is a syllogism composed of necessary propositions wherefrom something is concluded necessarily and evidently through causes or effects. Note here that there are three kinds of demonstration: one is said to be most perfect and most powerful (*potissima*); a second is demonstration of the reasoned fact (*propter quid*); a third, demonstration of the fact (*quia*). Three functions are attributed to demonstration. The first is to show the existence of an effect and its reason why, as is done in most powerful demonstration. The second is to manifest the cause of a thing, and demonstration of the reasoned fact does that. The third is to show the existence of a cause through an effect, and that is what is done by demonstration of the fact. [CA25v–26r].

These definitions are helpful for clarifying distinctions found in Galileo's notes. With regard to definition, in D1.2.13 Galileo mentions a distinction between definition when it is considered as a middle term and when it is considered as an instrument of scientific knowing; again, in D1.2.16 he says that definition can be taken in four ways: as the nature or quiddity of a thing, as a source of topical argument [see Sec. 3.6], as the middle term in a demonstration, and as an instrument of scientific knowing. The last usage in both instances is the one defined above, namely, an expression that clearly manifests the nature of a thing, for this is its primary function as a cognitive instrument. Secondarily, of course, it can serve as a middle term in a demonstration, but in that case it is not itself the instrument but is being put at the service of another instrument, namely, demonstration.

Similar distinctions with regard to demonstration occur in Galileo's exposition. In D1.1.2 he says that demonstration can be taken in two ways, either as a type of illative discourse (i.e., a type of syllogism or argumentation) or as an instrument of scientific knowing; he repeats this distinction in D3.1.2, there differentiating demonstration as it is a syllogism from demonstration as it is an instrument of scientific knowing. The second usage in both cases is again that defined above: a syllogism composed of necessary propositions wherefrom something is concluded necessarily and evidently through causes and effects. Only this type can be truly productive of science, for its formal and final cause, as explicitly stated in D1.1.2, is "the necessary relationship of the middle term to the subject, the predicate, and the conclusion." Without this element of necessity a demonstration would merely be a syllogism or a kind of illative discourse. Indeed, as will be made clear in the following chapter [Sec. 3.5a], a demonstration whose apodictic force is not grasped by an individual will be seen by that individual only as a syllogism or a probable argument, and so cannot function for him as an instrument of scientific knowing.

The foregoing account, then, give a good idea of how instruments of knowing were understood by Vallius-Carbone, and thus presumably also by Galileo. In one way they are all "instruments of scientific knowing," since all are somehow required if one wishes to have science or to know apodictically. In another way, apart from definition and demonstration, and possibly method, the others may be regarded simply as "instruments of knowing," since they aid in supplying knowledge even if this is not the perfect type of knowledge sought and attained in a science.

In the continuation of the text just cited [CA26r], Carbone mentions his treatment of method in his *Praeludia* to Toletus's introduction to logic. In view of the importance of this concept for understanding Galileo's views on logical methodology, and the fact that it is dwelt on not only in these *Praeludia* but also in Vallius-Carbone's *Introductio in logicam* and Vallius's *Logica*, we turn now to a fuller consideration of it and related concepts.

6. METHOD AND ORDER

The word *methodus* occurs three times in MS 27, twice in D1.2.3 and once in D1.2.6; all three occurrences are in Galileo's discussion of a question much argued by Renaissance scholars and mentioned in Chapter 1, namely, whether definition is subservient to demonstration, or vice versa. The value of these occurrences lies not so much in what they say about method but rather in directing us to the places in Galileo's source where method and related terms are discussed more fully and situated properly in a logical context.

a. *Method.* Carbone introduces the concept of method in his *Praeludia* to Toletus's logic of 1588, as just mentioned. An almost identical discussion is found in the *Introductio in logicam* of 1597, a fairly clear indication that Carbone used the same material he had appropriated from Vallius's lectures of 1587–1588 to prepare both the *Praeludia* and the *Introductio*.

In the latter work Vallius-Carbone consider method in a preliminary discussion of the instruments used by logic when directing the mind's operations. After listing the instruments and the mental operations associated with them, they first locate method within the more generic concept of order, then define method, and finally mention the various kinds of method included under their definition. The operations they list are the ones we have already discussed, namely, conceptualizing, judging, and reasoning.

This schematic treatment of method was amplified by Vallius in the introductory section of his *Logica* of 1622. There he decided to treat method as a species of order, the first species being order in general and the second, order of the particular type known as method. Possibly he did so after rethinking the relationship between these two concepts proposed by Zabarella in his *De methodis*, for there Zabarella inverts the two, regarding method as the more generic concept that includes under it both order and method in the proper sense [ZL138–139].

Whatever his motivation, Vallius's treatment of order in general makes use of the distinction between the order of nature and the order of doctrine: but these he now describes more fully.

> Taken in general, order is an instrumental habit of mind whereby one is able to arrange the parts of any discipline so that that discipline, to the extent possible, can be learned most easily and in an optimal way. So understood, there are two subspecies of order in general, one the order of nature and the other the order of doctrine. The order of nature may be further subdivided into the order of generation (or of execution) and the order of perfection (or of intention). The order of doctrine, on the other hand, is the order to be followed in a science.
>
> One may ask whether the scientific order should follow the order with respect to nature or an order with respect to us. The answer: since the sciences were invented to help us understand nature they should not follow the order nature employs but rather one that facilitates our better and easier knowledge of nature. Were the order with respect to nature to be followed, there would be only one scientific order, and that would be the order of composition; there would be no order of resolution. But a scientific order must be an order of doctrine, and an order of doctrine must be an order that first treats matters that are conducive to our understanding of others [VL1: 43].

This last consideration brings Vallius to an analysis of how knowledge of one thing can lead a person to knowledge of another. He thereupon outlines three different ways in which this can happen, and shows how this can further illuminate the difference between order and method:

> The first way in which knowledge of one thing assists in attaining knowledge of another is when the first thing known is actually the cause of knowledge of the second, the way in

> which substantial change is employed in the *Physics* to come to a knowledge of protomatter. The second is when the first thing known is not the cause of knowledge of the second and yet must be known beforehand; in this way one must known what an animal is if one is to understand the definition of man as a rational animal. The third way is when the first thing known is neither the cause of knowledge of the second nor necessary for attaining that knowledge, and yet can help by being conducive to its understanding; in this way animals are better known than plants or shrubs and so give a better idea of living things.
>
> A comparison of these three ways can cast light on the difference between order and method. Order consists in a forward movement (*progressus*) from things that are more known in the second and third ways, but not in the first, except possibly incidentally. Method, on the other hand, progresses from things more known in the first way; thus its starting point is what must be known with necessity and otherwise leads to certain knowledge of the matter to be understood [VL1: 43].

In this way of looking at the two processes, Vallius continues, order does not consist in the illation of one truth from another or in the proof of either, but only in the arrangement of the matters to be treated. These should be so arranged that the easier and those on which others depend come first. Then should come other considerations, whether they are actually prior in the order of nature, perfection, and intention or in the order of generation and execution. The reason for this is that a scientific order must take account of our ways of knowing things, and sometimes we know through what is first in nature, perfection, and intention, but sometimes not. Again, Vallius concludes, this is why Aristotle himself presents sciences in the order of doctrine and never follows the order of nature [VL1: 43].

b. *Order in the Analytics.* In explaining the acquisition of scientific knowledge through causes, the first way knowing one thing can assist knowing another, Vallius again observes a twofold ordering, and this too he subdivides into an order of resolution and an order of composition. Here, however, he explicitly applies his teaching to the ways in which these orders apply to the *Prior* and *Posterior Analytics*:

> Compositive order proceeds from first principles to their consequents or to what is composed of or from principles, and is characteristic of the speculative sciences. Resolutive order proceeds in the reverse direction, from an end proposed to the investigation of the first principles required to attain it, and is characteristic of the moral sciences, but also of the *Prior* and *Posterior Analytics*. In the *Prior Analytics* Aristotle first defines the syllogism, the end of that work, and then investigates its principles and causes. Similarly, in the *Posterior Analytics* he first defines science, the end of demonstration and definition, and then investigates the nature and causes of demonstration and definition. Based on this usage different definitions of compositive and resolutive order may be formulated. Compositive

order is that wherein one disposes the parts of a science in such a way that, starting from the first principles of the subject and traversing its secondary principles, one finally arrives at its ultimate principles; in this way scientific knowledge of the subject is acquired easily and accurately. Resolutive order, on the other hand, proceeds from any end proposed to an investigation of its principles [VL1: 43].

These orders are all contained under Vallius's first meaning of the term order, which is most generic and thus all inclusive [VL1: 43]. The second species of order is then method taken in the proper sense, and this, Vallius notes, is also called the way of doctrine, or the way of proving, demonstrating, or manifesting conclusions previously unknown from things already known. Method in this sense has many ways of proceeding, either from causes, or from effects, or from probable arguments, or from induction and similar forms of reasoning. In all of these, however, one must start from premises that are more known in the first way described above, namely, in the way that permits the deduction from them of what is less known. One of the major problems with method, Vallius here adds, is whether it applies only to demonstration (Zabarella's teaching) or whether it includes also definition and division (Vallius's view, appropriated, as already noted, by Galileo in his D1.2). In the order of doctrine, finally, one may proceed in either a resolutive mode (*modo resolutivo*) or a compositive mode (*modo compositivo*), provided one always starts from what is more known [VL1: 43].

7. RESOLUTIVE METHOD IN ARISTOTELIAN LOGIC

The foregoing mentions of resolution in the introductory sections of Vallius's *Logica* of 1622 may be complemented by notes taken from the *Praeludia* incorporated by Carbone in his *Additamenta* of 1597 – a series of notations to be differentiated from his *Praeludia* to Toletus's introduction mentioned above. The former is a prelude to the commentary Vallius gave on Aristotle's two *Analytics*, the *Prior* and the *Posterior*, in his lecture notes at the Collegio Romano in 1587–1588. It includes a more extended exposition of the definition and division of resolution as this concept is required for an understanding of both *Analytics*.

a. *Resolution in the Analytics*. Vallius-Carbone begin the *Praeludia* of the *Additamenta* with a reference to the opening passage of the *Posterior Analytics*, where Aristotle remarks that all teaching is based on prior knowledge [71a1] – a statement to be expanded into the entire treatise on foreknowledge appropriated by Galileo [see F2.1.4]. Among things that must be foreknown is the knowledge of terms, and since Aristotle entitled these books *Analytics*, one should begin with a notation concerning the title of the books and the definition of the terms contained in it:

> The Greek title of the earlier treatise is *Analutica pretera*, which translates into Latin as *Resolutoria priora*; presupposed is the word *Biblia*, meaning volumes or books, so that the sense is "prior resolutive books." The Greek *analusis* is directly transliterated into Latin and commonly used that way, though its proper translation is *resolutio*. It derives from the verb *analuo*, which translates as *resolvo*, resolve or untie, and is said not only of untying things bound together but more generally of decomposing whatever is composed in any way and so can be resolved into simpler and prior components. The term analysis therefore means an act whereby something composed, or posterior, is reduced to its principles or priors, and in this way the thing itself is said to be resolved [CA1ra].

As to the reality designated by the term, Vallius-Carbone note that this can be gathered from the etymology already given, and on its basis they offer a variety of descriptions of resolution:

> Resolution may be described in various ways: it is an examination of the causes of a thing, an inquiry into the proper causes of a true conclusion, a progression (*progressio*) from what is last to what is first, a kind of regression (*regressus*) back to the principles from which a thing comes or on which it depends. When something composed of elements returns to those elements the return is called a resolution. In these understandings, to resolve is not only to divide or untie a thing into its components but also to dissolve a composite entity into its primary elements so that its nature may be made manifest and correct judgments formed about it. A more complete description would be the following: resolution is the reduction of any entity to the principles on which it properly depends, whether this is done in a physical way or in cognition alone. Examples of the two types would be taking a body apart to identify its components and considering it only with the mind's eye to see of what it is made [CA1rb].

Vallius-Carbone note that a more complete enumeration of the kinds of resolution than is embodied in these descriptions can be given, and indeed that at least five different divisions can be made. These they enumerate as the one just mentioned, namely, (1) the division into real and cognitional, and then add the others: (2) into practical and speculative; (3) into metaphysical, mathematical, and logical; (4) into proper and improper; and finally a special type that stands by itself, (5) suppositional resolution. Of these the third division is directly pertinent to our concerns and deserves separate treatment on that account. Vallius-Carbone first characterize the difference between metaphysical and mathematical resolution as follows:

Metaphysical resolution is simply the reduction of sensible things to intelligibles, say, of bodily beauty to beauty of the soul, and of beauty of the soul to God. It is a type of vertical resolution that moves from terrestrial realities to the first and ultimate cause of all. Mathematical resolution, on the other hand, is the reduction of a conclusion to first principles, or of a conclusion to premises, and of these to indemonstrable principles. Practically all of the sciences and arts use this form of resolution, and so it is called mathematical not from the subject matter but from the fact that it is most clearly seen in mathematics. Physics uses the same type when it resolves a sensible body into parts, elements, causes, and principles. Grammar does the same when it resolves a sentence into words, words into syllables, and syllables into letters. So does rhetoric in analyzing a speech into major parts, the exordium, the narration, etc., and these into their minor parts, and the latter into periods, members, and so on. Other arts are taught in a practical manner, and thus they emphasize composition rather than resolution of the mathematical type. [CA1va–2ra].

With regard to these two types of resolution, Galileo is aware of the existence of metaphysical resolution but does not think it necessary for one to have a perfect demonstration, which for him must be made to proper causes but not always to ultimate causes [D2.5.11–12]. What Vallius-Carbone explain here as mathematical resolution, on the other hand, functions for Galileo and for them as the paradigm of how resolution should be carried out in all the speculative sciences. The more specialized sense he attaches to mathematical resolution will be examined in the following subsection.

Turning then to logical resolution, we find Vallius-Carbone's account of it to be more complex. They subdivide this kind of resolution basically into two types: that of the consequence (*resolutio consequentiae*), that of the consequent (*resolutio consequentis*). For them "consequence" has the meaning of inference or illation and refers to the connection that permits one to deduce the conclusion from the premises, whereas "consequent" designates the conclusion as opposed to the "antecedent," the premises from which it is deduced. As will be seen in their descriptions and in what follows, the first applies to the type of resolution treated in the *Prior Analytics*, the second to the type treated in the *Posterior Analytics*.

Resolution of the consequence: This is the reduction of the illation of an argument to the first principles on which it depends, namely, the universal rules of correct inference, such as the "said of all" (*dici de omni*) and the "said of none" (*dici de nullo*). This happens when one shows an argument to have first principles that are arranged properly according to figure and mode; thus, if one wishes to show that the following syllogism has a proper consequence, "Every animal is living; some bodies are animals; therefore some bodies are living," one must reduce it to the first principles that govern consequence, and from this make manifest its correctness.

> *Resolution of the consequent*: This is the resolution of what is inferred in an argument to its premises on the basis of the matter of which the conclusion or consequent is composed. This is done when one shows that an argumentation proceeds from premises that are true, causes of the conclusion, prior and more known than it is, and so on.... [CA1vb–2ra].

In the foregoing paraphrase only a small portion of the explanation of resolution of the consequent in the *Additamenta* is given. The fuller treatment there is of special interest, however, because it shows in detail how Vallius-Carbone would resolve the conclusion that man is risible, or capable of laughter, to its proper causes, an example used by Galileo when discussing related matters in D2.2, D2.3, D2.4, and D2.6. Actually they supply two examples, the first of which is simpler, concerned with man's corruptibility, and so can serve to introduce the second, concerned with man's risibility:

> Were one to construct the following demonstration, "Every animal is corruptible; every man is an animal; therefore every man is corruptible," further resolution would be needed because in this construction the proximate cause of man's being corruptible is not given. A resolution that would display the proper cause might be the fact that man is composed of contraries, and this should then be inserted into the minor premise as an additional middle term [CA1vb].

The further resolution they mention here is what logicians refer to as a densification of middle terms. The conclusion sought involves the subject ("man") and the predicate ("corruptible"). Between "man" and "corruptible" the first middle is "animal"; when inserted, this yields the series "Man / animal / corruptible." Vallius-Carbone recommend "composed of contraries" as a more proximate cause of corruptibility, and thus introduce that expression as an additional middle, in effect giving the new series: "Man / animal / composed of contraries / corruptible." (A modern revision might insert "composed of elements" between "animal" and "composed of contraries," since it is an animal's elemental composition that explains why its body can be decomposed through physical or chemical action and in that sense is corruptible.)

Vallius-Carbone then move on to the more complex example that occurs in Galileo's notes:

> The ideal of scientific knowing is not attained unless one is able to resolve a conclusion to all of its proper principles or middles. On this account, a person might be said to have perfect knowledge of man's being risible or capable of laughter if he knows that man is capable of wonder, and the latter because he is capable of discourse, and the latter in turn because he is rational; in this way all properties demonstrable of man can be shown to take their origin from his rationality. Thus, if one were to use the following demonstration, "Every animal

> capable of wonder is capable of laughter; every man is an animal capable of wonder; therefore every man is capable of laughter," the demonstration would be correct by reason of its form but not sufficient by reason of its matter. Further resolution is required because, as it stands, the demonstration does not make clear the connection between being capable of wonder and being capable of laughter. An additional middle or proximate cause should be inserted into the major premise, for example, the surprise element from which laughter arises when one is caught off guard; again, an additional cause should be added to the minor premise, namely, the recognizing of an effect before its cause, for it is this that gives rise to wonder. And, to proceed yet further, the reason why a man recognizes an effect before its cause is because he is a rational animal and cannot know except through discourse, and so his rationality is the radical first cause of all the other attributes occurring in the demonstration [CA1vb–2ra].

At this point they add the remark that one can see from this why an angel, who grasps knowledge in a single intellectual act, has no need of resolution (see Galileo's F3.1.16). Humans, being discursive, must densify middle terms between a subject and a predicate if they are to attain scientific knowledge of the conclusion it states (also note here the references to "human sciences" in F3.1.9, 16, 18). Vallius-Carbone thus propose the following complete resolution to make clear the connection between man's rationality and his risibility: "Man / rational animal / capable of discourse/able to recognize an effect before a cause / capable of wonder / able to be surprised / capable of laughter." Such considerations, in their view, make obvious the need for a book such as the *Posterior Analytics*, which explains why and how resolution of the consequent or resolution by reason of matter should be effected.

b. *Mathematical Resolution.* When explaining metaphysical resolution, as we have seen, Vallius-Carbone differentiate it from mathematical resolution, and set the latter up as the paradigm for practically all the sciences and arts. These disciplines conform to the ideal when, in their respective subject matters, they reduce conclusions to first principles, or conclusions to premises, or premises to indemonstrable principles. While citing examples of how this is done in physics, grammar, and rhetoric, however, Vallius-Carbone do not specify precisely how resolution is effected in mathematics. Possibly they avoid such exemplification because of disputes current in their day over the existence of true causes in mathematics [see Sec. 3.4c below], or, less probably, over an enigmatic prescription given by the Greek mathematician, Pappus, for the use of resolution and composition in that discipline.[12]

Galileo himself provides a convenient point of entry into Pappus's teaching, doing so in his *Dialogo* of 1632. There, during the first day of discussion, he has Salviati correct Simplicio's account of how Aristotle had arrived at the far from obvious conclusion that the heavens are immutable. Simplicio maintains that Aristotle first laid the ground for his arguments *a priori*, showing the unalterability of the heavens by means of self-evident principles, and that he afterward established the same conclusion *a posteriori*, arguing from sense knowledge and the testimony of the ancients [GG7: 75]. To this account Salviati replies:

> What you refer to is the method he used in writing his doctrine, but I do not believe it to be that with which he investigated it. Rather, I think it certain that he first obtained it by means of the senses, experiments, and observations, to assure himself as much as possible of the conclusion. Afterward he sought means to demonstrate it. That is what is done for the most part in the demonstrative sciences. This comes about because when the conclusion is true, one may by use of a resolutive method hit upon some proposition which is already demonstrated, or arrive at some self-evident principle. But if the conclusion is false, one can go on forever without finding any known truth – if indeed one does not encounter some impossibility or manifest absurdity. And you may be sure that Pythagoras, long before he discovered the proof for which he sacrificed a hundred oxen, was sure that the square on the side opposite the right angle in a right triangle was equal to the squares on the other two sides. The certainty of a conclusion assists not a little in the discovery of its proof – meaning always in the demonstrative sciences [GG7: 75–76].

Galileo's statement here, insofar as it applies to a true conclusion, coheres well with what has just been said about logical resolution, for the entire purpose of such resolution is to show how a conclusion can be shown to be true by resolving it to principles of the kind specified. It is when Galileo goes on to consider the case where the conclusion is false that his exposition, as several authors have pointed out, reminds one of Pappus.[13] For, in his *Collectio mathematica*, Pappus discusses how resolution might lead to a false conclusion as well as to a true one. The relevant passage, which is somewhat enigmatic, begins as follows:

> Resolution, then, takes the thing sought as if it were admitted and passes from it through its successive consequences to something which is admitted as the result of composition; for in resolution we admit the thing sought as if it were already done and we look for that from which it follows, and again the antecedent of the latter, until by so working backwards we come upon something already known or having the status of a first principle, and such a method we call "resolution," as it were, the solution backwards.[14]

Thus far Pappus's account agrees fairly well with Vallius-Carbone's description of logical resolution, particularly if one takes such resolution to require several stages and thus to involve "successive consequences."

Pappus then goes on to say that there are two kinds of resolution, one theoretical and the other problematical, and, when discussing each in turn, brings up the possibility of the false conclusion. For theoretical resolution he discusses this possibility in the following terms:

> [I]f what is admitted is true, the thing sought will also be true and the proof will be the reverse of the resolution; but if we come upon something admitted to be false, the thing sought will also be false.[15]

For problematical resolution he states the parallel as follows:

> [I]f what is admitted is possible and obtainable, that is, what they call in mathematics given, what was originally proposed will also be possible, and again the proof will be the reverse of the resolution; but if we come upon something admitted to be impossible, the problem also will be impossible.[16]

Although both of these statements are cryptic, they clearly describe ways in which mathematicians go over their proposed proofs, which are usually quite complex, to ascertain whether a theorem has been established apodictically or whether a problem has actually been solved using classical geometrical methods.

There is little doubt that by the time he wrote the *Dialogo*, and even before that in his disputes with Ludovico delle Colombe around 1615, Galileo was acquainted with these passages from Pappus [cf. GG4: 521] The same passages, though probably not known to Vallius in 1588, were also known to Jesuit mathematicians, and it is interesting to note what sense they made of them. Joseph Blancanus, one of Clavius's students who was intent on showing how mathematical demonstrations conform to the norms of the *Posterior Analytics*, discusses it in his *Apparatus ad mathematicarum studium* of 1620. There he characterizes mathematical resolution not as a general method applicable to all the sciences and arts, as do Vallius-Carbone, but rather as a special method that assists one in finding geometrical demonstrations quickly and easily.[17] Earlier he had pointed out that geometers use basically two methods in their proofs, one direct, that of ostensive demonstration, the other indirect, that of reduction to the impossible[18]; at this point he turns to explaining how resolution and composition are useful for uncovering either. He begins by noting that Euclid himself defined the twofold process as follows:

> Resolution is the taking of something inquired about as conceded, through consequences that yield some truth already conceded.
>
> Composition is the taking of something conceded, through consequences that enable one to conclude or assent to what is inquired about.[19]

Blancanus notes here that the same definitions can be found in Proclus and in Pappus. He then explains what Euclid means by them:

> Resolution is a *discursus* in which we investigate the truth of a theorem or the solution of a problem inquired about in this way. If a theorem is involved, we accept it as true and conceded, if a problem, we assume it as solved; that is, we suppose the former to be true, the latter to be possible and already effected. From this supposition we proceed by reasoning from what we supposed to be true to deduce consequences until we come to something either true or false. If what we come to is true and conceded, this is an evident sign that what we supposed, that from which the conclusion follows, is either true or possible. Such a consequence is based on the logical principle: truth can come only from truth if the matter and form of the reasoning process are correct.
>
> When the truth is found in this way, we effect the composition of the demonstration in the reverse order. That is, we construct a demonstration of the thing inquired about in a compositive order, reasoning back from the truth we have found to the conclusion sought. And if we come to something false or impossible, this is an evident sign that what we inquired about is false or impossible. Such reasoning is based on the logical principle: falsehood can come only from falsehood if the matter and form of the deduction are correct.[20]

Blancanus concludes his explanation with the remark that it is easier to see the method in use than it is to describe precepts for its use, and refers the reader to Euclid, Apollonius, Archimedes, and Pappus for clear exemplifications of it.[21]

From his citations, Blancanus's knowledge of the way resolution and composition are used in investigating geometrical theorems and problems owes much to the teachings of Marinus Ghetaldus, a mathematician who, like Blancanus, was a student of Clavius and a friend of Galileo.[22] None of these authors, it should be emphasized, set this particular method in opposition to the resolutive method on which Aristotle's *Analytics* is based. For them it is a special method used by mathematicians because it is adaptable to the subject matters they treat; it is not a method of general applicability, and in fact is of little or no use in the physical sciences. Zabarella, who was acquainted with this method, offers precisely such an evaluation of it:

> This type of mathematical resolution, with which one goes back over demonstrations already made and resolves later theorems into prior theorems and ultimately reduces the latter to first principles, is more an exercise for a scholar than it is a resolutive method in the sense of which we are speaking. For it is a process from the less known to the more known that would be completely useless to a beginner who was trying to grasp a science, since it gives rise to no new knowledge. But we are now treating of the kind of resolutive method that brings forth, from things more known, knowledge of matters unknown. The latter kind has a proper place in sciences other than mathematics, and particularly in natural science, for there the principles to be used in demonstration are unknown to us because of the limitations of our knowing

> powers, and so, as unknown, we cannot start from them. Thus we are forced by necessity to resort to what is, as it were, a secondary way (*via*) – the kind of resolutive method that leads to the discovery of principles, so that, having found them, we may subsequently demonstrate natural effects [ZL267].

In light of Zabarella's critique it would be a mistake to equate, as Jardine has done, the type of resolution referred to by Pappus with Galileo's use of the demonstrative *regressus* in his scientific writings.[23] As analyzed in Chaps. 5 and 6 below, Galileo used the regress to discover physical causes, not to demonstrate theorems or to solve problems in geometry. It is possible, of course, in view of the fact that he worked mainly in a mixed-science tradition wherein geometrical methods are used to solve physical problems, that Galileo unknowingly conflated Pappus's method with that of the *Analytics*. The two are quite different, however, and the textual evidence adduced by Jardine for Galileo's having done so is insufficient to support the overarching thesis he attempts to draw from it.[24]

8. INVENTIVE VS. JUDICATIVE SCIENCE

To complete this treatment of resolution in the *Analytics*, a further question should be addressed, and this is Galileo's use of the expression "inventive science" in F2.2.5. Usually an inventive science is thought of as opposed to a judicative science, and in the terminology of Galileo's day the resolutory books, those of the *Analytics*, were seen as different from the inventive books, those of Aristotle's *Topics*. This terminology, while based on Aristotle, is actually not Aristotelian but derives instead from the Stoics. Fortunately the usage is explained in a portion of the *Praeludia* to the *Analytics* that was appropriated by Carbone in the *Additamenta* of 1597, in a question entitled "Why the resolutory books are said to be judicative, the topical inventive." The reply of Vallius-Carbone to this question is extensive, but it contains useful insights into the topics treated in this chapter and so can serve as their summary and recapitulation.

a. *Modes of Knowing*. To explain the "judicative-inventive" terminological usage, Vallius-Carbone return to their previous discussion of the operations of the intellect and introduce an additional distinction, one that should be made prior to the division into the three operations already discussed. This they now identify as a distinction between the act of knowing and the mode of knowing:

> In human knowledge two things are to be considered: the act of knowing and the mode of knowing. By reason of the first, two acts are attributed to the human intelligence, the perception or apprehension of a thing and the judgment of what has been perceived whereby we judge that it is such and so, precisely as we think we have perceived or discovered it. By reason of the second, that is, by reason of the mode of acting, three acts may be distinguished: the first is simple apprehension or abstraction, whereby one thing is known without another; the second, composition or division, whereby having compared the things apprehended we join them or separate them; the third, discourse, whereby from one thing known we come to the knowledge of another previously unknown [CA3va–b].

Note in this citation the references to "human knowledge" and to "human intelligence," which cast light on what Vallius-Carbone here mean by "acts of knowing" as opposed to "modes of knowing." Their logic is obviously preparing students for the study of theology as well as philosophy, and thus they must take into account not only human psychology but also what might be called angelic and divine psychology as well. In Thomistic theology God's intellect and angelic intellects operate differently from human intellects, since only the latter gains knowledge through the senses and reasons discursively; on this account only humans require logic to regulate their particular "mode of knowing."

Having made this distinction, Vallius-Carbone elaborate on it as follows:

> The two prior acts are found in every intelligent nature, but the difference of acts taken from the modes of knowing is found in man alone. Since, therefore, logic is concerned with directing the operations of the intellect in their modes of acting, they can be divided both according to the prior acts, which pertain to the substance of intellection, into inventive and judicative, and according to the three acts taken from the modes directing the first, second, and third operations of the intellect. The first division of human acts with respect to their substance pertains not to logic but to psychology; the second, however, pertains to logic, since its function is to teach the modes of apprehending, composing, and reasoning. Again the divisions differ in that the two acts that are placed in the first division are found in every operation of the intellect, since in all areas in which the three operations are directed there is need for comprehension and judgment; but the acts taken from the mode of knowing are directed to the individual operations [CA3vb].

Note in the first sentence here the reference to "every intelligent nature," meaning by this God, angels, and men, and then the reference to "man alone," which restricts the scope of consideration. This new distinction thus casts further light on Galileo's references to "human sciences" mentioned earlier in this section, to be detailed more fully in Sec. 3.2.

b. *Topical Reasoning.* Having thus explained the *Analytics*, Vallius-Carbone now consider the way in which topical reasoning is characteristic of inventive or investigative science, and so is quite different from the type of reasoning just treated. Basic to their understanding is the distinction between necessary matter and probable matter, a distinction to be examined in more detail in Sec. 3.5a. Here they first identify the difference between the two matters:

> Since the human intellect, while it is joined to the body, is often unable to penetrate the natures of things and to perceive their intimate principles and necessary connections between causes and their effects, it must frequently be concerned with surface details and with extrinsic circumstances, and on this account makes use of probable rather than necessary arguments; whence there arises another type of matter found in arguments, namely, probable matter [CA4ra].

They then further elaborate on this difference, pointing out that the two types of matter differ again in two ways. The first way focuses on how they are oppositely related to judgment and invention, which they explain as follows:

> First, necessary matter, because it is ordered to science, which is one and certain in the sense that the demonstrator accepts only one part, namely, the true part, requires judgment lest he err in giving assent to the truth; it does not need invention, since there is but one middle term by which the conclusion is demonstrated. Probable matter, on the other hand, being arguable on either side, does not generate certain assent, and it is licit to use many middle terms in treating it, and on this account it needs invention rather than judgment. Nor is there need for judgment in those things in which assent is not drawn necessarily to one part, since in probable matter either part may be true or false [CA4ra].

The second way follows from this, for it makes necessary matter the basic concern of the sciences, leaving probable matter exclusively to dialectics:

> Second, necessary matter is left completely to the sciences, and logic has been separated from this domain since it reserves for itself only the judgment of instruments of knowing, as we have said. But probable matter, not being appropriated by any art or science as its proper consideration, since it is found in all disciplines, has been taken on by dialectics. This is not an art distinct from other arts, since it claims for itself only the treatment of probable matter [CA4ra–b].

With this division of labor established, Vallius-Carbone further explain how the *loci* or *topoi* that are discussed in Aristotle's *Topics* actually function in an inventive discipline such as dialectics:

> From this it happens that dialectics takes "places" (*loci*) or probable propositions from things

> themselves and expounds them generally and with certainty; using these, we are able to discourse about individual matters using probable propositions. And since the *Topics* is occupied with this invention of places and with method, it is said to be inventive and not judicative. Therefore logic is a faculty apart from the things of which science treats, whereas topics or dialectics is not; but since it has no definite place among the sciences, it has been rightly joined to logic, with which it has some affinity [CA4rb].

In their view, therefore, logic is an instrumental habit or faculty whose focus is on scientific reasoning and the necessary matter it requires; as such it stands apart from the sciences so as to be able to pass judgment on them. Dialectics, on the other hand, remains immersed in the matters of the particular sciences, helping to discover arguments bearing on those matters, but unable to come to apodictic conclusions concerning them. In this way it has some affinity with logic although, like rhetoric, it is a faculty that is basically different from logic. Thus Vallius-Carbone conclude:

> From what we have said one can gather that, although Aristotle nowhere distinguishes logic into judicative and inventive, nonetheless his analytical books can be called judicative and his topical inventive, as can be understood from their inscriptions. For the *Analytics* were instituted to this end, that we be enabled to bring a necessary judgment on the form and the matter of an argument, the *Topics* for this, that we may know how we can easily find arguments for discoursing with probability on any subject matter. With reason, therefore, is the former called "judging" and the latter "inventing" [CA4rb].

The foregoing is obviously an exhaustive analysis of the ways in resolution are employed in the *Analytics* and why it is called a judicative science, as opposed to an inventive science of the type explained in the *Topics*. Noteworthy throughout Vallius-Carbone's exposition is the clearcut distinction they maintain between logic and dialectics, a distinction Carbone will also extend to rhetoric in his many writings on that discipline. The principal goal of logic, as explained above in the sections on the nature of logic and its instruments, is scientific knowing; on this account Vallius-Carbone are at pains to differentiate it from dialectics, whose goal is mere opinion, and also from rhetoric, whose goal is simply persuasion. An individual who seeks only opinion or persuasion clearly has no need for the detailed material set out in the two volumes of Vallius's *Logica* of 1622. On the other hand, anyone who seeks the more demanding type of knowledge known as science would be foolish not to acquaint himself with the logical instruments explained in that work. That is why a logical methodology becomes a practical necessity for anyone intent on being a scientist in the Aristotelian sense, as explained above in Sec. 1.5.

NOTES

[1] On this usage, see Henry Veatch, *Intentional Logic: A Logic Based on Philosophical Realism*, New Haven: Yale University Press, 1952; also his *Two Logics: The Conflict Between Classical and Neo-Analytic Philosophy*, Evanston, Ill.: Northwestern University Press, 1969. The first of these acknowledges Veatch's use of the *Ars Logica* of John of St. Thomas (ix), the name under which John Poinsot was known in the Dominican Order; see the following note.

[2] The expression "mind-dependent" as a proper translation of *ens rationis* has been suggested by John N. Deely in his translation of *Tractatus de signis: The Semiotic of John Poinsot*, Berkeley, Cal.: University of California Press, 1985, 548–551. Poinsot's logic is later than Vallius's, but its general orientation is the same. The only work of this type now available in English, Poinsot's treatise may be consulted with profit by those desiring a fuller understanding of late scholastic logic. Other portions of the *Ars logica* have been translated by Y.R. Simon, J.J. Glanville, and G.D. Hollenhorst, *The Material Logic of John of St. Thomas*, Chicago: University of Chicago Press, 1955.

[3] To date the best systematic analysis of Galileo's logic is that of Maurice A. Finocchiaro, *Galileo and the Art of Reasoning: Rhetorical Foundations of Logic and Scientific Method*, Dordrecht-Boston: D. Reidel Publishing Company, 1980. Most of the analysis bears on Galileo's *Dialogue on the Two Chief World Systems*, some samples of which will be seen in Secs. 5.5 and 5.6 below. While the texts Finocchiaro analyzes are historical texts, and while he is aware that Galileo knew Aristotelian logic, he himself does not use Aristotelian logic in his analysis. The limitations this imposes on his work are discussed in the author's review of it in the *Journal of the History of Philosophy* 20 (1982), 307–309.

[4] The full title of this work is Ludovicus Carbone, *Introductio in logicam sive totius logicae compendii absolutissimi libri sex*, Venice: Apud Ioannem Baptistam et Ioannem Bernardum Sessam, 1597. For ease of citation references to it are inserted directly into the text, using the abbreviation CL followed by page number(s).

[5] The titles of these works read as follows: Ludovico Carbone, *Additamenta ad commentaria D. Francisci Toleti in Logicam Aristotelis: Praeludia in libros Priores Analyticos; Tractatio de Syllogismo; de Instrumentis sciendi; et de Praecognitionibus, atque Praecognitis*, Venice: Apud Georgium Angellerium, 1597; references to it are likewise inserted directly into the text, using the abbreviation CA followed by folio number(s). Paulus Vallius, *Logica Pauli Vallii Romani ex Societate Iesu, duobus tomis distincta*, Lyons: Sumptibus Ludovici Prost haeredibus Rouille, 1622; each volume of this is referenced separately, using the abbreviations VL1 and VL2, followed by page number(s). Ludovicus Carbone, *Introductio in dialecticam Aristotelis per Magistrum Franciscum Toletum Sacerdotem Societatis Iesu, Philosophiae in Romano Societatis Collegio Professorem. Additis in eandem introductionem praeludiis, eiusdem introductionis Tabulis*, Venice: Apud Paulum Meiettum, 1588; this too is cited in the text, using the abbreviation CT followed by folio number(s).

[6] The paraphrases are identified as such, so that the reader will not mistake them for translations. Generally they follow the text literally, except that some expressions are abbreviated or omitted (without indicating ellipses), and punctuation has been changed more liberally than it would be in a normal translation.

[7] Those acquainted with scholastic psychology will recognize that Vallius-Carbone here do

not enumerate the estimative or cogitative sense as a fourth internal sense distinct from the other three, as does Thomas Aquinas, but rather include its functions under that of the imagination or *phantasia*, thus staying closer to the text of Aristotle. Other variations from the Thomistic account, as will be seen below, are their teachings that the agent intellect and the receptive intellect in man are not two in number but only different activities of the one intellect, and that the intellective memory is a power distinct from the intellect itself. These teachings undoubtedly derive from Toletus's *Commentaria una cum questionibus in III libros de anima*, first published at Cologne in 1575 and often thereafter. For a brief account of Toletus's teachings on the soul and how it compares with those of other Renaissance commentators, see Eckhard Kessler, "The Intellective Soul," *The Cambridge History of Renaissance Philosophy*, eds. C.B. Schmitt et al., Cambridge: Cambridge University Press, 1988, 511–512.

[8] Thomas Aquinas, *Summa theologiae*, Prima pars, qq. 84–85; for an English translation, with notes and appendices, see St. Thomas Aquinas, *Summa theologiae*, 60 vols. ed. Thomas Gilby, New York: McGraw-Hill, 1964–1976. Vol. 12. *Human Intelligence*, tr. P.T. Durbin, 1968.

[9] Meaning by this the natural light of reason, and poorly translated by Drake as "my good sense," though this conveys the general idea – Galileo Galilei, *Two New Sciences*, tr. Stillman Drake, Madison, Wis.: The University of Wisconsin Press, 1974, 162.

[10] It is at this point, of course, that empiricists and idealists would later part company with the type of realism being explained here. The empiricist became a skeptic: he takes the position – fatal from an Aristotelian viewpoint – that he knows sensations or concepts, not things, and so he has doubts about any natures to which these intentions might correspond. The Kantian would replace that skepticism by agnosticism: not wishing to deny that things have natures, he maintains that he cannot know them but only their sensible appearances, and this – an Aristotelian might critique – because of some quirk in his psychological makeup. Galileo was certainly not agnostic in the Kantian sense, and he did not subscribe to the empiricist brand of skepticism either. For the most part his was a natural realism; he trusted in his ability to know things as they are, to grasp their natures in some way and possibly to define them, and, if so, then to demonstrate properties flowing from those definitions in the accepted scientific fashion. See the comments in the Epilogue.

[11] Note, in the second paragraph, the statements that the final cause of logic "is not demonstration alone" and that it is not "to distinguish the true from the false," contrary to positions that were held by Zabarella, as will be made explicit below.

[12] We say "less probably," because at the time Vallius was preparing the logic lectures of 1588, on which Carbone's *Additamenta* is based, Pappus's work had just been published in Latin translation and in all probability was not available to Vallius.

[13] Notably Jardine, "Galileo's Road to Truth," 306, 315, and, following him, Wisan, "Galileo's Scientific Method," 29; both cite the reading of Pappus provided by J. Hintikka and U. Remes, *The Method of Analysis: Its Geometrical Origin and Its General Significance*, Dordrecht-Boston: D. Reidel Publishing Company, 1974, 8–9.

[14] This translation is a redaction based on that of T.L. Heath, cited by Jardine, 306, but incorporating emendations made by M.S. Mahoney, in his "Another Look at Greek Geometrical Analysis," *Archive for History of Exact Sciences* 5(1968–1969), 322. We have here rendered analysis by resolution, synthesis by composition, in accord with the Renaissance Latin tradition.

[15] Following Heath and Mahoney, replacing analysis by resolution.

[16] Again following Heath and Mahoney, replacing analysis by resolution.

[17] See his *Apparatus ad mathematicarum studium*, Bologna: Typis Sebastiani Bonomii, 1620, 411.

[18] Blancanus, *Apparatus*, 400.

[19] *Apparatus*, 412.

[20] *Apparatus*, 412.

[21] *Apparatus*, 412.

[22] Ghetaldus's analyses will be found in his posthumous *De resolutione et compositione mathematica*, Rome: Ex Typographia Reverendae Camerae Apostolicae, 1630.

[23] In this connection, see Jardine's "Galileo's Road to Truth" (295–303), where he references many of Zabarella's texts but seems to have missed the text just cited.

[24] His main textual support is Galileo's discussion in the *Dialogo* to which reference has been made at the beginning of this section [GG7: 75–76]; to this he appends another text, a remark made by Salviati later in the *Dialogo* [GG7: 434–435], which is amenable to the same interpretation we have given the first (304–306). A subsidiary text, which Jardine imputes to Galileo (304–305), is actually taken from Benedetto Castelli's critique of Ludovico delle Colombe's treatise on floating bodies [GG4: 521]. Jardine admits that, while much of the manuscript for Castelli's critique is in Galileo's hand, this particular passage is not (305 n. 62). The fact that Castelli was a mathematician more than a philosopher could easily explain why his explanation of resolution would more resemble Pappus's than the kind of *regressus* explained in Galileo's D3.3.

CHAPTER 3

SCIENCE AND OPINION AS UNDERSTOOD IN MS 27

As has already been stressed, the notion of science that was current when Galileo began teaching at the University of Pisa in 1589 is quite different from that of the present day. Its predominant characteristic was its very stringent requirements for certitude and infallibility, requirements that set it apart from opinion, which results from probable reasoning and is revisable whenever more plausible arguments become available. In view of Galileo's claims for having achieved science and not merely opinion in the areas of his investigations, it is important to be clear on precisely how he understood these terms.

No treatise on science is preserved among the materials extant in MS 27, although such a treatise may have existed at one time. Fortunate it is that Carbone plagiarized this treatise from Vallius's lectures of 1587–1588 and published it in his *Introductio in universam philosophiam* of 1599. A similar treatise is to be found in Vallius's *Logica* of 1622, where it is reworked in fuller detail. These two works, both of which also treat opinion, are our basic sources for reconstructing Galileo's early views on science and so for gaining a better understanding of his remarks concerning it in MS 27.

So as to provide a framework in which these remarks may be situated, and drawing on the extensive materials preserved from Galileo's exemplar, we first discuss the nature and origin of the sciences, their various classifications, comparisons, and subalternations, and then in more detail the mathematical and the mixed sciences. We next turn to opinion, the type of knowledge generated by dialectics and opposed to science in the strict sense. Here we explain its nature and kinds and their precise relation to science; how it employs the dialectical syllogism and its middle term, the topic; and then the various kinds of topic, including that of antecedents and consequents, which is most similar to the HD method of modern science explained in Chapter 1. We conclude with a summary exposition of the nature of rhetoric and a discussion of how it may be used as an adjunct, along with dialectics, to induce assent to conclusions that cannot be established with apodictic proof.

1. THE POSSIBILITY, ORIGIN, AND CAUSES OF SCIENCE

Vallius-Carbone begin their treatment of science in the *Introductio in universam philosophiam* with an overview of the various meanings of *scientia* so as to restrict it to its most precise technical sense. Such an overview is required in view of Aristotle's own usage, for in some places he takes its Greek equivalent, *epistēmē*, in a very broad sense, in others, in a very narrow sense, and in yet others, in various senses intermediate between the two. Vallius-Carbone discern five different meanings in the Aristotelian corpus, which they characterize as follows: sometimes science is there taken to mean knowledge generally; sometimes it designates an intellectual habit that includes opinion and faith; sometimes, a habit more restricted in scope but still one that includes the understanding of principles along with conclusions deducible from them; sometimes, a habit that similarly includes art; and finally, the most restricted sense, a habit that simply employs causal reasoning to deduce universal and necessary conclusions about its subject matter. It is this last sense in which Vallius-Carbone are interested [CP121–122].

They next make further precisions about the way the term can be understood even in this sense. It is possible, they say, to take science to be the act of reasoning in which such conclusions are grasped, or the habit generated by repeated acts of this kind. They intend to focus on the second of these. Even then, the habit thus generated may be considered as a real entity that can be located in one of the ten categories, or as having a particular relationship to the intellect, which in turn may be threefold. One relationship is that of dependence on the intellect as its efficient cause and the matter or subject in which it inheres; another is that of being perfective of the intellect; and yet another is that of being effected in the intellect by demonstration, since a demonstration is a syllogism that is productive of science. A further complication arises from the fact that the person who possesses a particular science may have other interests and may intermingle matters that pertain to other disciplines with those of that particular science [CP122–123].

Taking into account all of these factors, one can treat science in a number of disciplines, in a variety of ways, and from different points of view. The concern of the logician, which is Vallius-Carbone's main interest, is thus different from that of the metaphysician, the psychologist, or the ethicist. They describe it as follows:

> To treat of science as it is an effect of, and the end of, demonstration is the function of the logician, and on this account Aristotle wrote much about it in the first book of the *Posterior Analytics*. For, since logic treats the instruments for acquiring science, and teaches what science itself is and what things are required for its attainment, logic is necessarily concerned with science. Add to this that the logician's task is to investigate the nature of demonstration and elaborate its various properties, which are taken from its end, namely, science. Therefore it must consider science even more than it must consider demonstration [CP124].[1]

a. *Possibility*. The next question Vallius-Carbone address is whether science in this strict sense is possible, a question that is still agitated in the present day. Whereas the modern problematic is largely based on Humean and Kantian theories of knowledge, to which reference has been made above [Sec. 2.3b, n.10], their sixteenth-century problematic focused instead on objections against science that were urged by the Aporetic Academics, that is, the Skeptics. Several of the difficulties they raised were identified by Carbone in his discussion of impediments to knowledge, as we have already seen [Sec. 2.1]. Now Vallius-Carbone take a closer look at "the foundations the Skeptics invoke when they say that we cannot understand and be certain of anything, and in this way destroy all science" [CP126].

The aporetic objections they enumerate may be summarized as follows: [1] human teaching is said to take its origin from the senses; but the human senses are dull, frequently deceive us, and represent things other than they really are; [2] not only do things sensed externally deceive us, but so do those perceived internally, for they seem true to us but are not – e.g., objects seen by sleepers, drunkards, and the insane; [3] the objects we apprehend are very similar to each other, and because of their similarity we cannot differentiate one from another, nor can we affirm that one thing really is not another; [4] the variable states of objects, changing as they do all the time, prevent us from apprehending them in an unchanging way; [5] the natures of things are hidden from us and our senses are unable to penetrate into them, and yet we are supposed to come to knowledge of them through the senses; and finally, [6] things themselves do not reach the human intelligence, but only their appearances and representations, and these are changeable and flexible; as a consequence our intellect understands nothing, or what it understands is quite different from things themselves [CP126–127]. What is interesting about these difficulties is that many of them anticipate problems that were later to engage the founders of modern philosophy. Several are also included in the "second impediment" to learning sketched in Carbone's introduction to Toletus's little volume already referred to [Sec. 2.1c].

Having enumerated these difficulties, Vallius-Carbone then detail various suppositions required for rational discourse about knowledge and briefly summarize the teaching on intentionality explained in the previous chapter [Sec. 2.3]. Here they focus on sense knowledge, detailing how the sense powers operate naturally and discussing problems of visual perception such as the bent oar and the colors seen in a pigeon's neck. Their point is that a basic truth is to be found in the senses, provided they are healthy, properly proportioned to their objects, and not obscured by obstacles or other impediments [CP129–130].

They next address the problem raised about the variability and changeability of the objects of experience. This provides the opportunity to explain further the realist epistemology on which their notion of science is based:

> Things that are subject to change, as long as they exist, never change so radically that their nature does not remain the same. Precisely as individuals, either they exist as such or they pass out of existence. It is thus false to hold that, if changed in the slightest way, they are no longer the same and do not have the same properties; experience contradicts this, as is apparent in the life of any animal. And since we see properties remaining the same in things that are changing, and know that these properties flow from their natures, we can affirm that their natures remain the same also. On this accounting all of the changes we discern in nature arise from the characteristics of individuals that come to be and pass away. But science is not concerned primarily and essentially with such characteristics; rather it is concerned with natures considered universally, in abstraction from their individuality. The intellect, moreover, does not err when considering things universally provided it perceives their proper formalities; these do not include the notes of individual things and so can be considered in separation from them. Nor does it follow, from the fact that we grasp such natures through their species and representations, that we do not know them but only their images. The species or representations of things are not what we know; rather they are the means whereby we know the things themselves. When we see a white object, we do not see the species but rather the object by way of the species [CP130–131].

Note the resonances in this passage with Galileo's statements in F3.1.14 and D2.12.15 that science does not consider individuals but rather abstracts from them so as to attain universal knowledge.[2]

Following this, Vallius-Carbone enter into an extensive discussion of the liar's paradox and various forms of self-contradiction that skeptics encounter when attempting to make knowledge claims of any type. This leads to their main conclusion, which affirms the possibility of science, after which they return to the six aporetic objections presented at the beginning of this subsection. Their replies to them, keyed by number to the arguments themselves, may be paraphrased as follows. (1) When conditions are present for sensing properly the senses are not deceived but are quite certain; and if occasionaly they are deceived, with their help and that of reason their functioning can be understood and the error corrected. Moreover, granted that the senses are incapable of complex truth, simple truth is still to be found in sense knowledge. (2) Persons who are asleep or drunk or mad have uncertain and changing sensations because of the unstable way their sense powers react to things sensed. (3) No one thing is so much like any other that it cannot be distinguished from it through some individuating characteristic, provided one uses the proper care. (4) As already explained, there are ways in which things can be seen as unchanging and so can yield science when considered universally. (5) We enter into the hidden natures of things not through our senses but through our intelligence; the senses, however, help by furnishing their properties, and these assist us greatly in grasping their natures. If this argument proves anything it is only that detailed knowledge of natures frequently escapes us, not that nothing can ever be perceived in a universal way. (6) As likewise explained, we do not know the species of things but rather the things that the species represent [CP135–136].

Note, in the reply to the first argument, the reference to simple and complex truth, which is related to the discussion of active and passive truth in Sec. 2.2b and is further explained by Galileo himself in D2.1, a matter to be treated in the next chapter. The reply to the fourth argument stresses the importance of abstraction and universality as essential to the concept of science here being defended, already noted as present in Galileo's F3.1 and D2.12. That to the fifth argument sets up the need for the demonstrative *regressus*, to be explained in detail in Chapter 4. And that to the sixth contains Vallius-Carbone's implicit rejection of theories of knowledge later to be proposed by Hume and Kant, to which reference has already been made in Sec. 2.3b.

b. *Origin and Causes.* In Galileo's day Platonism and Neoplatonic theories of knowledge were still regarded as live options by many, and thus it is not surprising that the treatment of the origin of human science in Vallius-Carbone focuses on two schools of thought, one maintaining that ideas are innate in man and the other that they are acquired by each individual in his lifetime. Within the first school they locate Plato, with his doctrine of *reminiscentia* or remembrance, and Avicenna, with his theory of a *dator formarum* or "giver of forms"; within the second they locate some Christian thinkers who hold that men acquire science through their own intelligence but require a special illumination from God to do so, and then oppose them to Aristotle, who does not insist on such illumination. They adopt the Aristotelian position, which they state as follows: "Human science is acquired through natural powers and through the natural light [*lumen naturale*]; thus, generally speaking, it is not necessary to seek any extrinsic cause" [CP141]. Again the natural light of the intellect, to which attention has been called in the previous chapter, assumes importance in their exposition. Note also that they are here speaking of "human science," an expression used by Galileo in F3.1.9, F3.1.16, and F3.1.18. This implies the possibility of an "angelic science" and a "divine science," touched on in F3.1.16 and D2.2.2, and also to be explained in what follows [Sec 3.2].

Having argued that science is possible, and having rejected that it arises in man by nature or from an extrinsic cause such as God, Vallius-Carbone proceed to specify the particular causes whereby it can be acquired naturally. They first introduce a distinction that will assume importance in what follows, that between actual and habitual science, touched on by Galileo in F2.3.1 and mentioned explicitly by him in D1.1.2:

> Science can be taken in two ways: first, for the act by which we know something, and by the knowledge or knowing of the thing itself, and this is called actual science; second, for the habit and quality acquired through the act and that inheres in the intelligence, and this is called habitual science. The former is present only when we are thinking; the latter perdures in us and with its aid we can cogitate whenever we wish. Again, science can be taken in two other ways: either for the simple apprehension of a thing whereby we know the thing itself; or for the assent we give when we make a particular negation or affirmation about the thing apprehended. For the present, the term science is taken in a special sense meaning the habit and the knowledge that is accompanied by assent, and about this there is a difficulty about the efficient cause that produces or can produce it, and also how it does so (IP149–150].

This leads them to an enumeration of all four of science's causes, beginning with the first three, preparatory to focusing on its efficient cause:

> For science, as for other things, there are four producing causes: matter, form, end, and agent. The matter of science is twofold: one is what it treats, the matter that constitutes the subject of the science, and if this is taken generally it is called the remote matter, if taken specifically, the proximate matter; the other is the subject in which the science inheres, and this is the human intellect, which is said to be a knowing intellect and an intellect in act. Its formal cause is that it is a kind of habit and quality; this cause will be explained more fully in the tract on the definition of science [Sec. 3.2 below]. The end is the knowing and perceiving of the subject matter [CP150].

The problem with the remaining cause, the efficient, they continue, is that here many causes can be assigned: God or an intelligence, one's teacher, one's own intellect, or simply one's knowledge of first principles. Without wishing to reject the first of these causes and the implied possibility of a revealed science, Vallius-Carbone focus now on science that arises from natural causes. For this, they proceed to argue, one's own intellect, and this alone, is the proper efficient cause of science.[3]

To explain this Vallius-Carbone set up an analogy between sense knowledge and intellectual knowledge:

> Anywhere there is a power that is indifferent to receiving or acting, there must be something that determines it to this or that reception or action; again, an efficient cause must be assigned for its particular determination. And since the human mind is a nude power, it needs an extrinsic form, called an intelligible species, to determine it in this way. This is necessary, for, just as objects that fall under the senses are sensed through sensible species, so also those that come to the intelligence require a species if they are to be understood [CP150–151].

The deeper problem, then, is what causes this intelligible species (i.e., the concept), since it itself is an agent in producing science. Their reply is that, apart from the concept and the object it represents, the agent intellect and the phantasm or percept are also agent causes in the origin of science:

> First, intelligible species or concepts are required for intellectual knowing, and these remain afterwards in the memory, which is a power not really distinct from the intellect; yet the intellect cannot understand through them without reverting to phantasms or images so as to terminate its understanding in a particular way.
>
> Then the agent intellect is necessary. This is not really distinct from the receptive intellect, not does it understand by itself; rather, intelligible species are produced by its light, with phantasms concurring objectively in their production. This light is related to the intelligible species as the light of the sun is to colors. Thus the concept is not produced in the phantasm as in a subject, nor is it a cause only of appearances, making universals appear in the phantasm; rather, while illuminating the phantasm the agent intellect efficiently produces the concept itself, not only extrinsically but also intrinsically, and causes it to exist in the receptive intellect. On this account the agent intellect alone does not produce the concept,that is, without the phantasm and the object, but does so along with the illuminated phantasm, and for this reason the object itself is said to act on the intellect [CP151–152].[4]

From these considerations they conclude to the following requirements for the production of scientific knowledge: the *terminus a quo* must be the absence of such knowledge and the *terminus ad quem* its acquisition; the transition from the first to the second must be effected by rational discourse; there must be a subject in which this change takes place, namely, the soul; and finally, there must be the efficient cause, namely, the intellect, as well as the thing to be known, the object, whence it follows that the object itself is the end of the knowing activity [CP153–154].

Vallius-Carbone then elaborate more fully on the way in which a science is acquired by the intellect's own natural process of discovery. They explain this as follows:

> The natural way of acquiring a science originates with the senses, from the natural light (*a lumine naturali*), and from first principles as from little sparks (*igniculis*). And since the basic notions and first principles that exist in the intellect depend on experience, and this in turn on sense knowledge, it can truly be said that sense knowledge is the first principle of human doctrine. Aristotle put it well: Nothing in the intellect that was not first in the senses. For from sense knowledge we gain the idea that a whole is greater than its part, since at one time or another we have perceived a whole and its parts through the senses. And finally, since the natural way of gaining knowledge is from things that are more known, and sense perceptions are of this kind, it is correct to say that all human knowledge takes its origin from the senses [CP154].

But here, they continue, an objection may be raised from Aristotle himself, who states that first principles are known either from sense, or from induction, or from custom, and thus it would appear that not all human science derives from the senses. (Observe that this teaching on first principles is found in Galileo's treatise on foreknowledge, at F2.1.4, F2.4.4, and implicitly in F3.2.3.) To this difficulty Vallius-Carbone reply that the three modes of knowing first principles are not enumerated so as to exclude sense knowledge from the other two, since induction is made from singulars that are apprehended by sense, and custom likewise derives from sensed instances [Sec. 4.6]. Thus both involve sense knowledge and more besides, and so all science takes its origin from the senses [CP154–155].

2. THE NATURE AND ATTRIBUTES OF SCIENCE

The material that follows at this point in Vallius-Carbone's treatise is devoted to the nature and attributes of science, in the course of which they explain what science is and how difficulties concerning its definition can be resolved. One of their main concerns is clarifying the ontological status of habitual science: they note that some see this as a quality, a type of habit acquired through repeated acts of scientific knowing, whereas others see it as the species or concepts themselves that persist in the intellect following such acts. The former group would include species or concepts under the habit, but as something subordinated to it; the latter would say that the species themselves count as the science without any superadded quality being required [CP157–158].

To resolve the problem Vallius-Carbone argue that sciences when viewed as habits are qualities, that when viewed as acts of knowing they are causal knowledge of particular conclusions, and that when viewed more precisely as qualities they are reasoning habits concerned with necessary matters. Their main conclusion they state as follows:

> Formally speaking sciences are not intelligible species or concepts themselves, but qualities and habituations, so to speak, that are acquired in the knowing power and whereby the intellect is rightly disposed to judge and dispute about reality. Thus science does not entail such concepts directly (*in recto*) but only indirectly (*in obliquo*), the way in which one relative term entails its correlative [CP159].

The arguments they advance in support of this position are many. Two of the more telling are these. Apart from the concepts that go to make up a science there is need for a certain promptness of the intellect in working with them, and this is something different from the concepts and is not acquired through them alone. Moreover, a habit is a kind of qualitative modification of the intellect; but concepts alone, since they are an accidental aggregate, do not amount to a qualitative modification; therefore they are not constitutive of habitual science [CP159–160].

After clarifying further the differences between actual and habitual science, Vallius-Carbone conclude with a final definition that sums up what they mean by habitual science and its subspecies, partial and total science, which assume importance in what follows:

> Science is a certain habituation or habit left in the soul after many acts whereby the intellect is disposed to gain understanding with certitude and facility. This habit is twofold: one is partial and is concerned with a single conclusion; the other is total and is concerned with all the conclusions of a particular science [CP163].

Their observations about this summary definition are perfunctory: they explain how definition may be included within the scope of science and how not; how science is to be differentiated from art and prudence; and how science is to be situated among the five habits of the intellect, which include, apart from science, art, prudence, understanding of first principles, and wisdom. One observation, however, is particularly of interest because of Galileo's implied reference to angelic sciences and divine science, which occur in F3.1.16 and D2.2.2, and to his much-cited comparison of human science with divine science in the *Dialogo* [GG7:

128–129]. This arises in reply to a question whether the definition of science that has just been given would also be applicable to God's science and that of the angels. Vallius-Carbone answer as follows:

> If we look to Aristotle's intention, the definition he gives applies only to a human science that is had through demonstration. This is obvious from what we have said above. Yet the entire definition can be accomodated to God's science and that of the angels, for both God and intelligences know the properties of things certainly and evidently through their causes, although they do not do so by discursive reasoning but by simple intuition [CP164].

The reference here to discursive reasoning finds an echo in Vallius-Carbone's treatment of logical resolution, described in detail in their *Additamenta* and summarized above in Sec. 2.7a, where they explicitly contrast angelic science with human science on the basis that the first requires no resolution whereas the second has need of it. And the contrast between our way of knowing and God's way of knowing by simple intuition anticipates Galileo's statement many years later in the *Dialogo*:

> As to the truth of the knowledge which is given by mathematical proofs, this is the same that divine wisdom recognizes; but I shall concede to you indeed that the way in which God knows the infinite propositions of which we know some few is exceedingly more excellent than ours. Our method proceeds with reasoning by steps from one conclusion to another, while His is one of simple intuition [GG7: 129].

a. *Unity of a Science.* Turning now to a consideration of the attributes or properties Vallius-Carbone attach to science, we find that they first devote considerable attention to the perfection or perfectibility of human science; following that they treat the question of the unity of a science; and then they cover in brief compass the remaining attributes – its evidence, truth, certitude, and preeminence. Because the question of what makes a science one or many assumes importance for our later discussion of the classification of the sciences, we depart from their ordering here and first consider the problem of the unity of a science. After this we discuss in more summary form the remaining attributes.

At the outset Vallius-Carbone make clear that they are discussing a total science, not a partial science, and that the precise difficulty arises not from the individual acts of scientific knowing that produce the total science, but rather from how the habitual science that results from them is to be characterized. Is this habitual science one or many in the sense that it is made up of a single habit of mind or of several? In support of the first alternative, one might argue that just as the intellect is a single power, so the habit that perfects it to understand a particular subject matter would seem to be single too. Again, the unity of a science should be taken from its formal object, and there is only one formal object for any particular science; therefore there should be only one habit corresponding to that science [CP171–173].

An alternative position would be that a total science is not a single habit but rather a complex of habits generated by the many demonstrations required to make a total science. How this would take place may be explained in different ways. Some hold that the concepts attained in actual science remain in the intellect and are themselves to be identified as habits; on this view there will be as many habits in a total science as there are concepts. Others say that the habits of a science are different from the concepts, but that more than one habit of mind is necessary to constitute a total science, with each habit having, in turn, its associated set of concepts [CP173–174].

The position taken by Vallius-Carbone acknowledges some element of truth in the first solution but favors the second way of explaining its alternative. The problem they see with the first position relates to the process whereby a science is acquired. Since science is produced by demonstration, those who embrace this explanation hold that the first demonstration seen by an investigator in a subject matter will generate in him not only the science of that subject matter but also the habit of the science. Subsequent demonstrations then will not produce one or more new habits, but rather will intensify the habit of mind that the first act of demonstration has already initiated.

The difficulty Vallius-Carbone see with this explanation is that it seems implausible when applied to a total science such as physics or logic. Each contains so many different objects of consideration – natural science, for example, the heavens, elements, compounds, organisms, even man – that it is impossible to encompass them all within a single intellectual habit. On this account they prefer to say that a total science requires more than one such habit. They then differentiate these habits not on the basis of concepts, as would the first way of explaining the alternative solution, but rather on the basis of the first principles from which different demonstrations can subsequently be made within the science. Thus a total science will not be a single habit; rather it will be constituted of several habits, each governed by its own set of principles, and each of which can be intensified as more and more demonstrations are seen within the area encompassed by those principles [CP174–181].[5]

b. *Other Attributes.* The remaining attributes Vallius-Carbone attach to science are its perfection or perfectability, its evidence, its truth, its certitude, and its preeminence. Though less important for our purposes, they touch on matters mentioned at various places in Galileo's logical treatises and are presented here in summary form.

The main problem concerning the perfection of human science, they say, is whether all things can be known by humans in a scientific way, and, if so, whether they be known only in general and confusedly, or in particular and distinctly. Their question is obviously restricted to matters known by the natural light of reason alone. From the point of view of certitude, they admit, human science is imperfect: not, however, in the way the skeptics understand this, namely, that men can know nothing for certain and must have opinion on everything. As opposed to the skeptical view they hold that men can have certain knowledge of some things in the world of nature because they can know them through their proper causes. The object of the human intellect, moreover, is being precisely as being (*ens ut ens est*), and thus everything knowable falls in some way under its purview. But in a more practical vein all things are not naturally knowable to humans in a scientific way, partly because human knowledge is restricted to what can be perceived through the senses, partly because the ultimate differences even of natural things are hidden from humans, and ultimately, because it is humanly impossible to grasp all the causes, external as well as internal, that affect natural entities in their being and coming to be [CP165–171].

Vallius-Carbone define evidence as a certain clarity and perspicuity whereby an argument or a sign is able to elicit conviction in the intellect, much the way in which a visible object seen at a proper distance and under appropriate light elicits conviction in the power of sight. It is not the same as certitude, since one can have certitude without evidence, as in divine faith, but one cannot have evidence without an accompanying certitude. There are two kinds of evidence, one appropriate to the senses, the other to the intellect. And evidence for the intellect is again twofold: one type is intuitive or immediate, when something is evident in itself, as are principles such as "The whole is greater than its part" and "Equals taken from equals the results are equal"; the other is discursive or mediate, when a thing becomes evident through something else, as a conclusion that is demonstrated from principles. The evidence that is the attribute of science is clearly discursive, not intuitive, evidence [CP192–193]. Many of these statements have counterparts in Galileo's D2.6.8.

Truth, as Vallius-Carbone explain it, is the conformity of the science or of the intellect possessing it with the thing known, the adequation of the mind with the object such that the object is apprehended as it is in reality. This is the complex truth found in the second and third operations of the intellect [Sec. 2.2]. They further subdivide complex truth into positive truth and negative truth: the first makes an affirmative statement about an existent, the second a negative statement about a non-existent. The truth appropriate to science is complex and affirmative truth, and from this negative truth follows as a corollary [CP193]. Here too their statements have counterparts in Galileo's questions, especially D2.1.2–3 and D2.1.7–8.

Vallius-Carbone define certitude as a firmness of the intellect in knowing that eliminates doubt or wavering about the knowledge attained; it differs from truth in that truth can be accompanied by doubt whereas certitude cannot. There are, moreover, two kinds of certitude: one is said to be extrinsic because, although the intellect gives its assent, it does so prompted by a command of the will; the other is intrinsic because the intellect gives assent on its own, forced as it were by the evidence presented or by its own reasoning, so that only a person deprived of the natural light would hold the contrary. The two certitudes differ in various ways: the extrinsic type can be false, as in the case of unfounded human faith, whereas the intrinsic cannot; the extrinsic necessarily depends on the will, whereas the intrinsic can actually be opposed to the will; the extrinsic does not invoke the natural light of the intellect, whereas the intrinsic does; and the extrinsic lacks evidence, whereas the intrinsic depends on it [CP193–194].

With these distinctions as a basis Vallius-Carbone state that science of necessity requires intrinsic certitude. The reason for this is that a science that is naturally acquired depends on the natural light of the intellect; it is also based on evidence that induces assent without fear of the contrary being true. The sources of this certitude are two. One is the middle term, and the more certain this is the more certain will be the scientific knowledge it generates. On this account the certitude of a demonstration of the reasoned fact is more certain than that of a demonstration of the fact, because the cause is more closely connected with the effect than the effect with the cause. This additionally gives rise to another distinction, that between things that are certain with respect to nature (when the cause is more known) and those that are certain with respect to us (when the effect is more known). Another source of certitude is the object of the science as this is considered from the viewpoint of its abstraction from matter, since matter can be a cause of uncertainty. Thus the more simple the object, that is, the more abstracted it is from matter, the more certain will be the science concerned with it. And although science's certitude depends on both of these sources, it depends more on the middle term than on the object because the middle term is the more proximate cause of the intellect's firmness in assent [CP194–195]. Again, on this topic there are many resonances between Vallius-Carbone's treatment and Galileo's remarks in D1.2.28 and D1.2.37, as well as those in D2.6.8–9.

On the last attribute, preeminence or nobility, Vallius-Carbone maintain that all sciences are good in their own right, including mathematics, even though it is said to abstract from goodness and the end. Yet one science can be ranked higher than another on one of three counts: either because its object is superior, the way the science of God is superior to that of the elements; or because its middle term or type of demonstration is superior, the way physics ranks higher than logic because it demonstrates through real and proper causes whereas logic does not; or because both its object and its middle term are superior, the way in which the science of man is superior to that of protomatter, since man is higher in the order of being than protomatter and one can prove man's properties by demonstrations of the reasoned fact whereas one can prove protomatter's existence only through a demonstration of the fact. Yet superiority is more cogently argued from the object of a science than from the middle term through which it obtains its proofs. This provides support for the view that knowledge gained from definition is superior to that gained from demonstration [CP195–196). Most of these points are touched on by Galileo in his lengthy comparison of definition with demonstration in D1.2.

3. CLASSIFICATION OF THE SCIENCES

How to classify sciences on the basis of the foregoing definition of habitual science requires investigation into how sciences may be grouped into species and genera, a topic much debated in medieval philosophy under the question of how the sciences are specified. This in turn is related to a problem already discussed, that of the unity of a science when the science is considered as an intellectual habit. As Vallius-Carbone now point out, the unity of a habit can be understood in three ways, namely, either as a numerical unity, or as a specific unity, and as a generic unity

[CP183]. Sciences that are specifically one pertain to the same species, whereas those that are generically one pertain to the same genus. The main problem they wish to treat is that of specific unity, and thus they discuss at this point how such unity can be ascertained.

a. *Specification*. The basic factors they take into account in this determination are the principles the sciences use in their demonstrations, the subjects of the demonstrations, and the properties that are manifested through the demonstrations. If all three are different in various sciences, they say, they have no specific unity; the same is true when the subject and the property are the same but the principles are different; and likewise when the subject is the same but the property and the principles are different [CP183].

A positive way of differentiating the sciences, they further note, is through their objects, considering these objects not materially but formally, i.e., in terms of various formalities that can be discerned in them. They identify three of these: one "through which" (*per quam*) the object is considered, another "what" (*quae*) is considered in it, and yet another the light "under which" (*sub qua*) it is considered. These they illustrate with the demonstration that man is corruptible, which they state as follows: Everything composed of contraries is corruptible; man is composed of contraries; therefore man is corruptible. The formality "through which" is the argument contained in the premises. The formality "what" is man as composed of contraries, for that is what makes him corruptible. And the formality "under which" is the aspect the natural philosopher treats in man, considering him as abstracted from singular matter but still containing sensible matter in his definition [CP184].

In light of these formalities, the key problem for Vallius-Carbone is whether the unity of a science should be judged from its object by the formality "what" or by the formality "under which." The second of these is usually expressed in terms of the various kinds of abstraction from matter effected by the intellectual light under which the object is considered, as explained in Sec. 2.1–2. Vallius-Carbone explain these kinds as follows:

> There are three degrees of abstraction, one called abstraction from singular matter, which is used by the natural philosopher or physicist, who abstracts only from singular matter. The second abstracts from matter in its consideration but not in its reality; the mathematician uses this when he considers quantified objects in general not as they exist but as they fall

> under his consideration. The third abstracts from matter both in its reality and in its consideration, and this is the way the metaphysician abstracts when he considers the natures of things separated from all matter [CP185].

This understood, they identify the two major positions as these: one holds that the sciences are differentiated generically by the three kinds of abstraction, the formality "under which," the other that they are differentiated by intrinsic principles, the formality "what" [CP186].

Vallius-Carbone begin their resolution by explaining how a total science is made up of several partial habits, all of which are concerned with one formal object "what." For this they cite the example of physics:

> Physics is a kind of total science that is made up of habits produced by demonstrations that are specifically distinct. But since these different habits are concerned with objects whose formality is the same, namely, a natural body, and since they define in the same way, through matter and form, and have the same way of demonstrating and proceeding from sense data and what is more known to us, they constitute one generic habit of mind [CP187].

They go on to generalize from this example and state the requirements for the generic unity of a total science:

> These different habits are not sufficiently characterized by their having different demonstrations, some through causes and others through effects, or some of accidents, others of substances. Rather the following four things are necessary to make for their unity. First, one subject considered under one formality. Second, a subject having proper principles and a proper mode of defining and proceeding. Third, a subject having many species, understood in the sense of proper formalities. (Thus medicine has one subject but not several species if its subject is considered formally, for the healable human body, the subject of medicine, does not have subspecies that are differently healable through the medical art.) Lastly, a subject having attributes or properties that are demonstrable. From all of these requirements the generic unity of a science should be ascertained [CP187].

On the basis of these considerations Vallius-Carbone specify in detail what they regard as key determinants for the numerical, specific, and generic unities of the sciences. Their positions may be consolidated and paraphrased into the following three:

> Numerical unity is taken from the unity of the subject in which the science exists; thus there are as many sciences as there are individuals in which the habit of the science exists.
>
> Specific unity is taken from the object considered under the precise formality "what," as just explained. It is not taken from the object considered materially, since there can be many sciences, say, of man; nor is it taken from the object considered formally as the "through which," since this differentiates sciences from other habits but not one science from another; nor is it taken from the object considered formally as the light "under

> which," that is, the type of abstraction it uses, since arithmetic and geometry differ specifically and yet they have the same degree of abstraction.
>
> Generic unity is likewise taken from the object considered under the formality "what," in this case a genus subject that contains a number of species under it. Again it is not taken from the object considered formally as that "under which," for this does not focus on the object precisely as such but rather on the condition that makes it accessible to the intellect [CP188–190].

Thus they favor the first position mentioned above, although their solution also accords with the way proponents of the second position claim the speculative sciences are specified.

b. *Speculative Sciences.* Traditionally the speculative sciences have been divided into three – physics, mathematics, and metaphysics – because their objects and modes of consideration seem clearly distinct. Vallius-Carbone become concerned with that division because it was being questioned in their day by some commentators, following the teachings of Antonius Bernardi Mirandulanus, a professor at Bologna and later a bishop, who died in 1565. Since Galileo mentions Mirandulanus in MS 27 at D1.2.14 and in MS 46 at B1, K38, K40, and L13, no doubt he was acquainted with his thought.[6] Mirandulanus questioned whether physics, mathematics, and metaphysics were really three distinct total sciences, as commonly taught, or whether they were only parts of one large science. He argued that just as logic is a single science made up of many different treatises, and physics a single science concerned with many subject matters (the heavens, elements, and compounds), so the three speculative disciplines are parts of a total science concerned with all of being. Some texts of Aristotle seem to support this idea, and, depending on how one views the object of a science, it may also be consonant with Aristotelian teaching on the specification of the sciences [CP229–232].

Vallius-Carbone take the occasion of Mirandulanus's teaching to explain how different formal objects yield the three speculative sciences of physics, mathematics, and metaphysics. They treat each in turn:

> Physics considers the natural or changeable body from what is known by the senses, since it does not abstract from sensible matter. As to its mode of proceeding, it puts sensible factors in its definitions, namely, matter and form; it abstracts, however, from individual matter. The reason behind this way of proceeding is that any type of knowledge must be proportioned to the things it knows; and since the nature of physical things is to be attainable by the senses and include matter, for them to be known they must be defined in terms of matter. For this reason the science of natural bodies considered precisely as natural makes use of matter, and in the beginning analyzes matter and through it proves the properties of physical things [CP234].

Next they turn to mathematics, explaining how its mode of consideration differs from that of physics:

> Mathematics, on the other hand – not having for its object the essences of things and not considering properties that flow from such essences, and moreover not considering causes and effects but only necessary connections between various properties that are attributed to its subject, that is, quantity as abstracted from sensible matter – has a mode of proceeding that is different from physics. And since it does not consider true causes, it does not have true definitions and demonstrations either. In using the expression "abstracted from sensible matter" we mean to focus on pure mathematics and not on the sciences intermediate between pure mathematics and physics, such as astronomy and perspective, which consider their objects as they fall under the senses [CP235].

Noteworthy here is Vallius-Carbone's implicit questioning whether pure mathematics can be a science in the strict sense on the grounds that it does not consider true causes and effects as found in the world of nature, a query we shall return to later. The same hesitation on their part has already been seen regarding the scientific status of logic, which they prefer to identify as an instrumental habit of mind rather than as a science precisely because its object is not real being but intentional being [Sec. 2.4]. Yet they do not place this restriction on the middle sciences, those intermediate between pure mathematics and physics, which will be treated in fuller detail in Sec. 3.4b.

Finally Vallius-Carbone note the distinguishing characteristic of metaphysics:

> But metaphysics, since it has for its object the quiddities and universal natures of things abstracted from matter not only in its reality but also in the mind's consideration, proceeds from principles that are most universal and that are self-evident by the light of nature [*lumine naturae*]. Concerning its object it should be noted here that it is commonly agreed that metaphysics considers not only being as such but also God and intelligences [CP235].

At this point Vallius-Carbone develop an idiosyncratic doctrine we have explained elsewhere but which is not relevant to the present exposition.[7] Whereas most commentators would say at this point that the formal object of metaphysics is being in common (*ens in commune*) as it abstracts from natural being and quantified being (the objects of physics and mathematics respectively), Vallius-Carbone question whether "being in common" is an object sufficiently adequate to include the study of being in general as well as that of God and intelligences in particular. Their conclusion is that it is not, that there is no way of assigning the same formal object and the same mode of procedure to the study of things so diverse in their natures. As a result they opt for the position that there are actually five total sciences, namely, a science of God, a science of intelligences, a science of being in common, a science of natural bodies, and a science of quantity as this is studied in mathematics [CP235–236, 249–250].

c. *Subalternation.* The subalternation of the sciences is a topic implied by the foregoing ways of classifying sciences, for some sciences may be regarded as superior to others, and when they are, their ordering according to higher and lower, otherwise referred to as their subalternation, becomes a matter of concern. Vallius-Carbone take up this subject immediately following their division of the sciences and treat it extensively, first describing what subalternation is and the conditions requisite for it, and then the type of science that results from it, namely, the subalternated or mixed science. We shall treat the first of these here, reserving the second for the following Section.

The basic description of subalternation is provided by Vallius-Carbone in terms of three factors that characterize sciences: their principles, their subject, and their end. For them these give rise to a threefold subordination of the sciences: one by reason of principles, another by reason of subjects, and yet another by reason of ends. Among the three kinds they then establish a hierarchy: subalternation on the basis of ends is less strict than subalternation on the basis of subjects, and that in turn is less strict than subalternation on the basis of principles.

Vallius-Carbone turn their attention next to the conditions requisite for subalternation. The common opinion in their day enumerated three requisites for this, namely: that the subject of the subalternated science be contained under the subject of the subalternating; that only an accidental condition be superadded to the subject of the subalternated; and that this condition not be a strict property or something that flows from the subject's essence. Some authors added further requisites, such as that the subalternated science supplies only demonstrations of the fact, whereas the subalternating science supplies demonstrations of the reasoned fact [CP268].

Vallius-Carbone adopt most of these requirements but provide their own interpretation of them. They first add a further requirement, namely, that the principles of the subalternated science must be proved in the subalternating science [CP269], a point that will be explained more fully below. They then combine the first and the fourth requisites noted above, as in the following statement:

> For true subalternation the subject of the lower science must be contained under that of the higher. First, Aristotle implies this when he says that the higher science provides the reason for the fact, the lower only the fact; but the reason for the fact is the same as the quiddity of the thing; therefore one cannot have perfect knowledge of the quiddity of the lower's subject without knowledge of the higher's; therefore the subject of one science will be contained under the subject of the other and will depend on it, in being as well as in knowledge. The same result can be arrived at by induction. The subject in perspective is the visible line; but the line is the subject of geometry; thus the line that is visible depends on the geometrical line both in being and in knowledge [CP269–270].

Vallius-Carbone depart from the common teaching, however, on one important particular, namely, the requirement of the accidental condition. Concerning this they write:

> The subalternated science frequently adds only an accidental condition to the subject of the subalternating science, but sometimes it adds an essential condition. The first part of this conclusion is conceded by all, and is gathered from Aristotle's saying that the subject of the subalternated science is in some way different from the subject of the subalternating. The same conclusion can be argued inductively, comparing perspective with geometry, music with arithmetic, and other subalternated sciences with their respective subalternating sciences. But sometimes the difference is essential, as in the case of a condition that makes the subalternated science a practical science and the subalternating science a speculative science, for such a difference results in sciences that are specifically distinct. This explains the second part of our conclusion. But it should be noted that a difference of this kind is not essential on the part of the subject, but rather on the part of the end [CP271].

This position coheres with materials developed by Vallius-Carbone in their extensive treatment of the distinction between speculative and practical sciences, a treatment omitted above in our summary of their classification of the sciences. In their view strict subalternation occurs not only within the speculative sciences, say, in the subalternation of perspective to geometry, but also can occur between practical sciences and their speculative counterparts, say, in the subalternation of ethics to the science of man. In this teaching Vallius-Carbone depart from Zabarella, who held that the only sciences that are subalternated in the strict sense are those that apply mathematics to the study of nature [ZL522–530].[8]

On the basis of these requirements Vallius-Carbone draw various corollaries about the ways subalternating sciences differ from their respective subalternated sciences. Among the noted differences are that subalternating sciences are more certain, more preeminent, and more perfect than their counterparts. The topic of preeminence they treat as follows:

> A subalternating science ranks higher than its subalternated science by reason of its middle term, because the former provides the reasoned fact and the latter only the fact; but the

science that gives the reasoned fact is superior because it knows the cause. But note that we say "by reason of its middle term," because by reason of its object the subalternated science might be superior, as in the case of astronomy as related to geometry. For the heavenly body that is considered by the astronomer is more preeminent than the quantity that is considered by the geometer [CP273].

Similarly, the topic of perfection elicits from them a clarification of Galileo's statement in D2.4.2 to the effect that subalternated sciences are imperfect:

> A subalternating science differs from a subalternated in that the former is perfect, the latter imperfect. The reason is that the former is independent by reason of object and principles, whereas the latter depends on the former for both. As a consequence the subalternated science cannot resolve its conclusions back to first principles unless it is conjoined to the subalternating science, and this type of resolution is necessary if one is to have a perfect science. Again, the subalternated science does not employ self-evident propositions, and so it cannot have perfect demonstrations, which must be made from evident principles. Finally, the subalternated science does not demonstrate properties of a first and adequate subject, as is apparent in astronomy, which demonstrates properties of celestial circles that are those of circles in general; and what it demonstrates it demonstrates from principles that are supposed and are not known in themselves [CP274].

Considerations of this type lead into a fuller discussion of how a subalternated science must know its principles, namely, whether it must grasp them by reason or whether it is sufficient to take them on faith. Vallius-Carbone argue that a subalternated science is not a true science if it does not grasp its principles by reason; thus, if it merely believes its principles, it is not science but faith [CP277–279]. They do add a qualification, however:

> Note here that the principles of a subalternated science can be known in two ways, either *a posteriori*, as that a round wound is more difficult to cure, which is known from experience, or *a priori*, through a cause, and only in the latter way is the subalternating science said to be a habit of principles with regard to the subalternated [CP279].

Thus in their view a subalternated science can be a science in the strict sense under the condition that it arrive at its principles by *a posteriori* reasoning. In that event it will be imperfect compared to the subalternating science, since with regard to the principles it will know them only as a fact and not as a reasoned fact. Yet it will still be a true science and thus different from faith.[9]

Vallius-Carbone complete their exposition of subalternation by arguing that physics and mathematics are not properly subalternated to metaphysics, that logic does not subalternate other sciences to itself, and that logic is not properly subalternated to any other science [CP283–284]. They further argue, contrary to the common view, that practical sciences are generally subalternated to speculative sciences, and instance mathematics and physics as the major subalternating sciences related to praxis. As speculative, the part of physics devoted to the science of man subalternates to itself two practical sciences: ethics, which depends on the knowledge it provides of man's soul, and medicine, which depends on the knowledge it provides of man's body [CP284–286]. But they admit, possibly by way of concession to Zabarella, that by far the greater number of subalternated sciences depend on mathematics.

4. MATHEMATICAL SCIENCES

Earlier we have noted Vallius-Carbone's characterization of mathematics as having nude quantity for its object, as not proceeding through true causes and effects, and thus as not qualifying as a science in the strict sense, although they exclude from this characterization the sciences subalternated to mathematics [Sec. 3.3b]. Their teaching on this matter has important bearing on how they conceive astronomy as a type of applied mathematics, and particularly on the way they see this discipline to be related to the part of physics concerned with the heavens. To explain these teachings, which assume great importance in our later chapters, we first explicate how they view the object of pure mathematics, after which we turn to their exposition of the sciences intermediate between pure mathematics and physics.

a. *Pure Mathematics.* A convenient entry point is their discussion of a problem based on the Aristotelian categories, namely, if, as Vallius-Carbone hold, there can be a total science of quantity, why not a separate science of quality and of all the other categories? Some authors, they say, reply that mathematics does not consider quantity in itself but rather corporeal substance insofar as it has quantity. They reject this response for several reasons:

> First, because the science of mathematics does not consider corporeal substance but rather nude quantity, and so it abstracts from being, the good, and the end. Second, because mathematics makes no mention of corporeal substance and its demonstrations conclude as if there were no corporeal substances, for it supposes quantity alone. Third, were it to consider corporeal substance it would have to treat its species, properties, and attributes, as any science should; but mathematics does not do this. Finally, this teaching would confuse

mathematics with physics, which has corporeal substance in the natural body for its object [CP242].

Note here their statement that mathematics abstracts from being and the good, which has a counterpart in Galileo's F3.1.18 and which they later discuss at greater length [CP294–296].[10] Also significant is their observation that mathematics "supposes" quantity for its object, which again suggests a concern with reasoning *ex suppositione*. Their reply to the difficulty follows, and this proves helpful for further clarifying what they mean by "nude quantity" when identifying it as the object of pure mathematics:

> There is a difference between quantity and the other types of accident. For quantity can be considered without substance and so can be abstracted from it; the other accidents enter into the intrinsic constitution of sensible substance. Again, quantity has many properties that can be considered in their own right without a relationship to substance, whereas other accidents do not. For although quantity is a property of substance, as are other accidents, precisely as quantity it has its own properties, such as having part outside of part, independently of its being a property of substance. As a consequence quantity can be treated by all three sciences: by metaphysics as it is a being, and indeed a supreme genus of being; by physics as it is a property of substance with a particular nature; and by mathematics as it has its own properties independently of substance. The remaining accidents are treated by metaphysics and physics in the first two ways but have no special science to consider them in the third [CP242–243].

Later, responding to a further difficulty over the objects of the total sciences, Vallius-Carbone clarify how mathematics considers its object:

> In mathematics the object is nude quantity; the mode of proceeding is through propositions that are thoroughly understood and through necessary connections between principles and conclusions, whether the former are true effects, true causes, true signs, or not, and without any consideration of motion and finality [CP250].

Stated in this way, their point seems not to deny the mathematician any contact with the real world, but rather to have him abstract from such contact so as to deal exclusively with quantity and its properties. This has further implications for the possibility of a subalternated science such as mathematical physics, as will be seen below.

Vallius-Carbone further state that mathematics, unlike physics, abstracts from sensible matter but that it does not abstract from intelligible matter. Their expression "intelligible matter" casts light on what they mean by "nude quantity" in their statements above. They explain:

> Intelligible matter is nothing more than continuous or discrete quantity, whether this be a line, a surface, or a body, precisely as it has the formality of matter, in the sense that

> something exists in it as a form or something is made from it as from matter, as a circle from its segments. Indeed, everything that exhibits such a formality in sensible things can be referred to as intelligible matter, although this more properly is said of quantity that is separated from substance [CP292–293].

Yet another point that requires clarification – and this in view of the Aristotelian teaching that mathematical entities do not undergo motion or change – is how pure mathematics abstracts from motion. Vallius-Carbone note that this kind of abstraction can be explained in a variety of ways:

> First, motion can be taken to mean sensible matter. Second, motion can be taken to mean generation, corruption, and other changes, the way motion results from natural powers; for it is certain that mathematics abstracts from local motion as it is made naturally to a particular place, since quantity, as considered in mathematics, does not entail motion of this kind. And if motion is taken more broadly still, for example to include growth of any kind, mathematics does not abstract from growth, because it considers how quadrature is augmented if a gnomon is added and the square figure retained. Likewise, if local motion is taken to designate any operation of parts such that distance, ratio, or velocity follows from it, such motion is considered by the mathematician, for under this formality motion can be proper to quantity in the way in which it is considered by the mathematician. Other motions are those that proceed from an intrinsic natural power, and these are not considered by the mathematician [CP293–294].

Note here that they do not exclude from the concern of the mathematician the imaginary motions considered by the *Calculatores* in what would later become the middle science of kinematics, but rather only natural motions that proceed from powers intrinsic to physical bodies.[11]

b. *The Middle Science of Astronomy.* With these points clarified, we may return to Vallius-Carbone's explanation of the way various sciences can be subalternated to mathematics. Unlike physics, which subalternates to itself only practical sciences, for them mathematics subalternates to itself both speculative and practical sciences. They first define why these are called "middle sciences":

> The subalternated scieces are said to be middle sciences (*scientiae mediae*) from the fact that they have a mathematical object applied to a physical thing, but they are properly mathematics because their subject is the essential condition rather than that which is only accidental, as stated above [CP285].

They then describe the speculative sciences subalternated to mathematics:

> Geometry subalternates to itself the following speculative sciences: perspective under the formality of lines, astrology under the formality of the appearances of the heavenly bodies and their sightings, astronomy under the formality of the stars. The latter two sciences differ by reason of end, because astrology considers things that arise from the motions of the heavens, astronomy only the motions of the stars themselves. Music is subalternated to arithmetic, being concerned with the sounding number [CP285].[12]

Following these they list the practical sciences:

> Under geometry is contained stereometry, which considers solid bodies; it subalternates to itself the art of measuring, of building, and other arts concerned with solid bodies. To the three higher subalternating speculative sciences yet others are subalternated: to astronomy, nautical and agricultural science and similar practical arts; to speculative music, practical music, and so on [CP285].

This general breakdown of the mathematical sciences leads to a further problem, namely, that of making precise the proper formality under which the mathematical sciences treat of quantity. Vallius-Carbone reply to this by first making the distinction between pure mathematics and the middle or mixed sciences that are subalternated to it. In their view the two branches of pure mathematics, geometry and arithmetic, consider continuous and discrete quantity respectively as existing in things, even though they abstract such quantity from physical reality. As opposed to this:

> The branches of mixed mathematics apply mathematical quantity to physical things; astronomy, for example, considers the quantity that is in the heavens or in stars, and the same can be said of perspective, music, and the others. But middle sciences do not directly consider both together, as if the object of perspective were the line along with the visual aspect, for in this way it would not be a true science. Thus in perspective one does not consider what vision is, or how it takes place; rather one considers directly only the line, but with the condition of its being applied to a physical object and precisely as it falls under sight. For this reason its formal object is only the quantity, that is, the line; and yet this would not be the object if it did not have the condition of being visible. This then is proper to subalternated sciences, that they add to the subject of the subalternating science some accidental condition, though this is not the formal consideration of the subalternated science; precisely as such the formal consideration does not differ essentially from that of the subalternating science. Therefore the formal object of the middle sciences is the same as that of the pure sciences, plus an accidental addition. For this reason astronomy has for its subject quantity as it can be applied to the heavens, not the heavens themselves. Otherwise it would not be different from physics [CP298].

This resolution of the difficulty leads Vallius-Carbone to attempt a fuller exposition of the precise formal object of astronomy, which they now describe as follows:

> Astronomy has for its formal object the quantity of the heavens, meaning by this both permanent and successive quantity, that is, three dimensionality and motion, for both of these are considered by the astronomer. Its formality, however, is to consider such quantity not as it is a type of accident of the heavens, for this pertains to physics, but rather as it gives rise to mathematical properties such as ratio, distance, shape, etc. The necessary condition is its application to the substance of the heavens; the remote material subject is the substance of the heavens. For even though this subject is not considered by the mathematician, it nonetheless is the subject in which the object of astronomy inheres, and therefore astronomy seems to have the substance of the heavens as its subject [CP298–299].

Having made this observation, Vallius-Carbone make the further concession that, though not strictly speaking entitled to do so, astronomers frequently arrogate to themselves the prerogatives of natural philosophers. They go on:

> Many mathematicians are deceived in this matter. So as to make astronomy preeminent they say that its subject is the heavens, whereas this is only its material subject, for if it were the formal subject it would be the science of physics, whose field it is to treat of the substance of the heavens and its properties. But since the sciences help one another, and astronomers wish to confer prestige on their science, they have taken on matters that pertain to natural philosophy. This happens in the other middle sciences, and for the same reason, and likewise in subalternated sciences as related to subalternating [CP299].

Following this solution of what was to become a pressing problem in their day, and on which more will be said below, Vallius-Carbone turn to a related question, namely, whether the middle sciences are more physical than mathematical. Some authors maintain that their object makes them more physical, while conceding that their mode of proceeding would make them more mathematical. Yet others hold that they are more physical absolutely, because their application to physical reality is in effect their formal consideration. While acknowledging both these positions, Vallius-Carbone take a stance opposite to them, namely, that the mixed sciences are in fact more mathematical. In support of this they cite Aristotle, who calls astronomy the most preeminent of the mathematical sciences, and who states that perspective and harmony do not consider sight and sound, but rather line and number; therefore mathematical entities are their direct object. And in the second *Physics*, text 20, Aristotle does not say that these sciences are more physical than mathematical, but only that among the mathematical sciences they approach closer to physics than do the others [CP299–300].[13]

At this point Vallius-Carbone enter into a detailed discussion of how the middle sciences differ from physics, and particularly how astronomy does so, for this offers them the major difficulty. They write:

> All middle mathematical sciences differ from physics, because even though they consider a physical thing, they treat it not as physical but as mathematical, as we have just explained; not in an absolute way, but as applied to a physical thing, where the application is like a necessary condition [CP300].

With regard to astronomy, they go on, this science seems to be closer to physics because it treats of the heavens, which are physical bodies. Yet one may still note the following differences between the two:

> First, physics considers the substance of the heavens, astronomy their accidents. Second, the former considers all the accidents of the heavens, the latter does not. Third, the former demonstrates accidents of the heavens as they are properties and terminations of a natural body, whereas astronomy does not. Fourth, astronomy considers only the quantity and proportions of motion, whereas physics considers its principles, how it is effected, and whether or not it is natural. Fifth, physics examines causes and through them demonstrates properties of the heavens, whereas astronomy does not. And if occasionally it seems to demonstrate through causes, either it demonstrates only from appearances and does not examine whether they are causes or not; or it argues from false and contradictory premises provided they suffice to save the appearances, as when it proves a conclusion through eccentric orbits and epicycles, which, as some maintain, do not even exist; or it does not demonstrate through causes properly as such. Sixth, astronomy generally offers proofs that are *a posteriori*, physics *a priori*. For example, the astronomer proves the earth to be round from the lunar eclipse and from other appearances; physics proves it because the earth is heavy, which causes all of its parts to tend toward the center and so aggregate into a spherical shape. Seventh, the physicist assigns a natural and proper cause for the heavens' being round, namely, because they are neither heavy nor light and are not made of the elements; the astronomer assigns only a common and remote reason, because lines drawn from the center of the earth to the heavens are equal. From all this it is apparent that astronomers do not consider the substance and qualities of the heavens, because all of these pertain to the realm of nature [CP301].[14]

These statements are typical of those made by philosophers in the late sixteenth century to protect their discipline from the enchroachments of mathematicians. Yet the ideas behind them are not essentially different from those sketched in Galileo's own *Trattato della Sfera* or *Cosmografia*, his teaching notes for the astronomy course he taught at Padua in the first decade of the seventeenth century. In introducting them he states:

> In the *Treatise on the Sphere*, more appropriately called *Cosmography*, as in the case of other sciences we must first point out its subject and then we will touch on the order and the method to be observed in the science.
>
> Thus we say that the subject of cosmography is the world, and we mean by this the universe, as indicated by the term itself, which means simply a description of the cosmos.

> But this does not include everything one might consider in the universe, for only a part pertains to the cosmographer. His part consists in inquiring into the number and ordering of the parts of the universe, their shape, size, and distances, and even more than these, their motions. Consideration of the substance and qualities of these parts is left to the natural philosopher [GG2: 211].

At the time he wrote this, therefore, Galileo apparently was willing to accept a division of labor between astronomy and physics such as that laid out in Vallius-Carbone's treatise on science.

One might raise the further question whether Galileo, on the basis of Vallius-Carbone's treatise, was deterred by it from doing original work in mathematical astronomy. Apparently not, for he was content to teach Ptolemaic astronomy as a middle science at Pisa, even though while there he already knew of Copernicus and his work [GG1: 43,47], and he continued to do so at Padua between 1602 and 1607 [GG19: 151–157]. His early interests seem rather to have been in mechanics, particularly the study of local motion, and here the strictures on causal analysis in a middle science were less severe, since the moving of weights obviously required causal agents of some kind.[15]

c. *Causes in Mathematics.* The foregoing accounts of the scientific status of pure and mixed mathematics, and of astronomy among the latter, reveal an ambivalence in Vallius-Carbone on the subject of causes. They wish to consider all of these disciplines speculative sciences, for which it would be essential that they demonstrate through knowledge of causes and effects, and at the same time they are reluctant, in the case of pure mathematics, to admit that it has true definitions, true demonstrations, or knowledge of true causes and effects. Apparently the meaning they attach to "true" in these qualifications is "physical" or "natural"; they further seem to restrict the sense of "cause" to agency or efficient causality, overlooking the possibility that the mathematician might achieve strict demonstrations through material and formal causality. This ambivalence seems to be a residue of an anti-mathematical attitude deriving from Alexander Piccolomineus and adopted by some early Jesuits, including Benedictus Pererius at Rome and the entire faculty at Coimbra. It was vehemently opposed by Clavius, who toward the end of the 1580's was able to introduce mathematics courses into the *Ratio studiorum* of the Jesuit Order and to prepare students for advanced work in that field. Still tensions continued to exist within the Order between its philosophers and mathematicians well into the seventeenth century.

The clearest statement of a revised Jesuit position on the nature of the mathematical sciences is provided by the student of Clavius mentioned above in Sec. 2.7b, Joseph Blancanus, in his *De natura mathematicarum scientiarum*, published at Bologna in 1615. There Blancanus argues that, while quantity in itself is studied by the physicist and the metaphysician, this is not the quantity studied by the mathematician. Rather the object of pure mathematics is terminated quantity, for it is the various terminations of quantity that give rise to the continuous and discrete entities studied in geometry and arithmetic respectively.[16] Intelligible matter, for him, is thus terminated quantity and not quantity absolutely considered. With this as its proper subject, the mathematician can provide essential definitions of mathematical entities and can demonstrate their properties through causes, both formal and material.[17] These demonstrations, contrary to the teaching of Piccolomineus, are most powerful (*potissimae*); Blancanus adduces in support of this teaching not only Aristotle, Plato, and Proclus but also recent authors such as Toletus and Zabarella.[18] He then goes on to refute in detail the "calumnies" brought against the mathematical sciences by Piccolomineus, some of which are reflected in the statements reported above from Vallius-Carbone.[19]

Turning to the middle sciences, Blancanus first notes an inconsistency in his adversaries, who wish to eliminate perfect demonstrations from pure mathematics but at the same time are willing to concede that they are found in the mixed sciences.[20] For his part the speculative middle sciences, such as astronomy and perspective, have most perfect demonstrations, as is seen in their supplying the reasoned fact for physical phenomena. He cites the demonstration of the lunar eclipse, and particularly the way in which that demonstration has been analyzed by Zabarella, as confirmation of their providing demonstrations that are most powerful.[21] And in the case of practical middle sciences, such as mechanics, demonstrations can be found in terms of all four causes, since as practical they always are concerned with an end to be attained, the efficient causes necessary to attain it, and the material and formal causes involved in its production.[22]

Blancanus's emendations to the teachings found in Vallius-Carbone's *Introductio in universam philosophiam* are noteworthy, for despite the fact that their material duplicates the treatise *De scientia* that was part of the teaching notes available to Galileo, and so aids in the understanding of MS 27, there is little evidence, as already intimated, that their views on mathematics or mathematical physics exerted a retarding influence on

Galileo. Part of the reason surely was the instruction Galileo had earlier received at Pisa from Buonamici, Fantoni, and possibly Mazzoni, as explained in the Introduction to *Galileo's Logical Treatises*. By the time he was working on his *De motu* of 1591, of course, he had clearly rejected the anti-mathematicism of Pererius. With regard to the pro-mathematical faction among the Jesuits, moreover, it is noteworthy that Blancanus and another Jesuit, Andreas Eudaemon-Ioannis, were in Padua at the beginning of the seventeenth century and while there had contacts with Galileo [Sec. 6.5]. Since Vallius himself was in the Veneto at that time, it is highly likely that he too knew of Galileo and his work in mechanics. Perhaps because of this, the views Vallius expresses in his *Logica* of 1622 on the nature of mathematics and the mixed sciences are much more benign than the materials in the *De scientia* plagiarized by Carbone. This is especially evident in Vallius's question inquiring whether or not subalternated sciences are true sciences. In the *Logica* he replies that they indeed are, whether they function in independence of their subalternating science and only grasp their principles *a posteriori*, or whether they know their principles as provided by the subalternating science, in which case their demonstrations are *a priori* and *propter quid* [VL2: 651, 656].

An even more fitting appreciation of the mathematical sciences is found in the brief epilogue with which Vallius concludes his exposition of subalternating and subalternated sciences in the *Logica*. Rather than disparage the astronomers for encroaching on the subject matter of physics, as in the passage cited above from Vallius-Carbone, he ends this on the note that all of the sciences mutually help one another:

> When sciences are so related that one is subalternating and another subalternated it is evident from the foregoing that they assist one another and that one depends on the other. When they are not subalternating and subalternated, moreover, they have much in common and again assist each other. First, all use dialectics and employ probable principles, since they do not have demonstrations in all matters, though they use probable arguments only when they lack better arguments. Second, all use metaphysics when they defend their sciences and their principles against attackers. Third, one science frequently makes use of examples taken from another, as in Aristotle's logic, where he often uses mathematical examples. Fourth, for more fruitful teaching the materials of different sciences are occasionally intermingled. In particular, physics presents an object for the science of God and intelligences because it proves through the motion of the heavens that God and intelligences exist, and since these are the objects of those sciences, they are presupposed by them, and they cannot prove them from their own principles. And physics is also concerned with celestial bodies and magnitudes, and thus physics and mathematics cooperate in various demonstrations.
>
> Finally, metaphysics, physics, and mathematics treat quantity in common. From this it

is apparent that the connection among the sciences is so great that one science cannot achieve perfection without the other, so that it either will not attain it at all or will barely do so, just as one virtue cannot exist in its perfect and natural state without the other virtues [VL2: 662].

5. OPINION AS RELATED TO SCIENCE

The concept of science detailed in the foregoing Sections can be better understood when it is set in contrast with opinion [Lat., *opinio*], which in the Aristotelian tradition is knowledge of a quite different type. Whereas the canons for attaining science are formulated in the *Posterior Analytics*, those for generating opinion are laid out in Aristotle's *Topics*. So as to make clear the difference between the two, in their *Introductio in universam philosophiam* Vallius-Carbone present several chapters on opinion in the concluding portion of the treatise on science. They further supplement this with an extensive discussion of the topics and the dialectical syllogism in their *Introductio in logicam*. Since Galileo frequently intermingles demonstrative and dialectical reasoning in his writings, a practice endorsed by Vallius and quite common in his time, we may draw from both these works to see how opinion was contrasted with science in the logical system that lies behind MS 27.

a. *Opinion and Its Kinds.* Just as Vallius-Carbone begin their treatise on science by listing various meanings of the term, so they open their discussion of opinion by listing its four different meanings. In their account opinion can mean either any knowledge whatever; or a type of knowledge one attains from someone else; or a confused kind of knowing that includes belief, opinion in the strict sense, and suspicion, but excludes science; or finally the strict sense itself, knowledge wherein one gives assent to a statement but not without fear that its contrary may be true. The last two meanings have further divisions: they may refer to an act of opining or the habit acquired through repeated acts of this kind; or they may refer either to immediate knowledge, which gives assent without a middle term or reasoning being involved, or to mediate knowledge, the result of a reasoning process [CP303].

Excluding the first two meanings, they give two definitions, one applying to the third and the other to the fourth, yet both being understood as types of habitual knowledge that involve a middle term. Their first definition reads as follows:

> Opinion is an imperfect intellectual habit accompanied by lack of evidence and certitude on the part of the middle term. It is said to be imperfect to separate it from the intellectual habits Aristotle describes in the sixth book of the *Ethics*, where, treating of habits that perfect the human intellect, he enumerates five: wisdom, science, art, prudence, and understanding. These habits are perfect in their kind, since they incline only to the truth and so perfect the intellect; the habit of opinion is excluded from them, because it has the possibility of being false. It is said to be with lack of evidence, to differentiate it from science, which requires evidence, as is apparent from what has been said about science above. It is said to be with lack of certitude, to differentiate it from things known by divine faith, which is most certain. And it is said to lack evidence and certitude on the part of the middle term to show the cause whence the imperfection arises, namely, a probable middle term [CP304].

This definition corresponds to what may be taken to be opinion in a broad sense, following Vallius-Carbone's initial characterization. Their stricter definition, on the other hand, reads:

> Opinion is an imperfect intellectual habit, uncertain on the part of the middle term, accompanied by fear. In this description the last phrase, accompanied by fear, is added to differentiate opinion from human belief, which can be had with lack of evidence on the part of a middle term, and with lack of certainty, and yet without fear. Thus one who knows that Venice is in Italy by human belief alone does so without fear of the contrary, because it is said to be so. And although human belief can be with fear, as when someone believes something on the authority of those who are not completely trustworthy, nonetheless it is not intrinsic to such belief that it be with fear, since we know by human belief a great number of facts that are most certain [CP304–305].

They go on to observe that Aristotle made no distinction between opinion based on human belief and that based on a probable middle term, although he did recognize that fear of the contrary was essential to his notion. Thus they conclude to two brief definitions consonant with Aristotle's thought: opinion is an assent that is true or false, without certainty and with fear, or alternatively, it is an assent that is true or false concerning a contingent object [CP305].

On the basis of these definitions Vallius-Carbone make further distinctions among kinds of opinion; these are similar to those they have previously made among kinds of science. For our purposes the most noteworthy are the distinction based on its material cause, the object or subject matter with which it is concerned; that based on its efficient cause, the way in which it is generated; and that based on the mode of its being acquired. With regard to the first, they differentiate between opinion concerned with a contingent subject and that concerned with a necessary subject not recognized as necessary. Related to this is the second difference, that based on the efficient cause of the knowledge: one kind is had through a probable syllogism; another is had through a demonstration when the argument is not recognized as a demonstration. An example of the first would be the person who believes that the sun is larger than the earth because an astronomer said so; of the second, a person who was presented with a demonstration of the fact but did not understand it. Had he understood the demonstration he would have had science; since he did not, he is in the same position as the person in the first case and has only an opinion on the matter. With regard to the mode of its being acquired, finally, one may distinguish between immediate and mediate opinion. Just as in the sciences there are certain propositions to which one assents immediately because they are evident, so also in the case of opinion; examples would be that mothers love their children and that the poor desire to be rich. Both propositions may admit of exceptions, and yet they do not require a middle term to convince one of their probable truth [CP305–306].

The foregoing distinctions may prove helpful for understanding some of the controversies that were to develop between Galileo and his Aristotelian adversaries, particularly regarding Galileo's scientific claims and the knowledge of mathematics required to understand them. The most frequent charge directed against him was that he had not offered demonstrations and that his arguments, being only probable, generated not science but opinion. Galileo's instinctive rejoinder was that he had presented demonstrative arguments but that they were not being grasped as such, frequently because the mathematics underlying them was not understood. Thus what was regarded as science by him might well be seen as mere opinion by his adversaries, even though both subscribed to the same Aristotelian canons.

A final problem addressed by Vallius-Carbone is that of the proper object of opinion: is this matters that are contingent as opposed to those that are necessary, and, if so, is it possible, as already intimated, for one to have an opinion about a necessary matter just as one may have an opinion about a contingent matter? To answer this question they first state their views on what is required for knowledge to be necessary and then set up a parallel account of how knowledge may be contingent. With regard to the necessary:

> The necessity of knowledge can be judged from four sources. First, from the necessity of the object, if the object is such that it cannot be otherwise. Second, from the cause through

> which the thing is known, namely, if it has a necessary connection with what is known. Third, on the part of the intellect, when it adverts to the fact that what it knows is necessary. Fourth, again on the part of the intellect, when apart from the latter type of knowing it also recognizes by a reflex act that it knows through a necessary middle term, that the thing cannot be otherwise, and that it is aware of this, with the result that it cannot be made to give up its assent [CP307].

In the same four ways, they continue, any particular knowledge can be said to be contingent:

> First, on the part of the object, when the object is contingent and is known under the aspect of being contingent, since it is possible to have necessary knowledge of a contingent object. Second, on the part of the middle term, when the middle is only probable and has no necessary connection with the thing known. Third, on the part of the intellect, which, although it uses a necessary middle term in the knowing process, does not see its necessity and assents to the conclusion with fear of its opposite. Fourth, again on the part of the intellect, when it thinks that an object that is necessary could be otherwise, or assents to it with fear of the opposite, or is aware that it does not give its assent necessarily [CP307–308].

With these matters understood, Vallius-Carbone note that the contingent can be said to be the proper object of opinion in much the same way that the universal is said to be the proper object of the intellect. This is so because the universal cannot be perceived by the senses, which grasps only singular things, whereas the intellect can perceive not only universals but singulars as well. Similarly there can be opinion not only of contingent matters but also of necessary matters as they appear to be contingent. Whence the proper object of opinion, as distinct from science, is the contingent; the necessary, on the other hand, is the object of science as such. On this account, although opinion is of the contingent and of the necessary as it appears contingent, there can be opinion of contingent matters that fall outside the concern of science [CP308].

Vallius-Carbone then conclude their reply to the proposed difficulty with the following summary statement:

> First, there can be opinion of necessary matters; second, opinion is always of an object considered under the formality of its being contingent; third, the contingent is the proper object of opinion, because it is not possible to have science of the contingent precisely as such [CP308].

The last part of this statement casts light on Galileo's observation in F3.1.13 that sciences are not concerned with contingent matters.

b. *Relation to Science.* In view of the aforementioned clear-cut differences between opinion and science, a few additional questions are raised by Vallius-Carbone about their relationships. One of these is the more practical, namely, whether probable arguments, which generate only opinion, add anything to a scientific exposition and whether or not they should be included in scientific discourse. The other is the more speculative, namely, whether opinion and science can coexist in the same intellect, at the same time, and on the same subject matter. Both of these questions have relevance to Galileo's writings, for he seems frequently to have intermingled probable and necessary arguments in the same treatise, and, on the important problem of the earth's motion, it is possible that he thought he had proved it scientifically and still had doubts about whether the earth actually moved.

Vallius-Carbone begin their discussion of the first question by noting that it is quite common, when trying to prove a point, to supply arguments that are probable along with those that are necessary. Some authors, they say, regard this procedure as improper. Their own view is set forth in a number of positions that enable them to evaluate and respond to this criticism. They first maintain that opinion does not increase science directly, that it is unable to do so, and that probable reasons are unable to dispose one towards science essentially and directly. They likewise state that, in the case where probable and necessary reasons are both used to prove a point, these do not produce a twofold assent or two habits of knowing, but one habit only, and this is not the twofold habit of opinion and science, but solely that of science.

These results notwithstanding, they argue that it is not superfluous to bring probable arguments in support of a conclusion that can be demonstrated; rather, probable arguments have considerable utility when used to reinforce demonstrations through causes. Their reasons are the following:

> First, because sometimes probable arguments dispose one to grasp reasons that are certain, and so help in this way. Second, because it is not easy, even for those who are learned, to recognize true and perfect demonstrations and the force of arguments. Third, because of variations in ability, for sometimes an argument will appear stronger to one person that it does to another. Fourth, because not all those who learn demonstrations recognize their nature and force, and so they do not really grasp them; on this account it is useful to reinforce them with probable arguments. Finally we would add that is not necessary to have many arguments if some are certain [CP323].

On this basis they are able to reply to the objection brought to the contrary. Probable arguments should not be contemned because they generate only opinion, which can be false, for it frequently suffices to have and to hold an opinion that is acceptable for probable reasons, especially when scientific knowledge is not attainable. On this account, even though opinion may not be completely perfect as a habit, nonetheless it has much to commend it, being quite conformable to the way most people arrive at knowledge [CP324].

The second question permits Vallius-Carbone to explore more deeply the problem of the coexistence of science and opinion in the same intellect and on the same conclusion. A number of arguments, they note, might persuade one that such coexistence is possible. For example, suppose the case where a person demonstrates a result and then finds another proof of it which he thinks is demonstrative, whereas it is not. In that case he would have scientific knowledge of it from the first proof and only opinion of it from the second. Again a person might first discover several probable arguments in support of a conclusion and then finally come upon a strict demonstration. Then he would not relinquish the knowledge he had gained from his first attempts even though he had subsequently obtained complete proof. In both cases, therefore, science and opinion would coexist in the same intellect and on the same conclusion [CP325].

While aware of these possibilities, Vallius-Carbone maintain that such coexistence is impossible. Their basic reason is that it would be repugnant to the certitude and evidence of science for it not to supply for the absence of these attributes in probable knowledge, particularly the fear of its contrary being true, for this is an essential characteristic of opinion in the strict sense. The argument applies whether one is thinking of the respective habits or of the acts whereby such habits are generated. Were coexistence possible one would have to be both certain and uncertain, in doubt and not in doubt, with evidence and without evidence regarding the same conclusion, and all of these states of mind involve a manifest contradiction [CP329–330].

This type of problem, as Vallius-Carbone further note, can be extended to the case where a conclusion is known both by science and by a faith that is not human but divine – the problem of the coexistence of science and divine faith in the same person. This has obvious application to the outcome of the process against Galileo in 1633, where the fact of the earth's motion was being argued on the basis of reason and also of divine faith. The common opinion of theologians at the time was that one cannot assent to the same conclusion by divine faith and by science in the same act: one either believes it by faith or knows it by science, and in the latter case one has no need for faith. Vallius-Carbone regard this opinion as only probable, however, thinking it more probable that science and divine faith can coexist in a person and so reinforce the certitude of assent to the conclusion [CP327–329]. They do not discuss the case where science and divine faith lead to contradictory conclusions, but since they teach that such faith generates greater certitude than does science [CP328], they undoubtedly would side with faith over science. This would be equivalent to holding that a statement assented to by divine faith must be certain, and that if its contradictory appears to be demonstrated by science, the demonstration must be flawed and the statement is eliciting assent only as a matter of opinion.[23]

6. THE PROBABLE SYLLOGISM AND THE TOPICS

Vallius-Carbone also teach that just as science is generated by the demonstrative syllogism, so opinion is generated by the probable syllogism. Galileo has an extensive analysis of demonstration in the second treatise contained in MS 27 but only mentions the probable syllogism in D1.1.4 and D1.2.3. Fortunately Vallius-Carbone have a detailed account of it in Bk. 5 of their *Introductio in logicam*. There, after labeling it the dialectical syllogism, they discourse extensively on the dialectical middle term, that is, the topic, and then consider the latter's definition and division. They conclude their logic course with a detailed description of the various kinds of topic and how they may be employed in dialectical discourse.

Vallius-Carbone describe the dialectical syllogism as one composed of probable propositions, that is, propositions that are verisimilar and worthy of acceptance and so can be regarded as true. Of such propositions they enumerate various kinds:

> The first are those that are admitted by all, e.g., that parents love their children, that shoppers want a bargain. Others are agreed on for the most part, e.g., that people prefer to be rich rather than poor. Others are admitted by the wise, and of these, some by all, e.g., that the good in itself is preferable to the useful; some for the most part, e.g., that the universe is one, that happiness lies in virtue alone. Yet others, by philosophers, that the universe had a beginning, as Plato held, or that sight is achieved by the reception of species, as do Aristotelians. For this reason matters that go beyond the opinion of all are not numbered among probables, e.g., that anything can come to be from anything else; that any one thing contains all others; that all things are one. Also included among probables are those propositions that can be deduced from probables [CL170–171].

Thus they conclude that "a syllogism composed of probable propositions, or of a probable proposition and a necessary proposition, or of necessary propositions that are regarded as probable, is said to be dialectical."[CL171] Note that they do not characterize a proposition as probable on the ground that it is concerned with contingent matter, for even one concerned with necessary matter is only probable if it is not seen as necessary. Thus its verisimilar character derives as much from the knower as it does from the thing known.[24]

Following this brief characterization of the dialectical syllogism Vallius-Carbone turn to its fuller development, focusing on its middle term, the topic, as this was first proposed by Aristotle and then subsequently developed in the Aristotelian tradition. It should be noted that Carbone himself, apart from the materials he appropriated from Vallius, wrote extensively on the topics as used in both dialectics and rhetoric.[25] Rather than use his monographs, however, we continue to follow the *Introductio in logicam* since this contained the material included in the set of logic notes available to Galileo.

Vallius-Carbone preface their discussion of the topics with the remarks that teaching makes use of opinion no less than it does of science, and indeed that many more things are held by opinion than are held by science; on this account it is important for students to be well informed about topics and probable arguments. Despite this, they continue, such instruction is generally skipped in the schools. They intend to remedy the defect by providing a fuller treatment of the subject than is to be found in other logic courses [CL172].

They begin with a description of the middle term in the dialectical syllogism and how it is related to the middle term in a demonstration:

> A dialectical middle, or argument, is a probability invented to induce belief; it is conjoined verisimilarly either to both extremes of a question or with one or the other so as to gain assent to what is to be proved, though without absolute necessity. In this the dialectical argument differs from the demonstrative, since the latter's middle goes with its extremes necessarily and thus generates an assent that cannot be doubted; the former's goes with them only probably. On this account a demonstrative argument can become a dialectical argument if one does not advert to its necessity, for anything that is necessary will be regarded as probable by those who do not grasp its necessity. Hence it is that the teaching on the invention of the dialectical middle can also serve for discovering necessary middles. For this reason, when treating of the invention of the demonstrative middle in the second book of the *Posterior Analytics*, Aristotle refers back to the teaching contained in the *Topics*. And in the latter he occasionally mentions that the treatment of topics is common to both the dialectician and the philosopher, that is, to the person arguing probably and to the one arguing demonstratively [CL173].

Noteworthy here is the close affinity Vallius-Carbone see between the probable syllogism and the demonstrative syllogism, repeating a refrain found in their *Introductio in universam philosophiam* to the effect that the two become interchangeable for those who are not expert in the subject matter being considered. Again, their observations here reinforce what has been said earlier about how inventive science and judicative science complement each other within the Aristotelian system [Sec. 2.8].

Vallius-Carbone conclude their introductory remarks with a brief definition of the topic and how it gets its name:

> This argument or middle term is commonly called a place, using the translation of the Greek *topos* or the Latin *locus*, and the books that treat of the invention of topics are called the *Topics* or, in Latin, the *Locales*, where the term designates the argument itself and not the seat of the argument or where the argument may be found. On this account a topic is generally defined as the seat of an argument or the place from which it can be obtained, for when topics are known arguments are easily discovered [CL173–174].

At this point in their text there is a marginal entry to Boethius's *De topicis differentiis*. The entry is significant, for the remainder of their exposition is not based on Aristotle's text but rather on the reconstruction of his teaching made by Boethius. This is manifest not so much in the definition of the topic as in its classification. Boethius's main division they now give, stating that there are two kinds of topics, the first of which is called a maximal topic and the second a maximal differentia. These definitions are not important for our purposes[26]; rather they lead to an enumeration of various categories of topics to be treated in some detail. This reads as follows:

> Some topics are artificial or intrinsic, taken from the matter that is being disputed about, whereas others are non-artificial or extrinsic, taken from extraneous considerations. Intrinsic topics signify either the thing from which the argument is sought or others conjoined with it or disjoined from it. In the first ordering are the topics of definition, description, and etymology. In the second are the topics of conjugates, parts and wholes, causes and effects, antecedents and consequents, and things coming before or accompanying or coming after. In the third are the topic of similars and dissimilars; greaters, lessers, and equals; and opposites and repugnants. A large number of extrinsic topics are listed by other authors but we shall posit only one, that of authority, and this can be subdivided just as are the other topics enumerated above [CL175–176].

Vallius-Carbone conclude this general overview with the remark that a twofold consideration for all these topics is possible, one common, when it is treated generally and not as applied to a particular subject matter, the other proper, when accomodated to a determinate matter. Dialectics, in their understanding, is concerned with the explanation of these topics precisely as common. They therefore devote the next fourteen chapters to their detailed elaboration.

7. TOPICS RELATED TO SCIENTIFIC ARGUMENT

Among the topics they treat not all are of equal value for understanding Galileo's logical methodology. Those that prove relevant include topics with some affinity to scientific argumentation; these are found in the second ordering of intrinsic topics enumerated in the last citation, namely, cause-effect and antecedent-consequent. Others that are invoked are located in the first and third orderings, those of definition and similarity-dissimilarity.

Topics relating to causes and effects are important because of the ways in which Galileo uses causal arguments in a dialectical way when he is not able to construct strict demonstrations from them. Those who have a superficial acquaintance with Aristotelian logic tend to equate a syllogism containing a causal middle with a demonstration, not realizing that although demonstrations use causes, not every causal explanation is demonstrative. Allied to their use in argument is the topic of antecedents and consequents. Earlier we mentioned the affinity of this topic to the HD method of modern science [Sec. 1.2a]. There seems little doubt that Galileo knew the correct norms for hypothetical reasoning, and perhaps used it extensively in his dialectical explorations. But one should not extrapolate therefrom that all of his reasoning was hypothetico-deductive in its modern understanding, for this would have eliminated the possibility of his achieving scientific knowledge in the strict sense. The topics of definition and similarity, finally, find use in Galileo's attempts to arrive at the natures of celestial phenomena such as comets and sunspots, as will be seen later in Secs. 5.3 and 5.4b.

a. *Causes and Effects.* Vallius-Carbone devote separate chapters to the topics of material, formal, efficient, and final cause, treating the effects of each along with the cause, and then supplying maxims and examples that illustrate how they may be used in dialectical argument. They presuppose that one knows the definition of cause and its four kinds from other parts of their logic.

Regarding the material cause they first explain three understandings of the term matter: that from which something is made; that in which something is present as in a subject; and that which is the object of some activity or what an agent treats. They then introduce distinctions between permanent matter, which remains in what is made from it, and transient matter, which does not, and between proximate matter, that from which a thing is made directly, and remote matter, that from which the proximate matter is prepared [CL184]. These notions understood, Vallius-Carbone illustrate how various dialectical arguments can be based on the material cause:

> First, from the positing of the cause one may deduce that the effect is possible: thus, if there are wood, stones, and cement there can be a house, though this mode does not conclude necessarily, since apart from the matter other causes are necessary. Second, by denying: from permanent matter by simply negating – there is no iron, therefore no sword; from transient matter, by using a past tense and a present effect – there was no flour, therefore there is no bread. The commonplaces or maxims: when a material cause is posited an effect can be posited; when matter is taken away, so is the effect. Third, by affirming the effect of a permanent material cause, but not by negating: there is a table, therefore wood, not the other way around. With transient matter it merely follows that the cause has preceded: there is oxymel, therefore there was vinegar and honey. Finally, from the attributes and effects of matter: the wood is dry, therefore it burns easily; man's body is composed of elements, therefore it can corrupt; lead is dense, therefore it contains much matter; dialectics is not the same as logic, therefore they have different subject matters [CL185].

Vallius-Carbone give similar treatment to the formal cause, first noting that it is threefold, essential, accidental, and exemplary, and then explaining how each of these can be used in argument. An example of the essential: the soul is present in the body, therefore a live person; the soul has left, therefore no longer a person but a cadaver. Of the accidental: the wood is round, therefore it rolls; this figure has the greatest volume for its circumference, therefore it is circular. Similarly one can argue from the effects of a form as follows: a sponge senses, therefore it is an animal; the heavens revolve easily, therefore they are spherical. The maxims for the formal cause are these: if a form is posited, so is the thing of which it is the form, with all its properties and attributes; if the form is removed, so are the others; and if the formal effects are removed, so is the form [CL186–187]

An efficient cause, continue Vallius-Carbone, is that from which an operation first proceeds; one kind is necessary, that from which an effect inevitably follows since it can arise from no other cause – in this way the sun is the cause of day; another is sufficient in the sense that it can produce the effect by itself, though the effect may be produced by another cause – in this way the taking of poison is the cause of death, fire the cause of heat [CL188]. Dialectical arguments may be taken from this cause as follows:

> The first mode, from a necessary and solitary cause, by affirming and denying: the earth is interposed between the sun and the moon, therefore an eclipse; the earth is not interposed,

> therefore no eclipse. From effect to cause: it is day, therefore the sun has risen; it is not day, therefore the sun has not yet risen; fish do not breathe, therefore they have no lungs. The commonplaces: when a necessary and solitary cause is posited or removed, so is the effect; and when the effect of such a cause is posited or removed, so is the cause.
>
> The second mode, from a sufficient cause by affirming and from its effect by denying: he took poison, therefore he died; he did not die, therefore he did not take poison. The commonplaces: when a sufficient cause is posited, so is the effect; when the effect is removed, so is the cause.
>
> The third mode, one may argue from a cause actually acting by affirming and denying, though this requires adding on the part of the effect a verb indicating the action: he is teaching, therefore a lecture; there is a lecture, therefore he is teaching.
>
> The fourth mode, from a cause able to act, for proving that the effect can exist by affirming, though this entails also positing the remaining requisites: he is a builder, therefore a building can be built; there is a building, therefore there was a builder.
>
> One can further argue from a conserving cause: these fish swim in the water, therefore they can continue to live; those are out of the water, therefore they cannot live; discord is absent from the city, therefore it will survive; the kingdom is divided against itself, therefore it will fall [CL188–189].

The end or final cause, lastly, is that for the sake of which something is done, and serves to explain the means and the other three causes. Vallius-Carbone explain its various kinds and how an entire range of dialectical arguments may be drawn from them, but these are omitted here as being more related to moral discourse and having little relevance to Galileo's scientific writings.

b. *Antecedents and Consequents.* Vallius-Carbone proceed directly from the topic of final cause to that of antecedents and consequents. They begin by noting that they take antecedent and consequent to mean anything that necessarily precedes or follows a subject under consideration. Thus to inquire whether or not a woman has borne a child one may investigate what necessarily precedes birth and follows after it, and these will provide topics from which one can construct arguments pro and con. As with causes, they propose a division of antecedents and consequents into various kinds: some are connected absolutely or necessarily; others are connected suppositionally; and yet others are recursive or reciprocal [cf. Sec. 1.5a]. They explain:

> There are two kinds of antecedent and consequent: the first precedes or follows in the order of attribution, the way man precedes animal, or in the order of time, the way the taking of poison precedes a death that results necessarily from it; the second necessarily results if something else is posited, the way the blossoming of fruit comes before its eating and the foundation of a building before its walls. Thus the animal and the death are consequences

> of the first kind, the fruit and the walls of the second. Antecedents of the first kind differ from the second in that the former precede or are followed in an absolute way, the latter only suppositionally (*ex suppositione*).
>
> There is a yet further division of antecedent and consequent; some are reciprocating, that is, convertible, as to be a testator for a dead person and to execute a will; others are not, as to be justice and to be a virtue, to give birth and to conceive [CL193].

On the basis of this classification, Vallius-Carbone first provide examples and maxims of absolute implication, including cases that are both reciprocating and non-reciprocating:

> The following examples of antecedents involve absolute attribution: he is a son, therefore an heir; courage is desirable, therefore it should be developed; laziness is not in accord with reason, therefore it is not a virtue; his throat was cut, therefore he died.
>
> If antecedents and consequents are reciprocating, one may go from the denial of the antecedent to the denial of the consequent and from the placing of the consequent to the placing of the antecedent; thus, he lacks charity, therefore he does not love God above all things; he loves God above all things, therefore he has charity; he does not love God above all things, therefore he lacks charity.
>
> The common topics on which the foregoing arguments are based are these: placing the antecedent necessarily involves placing the consequent; removing the consequent necessarily involves removing the antecedent [CL194].

Then follow examples and maxims of suppositional implication:

> These examples of antecedent and consequent are of the second kind: she did not conceive, therefore she did not give birth; she gave birth, therefore she conceived; a building is to be built, therefore foundations must be laid; foundations have not been put in place, therefore there will be no building; he was not an adolescent, therefore he will not be an adult; he is an adult, therefore he was an adolescent; he went bankrupt, therefore he had wealth; he had no wealth, therefore he did not go bankrupt; he wishes to attain eternal happiness, therefore he should believe in God and act virtuously. The maxims: removing the antecedent involves removing the consequent; placing the consequent involves placing the antecedent [CL194].

The logic behind these examples is obviously that of the valid modes of the hypothetical syllogism, *ponens* and *tollens*, which regulate modern HD-method and its procedures for verification and falsification. And just as the truth table for material implication in modern logic is verified in the examples involving absolute and suppositional implication, so that for equivalence can be seen to be operative in the reciprocating examples. The important thing to note, however, is that these forms of reasoning are not regarded as apodictic by Vallius-Carbone, but rather provide dialectical arguments that assist the process of invention and thus for discovering what might be the truth, not for actually demonstrating it.

c. *Definition and Similarity.* Within the topic of definition Vallius-Carbone first treat the questioning process whereby one arrives at definitions. This for them is done in the context of putting a predicate or attribute with a subject and testing it in various ways, both affirmatively and negatively. The affirmative part attempts to define both the attribute and the subject and determine whether or not they go together in the following combinations: the definition of the attribute with the subject, the definition of the subject with the attribute, and the definition of the attribute with the definition of the subject. The negative procedure is similar, except that one takes the negations or the contraries of the various subjects and attributes and their definitions and tests the same combinations. If this does not yield a satisfactory result, then one resorts to related topics, such as those of differentia, description, and property. That is, in place of definitions of the subject and the attribute one takes differentia associated with them and tries similar combinations. Or, alternatively, one takes descriptions of them and tests the various combinations again. Or, once more, one takes various properties or characteristics associated with them and does the same. In this way one is eventually able to arrive at some type of definition of the subject, even though this might be merely descriptive and not essential. The basic maxims that guide these procedures are the following: anything of which the definition (differentia, description, or characteristic) can or cannot be said applies also to the thing defined (differentiated, described, or characterized); whatever can or cannot be said of the definition (differentia, description, or characteristic) applies also to the thing defined (differentiated, described, or characterized) [CL176–179].

From this general technique, it is possible to branch out into a whole series of comparative procedures, such as considering similars and dissimilars, things equal, greater, or less, opposites, repugnants, and so on. Similarity is particularly fruitful in that it opens up the search to include analogies and proportionalities that frequently help in the defining process. In this context Vallius-Carbone take similars to mean any qualities, quantities, or natures that have elements in common or can be placed in some kind of proportional relationship to each other [CL196--197]. Whether consciously or not Galileo appears to have made good use of this topic for finding terrestrial models in terms of which he could understand celestial phenomena.

8. RHETORIC AND DIALECTICS

Closely allied to logic and dialectics is rhetoric, the art or science of persuasive reasoning. Since Galileo is frequently said to have used rhetoric in his polemical treatises and in his scientific expositions as well, before concluding this chapter we should at least raise the question of how he conceived rhetoric to be related to science and dialectics. Now rhetoric is mentioned only in passing in the logic course available to Galileo when he was composing MS 27. Usually the context is that of defining the scope and status of logic, which is said not to be a science or an art in the strict sense, but rather to be an instrumental habit, as explained above [Sec. 2.4]. As such a habit, logic is differented from dialectics and rhetoric by Vallius-Carbone, who describe the latter two as faculties or abilities that assist one in probable and persuasive reasoning respectively. Rhetoric in this understanding was highly developed at the Collegio Romano, and Carbone, who had studied there, turned out to be a master rhetorician. As we have noted, he composed several books on that subject, focusing on the rhetorician's use of topics and the ways in which his reasoning is similar to the dialectician's.[27] There are also indications that some of this material, like his *Introductio in logicam* and his *Additamenta*, was appropriated from the rhetoric notes of his teachers at the Collegio.[28]

Carbone is a good source from which to characterize rhetoric as it was probably understood by Galileo in his years at Pisa, for his work is representative not only of the Roman tradition but of that in the northern Italian universities as well. A better source, however, is an author cited favorably by Carbone, Antonio Riccobono, who was professor of rhetoric at Padua when Galileo went there to teach in 1592 and who became one of Galileo's friends. Riccobono composed a brief treatise on the nature of rhetoric in which he explained how rhetoric was similar to logic, dialectics, and the science of politics, while pointing out that it also differed from each in important particulars.[29] His work was directed against Mirandulanus, who has been discussed above, and to some degree against Zabarella, who taught logic at Padua while Riccobono was teaching rhetoric.

For Riccobono the major difference between dialectics and rhetoric is that the first is concerned with the probable [*probabile*] whereas the second is concerned with the persuasible [*persuasibile*].[30] People are persuaded by logical probabilities, but they are also persuaded by appeals to the emotions and to the character of the one persuading. Other differences noted by Riccobono are that the dialectician treats universal matters, the rhetorician singulars; the first uses question and answer in the form of a disputation, the second a continuous exposition directed to the common understanding of men; the first generates opinion, the second belief or persuasion; the first in some ways has a greater range than the second, since persuasibles always involve probabilities, whereas not all probabilities are persuasibles; and finally, dialectics is a general faculty for treating questions pertaining to any field of knowledge whatever, whereas rhetoric seems more restricted in its concerns, being exercised mainly in the political arena.[31]

On this last point, the proper subject matter of rhetoric, Riccobono presents a distinctive teaching that might have had an influence on Galileo. Most of Riccobono's contemporaries wished to restrict the matter of rhetoric to political discourse or to human affairs, since these are the subjects treated by Aristotle in his three types of rhetoric: deliberative, forensic, and epideictic. Zabarella, in fact, insisted that rhetoric was exclusively concerned with action and not at all with knowledge. But, while agreeing that human affairs constitute the principal concern of the rhetorician, Riccobono was unwilling to admit that they are his exclusive concern. Thus he writes:

> It is very true that the principal matter of rhetoric is things that humans do, and that the things humans do are included among the types of rhetoric. But Aristotle, speaking generally, does not seem to exclude other matters from the concern of the rhetorician, not even those that pertain to knowledge, provided the rhetorician treat them in a way that is appropriate for common understanding. For, when he proves that the function of rhetoric is not proper to other arts, he says that medicine is concerned with health and sickness, geometry with the properties of extension, arithmetic with numbers, and likewise the other arts and sciences in their proper subject matters, but the rhetorician with any matter whatever, in a way that is appropriate for persuading. He does not exclude what is treated in other arts, but makes them all the province of rhetoric itself if their matters are treated in rhetorical fashion.[32]

Therefore, just as all matters fall under the concern of the dialectician to the degree that they are probable, Riccobono would argue that all matters fall under the concern of the rhetorician to the degree that they are persuasible. So he would extend its ambit to include even scientific discourse:

> It is thus apparent that it is licit for rhetoric to use arguments drawn from common topics not only when dealing with civil matters, which are concerned with some proposed action, but also with natural science and indeed with any matter whatever. And this great utility of

rhetoric was recognized by Aristotle, namely, that the other arts do not persuade everyone, since they use the ways of speaking that are proper to the particular sciences; rhetoric, on the other hand, persuades all people, since it induces conviction and assent from common notions.[33]

One can only surmise whether or not this widened conception of rhetoric was known to Galileo. Moss has made the point that Galileo was among the first to use rhetoric in scientific discourse, and in the Copernican debates, particularly, he clearly did so to persuade everyone, not merely scientists.[34] It is thus not beyond belief that he obtained this conception of rhetoric from his colleague Riccobono.

In Galileo's day the notion of science in the strict sense, as we have seen in this chapter, was very demanding. It represented the highest level of human knowing, and thus had stringent requirements, more of which we shall outline in the following chapter. But the habit of science did not stand alone in the late sixteenth-century; it was buttressed by other intellectual habits, especially by dialectics and sometimes even by rhetoric. This circumstance may cast light on why probable reasoning, along with persuasive argumentation, came to play such an important role in Galileo's scientific treatises.

NOTES

[1] Note that this passage provides the rationale for Galileo's brief prologue to the treatise on demonstration [D]; see Sec. 4.4 below.

[2] See also the explanation in the previous chapter of first and second intentions and the role of the former in a realist epistemology, Sec. 2.3.

[3] Note the similarity of the four causes enumerated here by Vallius-Carbone to the four causes given by Galileo as causative of demonstration [D1.1.2], particularly when demonstration is considered as a type of illative discourse.

[4] How this process takes place is shown schematically in the life-powers model diagrammed on Figure 1 of Sec. 2.1b above.

[5] It is noteworthy that this last view seems to be corroborated by the advance of science through the centuries. Whereas, in ancient times, a natural philosopher might be thought competent to deal with all areas corresponding to the scope of Aristotle's *Physica*, later developments have shown the desirability of partitioning his work, as it were, and alloting the different areas mentioned above respectively to the astronomer, physicist, chemist, biologist, and psychologist, each of whom, it would seem, acquire different habits of thought in the development of their disciplines.

[6] For MS 46 see Galileo's *Early Notebooks*, 32, 112, 151, and 256.

[7] *Galileo and His Sources*, 130–131.

[8] A fuller discussion of the agreements and differences among Zabarella, Vallius, and Galileo, will be found in our "Zabarella and Galileo: The Transmission of Paduan

Methodology," *Giacomo Zabarella tra filosofia e scienza*, ed. Luigi Olivieri, Padua: Editrice Antenore, forthcoming.

[9] This is an important consideration when evaluating the scientific character of Galileo's *Two New Sciences* in terms of the canons provided here by Vallius-Carbone. In that work Galileo arrived at many of his principles by *a posteriori* reasoning; thus his would qualify as true science by these canons.

[10] Ludovico delle Columbe offers a similar evaluation of mathematics in his *Contro il moto della terra*, attributing it to St. Thomas Aquinas; see GG3: 255.

[11] It is noteworthy that Clavius, in a paper written around 1586 for the Jesuit Order justifying courses in mathematics in its houses of studies, argued that without mathematics "physics cannot be correctly understood," particularly not matters relating to astronomy, to the structure of the continuum, to meteorological phenomena such as the rainbow, and to "the ratios of motions, qualities, actions, and reactions, on which topics the *Calculatores* have written much." See *Prelude to Galileo*, 231 and 241 n. 79.

[12] In this and related passages, the terms astrology (*astrologia*) and astronomy (*astronomia*) are sometimes accidentally interchanged, possibly owing to Carbone's editorial work on Vallius's lecture notes. Here we preserve the usage found in Vallius's *Logica* of 1622, where he is explicit that astronomy studies the motion of the stars, astrology, events that arise from their motion [VL2: 660].

[13] The text of Aristotle here [194a7–9] is cryptic and it is difficult to know what it means. The common Latin translation reads: "Demonstrant autem et quae magis physica quam mathematica, ut perspectiva et harmonica et astrologia; e contrario enim quodammodo se habent ad geometriam." The very obscurity of the Latin leaves it open to a variety of interpretations.

[14] It should be noted, moreover, that the arguments advanced in the passage just cited overlook several important teachings found in the *Posterior Analytics*, especially how the demonstrative *regressus* works, how proofs in physics are generally *a posteriori* and rarely *a priori*, and how even demonstrations from remote causes can be strictly scientific. It is perhaps significant that when Galileo turned seriously to astronomy in 1609 he was able to exploit precisely these teachings to develop a "new science" of the heavens,as argued below in Chapter 5.

[15] With regard to the middle science of mechanics, there is no treatment of it in Vallius-Carbone's treatise on science. For an account of how mechanics was regarded in the mixed mathematics tradition of the Collegio Romano, see *Galileo and His Sources*, 136–139, 206–216.

[16] Blancanus, *De natura mathematicarum scientiarum*, 5.

[17] *De natura mathematicarum scientiarum*, 6–10.

[18] *De natura mathematicarum scientiarum*, 10–13.

[19] *De natura mathematicarum scientiarum*, 19–27.

[20] *De natura mathematicarum scientiarum*, 26.

[21] This particular demonstration is worthy of note because of the way Zabarella explains the interplay between internal and external causes in accounting for celestial phenomena. His explanation, abbreviated and paraphrased from chaps. 10 through 13 of Book 1 of his *De medio demonstrationis*, is as follows:

> [Chap. 10:] In every *demonstratio potissima* the middle term [M] will be the cause and the definition of the major term [P], but not of the minor term [S], except rarely. In most

> cases the middle term will be the cause of the major term; in a few cases it will be the cause of both the major and the minor terms; but it will never be the cause of the minor term alone. Indeed, one never seeks the cause of the minor term, for this pertains only accidentally to the demonstration.
>
> [Chap. 11:] Two kinds of accidents are demonstrated from their causes: those that have an internal cause and those that have an external cause. I call a cause internal when it inheres in the same subject as the accident, for example, a spherical figure is the cause of the moon's going through phases. An external cause, similarly, is in a place different from the subject of the accident, for example, the interposition of the earth is the cause of the moon's being eclipsed. Effects or attributes that result from an external cause always accompany the cause, but not the subject; for example, the moon is not always being eclipsed. Effects or attributes that result from an internal cause, on the other hand, always accompany the subject; for example, man is always risible.
>
> [Chap. 12:] When a cause is external, the middle term [M] is always the cause of the attribute [P] and never of the subject [S].
>
> [Chap. 13:] When a cause is internal, the attribute follows from the nature and the form of the subject, but it need not always be demonstrated from this form. The reason for this is that accidents proceed from their subject in a certain order, and sometimes one accident is the cause of another.
>
> Demonstrations always require the immediate and proximate cause of an attribute; in the case of a few accidents, the proximate cause will be the definition or nature of the subject, but this is not always so. For example, the spherical form is an accident of the moon but it is not its nature; thus the spherical form is the cause of its crescent phases. The cause of its spherical form, however, is the nature of the moon, if this were known [ZL550–553].

In Zabarella's day, of course, the nature of the moon was not known, but in the present day, with our knowledge of the moon's composition and the forces acting within it, we now can give a causal explanation even of its spherical form. For an analysis of such forces, see S.H. Dole and I. Asimov, *Planets for Man*, New York: Random House, 1964.

[22] *De natura mathematicarum scientiarum*, 29–31.

[23] On this point see our "Galileo's Science and the Trial of 1633," *The Wilson Quarterly* 7.3 (1983), 154–164.

[24] This way of presenting probable reasoning offers a neat way of eliminating the dichotomy some see between the "dogmatism" of a proof that is proposed as necessary and the "civility" of an argument that is presented only dialectically. In the final analysis the warrant for a demonstration is not that it is an "eternal truth" in the mind of the one proposing it but rather that it is received as conclusive by those it is intended to convince.

[25] His most important works in this category are *De oratoria et dialectica inventione vel de locis communibus*, Venice: Apud Damianum Zenarum, 1589: *De arte dicendi libri duo*, Venice: Ex officina Damiani Zenarii, 1589; and *Divinus orator, vel De rhetorica divina libri septem*, Venice: Apud Societatem Minimam, 1595.

[26] A clear explanation of these terms will be found in Eleonore Stump, *Boethius's De topicis differentiis*, Ithaca and London: Cornell University Press, 1978, particularly 201–204, where she discusses how they are related to Aristotle's topics. Also helpful in this regard is N.J. Green-Pedersen, *The Tradition of the Topics in the Middle Ages: The Commentaries on Aristotle's and Boethius' "Topics"*, Munich-Vienna: Philosophia Verlag, 1984.

[27] The more significant of these have been listed in n. 25 above.

[28] Moss, "The Rhetoric Course at the Collegio Romano."

[29] *De natura rhetoricae*, included as an appendix to his Latin translation of Aristotle's *Rhetoric*, Venice: Apud Paulum Meiettum, 1579.

[30] *De natura rhetoricae*, 214. *Persuasibile* being an odd term, it is somewhat remarkable that Galileo uses it in the preface "To the Discerning Reader" with which he introduces his *Dialogue on the Two Chief World Systems*, noting that he has "thought it good to reveal those probabilities that render this [the Copernican hypothesis] persuasible, given that the earth moves (ho giudicato palesare quelle probabilita' che lo renderebbero persuasibile, dato che la Terra si movesse)" [GG: 7: 30].

[31] *De natura rhetoricae*, 213–219.

[32] *De natura rhetoricae*, 211.

[33] *De natura rhetoricae*, 211.

[34] In her "Galileo's *Letter to Christina*: Some Rhetorical Considerations," *Renaissance Quarterly* 36 (1983), 547–576; for additional details, see her *Novelties in the Heavens: Rhetoric and Science in the Copernican Controversy*, forthcoming.

CHAPTER 4

DEMONSTRATION AND ITS REQUIREMENTS IN MS 27

Just as the dialectical syllogism treated in the *Topics* produces opinion, so the demonstrative syllogism treated in the *Posterior Analytics* produces science. In the previous chapter we discussed science, opinion, and the dialectical syllogism; now we turn to the demonstrative syllogism and its requirements. This topic is essentially the burden of the entire MS 27, where it is covered in great detail. Because of that detail it is difficult at times to see the forest for the leaves, to discern the main lines of a systematic treatment of demonstration. The aim of this chapter is to assist the reader with a more comprehensive account, to present in a more didactic way the definition and division of demonstration and the various requirements these entail. Since much of this information is contained in the manuscript itself the chapter may be regarded as a guide to its contents. It is also intended to supplement the materials contained in the manuscript, particularly topics of which knowledge is there presupposed. Some of this is contained in Vallius-Carbone's *Additamenta* and *Introductio in logicam*, some in Vallius's *Logica* of 1622, materials similar to those cited in previous chapters. There are also passages from other sources, including professors who taught the logic course at the Collegio Romano around the same time as Vallius, that cast fuller light on the teaching appropriated by Galileo. These are introduced at appropriate places in the exposition.

We begin with a summary of the material missing at the beginning of MS 27, a disputation on foreknowledge and foreknowns in general, as explained in the Latin Edition, pp. 117–119. Following this, we proceed through the contents of the first treatise, which is devoted to foreknowledge and foreknowns in particular. In MS 27 Galileo treats this in three stages, first discussing foreknowledge of principles, then that of subjects, and finally that of properties and the conclusions of demonstrations. We cover the essential content of his exposition in two sections, one devoted to principles and suppositions, the other to subjects and properties as these function in demonstrations. Then we turn to an exposition of Galileo's second treatise, that devoted to demonstration itself. This likewise is treated by him in three stages, the first dealing with the nature and importance of demonstration, the second with properties of demonstration, and the last with kinds of demonstration. We explain the first in sections devoted to definition and demonstration, its nature and species; the second, in sections devoted to causality, induction, immediate premises and their kinds, and the types of predication they involve; and the third, in a section devoted to the demonstrative regress.

1. FOREKNOWLEDGE IN GENERAL

Vallius-Carbone begin their discussion of foreknowledge (*praecognitio*) with some general observations about the way in which human knowledge is naturally acquired, presupposing materials treated in our previous chapters. They observe that just as in other natural processes some material is required on which agents can act, so it is with knowing. And since the logician examines the instruments wherewith science is acquired, it is incumbent on him to study things that must be foreknown, since these are the ways through which one comes to know matters that would otherwise be unknown. "Foreknowing," as the term indicates, means knowing something previously or beforehand. The term can be used in a variety of ways: for sense knowledge, since all natural knowledge takes its origin from the senses [Sec. 2.2]; for invention or teaching, since a person discovers knowledge on his own or acquires it from a teacher [Sec. 3.1b]; for knowledge on which other knowledge necessarily depends, the way one must know logic to have science in the strict sense [Sec. 1.5a]; for knowledge acquired in the first operation of the intellect, since knowledge used in the second and third operations depends on that of the first [Sec. 2.2]; and for principles known by the light of nature (*naturae lumine*) that are grasped as soon as their terms are understood [Secs. 2.1–2,3.1]. From these usages Vallius-Carbone extract two meanings of foreknowledge: one common, a knowledge that precedes other knowledge in any way whatever; the other proper, a knowledge required to attain new teaching, whether it actually generates such teaching or merely assists in its acquisition. The second is their major concern, and this implies two facets to foreknowing: one mainly auxiliary, whereby a person is directed or helped to attain new knowledge; the other effective or productive, whereby the new knowledge is actually attained [CA36r].

Related to this understanding of foreknowledge are two other notions Vallius-Carbone now clarify, the "foreknown" (*praecognitum*) and the "mode of foreknowing" (*modus praecognoscendi*). The first is the object with which the foreknowledge is concerned, such as the subject and predicate of a statement or a principle used in a proof. The other is the particular aspect under which that object is known, such as whether it is, what it is, or, if the object is a term, what the term means [CA36v].

a. *Kinds of Foreknowledge.* The two kinds of foreknowing in the proper sense are now labeled by Vallius-Carbone, using Averroes's terminology: the auxiliary type they call "directing" foreknowledge (*praecognitio dirigens*), the productive type, "acting" foreknowledge (*praecognitio agens*). These they further subdivide as follows.

Directing or helping foreknowledge, they say, can focus on what a thing is (*quid est quod dicitur*) and what a term means (*quid nominis*), or on whether something exists (*an sit*). The question of existence, in turn, can concern a thing that is the object of a simple question or one that is the object of a compound question. And these can be subdivided again according to the two instruments of scientific knowing, definition and demonstration. In definition one can inquire into the meaning of the term and whether what it designates exists; in demonstration, whether the subject of the conclusion exists and what its predicate means.

Acting or effective foreknowledge, they continue, can be classified in many ways. One division is into simple and discursive. Simple foreknowledge is foreknowledge of a definition; discursive is foreknowledge of either the premises of a syllogism, or the antecedent in an enthymeme or example, or the principles of a demonstration. Another division is into universal and particular: the first designates knowledge of common principles on which all conclusions depend, the second, knowledge of the proper principles of a demonstration. Yet another division is into knowledge that is acting for us (*secundum nos*) and knowledge that is acting in the order of nature (*secundum naturam*). Foreknowledge that is acting in the order of nature is knowledge of the cause of something's existence; this cause naturally precedes its effect and is the cause of our understanding that effect. Foreknowledge that is acting for us, on the other hand, is knowledge of something that leads us to knowledge of another thing; it is not itself the cause of that thing, although it is the cause of our knowing it [CA36v].

All of these subdivisions, Vallius-Carbone conclude, are reducible to the two kinds of foreknowledge mentioned by Aristotle in text 2 of the first book of the *Posterior Analytics* [71a12–17], namely, "is it?" (*an sit*) and "what is it?" (*quid sit*). (Note that this is the text referenced in

Galileo's F2.2.1.) For if the first question is applied to principles and one inquires whether they are true or not, this is acting foreknowledge since it produces knowledge of the conclusion; if, on the other hand, the first question is applied to subjects, it is merely directing foreknowledge since it usually points only to a subject that is known to exist. The second question, when applied either to the subject or the property, is directing foreknowledge, for it always inquires for the meaning of a term. Therefore the first question involves different kinds of foreknowledge when applied to principles and to subjects, the first relating to their truth and the second to their extramental existence; the second question, as opposed to this, always requires the same kind of foreknowledge, the directing foreknowledge supplied by the meanings of terms [CA37r]. It may be noted that these statements cohere with the teachings appropriated by Galileo in his F2.2.3, F3.1.8, and F4.1.5, although he does not there identify the respective foreknowledges as acting and directing. He does mention these two types, however, in F2.2.5, F3.6.3, and F4.1.12.

After examining the order in which different kinds of foreknowledges should be acquired, Vallius-Carbone consider whether foreknowledges are necessary, and, if so, on what grounds. With regard to the directing foreknowledge of the meaning of terms, they say that this is a practical necessity because of differences in languages and idiomatic expressions. Thus Greek terms require explanation for Latins; again, even within the same language, various disciplines develop their own terminologies, which must be understood if one is to discourse meaningfully about their subject matters. To a certain extent such foreknowledge is required even for a person to acquire scientific knowledge by himself, understanding by this requirement not a precise technical meaning of terms but one based on sense experience and expressed in ordinary language. With regard to acting foreknowledge, on the other hand, the necessity is absolute. The reason for this is that such foreknowledge is an efficient cause of subsequent knowledge. Since the effect cannot exist without its cause, if the antecedent knowledge does not exist the subsequent cannot exist either [CA39v].

There still remains a question relating to Aristotle's text, for in text 2 of the first book of the *Posterior Analytics* Aristotle does not say that one must know the existence of the subject of a science, and in fact, in text 24 of the same [76a32–b24], he states that sciences do not inquire into the existence of their subjects. Vallius-Carbone's reply to this difficulty is twofold. In general, they say, when Aristotle states that there are two foreknowledges, *an sit* and *quid sit*, he does not mean that these two are required for each and every science, but only that when foreknowledges are required, they are reducible to these two. With respect to the existence of the subject, on the other hand, they maintain that Aristotle did not intend this to be a general statement: what he meant was that some sciences do not inquire into the existence of their subjects because it is obvious to everyone that these exist and thus there is no point in raising the question of their existence [CA39v].

b. *Kinds of Foreknowns.* With the kinds of foreknowledge thus explained, Vallius-Carbone discourse more briefly on the kinds of foreknowns. Foreknowns themselves, they point out, are nothing more than the objects of foreknowing. And Aristotle himself, when treating of foreknowing, seems to have enumerated three such objects, namely, principles or axioms (*dignitates*), subjects, and properties. Yet various arguments can be brought against this number to show that there should be more. For example: apart from the principles of a demonstration one must also known their premises; again, the conclusion is in some way foreknown in the premises, and this too should be an object of foreknowing; yet again, there are demonstrations that do not prove a property of a subject, so in place of a property something else must be foreknown. And then, at the opposite extreme, there is Aristotle's statement, just discussed, to the effect that there are only two foreknowledges, and thus it would seem that two foreknowns should be sufficient. Since most commentators on Aristotle invoke arguments of this type, Vallius-Carbone propose to sort them out so as to arrive at a reasonable position [CA39v–40r].

With regard to Aristotle's statement in text 2, they reiterate, it is probable that his intention was not to enumerate the foreknowns themselves, but merely to indicate that all modes of foreknowing are reducible to two, namely, those relating to existence (*an sit*) and those relating to meaning (*quid sit*). Whatever Aristotle's intention might have been, however, it is clear that three foreknowns are required for a demonstration, namely: a principle (*principium*), a thing given (*datum*), and a thing inquired about (*quaesitum*). Under principle they would include all axioms and premises that function in a demonstration; under the thing given they would place anything that would be the subject of a demonstration; and under the thing inquired about they would include anything that can be concluded about the given subject. Similarly, for them there must be three foreknowns for a definition, namely: a thing defined, a genus, and a differentia. Their argument in support of this is the following: one cannot arrive at a definition or quiddity if one does not know in some way what one is attempting to define; and once one knows this, one further requires knowledge of a genus under which it may be located, and then knowledge of a differentia that will separate it off from other objects contained under that genus [CA40v].

All of the above statements relating to demonstration cohere with Galileo's discussion of the question he treats in F3.6, and particularly with his implied answer to the difficulties to be resolved there, for which, in F3.6.4, he references the missing disputation on the number of foreknowns.

2. PRINCIPLES AND SUPPOSITIONS IN DEMONSTRATION

At this point in the *Additamenta* Vallius-Carbone turn to a consideration of foreknowledges in particular and immediately address the problem of the foreknowledge required of principles, the matter with which Galileo's MS 27 begins [F2]. Galileo's treatment there, however, especially its first question [F2.1], is truncated when compared to that in the *Additamenta*. The latter prefaces the discussion of foreknowledge with an enumeration of the various kinds of principles, a division clearly presupposed for an understanding of Galileo's F2.1.4. To supply the missing material we first explain the kinds of principle that are under discussion in F2. Then, since suppositions are mentioned there as a type of principle, a type that turns out to be important for understanding the demonstrations in which Galileo was interested, we next take up the topic of supposition.

a. *Principles.* The general notion of principle, in the sense of the Latin *principium*, is that it marks a beginning, that from which or with which something starts or begins in any way whatever. Such a broad compass invites a variety of distinctions among types of principle. Vallius-Carbone recognize this at the outset and so propose a series of divisions. Some principles they say are complex, meaning by this propositions from which others can be deduced, and here they give the example, "Nature does nothing in vain." Others are non-complex: these are simple terms or things signified by them, such as a cause, a motion, or a nature. Non-complex principles can be further divided into those that are principles of knowing alone and those that are principles of being. Principles of knowing (*principia cognoscendi*), while not themselves causing, nonetheless lead to knowledge of things that do; an example would be the pulse, which the doctor uses to discern movements of the heart. Principles of being (*principia essendi*), on the other hand, function in the ontological order and not merely in the cognitive; any physical cause, such as the heart's motion itself, would therefore fit in this category [CA40vb–41ra].

Complex principles are of greater interest to the logician, and these may be divided in various ways. Vallius-Carbone here propose three divisions, one based on the extent of their use in the sciences, another on the extent of their being known, and yet another on their function in a demonstration or proof.

The first division embraces principles that are most common (*communissima*), such as "A thing cannot be and not be at the same time," used in all the sciences; others are common to many sciences but not to all (*communia multis*), such as "A whole is greater than its part," used in geometry and arithmetic; and yet others are proper to particular sciences (*propria*), such as "Nature is a principle of motion and rest," employed in the natural sciences. The second division is similar to the first but is only twofold: principles known to all (*omnibus*), and these are most universal principles such as the principle of non-contradiction; and principles known only to the learned (*doctis*), such as the principle "From nothing, nothing comes" [CA41ra].

The third division is of special interest because of its use in the *Posterior Analytics*. Vallius-Carbone describe it as follows:

> Principles are also divided into axioms (*dignitates*) and positions (*positiones*). Axioms are propositions known to all from the mere knowledge and explication of their terms; they are worthy (*digna*) of commanding assent from all, and must be known at the beginning of a science. Positions, on the other hand, are propositions that are not immediately evident, nor need they be known beforehand by the one being taught. Positions that assert that something is or is not such and so are called suppositions (*suppositiones*); those that neither affirm nor deny in this way are called definitions (*definitiones*). And if either of these propositions asserts something unknown to the learner or something he thinks is false, the instructur asks him to concede them in the beginning since they will be proved later in the science. These are then called petitions (*petitiones*) or postulates (*postulata*) the latter being the term used among mathematicians [CA41ra].

Note that this particular division of principles was known to Galileo, for it occurs in MS 27 at D2.3.7, although there he presents it as a division of immediate propositions [Sec. 5.7b] rather than as a division of principles.

It is also noteworthy that Vallius, in his *Logica* of 1622, gives essentially the same division, recognizing it as a division of principles, although he varies the wording somewhat from his earlier formulation:

> Axioms in this context [*Posterior Analytics*, A.10] are neither suppositions nor positions, but they are propositions that are known so readily that a person cannot refuse assent to them internally, though he might deny them with his lips. Suppositions are propositions that, even though they might be demonstrable, are accepted and conceded to be true by the learner because they seem so to him. Petitions can also be demonstrated, but the learner is petitioned to concede them, either because he is not convinced of their truth or because he himself holds a contrary opinion. A petition is also called a postulate, and both it and a supposition differ from a position only in that the latter has a wider meaning: it is a principle that is accepted without proof even though it can be proved, or one that is contrary to the opinion of another [VL2: 218].

Vallius then goes on to make further distinctions, indicating how the principles he has just described differ from terms or definitions:

> Terms or definitions are not suppositions because they are not propositions and do not affirm being or non-being. Rather they express meanings that are grasped or not. This is not true of suppositions, unless one takes the supposition to be that one's words have been heard. Suppositions are propositions from which a conclusion is deduced on the basis that they themselves are true.
>
> A petition and a supposition are further differentiated from a definition in that a supposition and a position are universal or particular, whereas a definition is neither, nor does it make any affirmation about a whole or a part [VL2: 218].

The point of the final clause of the last sentence might easily be missed by one who thinks that the proposition "Man is a rational animal" is a definition and that it applies universally to all men. The definition itself is not the whole proposition; rather it is only the latter part, the expression "rational animal." As such, in itself it makes no claim about universality or particularity of application.

A yet further qualification is introduced by Vallius at this point about the ways suppositions are used by mathematicians, and how this usage is sometimes misconstrued by philosophers:

> Nor is it licit to use false suppositions, nor do geometers make use of them, although some accuse them of this, saying that the geometer supposes a line to be a foot long whereas it is not, or that it is straight whereas it is not. For the geometer does not conclude from his pointing to the line that it has this property; rather he uses the line he is pointing to as a sign that leads to the knowledge of another line that has whatever conditions are necessary for him to effect his demonstration [VL2: 218].

The point being made here is that suppositions, as employed in the context of the *Posterior Analytics*, are simply speaking true propositions, even though they may not appear so or be evident to one learning a science.

b. *Suppositions and Hypotheses.* Since the Latin *suppositio* (or *subpositio*) is but a translation of the Greek *hupothesis*, meaning literally something placed under a position or a thesis, it is equally correct to translate it into English as hypothesis or as supposition. Galileo's usage in MS 27 and elsewhere shows a strong preference for the latter (either in the Latin or in its Italian equivalent, *supposizione*), following the practice of the professors at the Collegio Romano. Occasionally, however, he uses the Latin *hypothesis* or the Italian *ipotesi*. This usage is not problematic, though a problem does arise in English translation, particularly when dealing with expressions such as *ex suppositione* or *ex hypothesi*. At key places in his writings Galileo states that he is demonstrating *ex suppositione*; when he does, invariably translators interpret this to mean that he is reasoning hypothetically. But to reason hypothetically in the present day, particularly in scientific contexts, suggests the use of modern HD method – a logical procedure quite different from employing a supposition in a demonstration [cf. Sec. 1.2a]. This may explain why some Galileo scholars, working from his writings in translation, attribute this methodology to him rather than that of the *Posterior Analytics*. This is worth examining in some detail.

Aristotle himself used the Greek expressions *hupothesis* and *ex hupotheseōs* many times in his writings, most frequently in the *Organon* but also in his works on natural science. Most of the occurrences in the former are in the two *Analytics*, the *Prior* and the *Posterior*, where he is detailing how to attain knowledge that is necessary and not merely dialectical. In this context his term *hypothesis* turns out to have two meanings. The first is found in the *Prior Analytics*, where Aristotle is explaining the conditional or hypothetical syllogism, the second, in the *Posterior Analytics*, where he is explaining the categorical syllogism as used in a demonstration.

Reasoning *ex hupotheseōs* or hypothetical reasoning in the prioristic sense employs two clauses, one an antecedent, the other a consequent, called the *protasis* and *apodosis* respectively.[1] The rules governing this type of reasoning, expressed schematically as "If p, then q," have already been discussed in our treatment of antecedents and consequents [Sec. 3.7b]. There the distinction was made between three types of relationship or implication between antecedent and consequent: one absolute, another suppositional, and yet another reciprocal. It is the first of these (not the second) that is similar to the HD method employed in modern science. This type of implication is governed by two maxims, namely, placing the antecedent necessarily involves placing the consequent, and removing the consequent necessarily involves removing the antecedent. These may be schematized respectively as: "If p then q; and p; therefore q"; and "If p then q, and not-q; therefore not-p."

HD method, as was said, is similar to these but not the same. Actually it more resembles a fallacy, called the *fallacia consequentis* or fallacy of the consequent, for it reasons as follows: "If p, then q; and q; therefore p." In this usage p formulates a hypothesis that does not pertain to the order of appearances and thus cannot be verified empirically, whereas q states a consequent that is observable and can be so verified. Since the antecedent, p, cannot be verified empirically, use of the first maxim is ruled out; all that one can verify is the consequent, q, and so one must concentrate on it. The general idea behind the resulting method is that hypotheses that do not get empirical support in this way are eliminated as not true (on the basis of the second maxim), whereas those that obtain such support and are not disconfirmed are retained, since they *may* be true. And when hypotheses can be subjected to large numbers and varieties of experimental tests, and their consequents are repeatedly confirmed but rarely or never disconfirmed, it seems not only possible but probable that they indeed are true. The probability of their truth, in fact, is thought intuitively to increase in proportion to the number and variety of their confirmations. The atomic hypothesis (as well as hypotheses for the existence of electrons, molecules, black holes, genes, etc.) are all regarded in the present day as "verified" or "confirmed" – some would say "justified" – from this type of evidence.[2]

The technical use of *hupothesis* or *suppositio* in the *Posterior Analytics* is different from that in the *Prior Analytics*, although it too occurs in a premise. In the posterioristic use, however, it occurs in the premise of a syllogism that is categorical, not conditional or hypothetical. The categorical syllogism consists of two premises, one of which is a *thesis* and the other, placed under it, a *hupothesis*. As can be gathered from the foregoing, the *thesis* is a "laying something down" (whence the Latin, *positio*), and this is usually a definition; the *hupothesis* (whence the Latin, *subpositio* or *suppositio*) usually asserts the existence of the subject whose definition is affirmed and whose properties are being demonstrated. The *thesis*, to repeat, is frequently an immediate proposition and thus is not susceptible of proof by the instructor; the *hypothesis*, on the other hand, might be capable of proof, but it is usually assumed without proof by the instructor if the pupil accepts it and has no opinion to the contrary.

From this discussion one can see how *hupothesis* takes on different meanings in the two *Analytics*. In the *Prior Analytics* it refers to a statement that might be true or false and thus is problematic. In the *Posterior Analytics* it refers to a statement that is true, although it may appear problematic to the person unacquainted with its subject matter. Being true, it can function as a premise in a strict demonstration, and so can yield truth and certainty in the conclusion, not mere probability. The same cannot be said of a hypothesis used in the prioristic mode, particularly that of the modern HD method. Thus, although it is true that in both meanings of the term one can be said to reason hypothetically or *ex suppositione*, the strictly "scientific use," in the Aristotelian sense of *scientia*, is found only in the second. The possibility of this twofold use of *ex suppositione* in Galileo's day led to serious ambiguities, as will be seen in later chapters.[3]

Ex suppositione in this second and more rigorous sense was used by Aristotle in the *Physics* to justify his study of nature as an *epistēmē* or *scientia* and not a "likely story," as Plato had held. A problem arises here because *scientia* must have a necessary, universal, and unchanging character, whereas nature undergoes change and is contingent in its operations. How, then, can the ideal of *scientia*, necessary knowledge, be realized in the contingent subject matter with which physics is concerned? Aristotle's answer to this question hinges on the type of necessity that characterizes nature's activities. He distinguishes between a necessity that is absolute (*haplōs* or *simpliciter*) and one that is suppositional (*ex hupotheseōs* or *ex suppositione*). On the basis of that distinction he then elaborates the various types of causality to be studied in physics, explaining why all four causes require investigation, and particularly the end or the final cause, so as to clarify how demonstrations may be made in its contingent subject matter.[4]

Rather than continue here with Aristotle's text, we turn instead to the way the expression *ex suppositione* was understood by Galileo in physical contexts, and find this detailed, predictably, in Vallius's *Logica*. In the particular passage to which we direct attention Vallius takes inspiration from St. Thomas's commentary on the text of the *Physics*. Vallius's aim is to explain how one can obtain a strict demonstration in natural science by reasoning from a material cause or a formal cause. His explanation also ties in with Vallius-Carbone's treatment of antecedents and consequents involving implications that are suppositional, as opposed to absolute or reciprocal, as already explained in Sec. 3.7b. It reads:

> St. Thomas teaches in his fifteenth lecture on the second book of the *Physics* that necessity is twofold. One is unqualified and absolute, and in this way things that depend on causes prior in being, that is, whose causes precede the things themselves, are necessary. The other is from a supposition (*ex suppositione*), and in this way things that depend on causes later in being, that is, from the end, are necessary. For example, a man who desires health finds it necessary to take medicine. His taking of the medicine is indeed necessary, yet not in an unqualified and absolute way but from a supposition, because it derives its necessity not from a cause that precedes it in being but from health, which is later in being than the taking of the medicine. For with regard to existence the end is always later than the means. Since in natural things, however, there are two constituent parts, namely, matter and form, whatever has necessity in natural things can have it from the matter or from the form. Whatever has necessity from the matter has an absolute and unqualified necessity, because matter precedes its effect in being, and so man's composition from contraries is prior to his dying. Whatever has necessity from the form, on the other hand, does not have an absolute necessity, because the form and the end coincide, and the form as end does not precede its effect in the order of existence [VL2: 244].

An example given by St. Thomas in his commentary on the *Posterior Analytics* illustrates the distinction just made. When a person plants an olive seed, there is no absolute necessity that an olive tree will result from it, because nature's operations are contingent and many factors can intervene and prevent the tree from growing. Thus olive seed does not necessitate olive tree absolutely. Yet viewed in reverse, as it were, an element of necessity can be seen in the tree's production, for in the order of nature the olive tree's existence necessitates the previous existence of the olive seed. This type of necessity Aquinas refers to as *ex suppositione finis*, "on the supposition of the end," since the form of the olive tree is the end of the process of the tree's formation from the seed.[5] Supposing the fully formed tree, one can investigate all of the causes that are involved in its production, and on that basis, obviously suppositional, can obtain demonstrative or scientific knowledge of the olive tree.

It may be noted that this same type of reasoning is what permits one to attain scientific knowledge of infrequent happenings such as eclipses and rainbows. Eclipses and rainbows rarely occur and on the face of it would seem to be completely contingent; yet an element of necessity can be discerned in their production if one follows a method similar to that explained for the olive tree. On the supposition that an eclipse is to occur

– and here the eclipse itself is seen as a form or an end that terminates movements going on in the heavens – one can investigate all the causes involved in the eclipse's production and so come to demonstrative knowledge of the eclipse.[6] The same procedure lies behind the science of the rainbow.[7] In all three instances there can be demonstrations, strict demonstrations, although none of them involves an absolute necessity. They invoke rather a suppositional necessity, one perceived *ex suppositione finis*, which in the Aristotelian conception is the mode of demonstration most characteristic of natural science.

c. *Kinds of Supposition.* Thus far suppositions have been described as a subcategory of positions, thus different from axioms, and different at least nominally from petitions. The type of supposition found in reasoning about nature and natural processes has just been explained. The description of subalternated sciences in the previous chapter has further touched on the way in which such sciences obtain their principles from a superior science and thus suppose them, that is, use them suppositionally (*ex suppositione*) in their demonstrations [Sec. 3.3c]. Obviously, then, there are various kinds of suppositions, some appropriate to natural sciences, others to mathematics, yet others to subalternated sciences. Among the latter, the middle sciences, those subalternated to mathematics [Sec. 3.4b], are of particular interest in what follows, and these too employ suppositions of various kinds. A classification would thus seem to be called for.

Galileo does not present such a classification in MS 27, nor does Vallius in his *Logica.* But one of Vallius's successors at the Collegio Romano, Ludovicus Rugerius, who taught the logic course there two years after Vallius, in 1589–1590, attempted a more systematic account that proves helpful for our purposes.[8] He first divides suppositions into two types, those that are used in instruction and those that are used in proofs. "Suppositions for the learner," he writes, "are propositions that, even though they can be demonstrated, are not actually demonstrated but appear true to the learner." The other type he refers to as "suppositions absolutely taken," and he holds that these

> are principles that either cannot be demonstrated, but do require some confirmation or explanation, or that can be demonstrated by some kind of proof in another science, since the one who argues from them supposes them as demonstrated in the other science. They are like immediate propositions in the science in which they are supposed as demonstrated in the other, because in the former they do not have a middle term through which they can be demonstrated.[9]

The reference here is to subalternating and subalternated sciences, as Rugerius makes clear when discussing the principles employed in demonstrative syllogisms. All of these "must be foreknown," he states, "though not all in the same way." So he explains:

> Some that are most common for all disciplines should be most known to all; others need only be known for certain disciplines – for some are proved in a higher science, called the subalternating science, and thence they serve as principles in a lower science, which is spoken of as subalternated.[10]

He goes on:

> Moreover, there are some principles that are so known that they simply require the knowledge of their terms for them to be understood, as "Every whole is greater than its part"; others require induction and experiment, as "Fire is hot" and "Rhubarb purges wine." Some, again, can be proved from principles that are common or are taken from a higher science; others require demonstration *quia* or *a posteriori* or from a sign, as are those Aristotle uses to prove that there are three principles of natural things or that there is a prime mover. Some, moreover, are absolutely first and immediate for any particular science, provided they are more known in it – just as also the principles of one demonstration can sometimes be proved in another demonstration if they are not first and immediate, provided that they are known in the demonstration where they serve as principles.[11]

Rugerius's examples in this citation, as well as the reference in his last sentence to a "particular science," meaning by this part of a total science, and then to the possibility of multiple demonstrations within a science wherein one demonstration presupposes another, open up a number of additional ways in which principles can be "supposed" as foreknown. Some can be presupposed as established by induction and experiment, others as previously demonstrated *a posteriori* elsewhere, and yet others as proved in one part of a science and thus usable without proof in another part.

Collating these and similar commentaries on the *Posterior Analytics* and on Aristotle's other works, we arrive at the following ten categories of supposition:[12]

[1] Supposition of an end or form that is the normal completion of a natural process, which dictates a necessity to the matter
[2] Supposition of a principle used in natural science that can be proved in mathematics
[3] Supposition of a principle that can be established by induction and experiment
[4] Supposition of a principle that is capable of *a posteriori* proof, i.e., from effect to cause

[5] Supposition of a principle that can be proved in a particular science or part of a total science, and so is usable without proof in another science or part

To these five types we add five more for purposes of future reference. Four of these involve the middle sciences in one way or another, and, as will be seen, all are used explicitly or implicitly in Galileo's later writings. They may be formulated as follows:

[6] Supposition of a mathematical principle or definition that is posited for computation or calculation and is not true in nature
[7] Supposition of a mathematical principle or definition that is true and absolute and has a valid application in nature
[8] Supposition of one or more conditions involving the removal of impediments or of extraneous efficient causes that permit a principle or definition to be verified in nature
[9] Supposition of one or more conditions under which a mathematical priciple or definition will be verified in nature to a determinate degree of approximation

Of these, the sixth and seventh are discussed explicitly by Galileo in his *Considerations on the Copernican Opinion*, directed against Cardinal Bellarmine's 1615 interpretation of the suppositional character of arguments being advanced in support of Copernicus [GG5: 349–370]. The eighth is touched on in MS 27 at F3.1.9, F3.1.15, and D2.6.6. Along with the ninth, it is developed extensively by Galileo in working out his new sciences of mechanics and local motion, as will be seen in Chapter 6.

To complete the list, having cited places where Vallius-Carbone refer to the pure mathematician's use of supposition, we conclude with a tenth category that covers this usage:

[10] Supposition of pure quantity, continuous or discrete, as existing in intelligible matter and in which necessary connections or relationships can be ascertained

Vallius-Carbone mention this type when classifying the speculative sciences in their *Introductio in philosophiam* [CP235,240] and when characterizing pure mathematics as a science that supposes its subject matter [Sec. 3.4a, citing CP242]. Vallius himself defends it in his *Logica*, as noted above in Sec. 4.2a. Furthermore, Blancanus explains this kind of supposition in his *Apparatus*, and, like Vallius, maintains that this usage safeguards mathematics from the charge of falsification. When a geometer says that two lines are equal, he writes, he is referring to lines that are not sensible but intelligible. When demonstrating, therefore, the geometer supposes intelligible matter to be present and constructs lines, angles, triangles, etc., in it. These lines can be exactly equal when those in sensible matter are not. Thus, in effect, he "supposes" sensible lines to be intelligible so as to attain a scientific knowledge of them.[13]

d. *Foreknowledge.* With these matters understood, we now turn to Galileo's explanation of the foreknowledge of principles required for demonstration. He covers this topic in MS 27 in four questions, making up what he numbers his "second" disputation on foreknowledge and foreknowns [F2]. The entire disputation, as explained in the introduction to *Galileo's Logical Treatises*, can be correlated with passages in Vallius-Carbone's *Additamenta* of 1597.

The first of the questions [F2.1] asks whether every principle that is used in a demonstration must be known to be true beforehand. Galileo's reply to this is nuanced. First principles in the sense of axioms must be known in some way, that is, one must grasp them from the meanings of their terms or otherwise assent to them at least implicitly. Proper principles, on the other hand, must be foreknown without qualification on the basis of sense experience, induction, experimentation, or, in moral matters, from custom.

The second question [F2.2] is basically textual; it is raised because Aristotle says only that the existence or truth of first principles must be foreknown without mentioning the meaning of their terms. Hence the problem: should the meanings of their terms not be understood, how can they be recognized as true? Galileo's reply identifies first principles as complex, that is, as composed of a subject and a predicate, and concedes that the meaning of both terms must be understood for the principle to be employed in a way effective of scientific knowing.

In the third question [F2.3] the manner of knowing principles is addressed: should they be known actually or habitually? Galileo's answer invokes distinctions based on the type of principle involved and the circumstances of its use. Proper principles used in a direct or ostensive demonstration must be foreknown actually, and so must axioms when these are used in a reduction to the impossible. Otherwise axioms need be foreknown only habitually.

The final question [F2.4] inquires whether every principle used in a demonstration must be self-evident, or whether principles themselves are susceptible of proof. The problem has important ramifications for the natural sciences, whose principles are rarely analytical and so have to be established by antecedent reasoning of one sort or another. Galileo's response is that in some cases principles can be demonstrated *a posteriori*. In others, and here the case of a subalternated science is envisaged, they can be supplied by the science to which it is subalternated. In yet others, even though they cannot be proved in the strict sense, principles can be "manifested" or made obvious by an inductive process. How this works is explained below in the treatment of induction [Sec. 4.6] and further exemplified in the discussion of Galileo's definition of uniformly accelerated motion [Sec. 6.5b].

3. SUBJECTS AND PROPERTIES IN DEMONSTRATION

Having treated the foreknowledge required of principles, Galileo turns next to the two other things that must be foreknown in some way, namely, the subject (the thing given) and the property or predicate (the thing inquired about) [Sec. 4.1b]. Although both these terms find their primary meaning in the subject and predicate of the conclusion of a demonstration, the first, the subject, takes on a broader connotation in the context of foreknowledge. Here a subject is frequently used interchangeably with an object, as when an extramental subject is said to be the object of one's knowledge. In this usage subject means the subject matter of a discipline, the way in which one speaks of the subject of a science. Various questions can be asked about subject or object in this sense, and these are entertained in Galileo's third disputation on foreknowledge and foreknowns [F3]. This is made up of five questions, the first three concerned with questions relating to the existence of the subject of a science, the last two with questions about its meaning or essence or quiddity. The disputation as Galileo presents it is difficult to understand, for he uses many technical terms without explaining their meaning. Undoubtedly he does this because they are already explained in parts of the notes from which he is working that have not come down to us. For example, various distinctions relating to the subject of a science are found in the treatise on science to which Galileo makes reference in the prologue to the treatise on demonstration but which itself is missing from MS 27. For present purposes the necessary background material is sufficiently explained in Vallius-Carbone's *Additamenta* or in Vallius's *Logica*, much of which has been summarized in the preceding chapters.

a. *Subjects.* Vallius-Carbone begin their treatment paralleling Galileo's

F2 with the observation that many statements in the *Posterior Analytics* are concerned with the subject or the object of a science, whether the term science refers to a total science or a partial science, a distinction already examined in Sec. 3.2. Their objective, therefore, is to provide an overview of what these terms mean, so that statements about their foreknowledge can be properly understood [CA45v].

Subject, they explain, can be taken in a great number of ways – and they list nine. It may mean the matter on which natural agents act in the sense of a substratum, and in this way it is often employed in physics. It may mean a subsistent entity, or the terminal point of a motion or change, or an object of sense knowledge. Again, it may indicate something wherein other things exist, the way substance is the subject of accidents. Closer to their present concerns, it may indicate something different from a principle or a cause, or something that is presupposed to a science, or a term in a proposition, as subject is used in logic. Finally it may mean that of which properties are demonstrated in a science, and this is the sense in which it is used in the present disputation [CA45v].

In this last sense, further distinctions may be made when specifying the subject of a science, for example, the adequate or total subject, the principal subject, the partial subject. Definitions and exemplifications of these are found in Vallius's *Logica*. In its broadest meaning subject means a single generic entity considered in a science and whose principles, parts or species, and properties are investigated in it. An adequate or total subject is one that is neither broader nor narrower than the concerns of the science, in the sense that it includes everything considered in the science and nothing that cannot be reduced in some way to the science. As opposed to this, a partial subject is one that, while completely investigated in the science, does not include everything studied in the science, and so can be regarded as a part or species of the total subject. A principal subject, finally, while itself only a part and thus a partial subject, is nonetheless the most important of the parts and on this account is designated the principal [VL2: 557]. Vallius exemplifies these with the science of physics: its total subject is the natural body, and its principal subject, which is also a partial subject, is either the heavens or the elements [VL2: 160].

Significantly, Galileo's treatise on the heavens in MS 46 employs similar identifications of subjects. Like Vallius, Galileo states that the total subject of the *Physics*, which he too defines as that to which all things treated in physics are reduced, is the natural body; its partial subjects are any part or kind of natural body, such as the simple body and the composed body

[GG1: 17]. The total subject of *De caelo* is for him the simple body, meaning by this the heavens or the elements [GG1: 16–18]. (From this it may be seen that total and partial are relative terms, for the partial subject of a total science, *Physics*, is the same as the total subject of a part of that science, *De caelo*.) Galileo also mentions in passing, as a teaching of Nifo, that the heavens are the principal subject of *De caelo* [GG1: 16]. Its adequate subject, defined by Galileo as the object whose parts and properties are studied in the science, becomes for him the universe (*universum*) [GG1: 19] – a term coextensive with the natural body but not stressing the formality of nature. This becomes clear from the way Galileo defines the total subject of astronomy; he does this in his *Cosmografia*, where it is again the universe (*il mondo*, *l'universo*) [GG2: 211], but not considered as a natural body, as we have already seen [Sec. 3.4b]. The total subject of cosmography he there also divides into its principal parts, namely, the celestial and the elemental regions [GG2: 212].

b. *Existence and Meaning*. As already noted in Sec. 4.1a, when treating the foreknowledge required of the subject of a science Aristotle invokes a distinction between its existence (*an sit*) and its meaning (*quid sit*). This assumes importance in Galileo's F3 and F4, where Galileo refers to it as a distinction between the "is" of existence (*esse existentiae*) and the "is" of essence (*esse essentiae*). The latter terminology derives from Aegidius Romanus's commentaries on Thomas Aquinas and was much used in scholastic circles in the late thirteenth-century and thereafter. Galileo uses the distinction but does not explain it, whereas Vallius-Carbone do. They describe the "is" of essence first:

> Note that philosophers speak of two kinds of "is" (*esse*), one which they call the "is" of essence or of nature, the other that of existence. The "is" of essence is something all things have, that by which their nature is determined, as, in the case of man, to be a rational animal, for by this man's nature is constituted. This type of "is" is attributed to all things apart from any kind of existence, since the nature is not produced of itself, and if it is to be produced, it cannot be otherwise; for this "is" has an element of necessity to it. Whence it happens that from this "is" are taken propositions that are said to be of eternal truth [*aeternae veritatis*], such as that man is an animal possessed of reason and that animal is a substance endowed with sense knowledge [CA46rb].

The "is" of existence, on the other hand, they define as follows:

> The "is" of existence is nothing other than the existence of something already produced, that whereby it exists outside its causes. This existence is twofold: one actual, and that is the kind a thing has in actuality when it is produced; the other potential, and that is the kind a thing has

> in the causes from which it can be produced, as the animal in the seed. Some call this latter an existence in objective potency, because it is able to be realized at the proper time [CA46rb].

As can be seen from these definitions, the "is" of essence may be regarded as necessary whereas the "is" of existence may not, being basically contingent. It is for this reason that sciences are said to abstract from existence in their reasoning processes [F3.1.12].

On the basis of these preliminaries we may summarize Galileo's conclusions in F3 with regard to the foreknowledge required of a subject of a science or demonstration. F3.1 is crucial for understanding Galileo's later scientific methodology, particularly his use of suppositions in demonstrative reasoning and the techniques he developed for removing what he called "impediments" within the science of nature. Assuming that "is" can be taken either in the sense of essence or in that of existence, the text focuses mainly on the problem of existence and inquires whether a subject must be known actually to exist before one can have scientific knowledge of it. To use a modern example, can there be a science of dinosaurs if none actually exists at the present time? Galileo's reply is that first one must know the meaning or nature of the subject [F3.1.8], and then one must know its actual existence in some way, i.e., with certain qualifications relating to impediments [F3.1.9], though these need not apply to all subjects considered in the science [F3.1.10]. He adds that the question of existence does not apply to subjects treated in logic [F3.1.11], for these conclusions are restricted to the subjects of real sciences.

The point of the qualifications relating to impediments, as can be gathered from the parallel treatment in Vallius-Carbone [CA46vb–47ra], is that one need not know of the actual existence of the subject of demonstration for all times and places and under all conditions whatever [F3.1.9]. It is sufficient to know, for example, that roses actually exist in the summer in the earth's northern hemisphere, provided that there is no blight in the region that would kill them; under these suppositions it is possible to have a science of roses, even though no roses may actually be existent here and now. Galileo was interested throughout his life in *impedimenta*, i.e., accidental causes that interfere with the phenomena of nature, and devoted much of his experimental activity to eliminating them. As will be seen in Chapter 6, his study of naturally accelerated motion was largely concerned with identifying impediments such as friction and air resistance that might cause the actual fall of heavy bodies to deviate from the uniform acceleration imparted them by nature. On the supposition of such impediments being removed, he was convinced that he could have a true science of naturally accelerated motion and demonstrate properties of it as a subject [Secs. 6.5 and 6.8].

Following the general conclusion of F3.1, in subsequent questions Galileo draws more detailed conclusions relating to both the total or adequate subject of a science and its partial subjects. With regard to the first, his reply is that the total subject of a science cannot be demonstrated to exist within the science itself, either *a priori* or *a posteriori*, nor can its principal subject be demonstrated to exist in any way [F3.2]. On the question of partial subjects, however, he relaxes this stricture and holds that it is possible to demonstrate the existence of a partial subject within the science, though only by demonstration *a posteriori* [F3.4]. Galileo manifests knowledge of these conclusions in MS 46, for he states there that while the subject of a total science cannot be demonstrated to exist, that of a partial science can be [GG1: 19]. Both conclusions, moreover, have application to Galileo's science of local motion. As he proposes to develop this discipline in the *Two New Sciences*, its total subject is motion according to place (*motus localis*), and it is composed of three partial subjects as its species: uniform local motion, naturally accelerated motion, and a combination of the two, projectile motion. The existence of local motion does not need to be demonstrated in the science and can be taken for granted, whereas the existence of naturally accelerated motion as a partial subject can be demonstrated. But such a demonstration can be effected only *a posteriori*, by means of experiments and suppositional reasoning, as explained more fully below [Secs. 6.5 and 6.8].

Galileo's final two queries in this disputation relate not to the existence of the subject but rather to its definition or meaning. The first, posed in F3.5, is whether a science can manifest the real definition of its subject and then supply a demonstration of the reasoned fact of the subject's existence through such a definition. His reply to both questions is affirmative. A science can manifest the real definition of its partial subjects and even of its total subject, but of the latter only through the use of *a posteriori* reasoning; it can also supply a demonstration of the reasoned fact of the subject's existence in terms of the real definition [F3.5]. The second question, raised in F3.6, is textual: what does Aristotle mean at 71a12–14 when he says that two things must be known about subjects of demonstration, "that" they are (Gr. *hoti esti*, Lat. *quia sunt*) and "what" it is that is said of them (Gr. *ti to legomenon esti*, Lat. *quid est quod dicitur*)? Some commentators take the *quid* or quiddity of the latter expression to refer to the *quid rei* or real definition, others to the *quid nominis* or nominal definition. Galileo here opts for the second position, namely, that the quiddity referenced is merely the definition of the term and not that of the thing itself.

c. *Properties and Conclusions.* The fourth and last disputation in the treatise on foreknowledge [F4] is concerned with foreknowledges required of the properties demonstrated of subjects and of the conclusions deduced from the demonstrations themselves. Galileo here summarizes in two questions matter that is spread over four in Vallius-Carbone, two of which treat foreknowledges of properties and two foreknowledges of conclusions. Most professors at the Collegio Romano do not include the conclusion of a demonstration as matter to be foreknown, and Aristotle did not enumerate it among the foreknowns in his text, although some commentators thought it should be included there [Sec. 4.1b]. Since it was found in his exemplar, Galileo appropriated the question relating to the conclusion, joining this to his question on foreknowledge of the property, as he implies, only for convenience of exposition.

With regard to the term property (Lat. *passio*), it should be noted that this refers simply to the predicate of the conclusion, namely, what is attributed to or predicated of the subject as a result of the demonstration. Galileo's question in F4.1 is whether or not the existence of the property must be foreknown, a query suggested by the previous question on the existence of the subject. Yet the existence of a property is quite different from that of a subject. Being a type of accidental being, its mode of existence is that of existing in another as a subject; as such its mode differs from that of subjects, many of which are substances that exist by themselves and so are not dependent on others for their existence. In this context Galileo takes property to mean any attribute that can be predicated as the conclusion of a demonstration, and not property in the strict sense of the Latin *proprium*, which would be a term that is predicated of all subjects of the particular type, at all times, and of them alone [D2.8.4]. The only distinction he makes here is that between properties that are convertible with the subject and those that are not. Note that in the title of the question he does not inquire about the meaning or the quiddity of the predicate, although he does take account of that in his answer to it. There, after making appropriate distinctions about the kinds of demonstration that are relevant to proofs for existence, he argues that the nominal definition of a property must always be foreknown, whereas the real definition must be foreknown only in special circumstances. Its existence, on the other hand, may or may not have to be foreknown, depending on the kind of demonstration being considered and the type of property whose existence is in question. In demonstrations both of the fact and of the reasoned fact the property's existence must always be foreknown. In a most powerful demonstration, however, if the property is convertible with the subject it cannot be foreknown, whereas if it is non-convertible, though it is not necessary that it be foreknown, there is nothing to prevent its being so.

The question relating to the conclusion of a demonstration, as posed by Galileo in F4.2, can be considered to be part of the treatise on foreknowledge or, alternatively, part of the treatise on demonstration; some Jesuit commentators preferred the former, others the latter [Lat. Ed. 166–167]. Galileo's locating the question where he does reflects some sensitivity to these alternatives, since with it he completes the treatise on foreknowledge and prepares for that on demonstration. (He also delves further into the subject in D2.6.) The reply he gives here is based on distinctions between priority of time and priority of nature and between various ways of considering the premises of a demonstration in relation to its conclusion. Depending on how one understands the terms "before" and "at the same time," it is possible to maintain, on the one hand, that knowledge of the premises precedes that of the conclusion, and, on the other, that the premises and the conclusion are grasped simultaneously in the same intellectual act.

4. THE NATURE AND KINDS OF DEMONSTRATION

Having surveyed the contents of Galileo's first treatise in MS 27, that on foreknowledges and foreknowns, we pass now to his second treatise, that on demonstration. This begins with a brief prologue that is somewhat cryptic, though in the tradition of the Jesuit expositions of the *Posterior Analytics*. It was customary at the Collegio Romano to divide the matter covered by Aristotle in the *Posterior Analytics* into three treatises, the first the treatise on foreknowledge already discussed, exposing in considerable detail the first chapter of the first book, the second a treatise on demonstration, exposing the matter contained in the remainder of the first book, and the third a treatise on science, covering the additional matter of the second book. Such a procedure poses a difficulty from the viewpoint of Aristotle's text, since it postpones a consideration of science and its definition until after the treatment of demonstration. This is somewhat inconvenient since science is the goal or end of demonstration and, if knowledge of science is lacking, it becomes difficult to define demonstration. Galileo merely notes the omission, probably recording a similar remark in Vallius's lecture notes of 1588. But it is noteworthy that Vallius amplified his treatment in his *Logica* of 1622, for there he interpolates an additional treatise on definition between his treatise on demonstration and his treatise on science [see Table 1]. This innovation follows more closely the commentatorial tradition on the *Posterior Analytics*, for many commentators see its second book concerned at least as much with definition as with science, and possibly more with definition, while they are unanimous in the view that the first book is concerned with demonstration.

Galileo's coverage of demonstration, as he indicates in the prologue, contains three disputations, one on the nature and importance of demonstration [D1], a second on the properties of demonstration [D2], and a third on its species or kinds [D3]. All of these disputations, it would appear, were appropriated from Vallius's lecture notes of 1588. Of the seventeen questions they contain, however, only one, the second question of the first treatise, was preserved by Carbone more or less in its original form and so remains available to us for purposes of comparison. All the rest are no longer extant in this way. Fortunately, however, the missing questions were reworked and generally amplified by Vallius for his *Logica* of 1622, with the result that their original contents are partly discernible in the later version. Another fortunate circumstance is that the teaching notes of Lorinus, who taught the logic course at the Collegio Romano in 1584 (four years before Vallius), have been conserved both in manuscript and in a printed version dating from 1620.[14] Since it is quite likely that Vallius made use of Lorinus's notes when composing his own teaching materials in 1588, two avenues are thus opened up for reconstructing the exemplar that was likely available to Galileo, namely, Lorinus's earlier version of the materials and Vallius's later revision of them. Both of these sources are used in what follows to cast fuller light on Galileo's exposition.

The first disputation of the treatise on demonstration is made up of only two questions, one on the nature or definition of demonstration [D1.1] and the other on its importance [*praestantia*] compared with another instrument of knowing, namely, definition [D1.2]. We summarize their contents under three headings, the first focusing on demonstration itself, the second on its kinds, and the third on how it compares with definition.

a. *Nature of Demonstration.* At the outset of the question on the nature of demonstration Galileo first anticipates materials in the third disputation and provides an overview of its various kinds. He then gives the two classical definitions of demonstration found in the *Posterior Analytics* and explains the terms occuring in each. The explanation of terms in the first definition gives rise to four objections against that definition, to which Galileo replies in turn. He does not repeat this procedure for the second definition, probably because to do so would have been redundant: most of the second disputation of this treatise, that considering the properties of demonstration, is in fact concerned with elaborating and defending the second definition.

The kinds of demonstration enumerated by Galileo in D1.1.1 are five: the first two are general types, ostensive demonstration and reduction to the impossible, and the remaining three are special kinds, demonstration of the fact (*quia*), demonstration of the reasoned fact (*propter quid*), and most powerful demonstration (*potissima*). Each of the five types, Galileo observes, may be regarded either as a kind of illative discourse, that is, as an argumentative process going on in the intellect, or as an instrument of scientific knowing, that is, as a necessary demonstration productive of science [D1.1.2]. He also provides a causal analysis of these alternative ways of viewing demonstration that is similar to the causal analysis of logic given in Sec. 2.4a, which in turn presupposes the psychological background sketched above in Secs. 2.1, 2.2, and 2.3. In either way its efficient cause is the intellect, its remote material cause (in the sense of the subject in which it takes place) is also the intellect, and its final cause is scientific knowledge. Viewed as an illative discourse, its proximate material cause is terms and propositions and its formal cause is proper syllogistic arrangement according to mode and figure. Viewed as an instrument of scientific knowing, on the other hand, its proximate material cause is the subject, the predicate, and the middle term, and its formal cause is the necessary relationship of the middle term to the subject, the predicate, and the conclusion.

To clarify these statements, it may prove helpful at this point to review how a demonstration may be placed in syllogistic form so that its various components can be analyzed. In an ostensive demonstration, the type mainly envisaged here, the syllogism would be written "M is P; S is M; therefore S is P," where S and P are the subject and predicate of the conclusion, and M is the middle term. The propositions "M is P" and "S is M" are the premises of the syllogism. The first of these, "M is P," is called the major premise because it contains P, a term of broader extension, whereas the second, "S is M," is called the minor premise because it contains S, a term of narrower extension. This particular arrangement of the three terms, S, M, and P, is known as the first figure (referred to by Galileo in D3.3.14). When the conclusion is a universal affirmative proposition, both premises must also be universal affirmatives, and this mode in the first figure is called Barbara.[15] Then, viewed as an illative discourse, its material cause is the terms (S, M, and P) and the propositions containing them (i.e., the premises), and its formal cause is the way in which the syllogism is arranged "according to mode and figure," namely, in the mode Barbara and in the first figure. Viewed as an instrument of scientific knowing, on the other hand, its material cause is again the terms (S, M, and P), but its formal cause is the necessary relationships that obtain between these three terms as they go to make up the two premises and the conclusion. (As explained in Sec. 2.7a, the type of resolution explained in the *Prior Analytics* verifies the demonstration as an illative discourse, whereas the type explained in the *Posterior Analytics* verifies it as productive of science. The second type of resolution is more stringent than the first, for over and above the propositions being universal and affirmative, as required for the first type, the propositions must also be necessary to produce scientific knowing, as explained in D2.7.)

With this as background, we may return to the two definitions of demonstration given by Galileo in D1.1.3 and extracted from *Posterior Analytics* I.2 [71b17–26]. The first is that it is a syllogism productive of science, the second, that it is a syllogism consisting of premises that are true, first, immediate, more known than, prior to, and causes of the conclusion. Galileo's explanation and defense of the first definition [D1.1.4–10] clarify, at some length and in typical scholastic fashion, how it should be interpreted so as to be neither redundant nor circular. With regard to the second definition, on the other hand, Galileo here gives only a summary explanation: the premises must be "true," in the sense that truth can be properly inferred only from truth; "first and immediate," either in an actual or a virtual sense; "more known than" the conclusion, either with respect to nature or with respect to us; and "prior to and causes of" the conclusion, in the sense that the order of knowing must ultimately correspond to the order of being [D1.1.11]. This particular statement turns out to be programmatic for much of the second disputation of the treatise on demonstration, to be explained in what follows.

b. *Kinds of Demonstration.* Crucial to understanding this exegesis of the second definition is Galileo's, and hence Vallius's, interpretation of various statements about demonstration found in the text of the *Posterior Analytics*. In that text no systematic division of demonstration is given, but within the long Aristotelian tradition extending from Greek antiquity to the late sixteenth century many were supplied by commentators. The most famous in Galileo's time was that of Averroes, who enumerates the three species of ostensive demonstration mentioned in D1.1.1, namely, of the fact, of the reasoned fact, and most powerful. The rationale for the Averroist division is explained by Galileo in D3.1.8. There he identifies various textual bases in the first book: that for demonstration of the reasoned fact is Aristotle's statements in chaps. 2 through 12; that for demonstration of the fact is his statements in chap. 13 and onward; and that for unqualified or most powerful demonstration is his statements in chap. 27 to the end of the book.

While adopting this threefold division and its terminology, Galileo (following Vallius, who in turn follows Zabarella), rejects the Averroist teaching that these three types constitute distinct species of demonstration. Instead he argues in D3.1 that there are only two species of ostensive demonstration, of the fact and of the reasoned fact, and that most powerful demonstration is but a subspecies of demonstration of the reasoned fact, not a distinct species by itself.

By taking arguments of this kind into account as well as other statements in MS 27 we can discern the division of demonstration that lies behind both treatises appropriated by Galileo. This is shown in Table 2, along with the documentation on which it is based. The types of demonstration are there arranged in hierarchical fashion, with the most perfect at the top and the least perfect at the bottom. The basic division is into direct and indirect, or ostensive and negative demonstration, as set out in D1.1.1. Indirect or negative demonstration is the most imperfect, since it concludes not to a positive conclusion but to an impossibility or an absurdity. Within the ostensive category, demonstration of the reasoned fact is more perfect than demonstration of the fact because its middle term, M, is both causative in reality and causative of our knowing about reality. The middle term in a demonstration of the fact, on the other hand, is only causative of our knowing, sometimes because it points to a remote cause of the conclusion, proving that the result is true but not telling precisely why, at other times because it identifies an effect that can lead to knowledge of the proper cause. In the special case where cause and effect turn out to be convertible, demonstration of the fact can even prepare the way for demonstration of

Table 2

KINDS OF DEMONSTRATION IN MS 27

Direct or ostensive demonstration: proving a true conclusion from true principles [D1.1.1], containing under it two species:

- demonstration of the reasoned fact (*propter quid*): wherein the middle term is a middle in being as well as in knowing [D3.1.11]; this also has two subspecies:
    - demonstration through intrinsic causes, proving a property of its primary and adequate subject through principles that are actually indemonstrable, thus most powerful (*potissima*) or unqualified (*simpliciter*) [D3.1.14].
    - demonstration through causes that are true, proper, and proximate [D2.2.5] and at least virtually indemonstrable [D2.2.10], and not excluding extrinsic causes [D3.1.14]; or
- demonstration of the fact (*quia*): wherein the middle term is a middle in knowing only and not in being [D3.1.11]; it has two subspecies:
    - demonstration of a property from a remote cause [D3.2.3];
      and
    - demonstration of the existence of a cause from an effect:
        - either when cause and effect are convertible,
        - or when they are not; or again,
        - either when the existence is simple,
        - or when it is complex [D3.2.3]; or

Indirect or negative demonstration, which is either

- reduction to the impossible (*ad impossibile*): arguing from the concession of one impossibility to another that is more known [D1.1.1]; or
- reduction to the absurd (*ad absurdum*): arguing to the person (*ad hominem*) rather than to the issue [D2.1.5; cf. D2.5.10 and F2.3.4].

the reasoned fact by what is called the demonstrative *regressus*, explained by Galileo in D3.3.

To return now to Aristotle's second definition of demonstration, as already noted this is interpreted by Galileo and his sources in such a way as to be applicable, with suitable qualifications, to each of the kinds of demonstration enumerated in Table 2. The main difficulty arises with demonstration of the fact, which, on the face of it, does not seem to square with Aristotle's definition. For example, when demonstrating through a remote cause the premises will not be "first and immediate." One can remedy this defect by holding that the premises, while they are not "actually" first and immediate, are "virtually" first and immediate if they can be resolved to premises that themselves are first and immediate [D2.2–4]. Again, when demonstrating the existence of a cause from an effect, the premises must be "more known" than the conclusion. Now admittedly this cannot be true "with respect to nature," since causes are more known by nature than are effects. Still the premises can be more known "with respect to us," since in human knowing effects are usually more known, or more readily knowable, than are their causes. This also relaxes the requirement for the premises being "causes of" the conclusion, for then they need not be causes "of the being" of the conclusion but only causes "of our knowing" it, which is sufficient for a demonstration *a posteriori* [D2.6].

c. *Comparison with Definition.* The longest question in the treatise on demonstration, and indeed in all of MS 27, is D1.2. The question was probably part of Vallius's treatise on demonstration in his lectures of 1588, where his first disputation is concerned not only with the nature of demonstration but also with its importance – a consideration that invites comparison with definition. As explained in the Introduction to *Galileo's Logical Treatises*, when plagiarizing Vallius's material for his *Additamenta* of 1597 Carbone removed the question from its original treatise and placed it in another treatise, namely, that on instruments of knowing. The question is important for the fact that it touches on matter covered in the second book of the *Posterior Analytics*, whereas the other questions concentrate on the first book.

A detailed analysis of the question is not required here, since the problem it discusses pertains to the treatise on instruments of scientific knowing and has already been summarized in Sec. 2.5. The conclusions to which it comes are that definition in itself is better than demonstration; that definition, even as it is in us, is more important than demonstration; that definition and demonstration are related analogously as instruments of knowing; and that definition is the end of demonstration.

5. CAUSES AND EFFECTS

Causes and effects have already been mentioned as topics from which one may draw probable arguments [Sec. 3.7a]. Their more important use is not in dialectics, however, but in necessary or demonstrative arguments as explained in the *Posterior Analytics*. As we have seen, causes and effects function in demonstration as middle terms in the various types of ostensive proof shown in Table 2, and thus as the main vehicles used for attaining scientific knowledge. Considering Galileo's consistently expressed desire to achieve this type of knowledge in his investigations, it is a matter of crucial importance to grasp how causes and effects are understood in MS 27. But this much said, it should be noted that, while causes and effects are referred to many times throughout MS 27, there is no *ex professo* treatment of causality in that manuscript. The reason for this is that causes are matters of concern in the real sciences, and particularly in physics and metaphysics, where they are treated at length in Aristotle's works.[16] The logician, as such, is not an adept at identifying causes – even though, once identified, he is able to provide precepts for their logical use.

The many occurrences of the term cause in MS 27 are listed in the index to *Galileo's Logical Treatises*.[17] The main Aristotelian division into final, efficient, formal, and material is basic to Galileo's usage, for he is explicit that demonstrations may be made through all four causes [D2.2.4]. Moreover, since peculiarities arise in the demonstrative process depending on the type of cause used as a middle term, he mentions each type repeatedly throughout the manuscript. Galileo further groups the four causes into two types, intrinsic and extrinsic: the first are internal to the entities they cause and actually remain in them, and of this kind are formal causes and material causes; the second are external to it as agents or goals and generally do not remain, and of this kind are efficient causes and final causes. A distinctive element in the manuscript is Galileo's many uses of the intrinsic-extrinsic distinction. For example, he uses it when explaining how causes relate to definitions [D1.2.25 and D1.2.34], how properties can depend on extrinsic causes [D2.8.4], how true demonstrations can be made from both extrinsic and intrinsic causes [D3.1.18], and how even perfect demonstrations can sometimes be made from extrinsic causes [D2.10.6]. The distinction also figures importantly for him when subdividing demonstration of the reasoned fact into its two subspecies [D3.1.14], as treated below in Sec. 4.9a.

Another distinction much used by Galileo is that between causes in being and causes in knowing, as explained in D2.2.2 and D3.1.8, and the related distinction between causes that are more known in themselves and those that are more known to us, also in D3.1.8. This type of distinction, as already noted, enables him to see Aristotle's second definition of demonstration, which stipulates that demonstration must be made through causes, as applicable not simply to demonstration of the reasoned fact but to demonstration of the fact as well [Sec. 4.4b].

Causes in being are further divided by him in various ways: some are true and proper causes, others virtual and improper (or imperfect) causes, as noted in D2.2.3. True and proper causes are causes that produce effects in the order of nature and do so directly; these are the type required for demonstration of the reasoned fact, as mentioned in D2.2.5 and D2.12.4, and were consistently sought by Galileo in his scientific work, as will be explained below. Virtual and improper causes, as opposed to this, are the type of cause that may be thought to exist in God, as explained in D2.2.2, or that may be said of certain causal attributes that produce their effects through intermediate attributes and do not do so directly and formally, but only virtually, as explained in D2.2.11. The latter type he uses, like causes in knowing, to justify the application of Aristotle's second definition of demonstration to cases that *prima facie* do not appear to satisfy its stringent requirements.

Another frequently used division is that into proximate causes and remote causes. A remote cause is part of the causal chain that produces an effect without itself being the agent that directly produces the effect. For example, in the order of efficient causality the sun is a remote cause of human life in that it provides an environment warm enough to sustain such life, although it does not directly produce it; the same can be said of food, which is a remote cause in that it provides nourishment for human life, but not life itself. The proximate cause, on the other hand, is either the procreative act of the parents that directly produces the human organism, or the action of the heart and lungs that actually keeps the organism alive. In the order of formal causality, Galileo divides proximate causes into those that are actually proximate and those that are only virtually so. In D2.2.11 he exemplifies this with the cause of man's ability to laugh, noting that this consists actually in man's ability to wonder but is already found virtually in man's ability to reason. The concatenation of middle terms that justify this line of reasoning has already been sketched in the discussion of logical resolution in Sec. 2.7a.

On the precise relationships that exist among causes and effects several statements in MS 27 are of interest. The first is that some causes are convertible with their effects, a condition that must be fulfilled for the demonstrative regress to occur [D3.2.1, D3.2.3, D3.3.14]. Others are that causes have not only an intrinsic relationship to their effects [D2.8.7] but also a necessary connection with them [D3.3.7] – a connection that can be recognized by the natural light of the intellect [D3.1.17]. On this basis a natural cause that is sufficient to produce its effect will operate necessarily when not impeded [F4.2.9]; this resonates with another statement, namely, that a natural cause when not impeded will produce an effect equal to it in perfection [D2.2.6]. Such connections are behind Galileo's insistence that cause and effect are correlatives [F2.3.1, D3.3.7–8].

Attention has already been called to the problem of mathematical causes [Sec. 3.4c]. These did not pose a difficulty for Galileo when appropriating the materials in MS 27, for in its text he clearly differentiates them from physical causes; the former, he says, are more known in the order of being and in the order of knowing [D2.6.5], whereas the latter, though more known in the order of being, are generally less known in the order of knowing [D3.1.8.]. And Galileo is explicit that mathematical demonstrations are true demonstrations, even though they are neither perfect [D2.12.8] nor most powerful [D2.6.5]. These statements offer first-hand evidence in support of our view that Galileo was not influenced by Vallius-Carbone's pejorative statements about mathematics, mentioned above in Secs. 3.4a and 3.4c.

6. INDUCTION

Galileo makes several references to induction in MS 27, but because these are fragmentary and mainly *obiter dicta* in varying contexts, it is difficult to reconstruct his ideas on induction as a whole. Even Vallius does not analyze induction in his logic course, and thus recourse to his writings, or those of Carbone, is of no help. Fortunately, however, in his comparative study of Galileo's MS 27 and Zabarella's *Opera logica*, Everard de Jong has succeeded in tying in Galileo's references to induction with Zabarella's treatment of the subject.[18] On the basis of De Jong's work it thus becomes possible to give a coherent account of induction as it was being treated by Aristotelian logicians at the end of the sixteenth century, thus as it was understood by Vallius and Carbone, and through Vallius by Galileo.

a. *Zabarella's Teaching.* Zabarella takes up the problem of induction in his schematic exposition of the *Prior Analytics* and then in his *De methodis* and in his commentary on the *Posterior Analytics*. He defines it as the logical instrument whereby the less known universal is manifested from more known particulars [ZL170*]. There are two types, perfect and imperfect: the first takes all the particulars that are subsumed under the universal and so concludes with necessity; the second does not take all the particulars and so may not induce necessity [ZL171*]. Yet Zabarella allows that one need not have a complete enumeration of particulars to obtain a perfect induction, for in this genre too there are two types. In the first one explicitly mentions all the particulars; in the second one need mention only some or the majority of them, covering the remainder by a kind of generalizing or distributive term (*per dictionem aliquam distributivam*) [ZL171*]. It is the latter type of induction, says Zabarella, to which Aristotle refers in *Posterior Analytics* I.1 when he says that all scientific knowledge arises from preexisting knowledge [ZL171*].

Zabarella provides a more detailed account of how this works in his commentary on the last chapter of the *Posterior Analytics* [II.19], where Aristotle is explaining his notion of *epagogē*, the process by which the mind acquires knowledge of universal first principles. Here too Zabarella identifies this as a process by which the universal is gathered (*colligitur*) from particulars [ZL1277]. It does not involve a demonstration of the unknown from the previously known; rather it is the awareness (*notificatio*) of the thing from itself, a sort of transition from what is self-evident to sense to what is self-evident to the intellect [ZL1281]. What then takes place is the grasping of the less-known universal from the more-known particular in which it exists; both the particular and the universal are actually the same thing (*particulare et universale eandem rem esse*), only it is more known as particular than as universal [ZL1277].

The inductive process as so described is necessary to acquire the principles on which demonstration is based, for these must derive from sense knowledge and consequently from particulars [ZL1277]. But Zabarella notes that this is true only for principles that are based on things that are directly sensible, what we in the present day would call observables; on this account induction is generally linked directly with sense experience. Non-observables, such as protomatter, require some type of demonstration before they can be grasped at the level of intellect [ZL890] – an obvious reference to demonstration of the fact and, more specifically, to that from an effect. Axioms such as "The whole is greater than its part," on the other hand, are grasped inductively even though they may seem remote from sense experience. The reason one is not aware of this type of induction, Zabarella explains, is that it is going on continually from one's youth and so one is not consciously aware of making the ascent from particulars to the universal principle [ZL1280–1281]. It is in this way, he further maintains, that one comes to know mathematical axioms: none of these is innate, though they may seem so because the information on which they are based has been acquired over a long period from repeated sense experiences. Much the same is true of proper principles such as definitions, previously unknown but accepted once they have been explained by a teacher; these too have a similar foundation in sense knowledge and so are grasped by induction [ZL890].

b. *Galileo's Statements.* Against the background of this brief account it is possible to make sense of Galileo's remarks about induction in MS 27. These occur at nine different places in the manuscript, three in the treatise on foreknowledge and six in that on demonstration. Of the latter, two are in the first disputation, in the question comparing demonstration with definition as instruments of scientific knowing [D1.2]; one is in the last question of the second disputation [D2.12], where Galileo merely states that a premise in a syllogism he proposes is proved by induction; and the final three in the first question of the third disputation [D3.1], where he is arguing that there are but two species of demonstration.

The three statements in the context of foreknowledge are consonant with Zabarella's analysis of induction in his Tables on the *Prior Analytics*, where he states that Aristotle's reference to induction in *Posterior Analytics* I.1 applies to knowledge of the principle that all scientific knowing arises from preexisting knowledge – here taking "scientific knowing" for knowing imparted or received by way of instruction [71a1–10]. Like Zabarella, Galileo identifies this principle as one "known by induction, division, and hypothetical syllogism" [F2.1.4]. The sense of induction in this context is that of reasoning based on a complete enumeration of particulars, which would rely on "division" for its completeness and then on the *modus ponens* of the "hypothetical syllogism" (if it is true of each and every kind, it is true of all) to attain universality.[19] Galileo gives a similar enumeration in F2.4.4, though this time he refers to it as "some slight induction" (*aliqua levi inductione*) and lists it not in conjunction with, but as an alternative to, division and hypothetical syllogism. In the latter passage he does not name a specific principle but states only that it applies to "principles that are not opaque to understanding, which are for the most part those in the order of knowing." This coheres with Zabarella's identifying induction as a logical instrument (*logicum instrumentum*), which is his equivalent for an instrument of knowing [ZL170*]. For Zabarella there are only two such instruments, the resolutive method and the demonstrative method, the first of which includes induction and demonstration from an effect [ZL268–269]. Induction, in his view, is the much weaker resolution and is used only when principles are not completely unknown and so require but slight manifestation (*levi egent declaratione*) [ZL269]. The final reference to induction in the treatise on foreknowledge echoes this same passage. There, in F3.2.3, Galileo makes a distinction between matters that are completely known, those that are completely unknown, and those that are partly known and partly unknown. Matters in the last category cannot be demonstrated, he says, but they can be manifested by induction, which is precisely Zabarella's teaching.

In his question on instruments of scientific knowing [D1.2] Galileo makes two further references to induction, the first including it under the general category of argumentation in general and the second linking it, as does Zabarella, with demonstration from an effect. The first passage [D1.2.3] mentions induction among instruments that serve to direct the operation of the intellect in some way (*aliquo modo*), whereas the second [D1.2.5] excludes it from instruments that serve the intellect's operations perfectly and immediately (*perfecte et immediate*), which Galileo, following Vallius's teaching, identifies as only two, definition and demonstration [Sec. 2.5]. He then considers an objection, namely, that "induction and demonstration of the fact ought to make one instrument that is essentially different from these two" [D1.2.5]. Galileo rejects this suggestion on the ground that the knowledge obtained through induction and demonstration from an effect is *a posteriori* and imperfect (*est a posteriori et imperfecta*), whereas in this context he is considering only instruments that serve knowledge perfectly. His point is that the knowledge these instruments yield is not as perfect as that attained by definition or by demonstration of the reasoned fact, the latter being more perfect for being *a priori*. Note that his use here of "imperfect" is not meant to suggest that the knowledge they yield is open to error or is mere opinion, for Galileo explicitly rules out this interpretation in D3.1.10, to be noted below.

The reference to induction in the last question of the second disputation on demonstration implicitly invokes a process of division and complete enumeration, and it too is proposed as leading to necessary knowledge. The syllogism on which Galileo focuses is the following: "Demonstration of the reasoned fact must proceed from a true and proper cause; but it cannot proceed from a proper cause if it is not composed of essential propositions; therefore [every demonstration of the reasoned fact must be composed of essential propositions]." The minor premise, Galileo writes, "is proved by induction" [D2.12.4]. To see this all one need do is go through the various types of true and proper cause – intrinsic (formal and material) as well as extrinsic (final and efficient) – and verify that all of them involve essential propositions, as Galileo explains in the question that follows [Secs. 4.7 and 4.8].

The last three mentions of induction in MS 27 support the teaching that the inductive process is not open to error and so leads to necessary knowledge. They occur in Galileo's treatment of demonstration of the fact, a species of demonstration which he again, like Zabarella, links with induction and maintains is demonstrative in the strict sense. Here, however, his explanation of induction is similar to that given by Zabarella at the end of his commentary on the *Posterior Analytics* – one based on an intuitive grasp of a necessary connection between subject and predicate and not exclusively on an enumeration of particulars. The first occurrence is in a counter-argument proposed not by Galileo but by an adversary to his position; the latter wishes to disprove that anyone can know, by sense knowledge and induction, that risibility exists in a rational animal, a conclusion Galileo supports [cf. Sec. 2.7a]. The argument states simply that "induction does not prove anything necessarily" (*inductio nil necessario probat*) [D3.1.6]. To this Galileo's reply is that risibility is known to exist in man the way any property is known to inhere in its proper subject, simply from our knowledge of human nature:

> We know the connection of a property with its subject by experience, for, from the foundation of the world to our own times risibility has always been found with man; second, by induction, for it is true to affirm of each and every man that he is risible; third, by the light of the intellect (*lumine intellectus*), which recognizes that this connection is necessary... [D3.1.17]

Here he couples sense experience with a complete enumeration of particulars and also with the natural light of the intellect – all assuring the necessity of the proposition for which he seeks assent. It is noteworthy that Zabarella uses a similar concatenation, while not insisting on the complete enumeration, as does Galileo in the passage cited:

> In all such principles there is an essential connection of the predicate with the subject, and on this account, as Averroes often remarks, demonstrative induction happens of necessity. When we make an induction in necessary matter we do not enumerate all the singulars, because in knowing a few the intellect begins to see the essential connection between the two terms. Therefore the intellect, breaking off the enumeration of the remaining individuals, immediately gathers the univeral from those few, for the illation from an essential predication to a universal predication is necessary. Thus in cases such as this the essential connection between the terms is so manifest that the universal can be grasped from only a few individuals, and perhaps even from only one [ZL1281].

It would seem that considerations similar to the foregoing lie behind the last reference to induction in the manuscript, that namely at D3.1.10, where Galileo is again formulating an objection against the possibility of reaching a necessary conclusion with demonstration from an effect. He states the objection as follows:

> Aristotle teaches that demonstration must be made from universal premises, and so on; but if demonstration of the fact, which does not fulfill these conditions, were to be a true species of demonstration because it infers a necessary result from necessary premises, then induction also, since it infers a necessary result and does not generate error or opinion, would be true demonstration [D3.1.10].

The objection is somewhat trivial, attempting as it does to identify induction with demonstration, but it is noteworthy that in his reply Galileo does not deny that induction infers a necessary result. He maintains only that induction is not the same as demonstration because it is based on singulars and not on universal propositions, as a demonstration must be. He then adds, as a confirmatory argument, that "of and by itself" (*ex vi sua*) induction does not conclude necessarily. Here he seemingly has in mind an induction that is incomplete in Zabarella's enumerative sense (as described in his Tables on the *Prior Analytics*) or one that is not assisted by the natural light of the intellect in the sense of Zabarella's "demonstrative induction" (as explained in his commentary at the end of the *Posterior Analytics*, noted above).

Thus it would appear that, notwithstanding a few exceptions to which De Jong calls attention,[20] Vallius's and Galileo's understandings of induction are basically Zabarella's. The teaching of all three on this subject is further related to that on the demonstrative *regressus*, on which there seems to be complete agreement, as explained in the final section of this chapter.

7. THE PREMISES OF A DEMONSTRATION

Most of the materials covered thus far serve to explain the teaching on foreknowledge in chapter 1 of the first book of Aristotle's *Posterior Analytics* (Galileo's disputations F2, F3, and F4) and then, in a general way, the definitions of demonstration at the start of chapter 2 and how these can be accomodated to the kinds of demonstration Aristotle mentions in subsequent chapters of the first book (Galileo's D1). As we turn now to the remaining disputations in the treatise on demonstration we find that these are similarly selective. The second disputation [D2] is ostensibly devoted to the properties of demonstration and contains twelve questions in all. The first six examine in greater detail difficulties associated with the premises of a demonstration as described in Aristotle's second definition of demonstration in chapter 2 [D1.1.3]. Their focus is mainly on the remainder of the second chapter, as will be detailed in this Section. The last six questions take up problems relating to predication generally and its various types – essential, necessary, and universal – as treated in Aristotle's chapters 4 through 6; these will be explained in the following Section. The key problem raised in Aristotle's third chapter, namely, how to avoid circularity in demonstration, is postponed to the third disputation [D3], where it is taken up in connection with an exposition of Aristotle's chapter 13, devoted to the demonstrative *regressus*; this will be explained in our last Section.

As noted in the Latin Edition, pp. 249–253, D2 presents editorial problems occasioned by Galileo's leaving several of its questions unnumbered and failing to indicate a separation between questions required for their proper numeration. He numbered correctly the first four questions, D2.1 through D2.4, and also the last three, D2.10 through D2.12. Between these he inserted titles for five questions without numbering them, leaving spaces for the numbers to be filled in later. When doing so he apparently lost track of the material he was appropriating from Vallius, since the question with which he resumed numbering, D2.10, should have been D2.9 on the basis of the questions extant in the manuscript. A detailed study of Galileo's third unnumbered question [D2.7], however, reveals that this question is really two in one. When a division is made at the proper place and a new question number and title based on parallel texts are inserted, it turns out that the resumption at D2.10 was correct and that there really are twelve questions in the disputation. As reconstructed their queries are directed to establishing the following points: (1) that demonstration must consist of true premises; (2) that the premises must be first and prior to the conclusion; (3) what it means for premises to be immediate; (4) that demonstration must be made from immediate premises, and in a particular sense of immediate; (5) how immediate premises that are self-evident enter into a demonstration; (6) that demonstration must be made from premises that are more known than the conclusion; (7) that it must be made from premises that are necessary and universal; (8) that there are various modes of speaking *per se* or essentially; (9) how to recognize the first and second modes, and that there are not more than two such modes of predication; (10) how the modes function in a demonstration; (11) what a universal predicate is and what propositions are contained under it; and (12) how a perfect demonstration must be made from propositions that are essential, universal, and proper. Of these only the first six questions are of concern in what immediately follows.

a. *True and Primary Premises.* The stipulations that the premises of a demonstration must be true and that they must be primary and prior to its conclusion are the burden of questions D2.1 and D2.2. In the first of these, inquiring whether a demonstration must be composed of true premises, Galileo proposes to cast light on three problems that have been raised by commentators on the relevant passage in Aristotle [71b26–27]. These respect the kinds of truth being inquired into; whether or not all of these kinds are to be found in a demonstration; and how one is to understand Aristotle's statement that non-being, i.e., what does not exist, cannot be known.

In reply to these queries Galileo first differentiates ontological truth from epistemological truth and then divides epistemological truth into two types: simple truth, that found in sense knowledge and in the first operation of the intellect; and complex truth, that found in the second and third operations of the intellect [see Sec. 2.2]. Of these, the truth of the premises of a demonstration can only be complex truth, since this is the truth proper to a proposition [D2.1.3]. Yet this consideration does not eliminate simple or non-complex truth altogether, since apart from the premises the object of a demonstration, an extramental reality, must have a simple truth precisely as existent outside the mind [D2.1.7]. Does this then mean that there cannot be true propositions concerning things not actually existent in this way, such as the vacuum and the infinite? No, replies Galileo, for true statements can be made about them, and this even though there cannot be a science of them [D2.1.8]. What then of other non-existents, such as a rose in winter; are these too eliminated from the ambit of science? Not necessarily, he again replies, for Aristotle's statement that "what does not exist cannot be known" is not to be taken in an absolute sense. It simply means that what is not in a thing cannot be known to be in the thing, and this interpretation safeguards all the types of truth involved in a demonstration [D2.1.9].

The second question, that relating to the priority of the premises [D2.2], turns out to be more complex than the title suggests. As is clear from the first sentence of Galileo's response [D2.2.1], in posing the question he is actually equating "premises that are first and prior" with those that are "causes of the conclusion." Thus he conflates three different expressions found in the second definition of demonstration [D1.1.3] and treats them as a unit. The expressions are italicized in the following definition as given in the Latin Edition [32.1–3]: syllogismus constans *ex* veris, *primis*, immediatis, notioribus, *prioribus, et causis conclusionis*. This way of presenting the question, it should be noted, seems to follow Vallius's order of exposition in his lecture notes of 1588 and so would not represent an independent emendation on Galileo's part (see Lat. Ed., 208–209).

The difficulty, as presented, is that while causes are prior to what they cause in the order of being or of nature, they are not prior and more known to us. How, then, can they function properly in a demonstration where the requirement is that they be first and prior to us? A related difficulty is that there is perfect science of God and yet there are no causes in him [D2.2.1]. In his prenotes to an answer Galileo distinguishes various ways of speaking about causes as they may be used in demonstration, first implicitly differentiating the four species of cause and then explicitly noting a difference between causes that are true and proper in the order of being and those that are only virtual and improper [D2.2.2–3]. He then offers three conclusions: demonstration of the reasoned fact can be made from all four causes [D2.2.4]; these causes, however, must be true and proper in the order of being [D2.2.5]; but it is still possible to have demonstrations made from causes that are only virtual in the order of being, although these will be imperfect and verisimilar demonstrations [D2.2.7]. To clarify a difficulty concerning the use of causality in the much-cited demonstration of man's risibility from his rationality which has been discussed above [Sec. 2.7a], Galileo concludes by inquiring whether demonstrations must always be made from proximate causes.

His reply to this is still affirmative, but he now adds the proviso that "proximate" is to be understood either actually or virtually [D2.2.11], echoing a statement he has already made in D1.1.11 relating to the "first and immediate" clause of the definition [see Sec. 4.4a].

b. *Immediacy and Self-Evidence.* The remaining four questions in this first part of D2 continue to explore the implications of the clauses in the second definition, focusing now on the immediacy clause and its relation to the problem of self-evidence.

The third question [D2.3] inquires into the meaning of the expression "from immediates" (*ex immediatis*) and records various distinctions bearing on ways propositions can be said to be first and immediate. These Galileo presents in his first two notations, which explain what it means to be first with respect to a subject and first with respect to a cause [D2.3.1], and then how first with respect to a cause can again be taken in two ways: either for a proposition that is immediate with respect to a conclusion or for a proposition that cannot be proved *a priori* in the same genus of cause [D2.3.2]. The two distinctions can best be understood in terms of the examples just mentioned of man's risibility and man's rationality, which provide the key to Galileo's exposition. With respect to man as a subject of predication, risibility is first to a subject because no other subject can come between risibility and man when risibility is predicated of him. Likewise with respect to man, rationality is first to a subject for the same reason, but it is also first to a cause, because no cause can come between rationality and man through which rationality can be demonstrated of him. (Note that the causal part of this statement does not apply to risibility, since a formal cause can be assigned for man's risibility, namely, his rationality.) Yet even with regard to man's rationality one must be careful: it is possible to prove this from various effects discernible in man's behavior, and also, if one knows of it, through man's final cause, his ordination to the contemplation of God in eternal beatitude. In these cases, however, if effects are used his rationality is not being demonstrated *a priori* but only *a posteriori*, and if his ordination to beatitude is used, though the demonstration would be *a priori* it would not be in the same genus of cause, since it would be made through final causality whereas the demonstration being discussed is made through formal causality.

These distinctions understood, Galileo argues that in this passage [71b26–27] Aristotle does not take immediate predications to refer to those that are first to a subject or first with respect to a conclusion [D2.3.3]; rather he refers to propositions that have no others over them through which they can be demonstrated *a priori* in the same genus of cause [D2.3.4]. In this sense of immediacy there are only five kinds of immediate propositions: (1) those in which a definition or a part of a definition is predicated of the thing defined; (2) those in which a primary property is predicated of the definition of the subject; (3) those in which attributes are predicated of God; (4) those in which one category of being is denied of another; and (5) those in which one differentia is denied of another [D2.3.6]. Galileo excludes a sixth possibility proposed by Cardinal Cajetan, namely, propositions in which a primary property is predicated of its adequate subject. "Man is risible" would be immediate in this sense, but the predication would be first to a subject or first to a conclusion (an interpretation Galileo has already eliminated in D2.3.3); moreover, the property of risibility can be demonstrated through man's definition as a rational animal, and thus the predication has a proposition over it through which it can be demonstrated *a priori* in the genus of formal causality [D2.3.5].

The remainder of the third question, apart from a notation of Aristotle's different division of immediate propositions in 72a15–24 [D2.3.7] that has already been treated in our discussion of principles and suppositions [Sec. 4.2], makes brief reference to self-evident or *per se nota* propositions. This occurs when inquiring whether or not every immediate proposition must be self-evident [D2.3.8]. In reply Galileo invokes the distinction between things known with respect to nature and those known with respect to us, to be more fully explained in D2.6, and maintains that every proposition that is immediate must be self-evident with respect to nature, for otherwise it would not be immediate [D2.3.9]. With respect to us, however, only axioms or first principles are self-evident, because precisely as first they have no principles over them through which they can be proved [D2.3.10].

The fourth and fifth questions ask whether every demonstration must be made from immediate premises, and if so, whether all immediate and self-evident propositions enter into every demonstration. Galileo's answer to the first query is that every demonstration must be made "in some way" from immediate premises [D2.4.2]. Most powerful demonstrations require premises that are actually immediate in the sense of being actually indemonstrable [D2.4.3], whereas less perfect types can be made from premises that are only virtually immediate in the sense of being virtually indemonstrable [D2.4.4]. Galileo is explicit that Aristotle wishes to include the less perfect types along with most powerful demonstration in the definitions he has given at the beginning of chapter 2.

This understood, Galileo begins the fifth question by admitting that not all immediate and self-evident principles enter actually into every demonstration; the problem, using the terminology of the previous question, is whether axioms enter demonstratons virtually [D2.5.1]. To solve this he provides two notations explaining what the term "axiom" means [D2.5.4–5]. The conclusions to which he comes are that axioms can enter into a demonstration, but if they do, as sometimes happens in mathematical demonstrations, the demonstration will be imperfect and improper; when consideration is restricted to proper demonstrations, axioms cannot be employed in them as intrinsic components, either actually or virtually [D2.5.6–7]. He then appends to the question a related query, namely, whether for perfect knowledge of a conclusion one must resolve it all the way to first principles. Galileo's reply again invokes a distinction and yields two results. The first is that complete and perfect knowledge does require a resolution to all principles and causes, including the first and most universal [D2.5.12], and the second, that for proper knowledge, i.e., for a thing to be known perfectly in its own genus, knowledge of the causes in that genus suffices [D2.5.13].

The sixth question then turns to the "more known" clause of the definition (*ex notioribus*) and consists of two queries. The first explores the clause itself whereas the second considers alternative ways of comparing knowledge of the premises of a demonstration with that of its conclusion. The reply to the first query begins with two notations: one distinguishes the traditional meanings of "more known," i.e., with respect to nature and with respect to us [D2.6.1], and from this Galileo argues that singulars or less universals are more known with respect to us than are more universals in the order of causality, while being less known with respect to us in the order of predication [D2.6.3]. The other notation then discusses the premises of most powerful demonstration, from which Galileo concludes that this type of demonstration must be made from premises that are more known with respect to nature [D2.6.4], although this need not preclude that they also be more known with respect to us, for this occurs in mathematical demonstrations, even though the latter are not most powerful [D2.6.5].

In response to the second query, finally, Galileo lists six arguments against the premises being more known than the conclusion [D2.6.6]. He then explains various ways in which one type of knowledge may be regarded as better than, or superior to, another type; from these he concludes that knowledge of first principles is better than knowledge of the conclusion in the sense of being more evident [D2.6.9], whereas knowledge of immediate principles is better in the sense of being more independent and more concerned with a superior object [D2.6.10]. He then replies to the arguments that would seem to support the contrary opinion.

8. NECESSARY, ESSENTIAL, AND UNIVERSAL PREDICATION

At the outset of chapter 2 of the first book of the *Posterior Analytics* Aristotle indicates, as a requirement of scientific knowing, that its object "cannot be other than it is" [71b12] and thus that it be concerned with necessary matter. (This topic has been touched on above when differentiating the object of science from that of opinion, Sec. 3.5a.) He begins chapter 4 by returning to that requirement and noting that, if the conclusion of a demonstration is to be necessary, the demonstration must itself be composed of necessary premises. But, he goes on to note, to consider necessary predication one must also understand what is meant by both essential predication and universal predication [73a22–27]. This sets out his program of exposition for the remainder of chapter 4 and for chapters 5 and 6 as well.

Aristotle's project, predictably, serves to explain the structure of the remaining six questions that make up the second half of Galileo's D2. The first of these is concerned with necessary propositions [D2.7], the next three with essential predication and how it serves the purposes of demonstration generally [D2.8–10], and the final two with universal predication and how it serves the purposes of most powerful demonstration [D2.11–12].

a. *Necessary Propositions.* The task of D2.7 is one of explicating how and why demonstrations must be made from propositions that are necessary and "said of every instance" *(de omni)*. Both terms, "necessary" and "said of every instance," require explanation. Galileo begins by defining necessity as a kind of condition whereby things cannot be otherwise than they are, and then divides it into various kinds. One kind is unqualified necessity, which cannot be impeded by any power, not even the absolute power of God; another is natural necessity, which can be impeded by God's absolute power but not by his ordained power, that acting according to his ordinary law. Natural necessity is the main concern in the question, and this in turn has several degrees and divisions. The degrees are four: one associated with spiritual substances, another with celestial spheres, a third with elements, and a fourth with compounds formed from elements. The divisions, on the other hand, are two. One division of natural necessity is into absolute and qualified; another division is into non-complex and complex. An absolute natural necessity joins subjects and predicates that have an intrinsic ordination to each other, such as man and rational; a qualified natural necessity, on the other hand, joins those that have only an extrinsic ordination, such as swan and white. A non-complex natural necessity is that associated with the actual existence of a thing, which is simple, whereas a complex natural necessity is that associated with a proposition, i.e., a composite of subject and predicate formed by the intellect [D2.7.1].

A necessary proposition, according to the last definition, would exhibit necessity of the complex natural type, and this in turn could be either absolute or qualified, the latter of varying degrees depending on the subject matter with which it is concerned. Galileo concludes the foregoing division with the cryptic statement that such a proposition is the same as the universal posterioristic statement (*dictum posterioristicum de omni*) – the type of universal statement characteristic of the *Posterior Analytics*. A statement of this kind would be opposed to the universal prioristic statement (*dictum prioristicum de omni*), the type characteristic of the *Prior Analytics*, whose mode of resolution differs from that of the *Posterior Analytics*, as explained in Sec. 2.7. The basic difference between the two is that in a posterioristic statement the predicate goes with the subject and everything contained under it, and does so always and at all times, whereas in a prioristic statement abstraction is made from time, and indeed from content and truth also, since it is concerned only with logical form [D2.7.2]. Although the prioristic statement would reflect the type of necessity characteristic of the syllogism (viz., that of the *dictum de omni*), it is inadequate for purposes of demonstration and so is not further considered in MS 27.

To this point in D2.7 Galileo, in using the expression "universal posterioristic statement" (*dictum posterioristicum de omni*), has conflated the statement "said of every instance" (*dictum de omni*) with the posterioristic statement (*dictum posterioristicum*); although he has explained the posterioristic part, he has not explained the "universal" or de *omni part*. An explanation is required, however, because an ambiguity is latent in the Latin *dictum de omni* – literally "said of all," meaning said of each and every instance contained under the all. A predicate can be joined to a subject in a simply universal way, as when substance is predicated of man; thus "Man is a substance" is true of all men, since each and every man is a substance. Alternatively, a predicate can be joined to a subject in a commensurate or convertible way, as when rationality is predicated uniquely of man. "Man is rational" is true in the sense that man, and only man, is rational, and thus the universality in its case applies to each and every instance of both subject and predicate. In Galileo's usage, following Vallius's, the first kind, that of the simple universal, is referred to as the *dictum de omni*, i.e., the statement "said of every instance," whereas the second kind, that of the commensurate universal, is referred to as the *dictum universale*, i.e., the "commensurately universal" statement.

With this terminology presupposed, Galileo notes that the three expressions under discussion are hierarchically ordered, with the first being more inclusive of the others and each succeeding one less inclusive. Broadest in scope is the statement said of every instance (*dictum de omni*); after this comes the statement that is posterioristic (*dictum posterioristicum*), that is, necessary and essential; and finally, the commensurately universal statement (*dictum universale*), the most restricted in scope [D2.7.3]. His answer then to the question posed in the title is that every proper and perfect demonstration of the reasoned fact must be made from propositions that are said of every instance and are necessary [D2.7.4]. This still allows for the various degrees of necessity mentioned at the outset, including demonstrations of what would seem to be contingent effects, although Galileo does not explain in this place how these can be effected [D2.7.5–6]. Most powerful demonstrations, as will be seen in D2.12, must then be made of propositions that not only are said of every instance, are necessary and essential, but also are commensurately universal.

b. *Essential Predication*. Four modes of predicating essentially, that is, *kath auto* or *per se*, are explained by Aristotle in chapter 4 [73a35–b25] and again in chapter 18 of the fifth book of the *Metaphysics* [1022a15–37]. In D2.8 (the question whose title was omitted), Galileo enumerates these modes as the principal ones and supplies the rationale behind their enumeration. Predicates are either essential to the thing of which they are predicated, he writes, and so constitute the first mode; or they are accidental, and if so, are either common and thus eliminated, or proper, and then they constitute the second mode. If a thing exists in and by itself, this constitutes the third mode; and finally, if it is a cause in and by itself, the fourth [D2.8.1]. In successive conclusions he then explains the modes more fully. In sum, propositions in the first mode are essential when they predicate a definition or a part of a definition of a subject [D2.8.2]. Similar propositions in the second mode are those in which a property is predicated of a subject; here Galileo explains the various kinds of property and how they are predicated differently of their subjects, including those that depend on extrinsic as well as intrinsic causes [D2.8.4]. No propositions are essential in the third mode, since this is a mode of existing, not of predicating, and so applies only to non-complexes, i.e., to first or second substances [D2.8.6]. Finally, all four causes are found operative in the fourth mode, since all involve an intrinsic relationship to what they cause and so can provide the ground for an essential causal proposition [D2.8.7].

With the modes of essential predication thus explained, Galileo turns in the next question to providing rules for recognizing propositions in the first and second modes. These may be summarized as follows: (1) they must be necessary by at least a natural necessity; (2) they must be said of every instance; (3) the predicate must be said of the subject directly and naturally; and (4) the predicate must pertain to the true and perfect definition of the subject, or vice versa [D2.9.1–5].[21] Moreover, in his account there are only two modes of predicating essentially, the first and the second modes [D2.9.8]. The third mode is a mode of existing, not of predicating; similarly, propositions in the fourth mode do not express modes of predicating formally since they are rather modes of causing [D2.9.10–11].

The final question in this group then asks which of the foregoing modes serve the purposes of demonstration. Galileo begins his reply with a brief listing of opinions on the matter and a brief notation about the ways propositions making up a demonstration may be considered. He then formulates three conclusions, namely: the first and second modes of speaking essentially serve the purposes of all perfect demonstrations [D2.10.3]; the third mode does not serve the purposes of demonstration in a proper way, though it can do so in an improper way [D2.10.4]; and the fourth mode enters into demonstration in a variety of ways, using intrinsic as well as extrinsic causes, but particularly through the exercise of formal and efficient causality [D2.10.5].

c. *Universal Predication.* Directly after enumerating the four modes of essential predication in chapter 4 [73a35–b25], Aristotle gives the definition of universal predication explained above, namely, that in which the predicate is said of every instance of the subject, belongs to it essentially, and also precisely as such, that is, convertibly [73b27]. Galileo takes up this definition in D2.11, where he further describes the kinds of propositions that are contained under it. He first explains the definition with three successive notations [D2.11.1–3]. With regard to the propositions, he lists various opinions and then provides two more notations, the first describing grades of predicates and the second explaining more fully the difference between belonging to a subject essentially and belonging to it commensurately or precisely as such [D2.11.4–6]. The conclusion to which he comes is that universal or commensurate propositions are those in which the predicates belong to their subjects precisely as such or according to their proper formality [D2.11.7].

The last question then sums up the main conclusions that have been established in the disputation. Galileo begins by posing a series of difficulties, the first directed against the thesis that demonstrations must be made from propositions that are essential [D2.12.1], the second against their being made from universals [D2.12.2], and the third against their being made from propositions that are first and proper, that is, commensurate [D2.12.3]. He then offers three conclusions, adding after each his replies to the objections lodged against it. The first is that every demonstration of the reasoned fact must be composed of essential propositions [D2.12.4]; the second, that every most powerful demonstration must be composed of propositions that are both essential and commensurately universal [D2.12.8]; and the third, that a demonstration that is true and proper, though neither of the reasoned fact nor most powerful, can be composed of propositions that are not commensurately universal provided only that they are essential [D2.12.10]. He ends with a corollary stating that there cannot be demonstration or science of individuals, although, in a qualified way, there can be of God [D2.12.15].

9. THE DEMONSTRATIVE REGRESS

With this we move to the last disputation in MS 27, the third in the treatise on demonstration [D3] and devoted to a full analysis of the species or kinds of demonstration, an overview of which has already been given in Sec. 4.4b. The disputation is divided into three questions, the first two taking up problems relating to how the species are differentiated, the third showing how the differentiation makes possible the demonstrative regress (*regressus*). The demonstrative regress was a favorite topic of discussion among Paduan Aristotelians and is regarded by many scholars as playing an important role in developing the methodology of modern science.[22] As with the previous disputation there is no extant exemplar to which Galileo's treatment of these questions can be traced. However, there are sufficient similarities between Galileo's teaching and those preserved in Lorinus's logic course and in the revised version of Vallius's to show a clear dependence of Galileo's notes on the Collegio Romano, similar to that for the Treatise on Foreknowledges and Foreknowns. Moreover, in the case of the regress, a number of clues are available for tracing Galileo's exposition back through Vallius's and Lorinus's to Zabarella, the most eminent of the Aristotelian commentators at the University of Padua, as will be indicated in what follows.

a. *Two Species of Demonstration.* The first question in the disputation, D3.1, inquires into the number of species of demonstration. Galileo begins his answer with three notations, the first explaining two ways in which demonstration may be understood, either as a syllogism or as a demonstration proper [D3.1.1]; the second restricting discussion to the second way, precisely as it is an instrument of scientific knowing [D3.1.2]; and the third criticizing Averroes's teaching on the difference between knowing the cause of an effect and knowing the existence of the effect [D3.1.3]. Following this he presents two conclusions he regards as certain, namely, that demonstration to the impossible is not true and perfect demonstration and that, on the other hand, demonstration of the reasoned fact is true and certain demonstration [D3.1.4].

At this point Galileo launches into an extensive listing of opinions on the problem of demonstration's speciation along with the proofs offered in their support. The first opinion he identifies as that of Avicenna, namely, that there is only one species of demonstration, that of the reasoned fact; for this he gives arguments based on the text of Aristotle and a long additional series based on reason [D3.1.5–6]. The second opinion, which he attributes to Averroes and his followers, is that there are three species of demonstration, namely, of the fact, of the reasoned fact, and most powerful; in its support he likewise gives lengthy arguments drawn from Aristotle and from reason [D3.1.7–8]. The third and last opinion he associates with Themistius, Philoponus, Algazel, Aquinas, and others, holding that there are really only two species, of the fact and of the reasoned fact [D3.1.9].

With these preliminaries aside, Galileo replies to the question with four conclusions, buttressing each with numerous supporting arguments. The first is that demonstration of the fact is a true species of demonstration, though less perfect than demonstration of the reasoned fact, essentially because it proceeds from necessary premises and infers a necessary result [D3.1.10]. The second is that Averroes's most powerful demonstration is not a species distinct from demonstration of the reasoned fact because it adds nothing essential to the latter: both establish cause and existence and both have middle terms that are causes of being as well as of knowing; this suffices to differentiate them from demonstration of the fact, whose middle term is a cause of knowing only [D3.1.11–12]. Galileo's third conclusion then follows directly from his first two and agrees with the teaching of the third opinion, namely, that there are but two species of demonstration. For this he gives the confirmatory argument that we either seek a thing's existence, which we attain through demonstration of the fact, or we seek both its cause and its existence, and this we attain through demonstration of the reasoned fact [D3.1.13]. His fourth and final conclusion provides a further division of demonstration of the reasoned fact into two subspecies: one proves through extrinsic causes, the other through intrinsic causes. To this he adds that, when demonstration through intrinsic causes manifests a property of its primary and adequate subject through principles that are actually indemonstrable, this in itself constitutes a most powerful demonstration [D3.1.14]. The remainder of the question Galileo devotes to answering the arguments from authority in support of the first opinion, then those of reason, and finally those in support of the second opinion [D3.1.15–18].

The second question in the treatise [D3.2] is brief and is concerned with answering a twofold query, namely, how the two kinds of demonstration, that of the reasoned fact and that of the fact, are the same and how they differ, and how the latter type of demonstration, that of the fact, is further divided. Galileo's reply to the first query is that they are analogically (i.e., proportionally) the same, since both argue from true and necessary premises and have much the same properties. Still they differ essentially, both from their middle terms and from their ends [D3.2.1], as pointed out in the previous question. On this basis he explains how the two species are variously defined and labelled by different commentators [D3.2.2]. His reply to the second query is that there are several divisions of demonstration of the fact, of which he enumerates three: those from remote causes or those from an effect; those from convertible terms or those from non-convertibles; and those manifesting a simple existence, such as that of protomatter, a first mover, or an element, or those manifesting a complex existence, i.e., showing a proposition to be true by *a posteriori* reasoning [D3.2.3]. These, as well as the divisions of demonstration of the reasoned fact, are all summarized above in Table 2.

b. *Circularity and Regress.* The last question in this disputation, that on the regress [D3.3], is devoted to explaining the intimate relationship that obtains between the two species of demonstration established in the previous questions and how their proper use can generate new scientific knowledge without involving circularity or begging the question. To locate the problem in context, the teaching on the demonstrative regress arose from a proposal of ancient philosophers who wished the demonstrative process to exhibit perfect circularity in the sense that the conclusion of a demonstration would be known through its premises and the premises in turn would be known through the conclusion. Stated in terms of cause and effect, this would be equivalent to maintaining that the cause would be known perfectly through the effect and the effect reciprocally through the cause. The proposal was rejected by Aristotle, who in *Posterior Analytics* I.13 offered in its stead an imperfect circle whereby a premise or cause could sometimes be inferred through a demonstration of the fact, after which the conclusion or effect would be deduced through a demonstration of the reasoned fact. Rather than labeling this a *circulus* the Paduan Aristotelians referred to it as a *regressus*. As the name suggests, it involves arguing from effect to cause as the first step in a twofold progression, followed by a second step moving backward in the reverse order from cause to effect. Not all Aristotelians were satisfied with this teaching, some rejecting the first progression and others the second; but a substantial number, including Averroes and Thomas Aquinas, accepted it as valid and as offering an important insight into the way the *scientiae naturales* proceed in their investigations. Galileo subscribed to it, and so did Zabarella and the Jesuit professors of the Collegio Romano.

Galileo begins his exposition of the demonstrative regress with five different opinions, enumerated successively as those of Aristotle's predecessors, of the followers of Avicenna, of some moderns who follow Ugo Senensis, of Franciscus Neritonensis and his school, and of Aristotle himself [D3.3.1–5]. Following this he supplies two notations, the first of which relates to the requirements for demonstration, namely, that the proving part must be connected with the proved part in such a way that there can be a necessary inference from the one to the other, and, moreover, that the proving part must be prior and more known [D3.3.6]. The second details three different ways of understanding the relationship between cause and effect: formally, or precisely as cause and effect; simply as disparate entities; and as necessarily connected one with the other [D3.3.7].

These matters presupposed, Galileo states the position he himself follows in three conclusions, as follows: when the relationship between cause and effect is taken formally there can be no demonstrative regress; when cause and effect are taken disparately, that is, without being seen as necessarily connected, there is no circularity in reasoning; and when cause and effect are taken as necessarily connected, there can be a demonstrative regress provided the requisite conditions are observed [D3.3.8–10]. He then interjects an objection relating to the kinds of causality that would seem to permit perfect circularity and gives two replies showing why this is not possible [D3.3.11].

Galileo concludes the question with three queries and their respective responses. The first inquires whether the existence of the effect is manifested in the second of the two progressions that make up the regress, and this he answers with a qualified affirmative [D3.3.12]. The second asks which sciences use the regress, and his reply to this is that it is used most frequently in the physical sciences and almost never in the mathematical disciplines [D3.3.13]. The third then seeks the conditions required for the demonstrative regress to work properly, and these he enumerates as six in number [D3.3.14].

Of the various conditions, all except the fourth are unproblematic. The first three in effect restate matter that has already been covered in the first notation at the beginning of the question: the first, that there must be two progressions, one from effect to cause, the other from cause to effect; the second, that the demonstration of the fact, i.e., from effect to cause, must come first; and the third, that the effect must be more known than the cause. The fifth and six add two logical precisions: the fifth, that the cause and the effect must be convertible (a condition explicitly required by

Aristotle, 78a27–28); and the sixth, that the reasoning, when arranged in the form of a syllogism, must be in the first figure.

The fourth condition makes use of the distinctions given in the second notation at the beginning of the question and is implicit in the way its three conclusions are formulated, though this might not be grasped at first reading. It urges that an interval of time must separate the first progression (effect to cause) from the second progression (cause to effect), on the ground that the first progression does not terminate in knowledge of the cause precisely as such, that is, formally, as it *is* the cause of the particular effect, but grasps it only materially. This time interval is necessitated by Galileo's first two conclusions, because if the cause is grasped formally, according to the first conclusion, there can be no demonstrative regress; again, according to the second conclusion, only if cause and effect are taken "disparately" (another way of saying "materially"), can circularity in reasoning be avoided. During the time interval an intellectual activity must therefore occur – what Zabarella refers to as a work of the intellect (a *negotiatio intellectus*, or a *mentale examen*, ZL486). In the interval the investigator passes from knowing the cause only materially to grasping it formally, precisely as it is the cause, and indeed the unique cause (in view of the convertibility condition), of the particular effect. When cause and effect are seen as necessarily connected in this way, according to the third conclusion, the demonstrative regress becomes possible. The regress then, as Galileo admits, "is circular, but in an improper sense, since in it one progresses from an effect to material knowledge of the cause, and then from formal knowledge of the cause to the proper reason for the effect" [D3.3.14].

c. *Zabarella's Influence.* As De Jong has shown in his comparative study of Zabarella's *Opera logica* and MS 27, there are substantial similarities between the two works. This is especially verified in their treatments of the demonstrative regress, and in fact is explicable in terms of the mode of transmission of the teaching from Zabarella to Galileo by way of Lorinus and Vallius, both of whom make repeated references to Zabarella's writings. A few differences in terminology have obscured this overall agreement, but once these are clarified the agreement becomes undeniable.[23]

Two peculiarities in Galileo's account prove helpful in this respect. The first concerns the term *regressus* itself. As just explained, in the procedure it requires one argues from effect to cause as the first step in a twofold process, and follows this by a second step moving backward in the reverse order (and hence, a regress) from cause to effect. Now one oddity in Galileo's explanation is his repeated reference to this *regressus* as a *progressus*. This seems peculiar, for if a backward motion is being differentiated from a forward motion, why should a *regressus* be called a *progressus*? Here it is noteworthy that Zabarella, although more frequently employing the term *processus*, himself occasionally refers to each component of the *regressus* as a *progressus* [ZL495–496]. This usage is preserved in both Lorinus and Vallius, and so shows up in Galileo's manuscript [Lat. Ed., 294–298] – a *prima facie* evidence of the transmission from one to the other.

The second peculiarity is more difficult to explain but also more convincing. It occurs in Galileo's explanation of what happens between the two steps, i.e., between concluding the reasoning process from effect to cause with a demonstration of the fact and then starting with the cause thus discovered to formulate a second demonstration, this time of the reasoned fact. As we have seen, Galileo says that at the end of the first process one recognizes the cause only materially (*materialiter*), but then one comes to know it formally (*formaliter*), that is, precisely as the cause, and so can use it in a demonstration of the reasoned fact. Zabarella formulates the intermediate stage somewhat differently. For him the first process terminates only in a confused knowledge, a *cognitio confusa*, of the cause; the second stage begins when one has a distinct knowledge of it, a *cognitio distincta*, knowing it precisely as it *is* the cause, and so can argue *propter quid* [ZL486]. Now apparently Zabarella and Galileo are writing about the same twofold knowledge of causes, but whereas Zabarella uses *confuse-distincte* to make his differentiation, Galileo uses *materialiter-formaliter* to make his.

The source of this difference in terminology can be found in the parallel accounts of Vallius and Lorinus. When explaining the *regressus* Vallius begins with the distinction between *cognitio confusa et distincta* that is found in Zabarella. Then, when elaborating on the interval separating the two parts of the regress, Vallius interprets this "confused-distinct" terminology as equivalent to another scholastic distinction, that between the "material" and the "formal," between first recognizing a cause *materialiter* and then knowing it *formaliter* precisely as it is the cause [VL2: 345]. (Actually he attributes knowledge of the equivalence to Zabarella by inserting a marginal reference to him at this place in the text.) Thenceforth he conflates the material-formal distinction with the confused-distinct when explaining the methodology of the demonstrative regress.

Further light on Vallius's reading of Zabarella can be obtained from Lorinus's intermediate account, which was probably being perused by Vallius when writing his own. Lorinus likewise begins with Zabarella's confused-distinct terminology. But, when explaining the difference between recognizing a cause only imperfectly and confusedly and knowing it distinctly and more perfectly, Lorinus further suggests that knowing things "more plainly and distinctly" (*planius et distinctius*) means knowing them "under a better form" (*meliora forma*). This idea then leads him to write of knowing a cause *virtualiter* at first and subsequently grasping it *formaliter*. If a cause is known only "virtually," he writes, the effect is known only virtually also. But the second stage of the regress brings it about that what was previously not known distinctly comes to be known expressly (*expresse*). And, since the middle term contains the entire syllogism virtually, one can understrand why the cause is not grasped *formaliter* at the end of the first stage [LL555–556]. It is in these passages, as explained more fully in the Latin Edition (pp. 299–301), that Lorinus makes the transition from knowing a cause distinctly, through knowing it *meliora forma*, to finally knowing it *formaliter*. In this way he sets up the *materialiter-formaliter* distinction that is invoked by Vallius and appears ultimately in Galileo's manuscript.

This, then, is a brief overview of a Zabarella's influence on Galileo's understanding of the demonstrative regress, transmitted to MS 27 by way of Lorinus and Vallius. On the basis of such evidence it can be seen that Galileo's *logica docens*, as it bears on the *regressus*, is effectively the same as Zabarella's. And, since in many ways the regress doctrine recapitulates the most important portions of Aristotle's *Posterior Analytics* that are needed to establish a methodology of the physical sciences, we should not be surprised if Galileo's *logica utens* shows abundant signs of this doctrine being at work. Justifying this claim is the burden of the next part of this study.

NOTES

[1] For the texts in which these expressions occur and a more detailed exegesis, see our "Aristotle and Galileo: The Uses of *Hupothesis* (*Suppositio*) in Scientific Reasoning, *Studies in Aristotle*, ed. D.J. O'Meara, Washington, D.C.: The Catholic University of America, 1981, 47–77.

[2] This is the more or less standard account of the structure of scientific theories advanced by Rudolf Carnap, C.G. Hempel, and others: representative selections from their writings will be found in *Readings in the Philosophy of Science*, 2d ed.. eds. Baruch Brody and Richard Grandy, Englewood Cliffs, N.J.: Prentice-Hall, 1989.

[3] M.A. Finocchiaro discusses some of the ambiguities in his *The Galileo Affair: A Documentary History*, Berkeley: University of California Press, 1989; see the entries under "Hypothesis" and "Supposition" in the index. On the basis of the documents he analyzes relating to the trial Finocchiaro does not see any significant difference in the two terms, 334 n. 11. A difference is clearly noticeable, however, in Galileo's usage in MSS 27, 46, and 71 and in his scientific writings analyzed in Chaps. 5 and 6 below, where *suppositio* and its variants invariably refer to a premise in a posterioristic argument rather than to one in the prioristic mode.

[4] *Physics* B.8–9, especially 199b35–200a15. For a detailed analysis of this and related texts, see "Aristotle and Galileo," 53-57.

[5] In *I Posteriorum Analyticorum*, lect. 42, n. 3; see also lect. 16, n. 6, and In *II Posteriorum Analyticorum*, lect. 7, n. 2, and lect. 9, n. 11. English translations of these passages will be found in Thomas Aquinas, *Commentary on the Posterior Analytics of Aristotle*, tr. F.R. Larcher, Albany: Magi Books, 1970, 54–55, 148, 187–188, and 200.

[6] Aquinas touches on some elements of this causal analysis in the texts cited in the previous note; a more complete exposition is given by Zabarella in his *De medio demonstrationis*, portions of which have been paraphrased in note 21 of Chap. 3 above.

[7] The science of the rainbow (*scientia de iride*) was not known in detail to Aquinas, but its elements were worked out shortly after his death by another Dominican working at Paris, Theodoric or Dietrich of Freiberg. For a brief summary of this teaching as related to supposition, see our "Aquinas on the Temporal Relation Between Cause and Effect," *The Review of Metaphysics* 27 (1974), 573–574.

[8] The lecture notes of Rugerius are preserved in the Staatsbibliothek of Bamberg, Cod. Msc. Class. 62.1–2, herein referenced as *Logica* (1589). He begins his discussion of supposition on fol. 413v.

[9] *Logica* (1589), 413v–414r.

[10] *Logica* (1589), 414v.

[11] *Logica* (1589), 414v–415r.

[12] We have provided a fuller division in our "Aristotle and Galileo" (pp. 62–73, summarized in Table 2 on p. 74), but for purposes here the ones given are most noteworthy.

[13] Blancanus, *Apparatus*, 407.

[14] The manuscript is preserved in the Vatican Library, Cod. Urb. Lat. 1471, *Ioannis Laurinis [= Lorini] Societatis Iesu Logica*, 1584; the printed version is Ioannes Lorinus, *In universam Aristotelis logicam commentarii, cum annexis disputationibus Romae ab eodem olim praelecti*, Cologne: Sumptibus Petri Cholini, 1620, henceforth abbreviated as LL.

[15] Each of the figures of the categorical syllogism is designated by a mnemonic that contains three vowels and a variable number of consonants. In Barbara the three vowels are all A's: they designate that the three propositions of which the syllogism is composed – the major premise, the minor premise, and the conclusion – are all universal affirmative propositions.

[16] In the *Physics* the major treatment of the four causes is in II.3 [194b17–195b30; in the *Metaphysics* a similar explanation is given in V.2 [1013a24–1014a25].

[17] Alternatively, see the index to the Latin Edition, as well as that of *Galileo and His Sources*

for his use of causal terminology in other writings. Additional information is given in our "The Problem of Causality in Galileo's Science," *Review of Metaphysics* 36 (1983), 607–632.

[18] E.J. de Jong, *Galileo Galilei's Logical Treatises (MS 27) and Giacomo Zabarella's Opera Logica: A Comparison*, Ph.D. Dissertation, The Catholic University of America, Washington, D.C., 1989.

[19] Compare this sense of induction with that described by Zabarella in ZL171* and cited in the previous subsection.

[20] For example, De Jong notes that whereas Galileo and Zabarella agree that the principles of demonstration can be founded on induction, Galileo seems to hold that some principles can be known independently of the inductive process, whereas Zabarella denies this possibility (p. 349).

[21] Actually Galileo lists five rules here, but since his fifth rule recapitulates and makes more precise his fourth, we omit the latter and so list only four in our summary.

[22] This line of research has been pioneered by John Herman Randall, Jr., in the works cited in Chap. 1, n. 1.

[23] How the problem of these terminological differences has been overcome has been described in our essay, "Randall Redivivus: Galileo and the Paduan Aristotelians," *Journal of the History of Ideas* 48.1 (1988), 133–149.

LOGICA UTENS

CHAPTER 5

GALILEO'S SEARCH FOR A NEW SCIENCE OF THE HEAVENS

Equipped now with a fairly extensive knowledge of the *logica docens* appropriated by Galileo at the beginning of his teaching career, we move to the more difficult part of our study, that relating to his actual use of the scientific methodology it implies, his *logica utens*.[1] Before citing specific examples of such use we would stress a point made earlier, one on which Zabarella, Vallius, and Galileo all agree, namely, that there is a vast difference between *logica docens* and *logica utens* [Sec. 1.5]. The logic discussed thus far, the instrumental habit that directs the mind's operations to the attainment of truth, is *logica docens*, logic pure and simple. As opposed to this the *logica utens* that is used by an astronomer or a physicist to reach conclusions about the heavens or motion is, strictly speaking, not logic at all. When applied to subject matters in the real world it ceases to be logic and becomes instead the science of astronomy or that of mechanics. That is what *logica utens* means in an Aristotelian context: the use of logic in a scientific discipline – a use so intrinsic to the discipline that it becomes identified with the discipline itself.

Now Galileo's search for a new science of the heavens is particularly instructive, for it shows him using the regressive methodology explained in D3.3 in innovative and unusual ways. He himself does not refer to the *regressus* in his astronomical writings, though he does employ the related Italian expression, *progressione dimostrativa*, in his 1612 analysis of floating bodies [GG4: 67]. This is not unusual, however, for the Latin *regressus* has no direct counterpart in the Greek text, and perforce Aristotle himself did not use the term.[2] Yet Zabarella, in his commentary on *Posterior Analytics* I.13,[3] is able to identify the precise point at which Aristotle employs the regress in his study of the heavenly bodies. This he finds in the discussion of how a demonstration of the fact can be converted into a demonstration of the reasoned fact, exemplified in Aristotle's text with explanations of how one can arrive at the conclusions that the planets are near and that the moon is a sphere [ZL836–838].

Aristotle's use of astronomical examples to explain the possibility of this conversion has a significance that cannot be overemphasized. The phenomena of the heavens are so remote from experience that there is no way the causes of such phenomena can be discovered directly by the senses. If they are to be discerned, they must be found with the eye of the mind, by reasoning from effects that are sensible and so more known. To state this more strongly still: it is impossible to have a science of astronomy, in the Aristotelian sense of *scientia*, without making use of the demonstrative regress. One cannot begin *a priori* with demonstrations of the reasoned fact for the simple reason that the causes on which such demonstrations would have to be based cannot be sensed immediately. If they are to be known at all, such knowledge can come only through *a posteriori* reasoning. That is the basic reason why Galileo used, or attempted to use, the *regressus* in his writings on the heavens. He *had* to use it if he wished to establish the conclusions he was seeking, even those he wished to derive from his observations with the telescope.[4]

1. A DEMONSTRATIVE PARADIGM

Precisely how the regress works in cases such as these may be seen from a study of Galileo's vernacular course in astronomy, the *Trattato della Sfera ovvero Cosmografia*, which could have been written as early as 1591 and was used by him for private instruction at Padua down to at least 1606.[5] The context is his explanation in the *Trattato* of the aspects and phases of the moon and the ways in which these vary with its synoptic and sidereal periods [GG2: 251–253]. These phenomena depend only on relative positions within the earth-moon and the earth-sun systems and do not require commitment to either geocentrism or heliocentrism, being equally well explained in either. Basic to the explanation, Galileo notes, is that the aspects and phases are all effects (*effetti*) for which it is possible to assign the cause (*la causa*) [GG2: 250]. Among the causes he enumerates are that the moon is spherical in shape, that it is not luminous by nature but receives its light from the sun, and that the orientations of the two with respect to earth are what cause the various aspects and the places and times of their appearances.

The overall logical form of the argument that follows is implicit in Aristotle's remark in *Posterior Analytics* I.13, recognized, as just noted, by Zabarella as an instance of the demonstrative *regressus*. Following Galileo's characterization of this in D3.3 [Sec. 4.9], it involves two progressions, one from effect to cause and the other from cause to effect, separated by an intermediate stage wherein one sees the causal connection between the two as necessary and adequate to explain the phenomena.

Applying this form to the material Galileo covers in his *Trattato*, one can trace the following line of reasoning:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	Cause
moon's aspects and phases	moon's spherical shape, illumined by the sun, at various positions and times

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

The moon is not luminous by nature, it is externally illumined by the sun, and it is observed from many different angles; *only* a shape that is spherical and this illumination will cause it, under these circumstances, to exhibit the aspects and phases it does at precise positions and times observable from earth.

Second progression: *from the cause, recognized formally as the cause, to its proper effects.*

Cause	Effect
moon's spherical shape, illumined by the sun, at various positions and times	produces moon's aspects and phases, calculated using the laws of geometrical optics

The argument, as can be seen, combines both physical and mathematical reasoning and thus pertains to the mathematical physics of Galileo's day, the middle science of astronomy [Sec. 3.4b]. For the physical part, note that purely mathematical entities are not being discussed; what is under study is the moon, which is a natural body, whose shape is natural, and whose nature is such that it does not emit light as does the sun but shines by reflected light. For the mathematical part, the properties or aspects being studied in the moon are associated with its dimensive quantity and as such are amenable to treatment using theorems from the science of dimensive quantity, namely, geometry. In the above summary these properties are not stated explicitly, although they make up the bulk of Galileo's exposition in the *Trattato*, for there he spells out in detail how the various phases appear at different times depending on the relative positions of the moon, sun, and earth.

Here we skip over his calculations and merely delineate the basic insights that underlie them. These are that the moon itself is a sphere, not a mathematical sphere but a natural body whose shape is closely spherical, and that a spherical shape alone, of all possible geometrical figures, can explain how an externally illuminated body possessing it will manifest phases that are alternately new, crescent, gibbous, and full, and then gibbous, crescent, and new again, but each time with figures that are laterally reversed from those in the preceding series. The reason for the different appearances at different times and places in the heavens is that the interval between two new moons (the synodic period, that of the earth-moon system) is two days longer than the time required for the moon to return to the same configuration of stars and so to be again in conjunction with the sun (the sidereal period, that of the earth-moon-sun system). Thus the situation is complex and requires a knowledge of projective geometry as well as of lunar and solar movements for its comprehension. The same type of knowledge is required to compute the times of lunar and solar eclipses, and Galileo, interestingly enough, likewise explains these calculations in the *Trattato* [GG2: 246–250].[6]

In view of its importance and later use several observations should be made about this paradigm. First, it represents a strict demonstration based on causes and not a topical argument based on the *topos* of antecedents and consequents as explained in Chapter 3. Thus it produces science or true and certain knowledge in those who comprehend it, not merely opinion, in Galileo's understanding of these terms. The causes involved pertain mainly to the genus of formal causality, not that of efficient causality, although the exercise of efficient causality is presupposed to the demonstration. The form involved is that of dimensive quantity, an accidental form rather than a substantial form and proper to the middle science of astronomy.

A number of suppositions are also involved, some in the first progression, others in the intermediate stage. One is that the moon is a sphere, another is that it is illumined by light coming from the sun (the efficiency involved), yet another is that light rays may be approximated by straight lines, all of which function in the first progression. In the intermediate stage it is further presupposed that one either knows beforehand, or comes to see during that stage, properties of spheres under external illumination as well as characteristics of earth-moon and earth-sun motions known from observational astronomy. In light of these suppositions, pertaining to categories [2], [4], or [7] described in Sec. 4.2c above, one may say that the demonstration overall is made *ex suppositione*. Yet not until this intermediate stage, when the "work of the intellect" (*negotiatio intellectus*) is completed and the one engaged in the regress is assured of their truth, is it possible to entertain its second stage. There, assured of having knowledge "of the fact" with regard to all the suppositions, one can complete the regress and formulate the demonstration "of the reasoned fact." It is only at this second stage, therefore, that one attains scientific knowledge of lunar phases and not merely opinion concerning them. The requirements for a science in the Aristotelian sense have been met [Sec. 3.4] and one is entitled to make apodictic statements about this phenomenon in the heavens.[7]

2. DISCOVERIES WITH THE TELESCOPE

During the years between 1604 and 1609 Galileo became involved in an extensive experimental program in which he made substantial progress in another mixed science, the quantitative study of local motion. This program, discussed in the following chapter, may have sharpened his skills in using the demonstrative regress. But a far more important stimulus came in May of 1609 when he heard of a spyglass made in the Netherlands and set himself to construct an instrument of his own. His successful completion of this project late in 1609 and the publication in March of 1610 of his findings in *The Sidereal Messenger* launched him on a new phase of his career, one that would soon bring him fame throughout Europe. For purposes of this study the findings themselves are not as momentous as Galileo's reasoning from them to inaugurate his "new science" of the heavens. Others had constructed telescopes before Galileo, and some had even looked at the heavens with them, but none would formulate the "necessary demonstrations" to which Galileo would come on the basis of his observations.[8] How he could do so provides examples of the demonstrative regress in use, examples that prove more instructive than the paradigm sketched in the previous section.

The problem with the demonstration of the moon's phases, simply put, is that it is "dead science."[9] It yields results known and accepted universally, among astronomers at least, and Galileo's role in explaining them is that of a teacher, not that of an investigator. Here a question treated earlier assumes importance, namely, how "actual science" is acquired, first by those who discover it for themselves and then by those who learn it from others [Secs. 3.1 and 3.2]. In either case one has to "see" a demonstration, that is, one has to see how a particular effect is produced by a particular cause, and necessarily and uniquely by that cause. To "see" this type of connection is the work of the intellect, an example of which was given above when explaining the intermediate stage of the paradigm of the moon's phases. The task of the scientist, it turns out, is to use his intellect to see this for himself, whereas that of the teacher is to assist others to use their intellects to see it for themselves. Thus the limitation in the paradigm is that it requires one to separate out how Galileo first "saw" the material he covers in the *Trattato* and went through the *regressus*, from how he explains it to others so that they will be enabled to do it themselves.

When analyzing Galileo's discoveries with the telescope this difficulty is minimized. In this case the results he presents, unlike the moon's phases, are actual science, "live science" one might say, and he is describing the process he has just gone through to arrive at them. Thus we avoid some of the problems encountered when basing our analysis on a pedagogical treatise. In what follows we therefore apply his use of the demonstrative regress to his findings with the telescope, to the extent that we can do this from the extant documents. We restrict ourselves to three revolutionary conclusions he was able to infer from his findings, conclusions he claimed would be substantiated by anyone of sound mind who had access to an instrument as good as his own. These are the existence of mountains on the moon, that of four satellites of Jupiter, and that of the phases of Venus. The first two are documented in an early letter and in *The Sidereal Messenger* itself, the third in letters subsequent to its publication.

a. *Mountains on the Moon.* By November of 1609, acting on the news from the Netherlands, Galileo had made a series of telescopes with magnifying powers first of three, then of nine, then again of twenty times [GG3.1: 61]. With the latter instrument, between November 30th and December 18th he studied the moon as it went through its phases and made no fewer than eight drawings of the appearances he observed.[10] It is difficult to ascertain precisely how long it took him to infer conclusions from these observations, but in a letter from Padua to Antonio de' Medici in Florence written on January 7, 1610, Galileo wrote that from the data he had obtained "sane reasoning cannot conclude otherwise" than that the moon's surface contains mountains and valleys similar to, but larger than, those spread over the surface of the earth [GG10: 273]. Thus, within less than a month, by his own account, Galileo had demonstrated to his own satisfaction that there are mountains on the moon. He further improved the magnifying power of his telescope to thirty times and proceeded to make other discoveries, which he wrote up, along with those relating to the moon, for publication in *The Sidereal Messenger*.

As already noted, Galileo does not identify demonstrative regresses in his scientific works, and *The Sidereal Messenger* is no exception. Since a number of his expressions signal the use of the reasoning process described in D3.3, however, it is a relatively easy matter to discern in his account the regress sketched in the previous section. Following this we may summarize his argument as follows:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
sharply defined spots on the illuminated parts of the moon's surface, an irregular line at the terminator, with point of light emerging in dark parts	the surface of the moon is rough and uneven, with bulges and depressions [GG3.1:62-63]

Intermediate stage: *work of the intellect to ascertain if this really is the cause, eliminating other possibilities.*

Dark part of spots have their side toward the sun; shadows diminish as the sun climbs higher; points of light in the dark area gradually increase in brightness and size, connect finally with the bright area; "we are driven to conclude by necessity" that *only* prominences and depressions can explain the appearances "for certain and beyond doubt" [GG3.1: 64–69]

Second Progression: *from the cause, recognized formally as the cause, to its proper effects.*

Cause	**Effect**
changing illumination from the sun's rays on mountains of calculable height rising from the moon's surface	produces all of the observed appearances [GG3.1:69-70]

Like the demonstration of the moon's phases, this is not a topical argument but one that purports to yield certain knowledge based on true causes. The causes again are formal accidental causes (figure or shape), although they too presuppose the exercise of efficient causality (the passage of light rays). The figure or shape is that of a natural body capable of reflecting light, and the laws whereby it does so are those of geometrical optics. The irregular shape of the terminator, the boundary separating the light parts from the dark parts of the moon's surface, is not a mathematical line but one traced out by light rays impinging on the surface. Thus the demonstration is that of a mixed science, mathematical physics, not that of pure mathematics or of natural philosophy as such.

Suppositions are likewise involved in the demonstration. Many of these are those required for geometrical optics in general, such as that light rays may be treated as straight lines. Moreover, in the first progression the cause materially suspected, bulges and depressions on the moon's surface, may be seen initially as a supposition; by the conclusion of the intermediate stage, however, the conviction is generated that this supposition is true and so can serve as a premise in a demonstration of the reasoned fact.

The burden of the proof, here as in the previous demonstration, is carried by the intermediate stage, the work of the intellect. But here a special difficulty presents itself, and it seems to work contrary to the second stage of the demonstration. This is an observational difficulty that may be phrased as follows. The periphery of the moon is never seen as uneven, rough, and sinuous, the way the terminator appears through the telescope, but is exactly round and circular. Therefore there are no mountains on the moon, for if there were, its edges seen through the telescope would be ragged like a sawtooth. This difficulty either occurred to Galileo or was presented to him some time between January and March of 1610, for he raises it and proposes two answers to it in *The Sidereal Messenger*. The first is that chains of mountains close together, such as those that cover the moon's surface, create the impression of a flat and regular surface when seen from a distance, and this explains the circular appearance of its edge. The second is an *ad hoc* hypothesis: perhaps an "orb of denser substance than the rest of the ether" surrounds the lunar body and inhibits our vision so that we do not see the actual shape of the body; thus we perceive its edge as circular even though it really is not [GG3.1: 69–71].

The second reply was unfortunate, and Galileo later withdrew it. It caused difficulty for Christopher Clavius, who apparently seized on it to raise the possibility that the moon's surface is not uneven but that its body has denser and rarer parts that reflect light in ways that merely suggest a mountainous terrain [GG11: 93]. Others proposed similar counterexamples, but they were able to gain few adherents. With regard to the first reply, as Van Helden reports it was not until 1664 that telescopes of sufficient magnification and resolving power were available to show the remaining small irregularities in the moon's outline.[11] Giovanni Domenico Cassini observed and reported them in that year and thereby vindicated Galileo's explanation.

Other demonstrations concerning the moon are to be found in *The Sidereal Messenger*. We mention here Galileo's reference to only one, the presence of an ashen light (*lumen cinereum*) on the dark face of the new moon. Of this Galileo writes that he here wishes to assign the cause (*causam assignare*) of the light, although he had explained and given a causal demonstration of it to students and friends many years ago [GG3.1: 72] – a clear indication of his ongoing interest in the regressive methodology of the *Posterior Analytics*.

b. *Satellites of Jupiter.* On the very evening Galileo wrote to Antonio de' Medici that he had conclusively demonstrated the existence of mountains on the moon he noted a strange phenomenon, namely, that the planet Jupiter was "accompanied by three fixed stars" [GG10: 277]. That was on January 7, 1610, and it turned out to be a momentous observation. At the time Jupiter was close to opposition, at its closest approach to earth, and was the brightest object in the evening sky. The next evening Galileo turned his telescope on the planet again, hoping to see it that it had moved to the west of the stars, as astronomical computations then predicted [GG3.1: 80]. To his surprise this time he found it to the east of them. His attempt to resolve this apparent anomaly led him to a program of observing Jupiter and its strange companions whenever he could over a two-month period. By January 11th he had concluded that they were not fixed stars that could be used to determine the motion of Jupiter, but rather that they were small bodies, never observed before, that were moving along with Jupiter and indeed were actually circling it. "I therefore arrived at the conclusion, entirely beyond doubt (*omnique procul dubio*)," he writes, "that in the heavens there are three stars wandering about Jupiter like Venus and Mercury around the sun" [GG3.1: 81]. On January 13th he saw a fourth object for the first time, and by the 15th he had convinced himself that it was doing the same [GG3.1: 82]. Thus within a week of his curiosity having been aroused by the anomaly he had completed the demonstrative *regressus* and had convinced himself that Jupiter had four satellites revolving about it, as it made its own majestic revolution around the center of the universe.

This conclusion he continued to work out and confirm with repeated observations, some sixty-five in all, that occupied him until March 2, 1610. He quickly wrote these up, along with his description of the mountains on the moon and his other observations of stars and planets, and had them published at Venice by March 19th under the title *The Sidereal Messenger*. Their impact on the astronomical world cannot be over-estimated. Here for the first time was clear evidence that not all motions in the heavens are around the earth, as had been commonly thought. Moreover, whereas the earth had but a single moon, the planet Jupiter was now seen to have four, circling it and moving along with it in its passage through the skies.

Just as previously we have summarized the reasoning whereby Galileo was able to demonstrate the existence of mountains on the moon, so here we give in schematic form the demonstrative *regressus* he used to establish the existence of these satellites:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
four little stars accompany Jupiter, always in a straight line with it, and move along the line with respect to each other and to Jupiter	the stars are planets of Jupiter, circling around it at various periods and distances from it

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

Sixty-five observations between January 7 and March 2, analyzing in detail their variations in position, how they separate off from Jupiter or each other and merge with them in successive observations; inference to the only possible motion that explains these details; concluding "no one can doubt" (*nemini dubium esse potest*) that they complete revolutions around Jupiter in the plane of the elliptic, each at a fixed radius and with its characteristic time of revolution [GG3.1: 94].

Second Progression: *from the cause, recognized formally as the cause, to its proper effects.*

Cause	**Effect**
four satellites of Jupiter always accompany it, in direct and retrograde motion, with their own distances from it and periods of revolution [GG4: 210], as it revolves around the center in twelve years	seen on edge produce the appearance of four points of light, moving back and forth on a line with the planet and parallel to the elliptic.

Much the same observations may be made about this demonstration as about the previous two illustrations of the demonstrative regress. Like them it purports to be apodictic, not merely dialectical; it is concerned with the mathematical properties of natural or physical bodies and so pertains to the middle science of mathematical astronomy. It employs similar suppositions, mainly taken from projective geometry and geometrical optics, and of course is unintelligible to those unacquainted with those disciplines. More importantly, it supposes that the observational evidence presented by Galileo is correct and that it can be verified, as he claims, by anyone possessing a good twenty-power telescope. This supposition definitely slowed the acceptance of the demonstration in Galileo's day, but, considering the circumstances, it was verified and accepted by the scientific community in a short time. Within the year in fact, on March 24, 1611, the Jesuit astronomers at the Collegio Romano had confirmed Galileo's discovery, writing to their confrere, Cardinal Bellarmine, that

> four stars go about Jupiter, which move very swiftly, now all to the east, and now all to the west, and sometimes some move to the east and some to the west, all in an almost straight line. These cannot be fixed stars, for they have very swift motions, very different from those of the fixed stars, and they always change their distances from each other and from Jupiter [GG11: 93].

c. *Phases of Venus.* The last of the discoveries with the telescope to be treated here is that of the phases of Venus. While Galileo was making his observations of Jupiter, Venus was in the morning sky and not in a favorable position for viewing. Although he suspected that it was going around the sun, as he indicates in a passage of *The Sidereal Messenger* cited above, he had no way of confirming that suspicion. It was not until

October of 1610 that Venus appeared in the evening sky and Galileo could seek the confirmation he sought. Just as he was about to announce he had obtained it, he received a letter dated December 5, 1610 from his former student, Benedetto Castelli, inquiring whether Venus as seen through the telescope was "sometimes horned and sometimes not" [GG10: 481]. In Brescia at the time and lacking a telescope himself, Castelli apparently had the same thought as Galileo and saw this appearance as necessary proof of Venus's revolution around the sun. Galileo wrote back to Castelli at the end of December saying that he had been observing Venus with his instrument for about three months and described what he had seen. Earlier, on December 11th, he felt that he had enough data to send an anagram to Prague for Kepler forecasting his imminent discovery of Venus's phases, and on January 1, 1601, he wrote again to Prague unraveling the anagram and announcing it as a fact.

Galileo described his findings on Venus's appearances to Castelli in the following terms:

> ...about three months ago I began to observe Venus with the instrument, and I saw her in a round shape and very small. Day by day she increased in size and maintained that round shape until finally, attaining a very great distance from the sun, the roundness of her eastern part began to diminish, and in a few days she was reduced to a semicircle. She maintained that shape for many days, all the while, however, growing in size. At present she is becoming sickle-shaped, and as long as she is observed in the evening her little horns will continue to become thinner, until she vanishes. But when she then reappears in the morning, she will appear with very thin horns, again turned away from the sun, and will grow to a semicircle at her greatest digression [from the sun]. She will then remain semicircular for several days, although diminishing in size, after which in a few days she will progress to a full circle. Then for many months she will appear, both in the morning and then in the evening, completely circular but very small in size [GG10: 503].[12]

Thus the answer to Castelli was affirmative: sometimes Venus was horned, as Galileo describes it above, and sometimes it was not, namely, when it showed a full or half disk. It therefore emulates the figures of earth's moon, as Galileo put it when deciphering his anagram for Kepler, and so offers conclusive proof that it revolves around the sun.

The argument that would convince one of the truth of this conclusion is very similar to the explanation of the phases of the moon given above, although the geometry is different in the two cases. In a heliocentric system planets that come between the earth and the sun, such as Mercury and Venus (inferior planets), appear differently from those that are farther out and so do not, such as Jupiter and Saturn (superior planets).

As seen from a moving earth, inferior planets at some time during their synodic periods show crescent phases; superior planets, on the other hand, never have crescent phases, though they have gibbous phases when in quadrature with the earth and otherwise are seen as full. The basic reason is that inferior planets, like the moon, come between the earth and the sun and so can receive the partial illumination that shows up in the crescent phase. Always being within the earth's orbit, inferior planets cannot be in opposition to earth; instead they have two conjunctions with it, an inferior conjunction when closest and a superior conjunction when farthest away. During the first they are "new" and during the second "full"; in between they are in quadrature and exhibit the "half-moon" appearance. Thus they go through the same phases as the moon. The major difference is that at inferior conjunction the planet is very much larger than it is at superior conjunction, whereas the moon, maintaining the same distance from the earth, appears to be of the same size throughout the phases.[13]

In light of these considerations, the demonstrative regress for proving that Venus has phases may be summarized as follows:

First progression: *from effect to cause: the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
Venus manifests the same phases as the moon but changes in size as it goes through the phases, being smallest when it is full	resulting from the fact that Venus is in orbit around the sun and is seen at varying distances from the earth

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

The progression of shapes of Venus as it is observed through a good telescope, from full to semicircular to new and back to full again, with corresponding changes in sizes from small to large and back again, has only one possible explanation: Venus is located between the earth and the sun (that is, it is an inferior planet) and it is in orbit around the sun.

Second Progression: *from the cause, recognized formally as the cause, to its proper effects.*

Cause	**Effect**
Venus's motion around the sun as an inferior planet	explains its changes in sizes and shape throughout its orbit

Once again what is proposed is a demonstration, not a dialectical argument, based on suppositions drawn from projective geometry and geometrical optics as well as on the observational evidence presented by Galileo. With regard to the intermediate stage, the necessity of the conclusion does not follow directly from the observations and can be seen only with the eye of the mind. Both Galileo and Castelli seem to have been aware of this and the complication it introduced when presenting their case. In his letter Castelli had remarked that if the appearances were as he thought they would be, they would "be a sure means of convincing any obstinate mind" – using an expression, oddly enough, that is found in Galileo's MS 27 [F2.3.4]. In his reply Galileo reacted to this by noting that demonstrations can convince those "who are capable of reason and desirous of knowing the truth," but unfortunately that their adversaries were not of this type [GG10: 503–504]. Thus he was under no illusion about the ability, and the willingness, of his audience to complete the intermediate stage successfully and so agree with the conclusion he had demonstrated. The Jesuits at the Collegio Romano were fortunately not of this group, for in their report to Bellarmine mentioned above they offered complete confirmation of Galileo's discovery:

> ...it is very true that Venus wanes and waxes like the moon. And having seen her almost full when she was an evening star, we have observed that the illuminated part, which was always turned toward the sun, decreased little by little, becoming ever more horned. And observing her then as a morning star, after conjunction with the sun, we saw her horned with the illuminated part toward the sun. And now the illuminated part continuously increases, according to the light, while the apparent diameter decreases [GG11: 93].[14]

This was not until March of 1611, however, and thus of no immediate help to Galileo. But about the same time as Castelli was writing to Galileo, Antonio Santini, a telescope maker at Venice who had sent one of his telescopes to Clavius, also wrote to Galileo. He remarked that Clavius and his conferes were already observing Jupiter's satellites, and he was good enough to enclose their observations for four nights ending November 27, 1610 [GG10: 479–480].[15]

With regard to the second progression, it should be noted that this concludes only that Venus goes around the sun and makes no inference that the earth does also. Thus it disproves the Ptolemaic system without clearly confirming the Copernican alternative. Up to this time, apart from a promise in *The Sidereal Messenger* to show in his forthcoming work on the system of the world that the earth was in motion [GG3.1: 75], Galileo had in fact not openly embraced Copernicanism. Now, with the evidence of the moon's earth-like appearance at hand, the knowledge of Jupiter's moons, and finally of Venus's phases, he seems to have been convinced of the superiority of the Copernican system and began to say so publicly.[16]

3. THE SUNSPOT ARGUMENTS

Christopher Clavius died on February 6, 1612. With his death Galileo's relationship with the Jesuits, which had improved after his personal visit to the Collegio Romano, where he had in fact been feted with a solemn convocation on May 18, 1611, began to deteriorate. There were several reasons for this, one surely being Galileo's now taking up the Copernican cause and joining forces with Kepler at a time when Jesuit astronomers had adopted the Tychonian system.[17] Another was a dispute with a German Jesuit, Christopher Scheiner, who taught astronomy at Ingolstadt, and who published under a pseudonym three letters on sunspots in 1612. Too occupied with other observations, Galileo had paid little attention to the spots, which were being noticed by others at the time and of which he was already aware. Scheiner's letters caused him to investigate them more seriously, and this led to a heated controversy which would have repercussions with the Jesuits for many years to come.

Like comets, sunspots are celestial phenomena that are difficult to investigate, as attested by the fact that definitive knowledge of them is being reached only in our own day. Thus it is not surprising that many of the arguments between Scheiner and Galileo are dialectical in kind, leading at best to probable conclusions. Yet demonstrations are proffered by Galileo in his sunspot letters, and indeed in his interchanges he makes more frequent claims to have achieved "necessary demonstrations" than he does in many of his other scientific treatises. This is not unusual, for as pointed out in Sec. 3.6, necessary propositions can be used in probable argument. Upon analysis, moreover, most of the demonstrations to which Galileo refers are not ostensive; for the most part they rely on indirect proofs, using techniques of reduction to the impossible or to the absurd [cf. Sec. 4.4b]. Obviously there are many attempts at definition in the letters, and hypotheses and suppositions abound. And of some significance is Galileo's growing confidence, based on his previous successes with the telescope, that his mathematical physics can achieve results of philosophical importance, indeed, that through a study of various quantitative attributes he can arrive at knowledge of the "true constitution" of the universe [GG5: 102].

In view of the fact that the demonstrative regress is not employed to resolve the dispute about sunspots, as well as others to be discussed later in this chapter, we here employ a different format for displaying the procedures involved in probable argument. This parallels to some extent that of the regress, and thus it too is divided into three stages. The first is that of "possible explanation," wherein an appearance in the heavens leads the investigator to propose one or more possible explanations for its occurrence; the second is that of "dialectical inquiry," wherein probable or even necessary arguments are used to eliminate some of these possibilities and favor others; and the last that of "probable explanation," wherein an explanation regarded as probable (or more or most probable) is finally invoked to account for the observed appearances. These stages will be identified in what follows to show the degree to which various controversies could be resolved in the early part of the seventeenth century.

Galileo's reply to Scheiner's letters on sunspots likewise consists of three letters, published at Rome in 1613, entitled *Istoria e dimostrazioni intorno alle macchie solari e loro accidenti* (An Account and Demonstrations Concerning Sunspots and Their Properties). A key to the understanding of the reference to demonstrations in Galileo's title is contained in his first letter, where he states that it is easier to tell what the sunspots are not than what they are, and that it is much more difficult to discover the truth than it is to disprove the false [GG5: 95]. This is a clue that he was seeking indirect or negative demonstrations, not ostensive proofs of the type involved in the demonstrative regress. In his second letter he accepts a dichotomous division proposed by Scheiner and states as a necessary conclusion that the spots either rotate in a sphere of their own or are on the body of the sun and are carried along with its rotation. Of the two positions, he writes, "it seems to me (*per mio parere*) that the second is true and the first false" [GG5: 118]. His arguments are then based on properties, mainly quantitative, he has observed in the appearances; these correspond exactly (*puntualmente rispondono*) with their being on the sun's surface and nowhere else, and in this sense constitute necessary demonstrations [GG5: 127–128]. Aristotle himself would have agreed with these results, Galileo concludes, for he too based his reasoning on sense observation [GG5: 139].

What the subject of these demonstrations might be, however, is not completely clear throughout Galileo's letters. "Spot" is a vague term that connotes an appearance without identifying its cause or the substance that underlies it. Galileo acknowledges this difficulty and makes no claim to have solved it. He does, however, make a good attempt at defining the sunspots, employing all the dialectical techniques available to him [Sec. 3.7c]. The *topos* of similarity is the one to which he turns in an attempt to find an analogy in materials better known. Scheiner had done this in identifying them as stars (*stelle*); Galileo's view would be quite the opposite, for he sees them as resembling nothing more than clouds (*nugole*). He thereupon gives a detailed description of how sunspots change in size and appearance, and then how clouds do the same, to justify this characterization [GG5: 106–107]. In its light he qualifies somewhat his conclusion that the spots are *on* the sun's surface, preferring to see them as in some way contiguous to it. As he states it:

> If I am not mistaken, it is necessary to conclude that sunspots are contiguous with or very close to the body of the sun; that they are of material which is not permanent and fixed, but variable in shape and density; that they are movable to some extent by little irregular motions, and that they are all produced and dissolved, some in longer and some in shorter times. It is also manifest that their rotation is about the sun, though it remains questionable whether this happens because the sun itself rotates and carries them along with it, or whether the sun remains motionless and the spots are conducted by a rotation of some surrounding medium. It could happen either way [GG5: 133].

The tentative character of these statements belies any attempt to make out of them apodictic arguments of the type Galileo had been able to advance from his previous discoveries with the telescope.

The basic reasoning of the *Istoria e dimonstrazione*, consisting as it does of letters written at different times and concerned with ongoing observations of the sunspots, is difficult to summarize. Its argumentation may be reduced, however, to the following logical form:

Possible explanation: *from an appearance to one or more possible explanations of its occurrence.*

The observed appearances, following Scheiner, may be explained in one of two ways: as spots moving on the sun's surface or as spots rotating in a celestial sphere outside the sun, presumably "stars," i.e., planets.

Dialectical inquiry: *use of probable or necessary arguments to eliminate some possibilities or favor others.*

Geometrical optics provides necessary demonstrations that the spots are *not* outside the sun but are contiguous with its surface, namely: they appear thinner when near the edge than when close to the center; the distances they travel increases as they approach the center and decreases as they recede toward the edge; and they separate more and more as they approach the center – for one who knows *perspettiva*, "a clear argument (*manifesto argomento*) that the sun is a globe and that the spots are close to the sun's surface" [GG5: 119].

The spots display no parallax, showing identical arrangements from any places on which they are observed on earth, and thus are most remote from earth – a necessary demonstration that admits of no response whatever [GG5: 128].

The appearances of the spots are *not* those of stars but more resemble those of clouds that form and dissolve and so change size and shape; it is thus not certain that they return after a complete revolution, nor is it certain that the sun itself rotates on its axis, although it appears to do so [GG5: 133].

Probable explanation: *from an explanation now regarded as probable (or as more or most probable) to the appearance it explains.*

The spots are definitely *not* stars or planets rotating in their own celestial orbs around the sun somewhere between it and earth; it is probable that they are clouds in a medium surrounding the sun's surface; it is more probable that the sun itself rotates and carries this medium and its clouds along with it than that these have an independent circular motion around the sun.

With regard to this analysis, the most important thing to note is that it is disputational in form: it is directed against Scheiner and makes use of his two suppositions, reducing one of these to the impossible by indirect demonstration and leaving the other to stand without proposing direct proof. The "possible explanations" that provide the framework make no pretense to be exhaustive. The setting is parochial: the Jesuit had proposed one alternative that preserved the basic unchangeability of celestial matter; Galileo is intent on destroying that alternative so as to defend another, namely, a basic similarity between celestial matter and that of human experience.

The "dialectical inquiry" is interesting in that it provides a prototype of Galileo's writings on astronomy up until the decree against Copernicanism of 1616. Much is made in these writings, as we shall see, of "necessary demonstrations based on sense experience," and less is made of the conclusions at which they arrive. Here it is important to note that the same type of proofs from mathematical physics as has been seen earlier in the *regressus* examples continues to be employed, and even though their subject is somewhat ill-defined, they nonetheless conclude apodictically. Because they do, they rule out the possible explanation preferred by Scheiner. At the same time they do not provide enough information for one to advance beyond the fact of the spots' position to further knowledge of the reasoned fact. That is, although the spots are sufficiently localized, not enough is know about them to provide satisfactory explanations of why they appear as they do. Thus one is left in doubt and opinion concerning their properties, and can only conjecture about how they are produced and why they behave the way they do.

The "probable explanation," finally, is not completely devoid of necessity and couched only in probabilities. The necessity that is present, however, is the negative necessity resulting from an indirect proof: it tells, as Galileo forecasted in his first letter, where spots are *not* rather than where and what they are. Any positive content the explanations may have is regulated to some degree by the initial dichotomy on which they are based, one that surely does not exhaust all possible alternatives, as the present state of solar research reveals. Yet the probable conclusions they suggest are remarkably prescient, and one continues to be amazed at how much information Galileo was able to extract from the meager data his telescope provided him.[18]

5. TIDES, COMETS, AND EARTH'S CENTRALITY

In his *Letter to Christina* Galileo makes reference to "physical effects whose causes perhaps cannot be determined in any other way" [GG5: 311], without indicating what these effects might be. Historians are agreed that he had in mind the tides, which he speculated, as had one of his teachers at Pisa, Andrea Cesalpino, might be caused by the motion of the earth.[19] Apparently Galileo had been thinking of this for some time and had come to an explanation quite different from Cesalpino's. While in Rome he discussed this with a young friend, Alessandro Orsini, who had just been made a cardinal and who asked Galileo to commit it to writing. Galileo did so on January 8, 1616, in a letter now entitled *Discourse on the Tides*. The *Discourse* is important for the fact that it was written after Bellarmine's letter to Foscarini but before the decree against Copernicanism, dated March 5, 1616. It is Galileo's first attempt to formulate the tidal argument that would later cause him considerable difficulty with Pope Urban VIII, and his last attempt to demonstrate Copernicus's primary supposition before the issuance of the famous decree that thenceforth would effectively prohibit any advocacy of the earth's motion.

Apart from this initial version of the tidal argument, Galileo wrote few treatises on logical methodology as it pertains to astronomy until his *Dialogue* of 1632 – no doubt inhibited by the decree. Two additional evidences are available, however, that cast light on his knowledge and use of the *Posterior Analytics* in the interim. One is the documentation surrounding his dispute with the Jesuit Orazio Grassi on the nature and appearance of comets, occupying the period between 1618 and 1623, the other his letter to Francesco Ingoli of 1624, wherein he takes up Ingoli's earlier attempt to demonstrate the falsity of the Copernican system and shows the logical defects in his arguments. Both of these will be discussed briefly in what follows.

a. *Discourse on the Tides.* Galileo begins his letter to Orsini by noting the "amazing problem" posed by the tides and asserting that the only way to quiet the mind about them is to find their "true cause." This tranquillity of mind, he says, "is attained only when the reason advanced as the true cause of the effect accounts easily and clearly for all the symptoms and features that are seen in the effect" [GG5: 377]. Sensory appearance shows that the tides involve a true local motion in the sea, and thus to find their cause one must investigate the various ways motion can be imparted to water. This is an indication that Galileo's argument here will be dynamical, as opposed to the kinematical arguments based on projective geometry we have analyzed previously; thus, in place of the formal causes there identified he will now be seeking efficient causes. He further notes the complexity of the phenomena connected with the tides, and on this account will see if any of the possible movers "can reasonably be assigned as the primary cause of tides." To this he proposes to add secondary and concomitant causes to account for the diversity of the movements, invoking one of his causal maxims, namely, that "from a single and simple cause only a simple and determinate effect can derive" [GG5: 378]. Galileo's tone at the outset suggests a causal analysis using the canons of the *Posterior Analytics*, but it should be noted that his expression "can reasonably be assigned" is also compatible with dialectical argument based on the *topos* of cause and effect earlier explained [Sec. 3.7a].

The procedure Galileo follows is first to enumerate various causes that can effect the motion of water, among which he spends most time on the motion of its container, exemplifying this with the motion of "a large boat like those carrying fresh water from one place to another over salt water" [GG5: 380].[20] Examining all of these, he writes that he is

> greatly inclined to agree that the cause of the tides could reside in some motion of the basins containing seawater; thus attributing some motion to the terrestrial globe, the movements of the sea might originate from it. If this did not account for all particular things we sensibly see in the tides, it would thus be giving a sign of not being an adequate cause of the effect; similarly, if it does account for everything, it may give us an indication of being its proper cause, or at least of being more probable than any other one advanced till now [GG5: 381].

On this basis, then, he takes the motion of the earth hypothetically (*ex hypothesi*), and from its two motions, one of annual motion around the sun, the other of diurnal rotation on its axis, explains how it might function as a primary cause of the back and forth motion of water on its surface. Then he turns to "the particular details, so numerous and so diverse," of tidal motions in different areas to ascertain "the specific and adequate causes" of each [GG5: 383]. These he variously identifies as the gravity of sea water, the length and depth of the basin in which it is found, the frequency of its oscillations, and the ways these can coordinate with various parts of the earth's movement. He then examines "the properties of tides observed in experience" [GG5: 387], and gives an account of eight different phenomena based on his own observations or those of others, attempting in the process to correlate these generally with the specific and secondary causes he has identified. He thereupon completes the exposition somewhat abruptly, as follows:

> This was what I advanced as the cause of these motions of the sea in my discussion with you, Most Eminent Lord. It was an idea which seemed to harmonize mutually with the earth's motion and the tides, taking the former as the cause of the latter, and the latter as a sign of and an argument for the former [GG5: 393].

Before concluding Galileo then gives a brief consideration to another sign of the earth's movement, that of winds on its surface, and makes a plea for more extensive empirical reports from elsewhere, "for by comparing and collating them with the assumed hypothesis we could decide more firmly and ascertain more correctly the things that pertain to this very obscure subject" [GG5: 395].

Even from this brief summary it can be seen that Galileo has offered at best a sketch of an argument, without pretending to offer an apodictic proof. There is no mention of necessary demonstrations based on sense experience in the *Discourse*, nor is there any claim that the earth's motion is the only possible way of explaining the ebb and flow of the tides. Again, the precision of mathematical proof is absent: although a few references are made to the quantitative modalities of the tides' motions, none of these is accounted for with any cogency. To deal with efficient causes is a quite different matter from dealing with formal causes, and Galileo, being a mathematician more than a physicist at this stage, shows himself less expert with the former than with the latter. He is able to distinguish between primary and secondary causes, but his use of secondary causes at times approaches the *ad hoc*, and surely he was aware of this limitation in his argument. In any event, despite the use of effect-to-cause argument and the apparent return to a cause-to-effect proof, the elements of the demonstrative *regressus* are lacking. As worked out in 1616, the argument from the tides may have some plausibility, but it can hardly be regarded as a proof for the earth's motion and thus as substantiating the primary supposition that lies behind the Copernican theory.

For purposes of comparison, the tidal "proof" as formulated in the letter to Cardinal Orsini may now be summarized in a form similar to that of previous argumentations discussed in this chapter. In view of the fact that the *topoi* of cause-effect and antecedent-consequent are both invoked in the proposed proof, elements from the demonstrative *regressus* may be combined with those from the dialectical paradigm to yield the following reasoning:

Possible cause: *from an effect to one or more hypothetical causes that might be sufficient to produce it.*

Effect	**Possible Cause**
the ebb and flow of the tides in various oceans and seas on the earth's surface	is produced primarily by a two-fold motion of the earth, secondarily by auxiliary factors

Dialectical inquiry: *use of probable reasoning and correlations to specify in detail the causal factors that produce the effect.*

The motion of a container can explain the motion of water within it; the diurnal and annual motions of the earth produce unequal motions at different parts of the earth's surface; the oscillations set up in bodies of water by these unequal motions vary in period depending on the lengths and depths of the sea basins. These unequal motions also are of two types or have two components, one vertical, seen mainly at the extremity of the basins, the other horizontal, seen mainly at their middle; in very large seas differential factors further operate to produce more movement in some parts than in others.

Tidal periods of twelve hours are produced by the primary cause; those of six, four, three, and two hours are produced additionally by various combinations of secondary causes.

The motion of the moon is a fictitious cause that has nothing to do with tidal motions [GG5: 381–393].

Probable cause: *from one or more causes now regarded as probable to the actual production of the effect.*

Probable Cause	**Effect**
the twofold motion of earth, acting on bodies of water of different shapes and sizes	produces an ebb and flow of tides at characteristic periods in the respective basins

A few observations suggest themselves from this summary. The first relates to the tone of the argument's presentation. It preserves a form of address and a dignity appropriate to the cardinal to whom it is addressed, and thus is without the polemics seen in Galileo's disputes with the Jesuits and later in the *Dialogue*. The intention throughout is to establish some type of connection between the tides and the earth's motion, obviously not a necessary connection, but one that might elicit assent from a sympathetic hearer rather than from an adversary. Again, though the subject appears to be the earth and its motion, Galileo is essentially concerned with two other elements, water and air, to investigate how motions can occur across the interface of all three. This requires him to set aside his skills as an astronomer and move into the domains of geography and oceanography. These were not completely outside his competence, since at Pisa mathematics professors, and especially his predecessor Filippo Fantoni, covered these matters in their course work.[21] Usually they were prone to invoke mysterious "influences" from the heavens to explain natural phenomena, a procedure Galileo regarded with some skepticism [cf. GG1: 159], which may explain his dismissal of Kepler's favored explanation, the moon's influence, in one sentence [GG5: 389.14]. In any event, it is clear that these disciplines were in a rudimentary state at the time, particularly from the point of view of quantitative analysis, and thus there was little Galileo could draw on to establish even possible connections, let alone those that might be probable or provide the basis for an apodictic argument. The relation of the argument to the *Posterior Analytics* and to the materials covered in MS 27 is therefore tenuous at best, and one should not be surprised if their canons find little application in Galileo's account.[22]

b. *Discourse on Comets.* Much the same may be said about the arguments Galileo was to develop in his lengthy controversy with the Jesuit Orazio Grassi over the nature and movement of comets, except that in this case the tone is markedly different, being characterized as much by polemic and vituperation as by logical reasoning. As in the debate with Scheiner, the format of the presentation on Galileo's side is disputational: the subject now is comets, not sunspots, but both parties are in a state of relative ignorance about them. Scientific claims according to the norms of the *Posterior Analytics* are thus effectively ruled out at the outset. Under such circumstances the only logical tool available is dialectics, for it alone supplies the *topoi* whereby one can argue on either side of a question or a problem when the subject seems incapable of definition and the demonstration consequent on it.

Galileo's arguments relating to comets are contained in a *Discourse on the Comets* published at Florence in 1619, ostensibly authored by one of Galileo's disciples, Mario Guiducci, but composed mostly by Galileo. Only a few statements in the *Discourse* bear on the contents of MS 27. It does contain, however, an important critique of Aristotle's account of comets in the *Meteorology*, stating that it is full of suppositions that, if not completely false, at least stand much in need of proof [GG6: 53]. This accords with Galileo's teaching in F2.4 and F3.4, where he allows that not all principles of a science need be self-evident but that some can be established by prior proof. One such proof relates to the location of comets, supposed by Aristotle to be under the orb of the moon; Galileo-Guiducci here point out that "astronomers have conclusively demonstrated them to be far above the moon" [GG6: 64]. The demonstration they invoke is that from parallax, again provided by geometrical optics and well known to Jesuit astronomers.[23]

c. *Reply to Ingoli.* A final document that merits consideration here is a letter Galileo wrote in 1624 to Francesco Ingoli, in reply to a detailed refutation of the Copernican system Ingoli had given him in Rome eight years earlier, when that subject was being agitated there [GG6: 509]. The letter is noteworthy because it was written at a time when Galileo had resumed work on his "system of the world," probably having been encouraged to do so as a result of his conversations with Urban VIII. In tone the reply is polite and well reasoned, lacking the acrimony seen in the interchanges with Grassi. What is important about it is that it shows Galileo reviewing the logic of the arguments in support of the Ptolemaic system he himself had recorded in the Treatise on the Heavens of MS 46. A key defect, he now notes, is that the suppositions on which many are based have simply been taken for granted without being subject to critical examination. Thus they inadvertently involve a *petitio principii*. Once this is removed they no longer constitute an obstacle to the acceptance of the Copernican system.

The Ptolemaic objection Galileo examines with great thoroughness and with the aid of a diagram is that an observer on earth always sees exactly half of the spherical heavens, which would seem to provide clear geometrical proof that he himself is located precisely at their center. In one sense, Galileo notes, Ptolemy's argument not only has "some semblance of truth" but is "even conclusive from the viewpoint of the entire Ptolemaic system" [GG6: 526]. It concludes necessarily, however, only so long as one supposes that the earth is motionless and that the rising and setting of the stars derives from the turning of the stellar sphere. If these suppositions are set aside and one supposes that the earth is displaced from the center and moves around it, exactly the same appearances will present themselves. Thus it is not because half the sky is seen that one infers the earth to be immobile at the center, but rather because its centrality is assumed that one deduces that half the sky is seen [GG6: 527–528]. The clear implication is that *all* suppositions have to be examined critically, not merely those behind the Copernican system, if one is to arrive at the true system of the world.

Similar paralogisms are involved, Galileo continues, in physical arguments asserting that the heavier and denser simple bodies occupy the lower places in the universe. If one equates "lower" with being "closer to the center," then this very terminology prejudices the case from the outset. Here Galileo remarks that in doing so one "sins either in the form or in the matter of the argument" [GG6: 536], an implicit reference to the difference between the norms for resolution given in the *Prior Analytics* and those in the *Posterior Analytics* [Sec. 2.7a]. The defect in form comes from the ambiguity of the term "center," for it can mean either "center of the earth" or "center of the universe." If the second is confused with the first, one ends up with four terms in one's syllogism, a paralogism deriving from a defect in form. The defect in matter, on the other hand, derives from a failure to grasp the nature of gravity and the type(s) of inclination associated with it. One might demonstrate that the element earth is closest to the center of the terrestrial globe, but evidence now reveals that there are other centers in the universe, for example, that of Jupiter, and thus such a demonstration does not establish that the earth's center is the center of the universe. In fact two inclinations might be involved: "one would be that their parts have gravity, namely an inclination toward the center of their globe; the other would be an inclination of a whole globe toward the center of the universe" [GG6: 537]. It is remarkable here that as early as 1589, thirty-five years before replying to Ingoli, when considering the problem of the unity of the universe Galileo had already mentioned the possibility of multiple centers of gravitational inclination [GG1: 29]. But when one reads through his Treatise on the Heavens written at that time, and then compares it with his reply to Ingoli, one sees graphically how his logic has been sharpened in the intervening years, enabling him now to recognize *una petizione di principio* [GG6: 531] that had escaped him so many years before.

6. UNITING THE HEAVENS AND THE EARTH IN ONE SCIENCE

With this we come to Galileo's final attempt to urge the truth of the Copernican system, his monumental *Dialogue on the Two Chief World System*, which he had essentially completed by 1630 but which was not published until 1632. The work, one of the most famous in intellectual history, has been subjected to analyses too numerous to mention, let alone consider, in this study. For our purposes it represents Galileo's crowning achievement in astronomy, the furthest he would be able to go in uniting the heavens and the earth within a single *scientia* that might conform to the norms of the *Posterior Analytics*. Yet he was not successful in this attempt, nor did he ever claim to have succeeded in it. The objective of the *Dialogue* was more modest, namely, to offer the most convincing argument possible to support the earth's motion. To have gone to the point of even sketching a science of celestial mechanics Galileo would have had to anticipate his *Two New Sciences* and much more besides – achievements beyond his or anyone's grasp at that point in time.

In appraising the *Dialogue* we are fortunate in having at hand Maurice

Finocchiaro's *Galileo and the Art of Reasoning*, a book-length study wherein he analyzes in minute detail its logical argumentation.[24] The reasoning exemplified in the *Dialogue*, according to Finocchiaro, is dialectics *par excellence*. His analysis, coupled with Moss's analysis of Galileo's rhetoric, shows why one should expect the *Topics* and the *Rhetoric* to be more useful for comprehending what Galileo was doing in the *Dialogue* than either of the *Analytics*. In the main we therefore draw upon Finocchiaro's findings in this section, which deals with the first three days of the dialogue, and also in the next, which concentrates on the fourth day.[25]

Each one of the days has a theme and in fact attempts to establish a single conclusion. Over and above these, however, one might wonder if the *Dialogue* as a whole argues toward a unitary conclusion. Finocchiaro finds that it does, and this agrees with our appraisal also. Taking the results of his work, but presenting them in a form more homogeneous with the illustrations of Galileo's reasoning provided thus far, we would summarize the thesis of the *Dialogue* as follows:

> **Major premise:** Either the earth moves or it does not.
>
> **Argumentation:** The first alternative is preferable on two grounds: negatively, because all arguments in favor of its being at rest can be answered; positively, because, higher doctrine aside, there are sound rational arguments in favor of its being in motion.
>
> **Conclusion:** Therefore, based on rational argument alone, it is most probable that the earth moves.[26]

Note that the major premise, though not explicitly stated, sets up a dichotomy similar to those Galileo has used before; in this case it functions as a basic supposition underlying the argument. The latter is not a demonstration, nor *a fortiori* does it employ the demonstrative regress. It is precluded from being that because of the *proviso* Galileo makes in its preface, and this is confirmed in the letters authorizing the publication of the volume. The "higher doctrine," of course, is the teaching of Scripture as it was then interpreted by theologians. The detailed elaboration of the argument is worked out dialectically in four stages, each the subject of a day's discussion. The first focuses on the unity of the world, the second on the earth's daily rotation on its axis, the third on its annual revolution around the sun, and the fourth on a causal argument that supports both the earth's rotation and its revolution, namely, the argument from the tides.

a. *The Unity of the World.* The discussions of the first day are but a preparation for the book's thesis as a whole, for they provide a background against which the prevailing arguments for the earth's rest and the sun's motion can be critically evaluated. At a deeper level they aim to revise the understandings of nature and of natural motion as these appear in Aristotle's *De caelo* and even to correct them in light of Aristotelian teaching in the *Physics*; thus, seeing that the *Physics* is itself a work on natural philosophy, they are more philosophical than are other parts of the *Dialogue*. Their main thrust is toward the unity of the world, showing that bodies move naturally in both the heavens and on earth and do not require a radical distinction between curvilinear and rectilinear motion to do so, that the heavens are alterable just as is the earth and so there is no substantial difference between the celestial and the terrestrial, and finally, that the earth and the moon share a common nature. The methodological statements made in the course of the day are not numerous and they are all intelligible in light of the purpose Galileo has in mind.

Although "necessary demonstrations" are mentioned in the discussions, as they are in Galileo's previous writings, none figures prominently in the conclusion he wishes to establish. His chain of reasoning, based on the analysis in Finocchiaro,[27] may instead be summarized as follows:

Thesis: The observable universe has a natural unity.

Argumentation: This is so because (1) natural motions are the same in all bodies, and bodies move naturally in the heavens and on the earth; (2) Aristotle's classification of motion into circular and straight, up and down, is no longer tenable; (3) the absence of observable change in the heavens can no longer be maintained, for alterations are now discernible in the heavens; and (4) close examinations show that the moon and the earth are no different in nature.

Conclusion: Therefore, it appears that the universe is one.

The overall line of argument is not physico-mathematical but is philosophical in kind. Yet it has considerable persuasive force, incorporating as it does materials from Galileo's treatise on local motion later to appear in his *Two New Sciences*, and so can lay the groundwork for the more detailed reasoning to follow.

By way of more specific comment, it may be noted that Aristotle had invoked mathematical reasoning to establish his preliminary distinctions. Galileo does not wish to exclude such reasoning from the study of nature, since so much of his own argumentation depends on it, and so he simply makes the point that arguments such as "three is a perfect number" should be left to the rhetoricians and that proofs should be established with necessary demonstrations [GG7: 35]. He does not deny that Aristotle had offered such demonstrations, but only wishes to improve on them by explaining why bodies do not have, and cannot have, more than three dimensions [GG7: 36]. He readily concedes that the necessity of mathematical demonstration is not always to be sought in physical matters, but admits that he is not unwilling to use it if such demonstration is at hand [GG7: 38].

The alterability of the heavens enables Galileo to stress the importance of sense evidence in scientific reasoning, for he argues that had Aristotle seen the new effects and observations he would have altered his view that the heavens are unalterable [GG7: 75]. It is here that he first differentiates Aristotle's order of teaching from his order of invention, and then enters into his much-cited discussion of "resolutive method" [GG7: 75], analyzed in Sec. 2.7 above. He also argues convincingly that *a posteriori* argument ought to precede *a priori* argument in studies of the heavens [GG7: 76], precisely the procedure one would expect from someone acquainted with the demonstrative regress. Earlier in this same discussion Galileo shows his awareness of the difference between *logica docens* and *logica utens*, crediting Aristotle with an expert knowledge of the first but criticizing his lack of the second, and noting that mathematical demonstrations are to be found in the books of mathematicians, not in those of logicians [GG7: 60].

The comparison of the earth with the moon further enables Galileo to show how experiments and the construction of models can aid in establishing conclusions about natural phenomena. The experiments he describes relate to the ways one can show how light is reflected from one surface to another, and how these are relevant to understanding the illumination of the moon and its surface [GG7: 96]. He further explains how the moon's appearances can be duplicated by constructing a model with prominences and cavities on it. When properly illuminated this will show the same views and changes as are seen on its surface [GG7: 111–112], reviewing those already explained in Sec. 5.2a.

But, Galileo concludes, man is still limited in his knowledge of the moon; only in pure mathematics can he hope to attain the objective certainty found in divine science [GG7: 127–129; cf. Sec. 3.2]. The comparison of human and divine knowledge Galileo offers here posed difficulties for him with the Inquisition, but as Moss has pointed out, this was as much the fault of his rhetoric as it was of his science.[28] In order to give maximum persuasive force to his arguments he sensed the need to exalt the power of the human mind; many of his statements reflect this theme, for without it his arguments would ultimately be ineffectual. At the same time, to carry out the promise of his preface and acquiesce with the mind of Pope Urban VIII, he had to stress the limitations of the human mind; others of his statements support this counter-theme. Depending on the context, he would favor the one or the other, whichever he regarded as the more persuasive in the given circumstance.

The dialogue form, to be sure, gave Galileo the freedom he required to work with ambiguities such as these. But under the guise of giving equal hearing to both sides, through a skillful use of dialectics and rhetoric he was able to convince most readers of the efficacy of his arguments using the preferred theme. He employed many *topoi* in doing so, too many to be canvassed here. But his favorite was the extrinsic topic, the argument from authority [Sec. 3.6]. By every device at his command he discredited the authority of Aristotle and his followers; in its place he amplified his own, stressing the superior knowledge, methods, and virtues of the "academician" in solving the many problems attendant on understanding the earth's motion.

b. *Earth's Daily Rotation.* The burden of the second day's discussion is examining the arguments on the Copernican side favoring the earth's diurnal motion, that is, a daily revolution on its axis. To do this Galileo has to invoke experiments to support the position, but he also has to be wary of sense knowledge, since there is little sensory evidence of the earth's rotation. Thus he has to advance the claims of reason over those of the senses. Galileo notes that the argument from simplicity is favorable to Copernicus, but one cannot regard this as a necessary demonstration, since it confers only a greater probability on the conclusion [GG7: 144].

The better path is to examine all of the known objections to the earth's rotation to show how they are completely lacking in force and so cannot be used to reject the Copernican thesis.

Carrying out this plan, as Finocchiaro construes it,[29] the basic argument of the second day can be put in the following format:

> **Major premise:** Either the earth has a daily motion of rotation on its own axis or the entire cosmos rotates around the earth.
>
> **Argumentation:** The problem posed by the earth's rotation is that: (1) it goes counter to Aristotle's authority; (2) the arguments favoring it are indirect but only probable, since they derive from the difficulties besetting the contrary view; and (3) the objections to it are apparently insuperable.
>
> The best resolution of this problem is that: (1) Aristotle's text on this matter involves equivocation; (2) all the phenomena alleged as counter-evidence can be saved whether the earth is rotating or not; and (3) none of the proferred objections has compelling force once the nature of motion is understood.
>
> **Conclusion:** Therefore, it is preferable to hold that the earth rotates on its axis.

Again note that the major premise involves a basic supposition, stated in the form of a dichotomy, to which assent would readily be given in Galileo's day.[30] The dichotomy is not given explicit formulation in Galileo's text and it is not apparent at the outset whether the Copernican thesis is being juxtaposed to the Aristotelian, the Ptolemaic, or the Tychonian alternatives. As the argument develops, however, it can be seen that Aristotle is the main target and that it is his dynamical concepts that are being called into question. In other words, what is being argued is not a type of mathematical astronomy where systems that are kinematically equivalent, to use the modern expression, are being compared. Rather, what is at issue is the physics of motion, and of bodies on the earth's surface at that, as this is relevant to deciding the problem of the earth's rotation.

Basic to Galileo's rejection of Aristotle's physics is the latter's contention that only two types of motion are possible, natural and violent. Since a rotary motion could not be natural for the earth, its motion would have to be violent, and so could not be perpetual [GG7: 159]. Along with this, Aristotle did not have a proper understanding of projectile motion, and thus was incapable of seeing how a previously impressed impetus can explain the continued motion of objects [GG7: 173]. In this context Galileo brings up the experiment of a stone dropped from the top of a ship's mast [GG7: 179], arguing from this analogy that tests designed to prove the earth's rotation yield the same result whether the earth is moving or at rest. After discussing a similar experiment, that of a crossbow fired from a moving carriage [GG7: 194–197], Galileo points out the fallacy of supposing as true the precise matter being investigated, along lines already sketched in his reply to Ingoli [Sec. 5.4c]. The same result will come, he concludes, from all experiments attempting to disprove the earth's rotation. Those who understand that the earth communicates its own motion to all objects on its surface will have no difficulty interpreting the experiments' results [GG7: 209].

For the method to be used in discovering physical principles that are different from Aristotle's, Galileo stresses the over-riding importance of geometrical reasoning [GG7: 244,299]. The main difficulty comes in applying abstract mathematical knowledge to physical reality, and this has to be done conditionally (*condizionatamente*) – another way of describing reasoning *ex suppositione* [GG7: 233]. An important technique for achieving this result is to "deduct the impediments of matter" [GG7: 234]; this involves a special type of suppositional reasoning in which Galileo pioneered (Type 8 of those listed in Sec. 4.2c above), to be explained in the following chapter. As will be seen there the resulting arguments conclude necessarily and are truly demonstrative, though in themselves they are not sufficient to demonstrate the earth's motion.

The two principal objections against the earth's rotation have to do with the lack of a cause to explain it and with the inadequacy of sense experience to justify it. Galileo's attack on the first is to acknowledge that only two types of cause can be invoked here, either an internal or an external principle, and that he does not know which is involved but that it is probably the same as the motive power that moves Mars, Jupiter, and the heavenly spheres [GG7: 260]. This leads him to make skeptical remarks about the difficulty of knowing the essence of any motive force, including gravity, thus employing the "limitations of the human mind" theme to evade the objection. The second objection is handled by the counter-theme: our senses deceive us when we attempt to judge whether a motion is truly straight or only apparently so, but the human mind can transcend these limitations of sense. That is why the Copernican view requires one to deny one's own senses, for reason alone, and particularly mathematical reason, is powerful enough to adjudicate between the apparent and what is truly the case [GG7: 279–281].

c. *Earth's Annual Revolution.* With this Galileo moves on to the third day of his dialogue, tackling the more debatable feature of the Copernican system, the earth's yearly revolution around the sun. Here again his basic strategy is to set up a dichotomy and to show, by argument, that the evidence supporting the earth's movement is more probable than that supporting the sun's. The outlines of his argument, again based on the detailed analysis provided by Finocchiaro,[31] may be shown as follows:

> **Major premise:** Either the earth makes a yearly revolution around the sun or the sun makes a yearly revolution around the earth.
>
> **Argumentation:** The evidences for the earth's revolution around the sun, though indirect, are very cogent, namely: (1) the heliocentrism of planetary motions generally; (2) the retrograde motion of the planets in particular; and (3) the motion of sunspots.
>
> The arguments against the earth's revolution and in favor of the sun's motion, though not easily dismissed, are inconclusive, namely: (a) the biblical passages citing the sun's movement; (b) the absence of parallax and the lack of change in stellar dimensions and other celestial appearances; (c) the sun's apparent motion; and (d) the inconceivability of a body like the earth having several natural motions.
>
> **Conclusion:** Therefore, it is preferable to hold that the earth makes a yearly revolution around the sun.

The strongest objection Galileo must face in arguing for the earth's motion around the sun is the absence of parallax, an evidence that the Greeks, including Aristotle, had sought but could not find, and so they concluded to the earth's immobility. His tactic is to meet this difficulty head-on at the beginning of the third day's discussion by renouncing the method he has extolled up to this point, namely, the use of quantitative measurements. Such measurements are an unreliable guide in astronomy, he now states, for even the smallest error made by an observer with an instrument will change a location "from finite and possible to infinite and impossible" [GG7: 317]. Thus he is forced to relinquish the means he would ordinarily depend upon. But the telescope, fortunately, has furnished a different type of evidence, not hitherto available, that can supplement the insight Copernicus used to see the truth of heliocentrism. Observations of the planets, which are "most evident and offer conclusive proof," exclude the earth from the center of the universe and put the sun there in its place [GG7: 349].

Among these evidences, the most certain attest that Venus and Mercury revolve around the sun, particularly the changes in Venus's shape, which "concludes necessarily" [GG7: 350]. The anomaly of the moon going around the earth every month and the earth circling the sun every year is also removed when one sees Jupiter making a similar twelve-year orbit accompanied by its four moons [GG7: 367–368]. Not only do the planets move around the sun, therefore, but it is "very probable and perhaps necessary to concede" that the earth circles it also [GG7: 368]. Confirmatory evidence comes from the ease and simplicity with which the earth's annual revolution can remove the anomalies in the movements of the five planets – their complicated progressions, stations, and retrogressions – reducing them all to equable and regular motions [GG7: 372]. All of this adds up to the conviction that, in the final analysis, "the illnesses are in Ptolemy, the cure for them in Copernicus" [GG7: 369].

The same appeal to simplicity underlies Galileo introduction at this point of the sunspot argument. He now claims that the changes in the paths the spots trace across the face of the sun are more readily explained from the positions from which they are viewed on the moving earth than from some contrived motion attributed to the sun. Combined with other divergent phenomena, especially the motions of the planets, they "easily and clearly reveal the true cause (*la vera cagione*)" of their appearance, namely, the annual revolution of the earth around the sun [GG7: 383].

7. THE REALITY OF THE EARTH'S MOTION

Despite occasional mentions of certitude, necessity, and true causes in the discussions of the first three days of the *Dialogue*, it should be obvious from the above that up to this point Galileo has not demonstrated the reality of the earth's motion. The demonstrations introduced periodically into the discussions are those analyzed in the first three sections of this chapter, all of which employ the demonstrative regress and pertain to the middle sciences, being physico-mathematical in character. The causality on which they are based is formal causality, that of a quantitative form, which enables one, using mainly projective geometry, to eliminate alternative explanations and show that there is a unique cause of the celestial appearance they investigate. This works for the presence of mountains on the moon, for the rotation of satellites around Jupiter, and Venus's rotation around the sun. It does not work, however, for the earth's motion, either in diurnal rotation or annual revolution. Galileo's strategy in both these cases is to suppose two possible explanations for various effects, explanations that are seen as dichotomous, and to argue for the greater probability of the one compared to the basic implausibility of the other. The *topoi* most useful here are similar-dissimilar, coupled with antecedent-consequent, which offer powerful support to analogy arguments based on Jupiter as a model "solar system" and to proportionality arguments based on periods and radii of revolution around a center, as suggested in Sec. 3.7. Yet these are all dialectical, and the best he can conclude from this strategy is that it is more probable that the earth moves, not that it actually does so.

A key difficulty of which Galileo was undoubtedly aware was that all of his "necessary demonstrations based on sense experience" could be readily integrated into the Tychonian system, wherein the earth was still regarded as immobile at the center of the universe. In that system not only the moon but also the sun rotated around the earth, with the sun being circled by the five planets during its rotation. Jupiter would in turn be accompanied by its satellites, and the sun itself could be assigned rotations and librations to account for the movement of sunspots across its surface.[32] Thus one might be certain of mountains on the moon, the satellites of Jupiter, the movement of Venus and Mercury around the sun, and even the motion of sunspots, without being committed by these to the earth's motion. The basic reason for this is that demonstrations based on projective geometry yield certitude in the instances mentioned but are not judicative in cases where relative motion is involved. Projections of light rays remain the same whether one assumes that the earth is at rest, or that the sun is at rest, or that either or both move with respect to the other. These alternatives still remain suppositions that may be true or false independently of the uniformity of the results deducible from them.

Realization of this state of affairs probably explains why Galileo embarked on the dangerous course of running afoul of the Inquisition by adding a fourth day in which he would propose anew his argument from the tides. This would be the "ingenious fancy" (*fantasia ingegnosa*) he had mentioned in his preface [GG7: 30], a topic the censor of the *Dialogue*, on the pope's instruction, had told him to avoid [GG19: 327]. The reason for the Church's stricture was obvious: to argue from the tides would be to bring the treatise out of the mathematical realm of saving the appearances and into the physical sphere of the causes and effects of natural motions. Galileo was by now aware that his demonstrations of the first two days had simply countered the obvious objections against the earth's motion without giving convincing evidence in its support, and further, that those of the third day had shown the untenability of the Ptolemaic hypothesis but had given no proof for the Copernican system vis-a-vis that of Tycho Brahe. Since Catholic astronomers who acquiesced to the decree of 1616, and particularly the Jesuits, were using Galileo's own telescopic evidence as support for the Tychonian system, he probably felt that his own work would come to naught if this alternative were not eliminated. Physical proof of the earth's rotation would, of course, settle the issue in favor of Copernicus, and so he decided to risk it. He surely knew that the tidal argument he developed, after sixteen years of work, is inconclusive, for it was so regarded in his day, just as it is in our own. But he still counted on its persuasive force, its being *persuasibile*, as he explicitly states in the preface [GG7: 30.22].[33] The discussions of the fourth day are thus important methodologically, for they show Galileo using the *topoi* of cause-effect, antecedent-consequent, and similar-dissimilar to induce assent to a conclusion that those of his readers who were well disposed might regard as scientific.

a. *The Causal Argument.* Having rejected earlier explanations of the tides as unsatisfactory, Galileo observes that in natural questions of this type it is a knowledge of effects that leads to "the investigation and discovery of causes" [GG7: 443] – a hint at the procedure of the demonstrative *regressus*. It is therefore necessary to have a full knowledge of the effects, for if among those one is able to discover those that are "principal," from these it will be possible to discover the "true and primary causes" [GG7: 443–444]. Furthermore, although an identification of all the "proper and sufficient" causes of these effects may not be possible, if one carefully studies "effects that are similar in kind" one will be led by this ultimately to "a single true and primary cause" [GG7: 444]. Galileo's inference here is supported by his favorite causal maxim, namely, there can be only one true and primary cause for any one effect. Under its guidance he embarks again on the program he had outlined in his letter to Cardinal Orsini of 1616 [Sec. 5.4a], only this time fleshing out more of the details.

The arguments are quite complex and so they are merely schematized here, following as heretofore Finocchiaro's analysis.[34] In view of Galileo's hint at the methodology of the demonstrative regress, we again combine elements of the demonstrative paradigm with those of the dialectical paradigm as in our previous analysis of the letter of 1616:

Possible cause: *from various effects to one or more hypothetical causes that might be sufficient to produce it.*

Effects	**Possible Cause**
the ebb and flow of the tides in various oceans and seas on the earth's surface	produced primarily by a twofold motion of the earth, secondarily by the fluid properties of water

Dialectical inquiry: *use of probable reasoning and correlations to specify in detail the causal factors that produce the effects.*

Previous theories about the cause of the tides must be rejected: differences in sea depth alone cannot produce and sustain tidal motion; lunar attraction would not produce tides in some parts of a sea and not in others; the water involved is not heated by the moon; miracle explanations are only a last resort; and undersea sources are inadequate as causes [GG7: 442–448].

The primary cause of the tides is the daily accelerations and retardations produced in every part of the earth as the diurnal component is added or subtracted from the annual component of the earth's motion; this is so because water in a container can be made to move like the tides by accelerating and retarding the container [GG7: 449–453].

The secondary cause is the fluid properties of water: water tends to oscillate before reaching equilibrium; its oscillations take less time the shorter the length of the basin and the greater its depth; it moves vertically at the extremities and horizontally at the middle of the basin; and different parts of the same body of water can move at different speeds simultaneously [GG7: 454–456].

The basic tidal effects that can be explained as resulting from the interaction of the primary and secondary causes are: the absence of tides in lakes and small seas; the six-hour tidal interval in the

Mediterranean and presumably the different periods in other seas; the absence of tides in seas that are narrow in an east-west direction; the fact that tides are greatest at the extremities and least at the middle of a gulf; the great currents through certain straits; the violent agitations and vortices in certain straits; and the unidirectional flow of currents through certain straits [GG7: 457–461].

The behavior of winds provides no evidence against the earth's motion because air unlike water does not retain an acquired motion; and because the turning of the lunar orb could not produce either the prevailing westward winds that do exist or the back and forth motion of the tides [GG7: 462–469].

Probable cause: *from one or more causes now regarded as probable to the actual production of various effects, as follows*:

Probable Cause	**Effects**
variations in speed of the earth's diurnal and annual motions or both	produce the diurnal period of the tides
monthly variations in the speed of the earth's annual motion caused by changing relative positions of earth, sun, and moon	produce monthly variations in the diurnal period
annual variations in effective speed resulting from the inclination of the earth's axis to the plane of its orbit	produce annual variations in the diurnal period
variations in the dimensions of the containing basin	produce particular periods in particular basins [GG7:470-487].

As can be seen on even cursory examination, the overall argument as outlined above is basically the same as that offered in the *Discourse on the Tides*. The elaboration is mainly in the provision of quantitative details for explaining monthly and annual variations in the tides that were ignored in the *Discourse*. Some of these details, glossed over in the *Dialogue*, unfortunately run afoul of the maxims for antecedent-consequent reasoning and so prejudice Galileo's case rather than strengthen it. Before coming to them, however, we should note an objection to the procedure that *is* recognized there explicitly and proves relevant to our study. This is that the reasoning throughout is hypothetical (*ex suppositione*) and so is entirely dependent on what has been assumed, namely, the two motions attributed to the earth [GG7: 462]. Not only is the assumption gratuitous, the objection continues, but it can be used to deduce consequences that are contrary to fact, such as motions of the air on the earth's surface analogous to those of the water, which should be perceived as winds [GG7: 462–463]. This is Galileo's only explicit mention of reasoning *ex suppositione* in the *Dialogue*, although he had hinted at this procedure during the second day's discussions when treating of the application of mathematics to the study of nature [Sec. 5.5b]. There he showed how it could be used to generate certain proof, whereas in this context the expression is used by an objector to his teaching and so has the sense Bellarmine employed against the Copernican hypothesis generally. Rather than resort to his usual reply, namely, that some suppositions are true and others false and that his are the former, Galileo instead attacks the validity of the inference from antecedent to consequent, showing how winds and like phenomena would not actually result from the primary and secondary causes he has proposed [GG7: 463–470].

Another point that is noteworthy is Galileo's use of a model or analogue to analyze the causality behind the tidal motions. In the *Discourse* he had begun with the model of a barge carrying water and then emended this with the epicyclic model of the rotating earth making an annual revolution around the sun. About this he then wrote:

> Though many will consider it impossible that we could experiment with the effects of such an arrangement by means of machines and artificial vessels, nevertheless it is not entirely impossible; I have under construction a machine, which I shall explain at the proper time, and in which one can observe in detail the effects of these amazing combinations of motions [GG5: 386].

Despite this promise of supplying fuller details later, William R. Shea has called attention to the similar passage in the *Dialogue* where Galileo claims to have constructed such a model but gives no particulars about it whatever, leaving it to the reader's imagination to verify how "its amazing combinations of motions can produce" the desired effects [GG7: 456].[35] In Shea's view, one may reasonably doubt the veracity of Galileo's claim for his model, particularly in view of the fact that several of his predictions do not agree with what is observed in tidal phenomena.[36]

b. *The Pope's Alternative.* Apart from difficulties of this type, many of them associated with Galileo's quantitative emendations to his earlier tidal proposal, mention should finally be made of a key objection, well known to Galileo, but whose handling of it had drastic consequences for his own person. The objection probably dates back to Galileo's discussions with Pope Urban VIII in 1624, when Galileo proposed the tidal demonstration to him and the pope countered with a nominalist argument based on the limitations of the human mind. Immediately following his exposition of the tides, in an all too brief conclusion to the fourth day's discussions, Galileo puts the pope's alternative in the mouth of Simplicio. The latter admits that he does not understand the technicalities involved, but from what he does understand, and even considering how ingenious (*ingegnosa*) the explanation may be, he does not regard it as true and conclusive. "Indeed," he goes on,

> I always keep before my mind's eye a very firm doctrine, which I once learned from a man of great knowledge and eminence, and before which one must give pause. From it I know what you would answer if both of you [i.e., Sagredo and Salviati] are asked whether God with his infinite power and wisdom could give to the element water the back and forth motion we see in it by some means other than by moving the containing basin; I say you will answer that he would have the power and the knowledge to do this in many ways, some of them even inconceivable by our intellect. Thus, I immediately conclude that in view of this it would be excessively bold if someone should want to limit and compel divine power and wisdom to a particular fancy of his own (*una sua fantasia particolare*) [GG7: 488].

There is no doubt that the "man of great knowledge and eminence" mentioned here was Urban VIII himself, and the description of the *fantasia* as being *ingegnosa* suggests that even Galileo's use of the label *fantasia ingegnosa* in his preface reflects an evaluation the pope earlier had made of his argument from the tides. In effect this rejection on the basis of divine power and knowledge is the "last word" in the dialogue, but its very brevity and its coming from the mouth of Simplicio dilutes any force it might have had, particularly in light of the disproportionately lengthy arguments Galileo had been developing on the opposite side. In fact, the placement and brevity of his favorite argument so infuriated the pope that he quickly initiated proceedings against Galileo, the outcome of which would be the celebrated trial and condemnation of 1633.

The concluding paragraphs of the *Dialogue* create the impression that Galileo himself agrees with Urban's statement about the human mind's limitations, but the persuasive force of what has preceded surely negates that impression. Galileo did not fool the pope, nor did it fool those who read the *Dialogue* to evaluate it for the Inquisition; that is why the last few paragraphs came to be called "the medicine of the end" [GG19: 326]. And yet Galileo had good reason not to agree with the pope on this matter, for it imposed such a severe limitation on human reason that it made *any* science of nature virtually impossible, and surely a science of astronomy. What Urban was ruling out was the demonstrative *regressus* itself, on the ground that the first progression can never be made because one cannot reason *a posteriori* from effect to cause. No matter what cause one might assign for a given effect, if the pope's view is adopted it always becomes possible to say that such an assignment limits God's knowledge and power, that God can produce the same effect by a cause completely beyond human comprehension. This reply bears comparison to present-day arguments invoking logical possibility to rule out any such thing as natural necessity.[37] Galileo rejected the Renaissance counterparts of such arguments in his questions D3.1 and D3.3 of MS 27. Before him Christopher Clavius had maintained that those who invoke the argument, "Doubtless another cause, at present unknown to us, can be found for those effects," destroy the very possibility of astronomy's being a science.[38] And after him Sir Isaac Newton voiced a similar sentiment to protect his *Mathematical Principles of Natural Philosophy*, stipulating rules that would not permit the imagining of hypotheses to nullify the celestial mechanics he had developed by reasoning from natural effects to their natural causes.[39]

That Galileo himself did not give up on the possibility of a natural science in spite of Urban VIII's strictures is clear from the penultimate sentence of the *Dialogue* itself. There Sagredo, after suggesting the possibility of yet further sessions to clear up problems earlier touched on, indicates that he is "looking forward with great eagerness to hear the elements of our Academician's [i.e., Galileo's] new science of local motion, natural and violent" [GG7: 489]. Sagredo's intuition was correct: Galileo's dialectics had brought him very close to a new science of astronomy, for many of the theorems Newton was to develop in his "System of the World" were already nascent in the discussions recorded in the *Dialogue*. What was not yet clear was how the principles implied in those discussions could be articulated. Galileo, of course, had a fairly good idea of how that was to be done; he needed only the time to put together his *Two New Sciences* to give the rest of the world the clues that would make the "new physics" a reality.[40]

NOTES

[1] Since this volume is in effect a sequel and further development of materials already presented in *Galileo and His Sources*, in this chapter and the next we now survey and summarize essentially the same texts as in the previous work. The difference between the two treatments is that in *Galileo and His Sources* we were intent on showing how Galileo's scientific writings were in basic continuity with Jesuit teachings at the Collegio Romano, whereas here we have the much less demanding task of showing internal consistency in Galileo's works themselves, and particularly how the *logica docens* of MS 27 manifests itself in the *logica utens* of his later treatises.

[2] The expression closest to this in Aristotle's text is *kuklos* (Lat. *circulus*), when he speaks of demonstration being circular in I.3, 72b25–73a6. Probably on this account Galileo refers to the *regressus* as a *circulus imperfectus* in D3.3.5.

[3] Identified as I.12 in Zabarella's division of the text.

[4] This coheres precisely with the sentiment of Christopher Clavius when he wrote, at the conclusion of his commentary on the *Sphere* of Sacrobosco:

> Finally, we may conclude our project as follows: just as in natural philosophy we arrive at knowledge of causes through their effects, so too in astronomy, which treats of heavenly bodies very far distant from us, we can only attain to knowledge of the bodies themselves, of how they are arranged and constituted, through study of their effects, that is, through their movements as perceived by us through our senses [Rome: 1581, 450].

[5] An analysis of this treatise in the context of the logical teaching contained in MS 27 will be found in *Galileo and His Sources*, 255–261.

[6] For a clear modern explanation of these phenomena, with diagrams, see Otto Struve, *Elementary Astronomy*, rev. ed., New York: Oxford University Press, 1959, 74–80. We cite this edition because it is the last to demonstrate such properties of the moon on the basis of how it appears from earth and prior to its close observation by satellite. From Struve's text one gains the impression that astronomers of his day regarded these arguments as apodictic and as providing certain knowledge of the causes of the moon's appearance long before satellite data became available.

[7] A denial that the demonstrative regress works in this simple case effectively rules out the possibility of astronomy ever being a science in Aristotle's apodictic sense. As noted in Chap. 1, McMullin rejects on principle Aristotle's teaching in *Posterior Analytics* I.13 [Sec. 1.2f] and so, along with that teaching, must deny the validity of the proof for the moon's phases here elaborated, as well as the validity of Galileo's proofs detailed in the next section. The simplest reply to such a rejection is the argument *ad hominem*. Is McMullin himself *certain* that the moon is a sphere, that it has mountains on its surface, that Jupiter has satellites, that Venus has phases, and so on? If not, then he is consistent with his critique of Aristotle, but he must hold as a consequence that planetary astronomy is not an apodictic science but only opinion, highly probable opinion, but opinion nonetheless. If he *is* certain of these conclusions, then his problem is that of identifying at what point in the history of thought, and by what reasoning process, he, or others before him, became convinced of their truth. Should he situate his discoveries prior to the age of satellite exploration (thus ruling out the radical empiricist alternative, that such conclusions were not reasoned to but

grasped directly in sense experience), he will have arrived at a demonstrative *regressus*, whether he recognizes it under that name or not. See the previous note.

[8] For the background to Galileo's discoveries see Albert van Helden's new annotated translation of Galileo's *Sidereus Nuncius, or The Sidereal Messenger* (Chicago and London: The University of Chicago Press, 1989, 1–24. We generally use Van Helden's analysis in what follows.

[9] This expression is sometimes heard in philosophy of science circles with the connotation that being "dead" it is trivial or unimportant, the really interesting science being that done at the frontiers of knowledge. This is the reverse of the Aristotelian view: "live science" is probably opinion and not science at all, whereas any topic on which scientists have "closed the book," as it were, probably constitutes valid science in the Aristotelian sense.

[10] Van Helden, *Sidereus Nuncius*, 9–12, 39–57.

[11] *Sidereus Nuncius*, 21–22.

[12] Following Van Helden's translation, 105–106.

[13] For the geometrical details, see Struve, *Elementary Astronomy*, 98–101.

[14] Again using Van Helden's translation, 111.

[15] See Stillman Drake, *Galileo: Pioneer Scientist*, Toronto: University of Toronto Press, 1990, 136.

[16] Drake, Galileo: *Pioneer Scientist*, 142.

[17] Called such because it was proposed by Tycho Brahe as a compromise between the Ptolemaic and the Copernican systems that could be reconciled more readily with Aristotelian and Scriptural teachings.

[18] For further methodological observations about the letters on sunspots, see *Galileo and His Sources*, 289–291.

[19] Cesalpino did so in his *Peripateticae quaestiones*, first published at Venice in 1571 and again in Geneva in 1588. According to Cesalpino the ebb and flow of the tides was caused not by the moon but by the movement of the earth; likewise he thought he could explain the motion of trepidation attributed by astronomers to the eighth sphere by a similar movement, seemingly one of slow oscillation. Some details relating to Cesalpino's teaching will be found in Helbing, *Buonamici*, 57, 200.

[20] This passage suggests that the argument occurred to Galileo on the basis of his experience with barges carrying fresh water to Venice from the mainland during his early years at Padua; see Stillman Drake, *Galileo at Work: His Scientific Biography*, Chicago: The University of Chicago Press, 1978, 37.

[21] For details on Fantoni, see Essay 10 in C.B. Schmitt, *Studies in Renaissance Philosophy and Science*, London: Variorum Reprints, 1981.

[22] Additional comments about the early discourse on tides and the context in which it was written will be found in *Galileo and His Sources*, 291–295.

[23] Again, further details are given in *Galileo and His Sources*, 295–298.

[24] Publication details are given in note 3 of Chap. 2 above; the work is cited hereafter as *Art of Reasoning*.

[25] Our own review of the *Dialogue* in the context of the terminology of MS 27 will be found in *Galileo and His Sources*, 299–311.

[26] Finocchiaro, *Art of Reasoning*, 29.

[27] *Art of Reasoning*, 33–35.

[28] See her "Galileo's Rhetorical Strategies in Defense of Copernicanism," 99–103.

[29] *Art of Reasoning*, 35–39.

[30] Such assent is mentioned mainly to eliminate from consideration arguments about the earth's motion deriving from Newtonian or relativistic mechanics that sometimes intrude themselves into discussions of Galileo's proofs. However valid or interesting these arguments may be, they are irrelevant for understanding the logic Galileo himself used and thus are not dwelt on here.

[31] *Art of Reasoning*, 39–42.

[32] The equivalence of the Tychonian and Copernican systems in this regard is explained by Keith Hutchinson, "Sunspots, Galileo, and the Orbit of the Earth," *Isis* 81 (1990), 68–74, replying to an earlier discussion by A. Mark Smith, "Galileo's Proof for the Earth's Motion from the Movement of Sunspots," *Isis* 76 (1985), 543-551. Smith's essay contains diagrams that are helpful for understanding the kinematical relationships involved.

[33] This term, it may be recalled, was used by Antonio Riccobono, professor of rhetoric at Padua and Galileo's friend there, to characterize the formal object of rhetoric and thus to differentiate it from dialectics; see Sec. 3.8.

[34] *Art of Reasoning*, 42–44.

[35] See his *Galileo's Intellectual Revolution: Middle Period, 1610–1632*, New York: Science History Publications, 1972, 177.

[36] More serious criticisms directed against the tidal argument include that of Ernst Mach, in effect one already lodged by Galileo's contemporaries, namely, that the centripetal acceleration deriving from the earth's rotation is constant over its entire surface and so cannot combine with a linear acceleration to cause a tidal variation. Another is Galileo's contemptuous attitude toward Kepler and the lunar explanation of the tides, causing him to deny the dependence of their half-monthly period on the moon and to omit entirely the monthly period whereby the tides occur later each day by the same time as the moon's transit. Antonio Rocco, who had read the *Dialogue* carefully shortly after it was published, called attention to Galileo's many claims of relying on sense experience at the beginning of the work but his conspicuous omission at the end of the evidence such experience provides [GG7: 712].

[37] The similarity becomes clear when examining arguments brought against the intermediate stage of the *regressus* by those, such as Ernan McMullin, who maintain on logical grounds that it is impossible ever to arrive at a unique causal explanation for any natural phenomenon. Logical possibility allows for any state of affairs that does not involve an explicit contradiction, and oddly enough, this squares with the only limitation theologians would place on God's absolute power. This puts McMullin in Urban VIII's corner: both reject the very possibility of Galilean science on *a priori* grounds, though they use a different language to do so.

[38] In his commentary on the *Sphaera* of Sacrobosco (Rome: 1581), 451.

[39] That is, his celebrated *Regulae philosophandi* with which he begins Book III, The System of the World, of the *Principia*.

[40] The question naturally arises whether Galileo thought that he had demonstrated the earth's motion in the *Dialogue* itself, without benefit of the principles he would later make explicit in the *Two New Sciences*. From the arguments analyzed above it seems clear that he did not, that he realized that the arguments he had advanced in the *Dialogue* were dialectical rather than scientific in the strict sense. An unexpected confirmation of this view is found in a notation he made in his own hand on the flyleaf of the first edition of the *Dialogue*; it has been transcribed by Favaro and translates as follows:

> Take note, theologians, that in your wish to make a matter of faith out of propositions relating to the motion and rest of the sun and the earth, you run the risk of having in time to condemn as heresy propositions that assert that the earth standsstill and the sun changes its place – at such time, I say, as it will have been demonstrated on the basis of sense experience and with necessity (*sensatamente e necessariamente*) that the earth moves and the sun stands still [GG7: 541].

This seems an implicit admission that, at least at the time of writing this, Galileo realized he had not yet achieved his goal of offering a "necessary demonstration" of the earth's motion based on "sense experience," although he remained convinced that one day such a demonstration would come within man's grasp.

CHAPTER 6

GALILEO'S NEW SCIENCES OF MECHANICS AND LOCAL MOTION

As has been seen, Galileo's observational genius with the telescope had supplied him with much new data on which a new science of the heavens could be based. Yet, at the time he published the *Dialogue*, he lacked, or at least did not have in usable form, a terrestrial mechanics that could integrate these data within a systematic structure. Earlier his experimental genius and his skill at developing measuring techniques had stood him in good stead when he had considered not the motions of the heavens but those of bodies close at hand. As his manuscript fragments now show, in the period before his discoveries with the telescope he had remarkable success laying foundations for a new science of local motion based on experiment. Thus he seems to have realized that, before he could provide a new astronomy that would replace Aristotle's, he first needed to articulate such a solid experimentally-based science. That probably explains why, after the humiliating defeat he had suffered at the hands of Urban VIII, he returned to his earlier interests and resumed work on the manuscript that was to become the *Two New Sciences*.

However that might be, abundant historical evidence is at hand to show that Galileo's first serious scientific efforts were in the study of local motion and terrestrial mechanics, and that it was only with the benefit of the knowledge he had gained in those efforts that he had presumed to tackle the problem of the earth's motion. He undoubtedly had begun experimentation with pendulums, with falling bodies, and with the inclined plane already while at Pisa. Then at Padua, particularly in the first decade of the seventeenth century, he embarked on a systematic research program that, by 1609, had supplied most of the conceptual apparatus on which the *Two New Sciences* of 1638 would be based. His discoveries with the telescope and the fame that came to him in 1610 with *The Sidereal Messenger* unfortunately sidetracked him from this enterprise and engaged him in the long polemic on the Copernican issue that did not end until the trial of 1633. Only after that, and in the peaceful surroundings of his "house arrest" at Arcetri, could he get back to work on the science of local motion and produce the masterwork of his career.

In what follows we treat this somewhat disconnected development in its chronological order, discussing first Galileo's early work at Pisa, then his experiments on motion at Padua and those in hydrostatics at Florence, and finally the summary presentation of his previous work, by that time largely editorial, at Arcetri. The thesis to be developed is that his successful incorporation of an experimental program into the techniques of suppositional reasoning and demonstrative regress appropriated in MS 27 and analyzed in Chapter 4 enabled him to develop a new science of local motion. This despite the fact that similar techniques, based on observational evidence but without the benefit of quantitative analyses involving experimentation, earlier had failed him in his attempt to produce, or to convince others that he had produced, a new science of the heavens.

1. ARCHIMEDEAN BEGINNINGS

When Galileo left off his studies at the University of Pisa in 1585 he had written nothing, or at least had left nothing in writing, that would give indication of his latent abilities as a mathematician. During the four years that intervened before his returning as a lecturer in mathematics, surviving materials show that in 1586 Galileo wrote a treatise in Italian on a small balance useful for determining specific gravities, *La bilancetta* (The Little Balance) [GG1: 215–220]; a more technical work in Latin entitled *Theoremata circa centrum gravitatis solidorum* (Theorems on the Center of Gravity of Solids) [GG1: 187–208], completed in 1587 but probably begun earlier; some tables listing specific gravities of metals and jewels measured in air and water [GG1: 225–228]; and a few notations on a work of Archimedes entitled *De sphaera et cylindro* (On the Sphere and the Cylinder) [GG1: 233–242]. All of these show a clear interest on Galileo's part in the principles and methods used in Archimedean science.

If the thesis relating to the time of appropriation of MS 27 described in *Galileo's Logical Treatises* is correct, none of these compositions was influenced by the logical teachings contained in Galileo's Jesuit sources. Yet a close study of their terminology reveals that they treat mathematics and mechanics as sciences in the Aristotelian sense, capable of yielding demonstrations on the basis of appropriate suppositions. This is consonant with the instruction Galileo would have received at Pisa, where philosophers such as Francesco Buonamici and mathematicians such as Filippo Fantoni saw the *Posterior Analytics* to be relevant to their disciplines. Thus the logic they themselves endorsed accords well with the contents of MS 27.[1]

From the terminology Galileo employs in *La bilancetta*, one can see that at this stage of his life he was convinced of the demonstrative character of Archimedes' reasoning when determining that the king's crown was not of pure gold but rather a mixture of gold and silver [GG1: 215–216]. The concepts Galileo employs are mathematical, but they are not purely such; he speaks also of *gravità* and its effects, namely, weight and motion, and he is assured that an element has no weight, and thus no motion, in its proper place. The case that interests him is one of static equilibrium, but this does not disguise his quest for knowledge of the physical causes that produce motion and rest. Thus he is pursuing the model of a middle science, in the sixteenth-century understanding of *mechanica*, to justify his solution of Archimedes' problem.

The *Theoremata* on centers of gravity of solids is more systematic than *La bilancetta* and gives better indication of Galileo's considerable ingenuity in devising proofs not found in Archimedes. Apparently he had worked through the latter's *On the Equilibrium of Planes*, the title then being used for *On the Center of Gravity of Planes*, to its last theorem, which shows how to calculate the center of gravity of a plane parabolic section. He there conceived the idea of extending the treatise to include a triangular section whose center of gravity would correspond to that of a cone, and in fact worked out a solution through the use of a lemma he devised using mean proportionals. At the same time he seems to have decided to expand this treatment into a series of propositions and proofs that would serve as more general principles from which the solution would follow, thereby supplementing Archimedes' exposition of the center of gravity of planes with another on the center of gravity of solids. Galileo left some propositions of the resulting *Theoremata* with Clavius when he visited him in Rome in 1587. He also showed them to a number of prominent mathematicians, whose favorable reaction quickly gained him a reputation in their discipline.

Although the reasoning of the *Theoremata* is geometrical throughout, the considerations are also physical and the main demonstration Galileo proposes is that of the mixed science of mechanics. It is noteworthy that a proposition in his proof, that stated at GG1: 188.33, was questioned by both Clavius and Guidobaldo del Monte as involving a *petitio principii*, and seems to have initiated Galileo's interest in the logic of proof he later set out in MS 27.[2] Other important features of the *Theoremata* are Galileo's use of approximation methods pioneered by Archimedes, his handling of limiting cases, and the simplicity and elegancy of his proofs.

These are usually not ostensive demonstrations, however, but rather indirect proofs that establish the truth of a proposition by reduction to the impossible [Sec. 4.4b]. Thus, to find the center of gravity of a parabolic conoid, Galileo first shows that this must be situated on the axis of the conoid between the center of gravity of a series of cylinders circumscribed around it and that of a series of cylinders inscribed within it, and that the distances separating the respective centers on the axis can be made smaller than any assigned length. His proofs then consist in showing that to hold any position other than the one he has assigned for the conoid's center of gravity would involve an absurdity, and thus that his solution to the problem is the only one possible [GG1: 189–195]. The technique obviously bears comparison to Galileo's later use of dichotomous division and elimination of a possible alternative in the solution of the astronomical problems discussed in the preceding chapter [Secs. 5.3 and 5.5].

2. THE EARLY TREATISES ON MOTION

As explained in the preface, Galileo's treatises on motion now bound in MS 71 were written more or less in continuity with MSS 27 and 46 and may show influences of the methodological teachings in MS 27. This is probably not true of his earliest attempt to treat motion, the dialogue on that subject also found in MS 71, which scholars agree could have been begun earlier. On this account in what follows we first provide an analysis of the dialogue and then move into a more extended discussion of the remaining contents of MS 71, whose logical methodology proved seminal for much of Galileo's later writing on motion and mechanics.[3]

a. *The Dialogue on Motion.* This dialogue, Galileo's earliest, is untitled, written in Latin, and patently incomplete. Set at Pisa, it records discussions on the motion of heavy and light bodies between Alexander, who is obviously Galileo, and an interlocutor named Dominicus. The latter says that he would like to discuss motion, not in general terms (a possible jibe at Buonamici, who did just that[4]), but more specifically problems relating to the motion of heavy and light bodies that are amenable to mathematical treatment, on which Alexander can help because of his familiarity with "the divine Ptolemy and the most divine Archimedes" [GG1: 368]. The problems are: (1) whether there must be rest at the turning point of motion; (2) why wood falls faster than iron when dropped from a height, if one admits this as a fact; (3) why falling motion is swifter at the end than at the beginning, and forced motion the reverse; (4) why a given body falls more swiftly in air than in water; (5) why cannons shoot farther when inclined to the vertical than when pointed horizontally; and (6) why they shoot heavier balls more swiftly and farther than light ones, though the latter are easier to move. All of these questions interested Buonamici, Fantoni, and another of Galileo's professors, Girolamo Borro, and could well have been the subject of recent disputations at the university. Galileo's replies to them are poorly organized, and by the time the dialogue breaks off only three have been answered, namely, the fourth, the first, and the third, in that order.

The fourth topic is of key interest because it enables Galileo to provide a series of demonstrations relating to the flotation and submergence of bodies similar to theorems he has already provided in his work on the *bilancetta* [GG1: 379]. These theorems, he says, though not different from those demonstrated by Archimedes, will be supported "by demonstrations that are less mathematical and more physical" and will be based on "suppositions that are clearer and more manifest to the senses" than those employed by Archimedes [GG1: 379]. In the subsequent discussion he does not identify these demonstrations and suppositions explicitly. The following, however, is a likely reconstruction of what he means by these terms:

> **Suppositions:** Heavy bodies move by reason of their heaviness (*gravitas*), and light bodies by reason of their lightness (*levitas*) [GG1: 378].
>
> Solid bodies immersed in water are either (1) of heaviness equal to the water, or (2) lighter than the water, or (3) heavier than the water.
>
> **Theorems demonstrated:**
> (1) A solid body of heaviness equal to water, completely submerged in water, will move neither upward nor downward.
> (2) Solid bodies lighter than water, if let down into water, are not completely submerged, but some part of them protrudes from the water.
> They sink up to the point where the volume of water equal to the volume of the submerged part has the same weight (*gravitas*) as the submerged part.
> If forced under water and completely submerged, they are buoyed

up with a force (*vis*) equal to the excess of the weight of a volume of water equal to the volume of the submerged solid over the weight of the solid itself [GG1: 382–383].

(3) Solid bodies heavier than water continue to move downward if they are let down into water.
They move downward with a force (*vis*) equal to the amount by which a volume of water equal to the volume of the solid body is lighter than that body [GG1: 383].

Conclusions: Heavy bodies move downward insofar as they are heavier than the medium through which they move; hence their heaviness in comparison with the medium is the cause (*causa*) of this downward motion. Similar reasoning leads to corresponding conclusions about bodies lighter than the medium [GG1: 378].

A solid heavier than water is lighter in water than in air by the weight, in air, of a volume of water equal to the volume of the solid. Thus a body will always move downward more swiftly (*celerius*) in air than in water [GG1: 384–385].

For each of the above theorems Galileo provides a geometrico-physical demonstration, frequently employing a double reduction to the impossible such as he uses in the *Theoremata*. Galileo does not elaborate on why his demonstrations are more physical than mathematical, or why his suppositions are clearer and more manifest to the senses than those employed by Archimedes. It seems evident, however, that he is explicitly locating himself in the domain of a mixed science by talking from the outset about water and air as physical elements, and then by discussing flotation phenomena in them that can be perceived directly by the senses. His mathematics is thus not abstract and speculative, but is easily comprehended as explaining physical events known from ordinary experience.

The dialogue then moves to a consideration of the turning point of motion, and here Galileo's methodological statements are noteworthy. Starting from five suppositions relating to the motive forces and resistances encountered by an object thrown upward, four of which are expressed in quantitative form, he argues that the object's motion will be continuous and that no rest will intervene when it turns to begin its downward course. Dominicus is most impressed by this procedure, and says that he must agree that "these demonstrations conclude necessarily, since they depend on the most manifest and most certain principles, which cannot possibly be denied" [GG1: 391]. Galileo then proceeds to contrast his method with that of Aristotle in his treatment of the void, where the latter "employed a kind of geometrical demonstration" and yet came to an erroneous result [GG1: 396]. The form of argument Aristotle used was correct, says Alexander, speaking for Galileo, and it would have led to a necessary conclusion "if Aristotle had demonstrated what he had assumed or if, at least, his suppositions were true even though not demonstrated" [GG1: 397]. He then argues that Aristotle regarded his assumptions as axioms, whereas in fact "they are not obvious to the senses, nor have they ever been demonstrated as true, nor are they even demonstrable," and proceeds to show their falsity [GG1: 397]. These remarks manifest an implicit awareness of the matters contained in MS 27, and especially Galileo's concern over the truth of premises on which arguments are based, possibly triggered by his first encounter with Clavius.

Galileo's argumentation relating to the problem of rest at the turning point of motion proceeds along similar lines. It too is based on five suppositions [GG1: 389–390]. From these Galileo proves a theorem stating that the force impressed by a mover is continuously weakened in forced motion, with the result that in any given motion no two points can be assigned in which the impelling force is the same. The proof is not ostensive but employs a double reduction to the absurd. The end result is that the physical part of the proof is carried mostly by the suppositions, whereas the mathematical part assures rigor by showing the inconsistencies that any departure from these suppositions will necessarily entail. When discussing physical forces arising from air resistance, however, Galileo invokes the distinction between essential and accidental causes, a distinction he uses repeatedly and that assumes importance in the development of his tidal argument for the earth's motion, as explained in the previous chapter. The context is a scholastic argument to the effect that air resistance acts against the body's weakened force at the apex of its motion and brings it to rest for a brief period at the turning point. Galileo's reply is that rest might occur in that way, but if it does, one cannot conclude that "rest occurs necessarily," for in this event the air is only an accidental cause (*causa per accidens*) of the body's coming to rest. He further asserts that Aristotle himself would have considered such a resistance as merely accidental, and so would have disregarded it in a causal analysis. Although he does not state this, his implication is that his own suppositions invoke essential causes (*causae per se*) and so permit one to conclude necessarily that rest does not occur at the turning point [GG1: 392–393].

The last of the questions treated in the dialogue on motion addresses the problem of the cause of acceleration in free fall and makes use of the preceding discussion to argue that the increase of speed results only from the continual decrease of a force previously impressed on the falling body. Galileo illustrates the argument with a diagram showing a body whose weight is 4 moving vertically upward along a line from A to B. After leaving point A the impelling force throughout the motion will have to be greater than 4, but at point B, since the upward motion stops there, it must be equal to 4. The downward motion commences at B, since the impelling force continues to diminish even when that point has been reached. The body then encounters less resistance to its weight, and "since this resistance is continually diminishing, the result is that the natural motion is continually accelerated (*continue intendatur*)" [GG1: 405].

To the objection that this solution is applicable only in the case of a natural motion that is preceded by a forced motion, Galileo replies that the cause of the acceleration is the same (*eandem ob causam*) even when a forced motion has not immediately preceded. His reason is that, even when a body begins from rest at point B, it is already being impelled upward by a force (*vis*) equal to its specific weight (*propria gravitas*) at that point; whenever this force is removed by whatever it is that lets the body fall, exactly the same sequence of events follows, and the motion of the falling body is found to be continually accelerated [GG1: 406]. In the course of this explanation Galileo enunciates an infinitesimal principle he will use to good effect in his later writings. In arguing that a force greater than 4, the measure he assigns to the body's weight, will always have been required to bring the body to the position from which it begins to fall, Galileo states that "an infinitely small force can always be impressed on the body to move it over any minimal distance whatever" [GG1: 406.11–13]. As a consequence, he says, the force that impels the body in forced motion over a zero distance is 4. Therefore the falling body takes its departure from rest with an upward force impressed on it that exactly counteracts the downward force of its own weight [GG1: 406].

b. *The Older* De motu: *Falling Motion.* The reformulation of the materials contained in the dialogue makes up the major portion of MS 71, usually referred to as the older *De motu* (*De motu antiquiora*) to distinguish it from the treatises on motion contained in the *Two New Sciences*. Three versions of this treatise exist, one complete and thus the most extensive, the other two containing either preliminary sketches or later revisions of the treatise. For purposes here disputes over the temporal ordering of the fragments can be disregarded in view of the fact, as has been argued, that all were written in reasonable temporal proximity to MS 27.[5] The approach to be followed will thus be more systematic, focusing particularly on the types of supposition Galileo employed in the study of local motion and on the procedures he developed to investigate them, particularly his seminal work with the inclined plane. Also his speculations about the possibility of a neutral motion, intermediate between natural motion and violent motion, assume importance for the way he would later deal with what is now called inertial motion.

The key to the organization of the complete treatise or essay *De motu* is contained in an item indicated on the plan with which MS 71 begins, namely, a statement that in motion three things are to be considered: the movable object, the medium through which it moves, and its mover or motive force [GG1: 418]. Internal evidence indicates that the essay is divided into two books, the first made up of thirteen chapters and the second of the remaining ten. A further perusal of its contents shows that the first book treats mainly the movable object and the medium through which it moves, whereas the second book concentrates on the movers or motive powers that cause its motion. The work thus shows an orderly progression through complex subject matter, quite different from the rambling account in the dialogue.

The first six chapters essentially duplicate the treatment of heaviness and lightness in the dialogue, giving reasons for the arrangement of the elements in the universe and their quantitative distribution, along lines similar to those found in Jesuit expositions of *De caelo*. The first demonstration then proves a proposition basic to the treatise as a whole, namely, that "bodies of the same heaviness as the medium in which they are situated move neither upward nor downward" [GG1: 254], with the proof taking the form of a *reductio ad inconveniens* [GG1: 256.13]. A second demonstration purports to show how and why motion upward results from *levitas* [GG1: 257], a conclusion treated quite differently in the alternate versions of the treatise.

Chapters 7 through 9 treat the natural motion of heavy objects considered from the viewpoint of the movable body itself. Here Galileo raises more interesting questions than those in the dialogue and goes beyond the simple quantitative methods used in it to parallel those employed by Giovanni Battista Benedetti in his *Diversarum speculationum* of 1585. The similarity is so striking, and the anti-Aristotelian tone of Benedetti's work becomes so much more explicit in Galileo's composition, that it seems likely he and Jacopo Mazzoni were reading extensively in Benedetti and absorbing the latter's critical mentality.[6] Here also problems begin to surface relating to the closeness of fit beween mathematics and physics that were to occupy Galileo until the end of his life.

There can be no doubt that by now Galileo is very much concerned with a causal analysis of natural motion, for chapter 7 starts out by inquiring "what causes the swiftness and slowness of natural motion" [GG1: 260]. Yet he quickly makes the admission that, though "what we seek are causes of effects, these causes are not given us by experience" [GG1: 263]. In other words, natural causes are in large part hidden causes, and they can only be discerned from a careful study of the effects they produce – precisely the situation that would require one to use the demonstrative *regressus* outlined in D3.3 [Sec. 4.9].

To ascertain the extent to which Galileo might employ such a regress, we will now review a few representative details of his reasoning in this second section of his first book. The argumentation throughout this section is extensive, making it impractical to attempt analyzing it in its entirety. As a feasible alternative, we shall examine only the reasoning by which he arrives at two key conclusions. These are: (1) that in the same medium bodies of the same material but of unequal volume move naturally with the same speed, and (2) that in the same medium bodies of different materials fall at different speeds. Both are explicitly directed against Aristotle's teachings, and the general technique Galileo uses is to set up a dichotomy between those teachings and his own, reduce the former to an impossibility or an absurdity, and then urge the truth of his own position.

I. The dichotomy behind the first conclusion is that *either* bodies of the same material and of different volumes fall with the same speed in the same medium, *or* they fall at different speeds following the rules given by Aristotle in *De caelo* 3.2 and 4.4 [GG1: 263]. Galileo here develops two disproofs of the Aristotelian position: the first is based directly on the Archimedean buoyancy principle and is similar to the arguments developed in the dialogue on motion discussed above; the second is based on a supposition taken from the *Quaestiones mechanicae*, thought in

Galileo's time to be authored by Aristotle but now known to have been written by a member of his school in the generation after his death. Actually the supposition on which Galileo's argument is based is one used in the *Quaestiones mechanicae* to solve the problem posed by the "wheel of Aristotle." The case discussed there is that of a body moving with a connatural motion while being tied to another body moving with a forced or accidental motion, and in this case the supposition is valid. Galileo applies it here to two bodies that are moving naturally, where it is not valid, and from this he reasons that Aristotle involves himself in a contradiction.[7]

The positive argument Galileo develops is important, for it pursues a search "for causes not given us in experience," as already noted, and thus suggests the use of the demonstrative regress. Though there is no straightforward application of regressive method in his text, his purported use of it can be adapted schematically to the form employed in the previous chapter and summarized as follows:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
Bodies of the same material and of unequal size fall at the same speed in the same medium	speed of fall is deteremined by the weight of the body in the medium through which it falls.

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

Supposition: heavy bodies move downward by reason of their weight (*gravitas*), and thus their speed of fall is directly proportional to their weights [GG1: 262].

To hold, as Aristotle does, that speed of fall is directly proportional to absolute weight contradicts experience, since if two stones are dropped from a high tower, one twice the size of the other, the larger does not reach the ground when the smaller is only halfway down [GG1: 263].

The essential cause (*causa per se*) of the body's speed is thus not its absolute weight but its specific weight, that is, its weight less the weight of a volume of the medium equal to its own volume. Such specific weight is the same for all bodies of the same material and of unequal size falling in the same medium [GG1: 264].

Accidental causes (*causae per accidens*) such as the shape of a body may cause variations in speed, but these are slight and may be neglected [GG1: 266].

Second progression: *from the cause, recognized formally as the cause, to its proper effects.*

Cause	**Effect**
Bodies of the same material and of unequal size have the same specific weight in the same medium	neglecting accidental causes, they fall at the same speed in the same medium [GG1:266]

Note here the genus of causality on which the proferred demonstration is based. Galileo is still working under the Aristotelian supposition that downward motion is caused by a motive power in the heavy object, its *gravitas*, and thus his argument invokes an efficient cause. His departure from Aristotle is not on the cause itself, but rather on a particular quantitative modality it manifests under the circumstances, namely, how effective it is in moving the body in the medium surrounding it.

II. The second conclusion to be examined respects the ratio of the speed of fall of bodies of different material in the same medium. Here again Galileo begins with an Aristotelian rule, then disproves that rule, and proceeds to derive various correct rules that now follow from a consistent application of the Archimedean buoyancy principle. As implicitly used in the previous argumentation the Archimedean principle states that, rather than the speed of fall being regulated by the *gravitas* or weight of the body, it is regulated by the body's *propria gravitas*, its weight in the particular medium through which it moves. Already enunciated by Benedetti,[8] this becomes what Galileo now identifies as the "true cause" of the speed of fall of bodies in the various media [GG1: 272–273].

Despite this avowal, however, Galileo has some reservations, for at this juncture he returns to the second problem enumerated at the beginning of the dialogue, namely, why it is that a lighter body made of wood when dropped from a height falls more swiftly than a heavier one made of iron, "if one admits this as a fact" [GG1: 368]. In that context his wording portrayed the factual status of the problem as questionable. Now he accepts it as a truth that contravenes "the general rules (*universales regulae*) governing the ratio of speeds of motion of bodies," and admits that "a great difficulty arises at this point, because these ratios will not be observable by one who makes the experiment (*periculum*)" [GG1: 273]. He goes on:

> For if one takes two different bodies, which have such properties that the first should fall twice as fast as the second, and if one then lets them fall from a tower, the first will not reach the ground appreciably faster or twice as fast. Indeed, if an observation is made, the lighter body will, at the beginning of the motion, move ahead of the heavier and will be swifter [GG1: 273].

The mention of bodies dropped "from a tower" suggests the Leaning Tower of Pisa and the likelihood that Galileo himself performed the experiment, as he claims later on in the treatise when refuting an experiment alleged to have been performed by one of his former teachers, Girolamo Borro [GG1: 333–334]. Here he apparently thinks that departures from expected results will be explicable and so he ascribes them to accidental causes. They are to be treated as "quasi-monsters" (*quodammodo prodigia*) that arise in nature but from unnatural causes – a teaching to be found in Aristotle's *Physics* [199b5]. Yet the fact that he lacks experimental confirmation clearly bothers Galileo, and this might count against the possibility that he himself thought he had achieved a strict *regressus*, despite the claims he makes for having found the "true cause" of falling motion.

In any event, his argument here may be put in the regressive form in much the same way as the previous one:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
Bodies of the same size but of different materials fall at different speeds in the same medium	since their speeds of fall are determined by their specific weights in that medium

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

Supposition: heavy bodies move downward by reason of their weight (*gravitas*), and thus their speed of fall is directly proportional to their weights [GG1: 262].

To hold Aristotle's rule that the ratio of speeds of the same body moving in different media is equal to the ratio of the rarenesses of the media leads to absurd consequences [GG1: 268–269].

Rather, a heavy body whose density is greater than the medium is urged downward in that medium with a force measured by the difference between the weight of the body and the weight of a volume of the medium equal to the volume of the body, that is, by its specific weight in that medium [GG1: 271].

The true cause of the speed of fall of a heavy body in a medium is the specific weight of the body in that medium [GG1: 272].

Second progression: *from the cause, recognized formally as the cause, to its proper effect.*

Cause	**Effect**
The ratios of specific weights of the same body falling in different media	determines the ratios of the speeds of fall of that body in those media
The ratios of specific weights of different bodies falling in the same medium	determines the ratios of the speeds of fall of those bodies in that medium
The ratios of specific weights of different bodies falling in different media	determines the ratios of the speeds of fall of those bodies in those media [GG1:272-273]

Note again that the basic cause invoked here is an efficient cause, specific weight (*propria gravitas*), very similar to Aristotle's "false cause" that is rejected by Galileo. The difference is in the motive effectiveness of that cause, here attenuated by the upward force of the medium's buoyancy.

c. *The Inclined Plane: Suppositions.* Book Two of the *De motu* breaks new ground, for it begins with a topic mentioned in the plan for the treatise, namely, "the ratios of motions along inclined planes" [GG1: 418], which had not been discussed in the dialogue on motion. Although treatments of weights on inclined planes were common in mechanical treatises of the time, including those of Jordanus Nemorarius, Niccolò Tartaglia, and Guidobaldo del Monte, Galileo's originality would seem to lie in his claimed success at deriving the ratios of motions along such planes "from principles of nature that are known and manifest" [GG1: 296].

He puzzled over the question, Galileo says, why a heavy body descends faster along a plane that is more inclined to the horizontal, seeking "to resolve the demonstration of this fact to its proper principles," namely, those that initiate a natural motion [GG1: 296]. Galileo's use of "to resolve" here signals his use of a resolutive method, as described above in Sec. 2.7, to reduce effects to their essential causes. The cause that accounts for the swiftness of descent he then identifies as the weight (*gravitas*) the body has by reason of the incline on which it is situated, and this leads him to investigate what "such weight" (*talis gravitas*) will be [GG1: 297]. His calculations, made with a simple geometrical diagram that enables him to use mean proportionals, yield the conclusion that "the same weight can be drawn up an inclined plane with less force (*minori vi*) than vertically, in proportion as the vertical ascent is smaller than the oblique" [GG1: 298]. From this follows the famous *De motu* theorem, one of the cornerstones on which the *Two New Sciences* would be built:

> Consequently the same heavy body will descend vertically with greater force (*maiori vi*) than on an inclined plane in proportion as the length of the descent on the incline is greater than the vertical fall [GG1: 298].

Immediately after asserting this remarkable proposition, Galileo qualifies it by pointing out that his demonstration is valid only on the supposition that there are no accidental factors present to perturb the result. "One must suppose," he writes, that the plane is "in some way incorporeal or at least exactly level" and that the ball is "exactly spherical." Under these suppositions, he goes on, one can even show that any body on a plane parallel to the horizon will be moved by a minimal force, and indeed, "by a force less than any other force" [GG1: 298–299]. He proceeds to prove this by a simple mathematical argument made with the express assumption that no accidental resistances are present, and concludes that "the motion of such a body would be neither natural nor forced" [GG1: 300]. In a marginal addition he speculates how such a motion should properly be described, and prefers to call it "neutral motion" rather than "mixed motion" (*iste melius dicetur neuter quam mixta*) – a characterization consonant with the "intermediate motion" (*motus medius*) found in the Collegio Romano lecture notes.[9]

Here Galileo apparently senses a need to defend his *suppositiones*, for it may appear, he writes, that he has used false propositions to defend a true result, precisely a charge he had earlier directed against Aristotle [GG1: 277–278]. It is in this context that Galileo takes his oft-cited refuge under "the protecting wings of the superhuman Archimedes," whose name he never mentions "without a feeling of awe" [GG1: 300].

Archimedes, he recalls, made precisely the same type of supposition in his *Parabolae quadratura*, for there he treated weights suspended from a balance as making right angles with the balance even though they do not exert their force in parallel lines but actually converge toward the center of the earth. One could maintain, he now says, that the angles are right angles, or that this is an immaterial consideration since all that is necessary for the proof of the balance theorem is that the angles be equal. An alternative defense of Archimedes, he goes on, would be that he had simply employed geometric license (*geometrica licentia*), as he had quite clearly done in other situations, such as when supposing that surfaces have weight, or that one surface is heavier than another, although in point of fact a surface can have no weight [GG1: 300].

Galileo further cautions that it may not be possible to verify experimentally that a sphere can be moved horizontally by a minimal force because of external resistance and the fact that no plane on the earth's surface is strictly speaking horizontal [GG1: 301]. Similarly the ratios he has calculated for motion down an incline may not be observable either. The reason for deviations, he says, is that his "demonstrations generally are based on the supposition that there are no extrinsic impediments" [GG1: 302]. Such *impedimenta* are all so many accidental causes for which rules cannot be expected to account, since they can occur in countless ways and so invariably affect experimental accuracy. From these statements it is clear that Galileo was convinced that the *impedimenta* and the *accidentia* found in the universe of sensible matter would have to be transcended if one were to arrive at a mixed science of mechanics, and that the way to do so was now obvious. The geometrico-physical demonstrations of such a science would supply the answer, but they would have to be explicitly identified as demonstrations made *ex suppositione*, manifesting a clear knowledge of the many impediments their suppositions would be designed to eliminate.

Since Galileo has explicitly identified his method of deriving ratios of speeds along inclines as resolutive, it will be helpful again to outline the demonstrative regress that here seems to be involved.

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
Heavy bodies descend along planes inclined to the horizontal	because their heaviness on the inclines increases with the

more swiftly the greater the angle of inclination.	angle of inclination

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

A heavy body tends to move downward with as much force (*tanta vi*) as is necessary to lift it up, and this force will be greater the greater the weight (*gravitas*) of the body on the incline [GG1: 297].

Geometrical analysis shows that the ratio of the force required to overcome weight on an incline to that required to overcome weight vertically is as the ratio of the vertical height to the oblique distance along the incline [GG1: 298].

Therefore a body will descend vertically with greater force (*maiori vi*) than when on an incline in proportion as the length of descent on the incline is greater than the vertical fall [GG1: 298].

Suppositions: (1) that heavy bodies move downward by reason of their weight (*gravitas*), and thus their speed of fall is directly proportional to their weights [GG1: 262].

Again, (2) that there is no accidental resistance (*nulla existente accidentali resistentia*) occasioned by the roughness of the moving body or of the inclined plane, or by the shape of the body; that the plane is, so to speak, incorporeal, or at least that it is very carefully smoothed and perfectly hard; and that the moving body is perfectly smooth and of a perfectly spherical shape [GG1: 298–299].

Further, (3) under such conditions, that any heavy body can be moved on a plane parallel to the horizon by a force smaller than any given force whatever [GG1: 299–300].

Second progression: *from the cause, recognized formally as the cause, to its proper effects.*

Cause	**Effect**
The greater the angle of incline on which a heavy body rests	the greater the force with which it moves downward
The weight of a heavy body on an incline is to its vertical weight as its vertical height is to the length of the incline	the ratio of its speeds down the incline will be as the ratio of the length of the incline to its vertical height

The weight of a heavy body on a plane inclined to the vertical by any angle no matter how small	under the supposed conditions will move the body down the incline with a natural motion
A force smaller than any given force will suffice	To move a body along a plane exactly parallel to the horizon [GG1:298-300]

Note yet again that the type of causality involved here continues to be efficient, that exercised by a motive power (*gravitas*), now attenuated not by the buoyancy of the medium but the positional weight of the heavy body on the incline. Observe also the use of infinitesimal angles and infinitesimal forces in the third and fourth conclusions, which Galileo will apply to good effect in his early treatises on mechanics, to be seen in the following Section. And note finally that Galileo regards these arguments as demonstrations on a par with Archimedes' demonstration of the law of the lever, based as they are on suppositions that may be regarded as physically true even though they might not satisfy the rigor demanded by a pure mathematician.

d. *Agent Forces.* The concluding section of Book Two is concerned with the mathematical properties of falling and projectile motion, and in it Galileo applies the principles he has been developing to refute commonly held opinions of his day. The major problem with falling motion is explaining why it accelerates toward the end of the fall; his adversaries, he says, err for a variety of reasons, among which is their confusing *causae per accidens* with *causae per se* [GG1: 317]. His preoccupation will be uncovering the true cause (*vera causa*) of the acceleration, and to do this he will employ a resolutive method (*resolutiva metodo*) [GG1: 318]. Since he has already shown that the velocity is a function of the *gravitas* and *levitas* of the moving object, he will consider the problem solved if he can show how and why the falling body is less heavy (*minus grave*) at the beginning of its fall. And since its natural heaviness must remain unchanged, the only reason why it could be *minus grave* would be because the diminution was *praeternaturalis* and introduced from without [GG1: 318]. The explanation is easily seen in the case of a heavy object thrown upward, for in this case the *virtus impressa* overcomes the body's weight all during its upward course, though it diminishes gradually until it equals the downward force at the top of the body's trajectory. After the turning point, however, the externally induced force diminishes yet further and the natural weight becomes more and more felt. The gradual predominance of the natural weight over the exraneous lightness thus explains the body's downward acceleration.

Here again we have a case where Galileo's appeal to the resolutive method suggests that he is employing the demonstrative regress to find the "true cause" of the increase of speed during the body's fall. The argument he develops may thus be phrased in the following regressive format:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
There is an observable increase in the speed of natural falling motion toward the end of the motion	because the falling body is less heavy at the beginning of its motion than it is at its end.

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

Supposition: heavy bodies move downward by reason of their weight (*gravitas*), and thus their speed of fall is directly proportional to their weights [GG1: 262].

The explanations offered by Aristotle and others invoke only accidental causes and do not arrive at the essential cause of the acceleration [GG1: 317].

That is: the weight of the body does not increase as it approaches its proper place; the body is not pushed by the medium rushing in behind it to fill the void created by its motion, since it is only accidental that it moves in a plenum; nor does the body encounter less resistance by having to separate fewer parts of the medium as it approaches the end of its motion [GG1: 316–317].

Rather, the natural and intrinsic weight (*naturalis et intrinseca gravitas*) of the body remains constant. Thus it is necessary to find some external force (*vis extrinseca*) that lightens the body at the beginning of its fall. This can only be the impelling force (*virtus impellens*) or lightness that sustains the body before it begins to fall and continually diminishes throughout its fall.

Such an impelling force is found not only when bodies are thrown upward before their descent, but also in cases where natural fall is not preceded by such a forced motion [GG1: 318–320].

Second progression: *from the cause, recognized formally as the cause, to its proper effect.*

Cause	**Effect**
The continual increase in effective weight of the body as this impelling force weakens and acts less against the body's essential weight	causes the body to move faster and faster throughout its fall from beginning to end [GG1:319].

Note once again that efficient causality continues to carry the force of the demonstration. Also noteworthy is that Galileo himself adverts to the "work of the intellect" involved in the intermediate stage of this regress when he reflects on his own thought processes in the following terms:

> Now, while engaged in seeking for the cause of this effect [i.e., the acceleration]...I was troubled for a long time, and did not find anything that fully satisfied me. And, indeed, when I discovered an explanation that was completely sound (at least in my own judgment), at first I rejoiced. But when I examined it more carefully, I mistrusted its apparent freedom from any difficulty. And now, finally, having ironed out every difficulty with the passage of time, I shall publish it in its exact and fully proved form [GG1: 316].

This statement occurs, of course, only in manuscript. As it turned out Galileo never did publish it, possibly a sign that he continued to have doubts about its truly demonstrative character. These were not to be overcome until he succeeded in obtaining experimental verification of yet other mathematical properties of falling and projectile motion. But at this early stage there was still room for doubt. One need only recall the point made in Sec. 3.7 above, and surely known to Galileo, namely, that though demonstrations employ causal argument, not every causal argument is demonstrative, for it might be dialectical. The latter possibility proves to have been the case here, as we now known, and this in itself gives adequate ground for Galileo's hesitation.

3. THE EARLY TREATISES ON MECHANICS

Galileo's course in mechanics, based on the tradition of the Aristotelian *Quaestiones mechanicae*, survives in two early versions, one probably dating from 1593 and the other certainly from 1594, and in a finished version that is more difficult to date but was probably written before 1602. If these datings are correct, the course itself is associated with his teaching at the University of Padua rather than at Pisa. It takes on particular interest on this account, for it clearly shows a continuity of thought that carries over from his Pisan to his Paduan period. Since the versions of 1593–1594 register few points of disagreement, they may conveniently be treated here as one under the rubric of being Galileo's "first" mechanics, after which a fuller analysis will be made of the finished version, which he entitled *Le meccaniche*.

a. *The First Mechanics.* The titles appended to both versions of this work indicate that it is a mechanics of machines or of instruments and thus a practical mechanics that would usually follow the more theoretical treatment of centers of gravity, according to the breakdown of the science of mechanics accepted in his day.[10] Its opening chapter paraphrases the introduction to the *Quaestiones mechanicae* and differs little from similar treatises on the subject dating from the same period. The science of mechanics, writes Galileo, teaches the reasons (*le ragione*) and furnishes the causes (*le cause*) of the marvelous effects we see coming from various instruments, moving and raising great weights (*pesi*) with the slightest force (*forza*) [GM270].[11] Wishing to present an orderly treatise on this subject, he will first examine the nature of the primary and simpler instruments and then show how compound machines may be reduced (*si reducano*) to them. All of these machines, he adds, can in turn be reduced to the balance, and thus its understanding is basic to all the rest [GM270].

The explanation of the balance that follows is similar to that in *La bilancetta* of 1586. Galileo treats first in chapter 2 of the balance with equal arms, then extends the explanation in chapter 3 to that with unequal arms, saying that his results are demonstrated "not only by experiment (*esperienza*) but also by reason," the latter as expounded by Archimedes in his work on the equilibrium of planes [GM271]. The propositions established here are applied to the lever in the next four chapters, and it is in this application that Galileo returns to an idea he had already mentioned in the *De motu antiquiora*, namely, that of "a minimum force or force smaller than any assigned force" [GG1: 299]. The context is a clarification of the statement that a force of 200 will move a weight of 2000 if applied with a leverage of ten times the distance of application. Galileo immediately qualifies this to state that such force will merely sustain the weight and thus "it is not absolutely true" to say that the force will move it. Yet considering, he goes on, that any minimal moment (*minimo momento*) added to the counterbalancing force will produce a displacement, by not taking account of this insensible moment (*momento insensibile*), one can say that motion will be produced by the same force as sustains the weight at rest [GM272–273]. This is an important statement, for it shows that Galileo was continuing to reject the rigorist position of Guidobaldo del Monte and others, which would not allow this minimal force to be neglected. The use here of what is clearly a supposition, one permitting the mathematical physicist to neglect infinitesimal forces in his calculations, opened the door for him to treat both dynamic and static cases by the same mathematical principles. Effectively he had begun to bridge the gap between Archimedean statics and the Aristotelian tradition of *De ponderibus* recently revived by Tartaglia, and was moving in the direction of a unified science of mechanics.

The same topic is rejoined in chapter 12, where Galileo reduces the operation of the screw to the principles that govern the inclined plane, since the screw is nothing more than an inclined plane wrapped around a cylinder. He observes that a heavy body descends by its natural tendency, whether it falls directly downward or works its way along any surface inclined even slightly to the horizon [GM276]. On the other hand, given a plane without any inclination at all, heavy objects placed on it will remain at rest. It is also true, he goes on, that a minimal force (*minima forza*) in such circumstances will suffice to move them from their place [GM276]. This is a repetition of the teaching contained in *De motu antiquiora*, just referred to. Galileo again states it explicitly: a body on a level surface "can be moved, not by itself, but by a minimal force applied to it from without" [GM277]. With regard to the calculation of the amount of force necessary to move it along the incline at various angles, Galileo observes that to determine this demonstratively (*dimostrativamente*) would be somewhat more difficult. Thus he will pass over the matter at this point, merely noting the conclusion, namely, "that the weight to be moved has the same ratio to the force moving it as the length of the inclined plane to the perpendicular height to which the weight will be raised" [GM277].[12]

b. *Le meccaniche*. His more fully developed treatise on mechanics, titled *Le meccaniche* in some manuscripts, Galileo models on the mechanical treatises of Tartaglia, Commandino, and others. As in all "demonstrative sciences," he writes, it is necessary to begin with "definitions" and with "primary suppositions," from which will spring the "causes" and "true demonstrations" of the "properties" of mechanical instruments [GG2:

159]. The definitions he gives are those relating to the motion of heavy bodies – *gravità*, *momento* (revised from his usage in the earlier versions to now mean a propensity to move downward), and *centro della gravità* – the last of which he takes from Commandino. His suppositions are likewise three in number, and all are concerned with various aspects of the center of gravity. The first is that a heavy body will move downward along a line joining its center of gravity to the universal center of heavy things; the second, that a heavy body not only gravitates on its center of gravity but also receives impressed forces at that center; and the third, that the center of gravity of two equally heavy bodies is in the middle of the straight line joining their respective centers [GG2: 160]. In explaining the third supposition Galileo remarks that, if two equal bodies are suspended at equal distances from a point, they will have their point of equilibrium at this common juncture, provided the equal distances are measured with perpendicular lines drawn from the weights to the common center of heavy things. Here again he has sided with Archimedes and Tartaglia against the more stringent position of Commandino and Guidobaldo del Monte, who would not admit the possibility of perpendiculars drawn to a common center because such lines would obviously not be parallel.

Using his third supposition Galileo proceeds to demonstrate the general principle of the balance, namely, that unequal weights hanging from unequal distances will weigh equally whenever the said distances are inversely proportional to the weights [GG2: 161]. The proof he offers is a generalized one that does not assume a uniform shape for bodies distributed along the beam of the balance – an assumption he justifies with the Aristotelian argument that shape (*figura*) pertains to the category of quality and is thus powerless to alter weight (*gravezza*), which derives from the category of quantity. He then applies the principle to the steelyard, the lever, the windlass, and the screw.

In his treatment of the screw Galileo becomes yet more explicit on the way he sees mechanics preserving its scientific character. He begins with a conjecture (*speculazione*) that he recognizes as somewhat remote from the study of the screw but which he feels is fundamental to an understanding of the instrument [GG2: 179]. This is that any body retaining its heaviness has within itself a propensity (*propensione*), when free, to move toward the center, and to do so not only when falling perpendicularly but also, when unable to do otherwise, by any possible motion toward the center. Thus, given "a surface that is very clean and polished like a mirror, and a perfectly round ball," the ball will move down the surface if the latter has some tilt, even the slightest. If the surface is exactly level, however, and equidistant from the plane of the horizon, the ball will remain still, though it will retain "a disposition to be moved by any force no matter how small." To make the point even more dramatically, Galileo reiterates that if the surface is tilted "only by a hair," the ball will spontaneously move down it or, conversely, resist being moved up it. Only on a perfectly flat surface will the ball "be indifferent and remain questioning between motion and rest" – a statement I.E. Drabkin has pointed to as Galileo's proto-inertial principle.[13] Yet when there, "any slightest force" will suffice to move it, and conversely, "any slightest resistance" will be capable of holding it still [GG2: 179–180].

This line of reasoning leads Galileo to consider what he regards as an "indubitable axiom," namely, that if "all extraneous and accidental impediments" are removed, a heavy body can be moved in the plane of the horizon "by any minimal force whatever" [GG2: 180]. Apparently not wishing to set himself in opposition to his patron, Guidobaldo del Monte, on this point, Galileo directs the reader's attention to Pappus and says that the latter failed in his attempt to solve problems of this kind by his supposition that "a given force" (*una forza data*) would be required to move a heavy body on a horizontal plane. But this supposition is false, Galileo insists, for "no sensible force" is required for such motion when all "accidental impediments" – which are not the concern of the "theoretician" anyway – have been removed [GG2: 181].

With this axiom as a basis, Galileo proceeds to attack the problem of the force required to move an object up an inclined plane. In the earlier versions he had passed over this exercise as somewhat too difficult, but here he proceeds to solve it with the same geometrical method he had earlier used to work on motion down the incline. The only physical principle he imports into his analysis is the one he has stressed repeatedly in the preceding discussion, namely, that the force required to move a weight need only insensibly (*insensibilmente*) exceed the force required to sustain it. From this, plus his geometrical analysis of various paths of possible descent, he is able now to derive the conclusion he had only stated in the earlier versions: the force required to move a weight up an incline "has the same proportion to the weight as the perpendicular dropped to the horizontal from the end of the plane has to the length of the plane" [GG2: 183; cf. GM277]. And with the aid of this principle he is able to solve not only the problem of the inclined plane but that of the wedge and the screw also.

The reasoning Galileo uses for his resolution is based on the geometrical equivalence of all three instruments, the screw, the wedge, and the inclined plane, and may be represented schematically as follows:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
A heavy body is lifted upward more easily along an incline than it is along the vertical	because its effective weight along the incline is less than its vertical weight

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

Suppositions:

(1) that the inclined plane and the wedge may be represented as a right triangle whose base is AB, whose height is BC, and whose hypothenuse is AC, and that the screw may be regarded as an inclined plane wrapped around a cylinder ABCD, thereby generating a helical line whose height is BC and whose length is AD;

(2) that both the body to be lifted and the surface of the incline are smooth and polished, and that all extraneous and accidental impediments have been removed;

(3) that the force F_i required to move a weight W up an incline need only insensibly exceed the force F required to sustain it on the incline; and

(4) that the drawing of a body up a stationary incline is equivalent to leaving the body stationary and moving an inclined plane under it, as in the driving of a wedge or the turning of a screw.

Axiom: the force F required to sustain a weight W on an incline "has the same proportion to the weight as the perpendicular dropped to the horizontal from the end of the plane has to the plane's length," i.e., $F/W = BC/AC$ [GG2: 183].

Second progression: *from the cause, recognized formally as the cause, to its proper effects.*

Cause	**Effect**
A force F_i insensibly exceeding F, where F has the same ratio to W as BC has to AC	will suffice to move weight W up the length of the inclined plane AC [GG2:183]

A wedge driven under a heavy body W and raising it with the same force F_i	will suffice to lift weight W to the height BC [GG2:183-184]
A screw generating a force F_i that insensibly exceeds F, where F has the same ratio to W as AC has to length of the helical line AD	will suffice to life weight W the height of the cilinder AC (GG2: 184]

What is noteworthy about these demonstrations is their similarity to those formulated in the older *De motu* treatise [Sec. 6.2b], where Galileo was analyzing the natural speed of fall of heavy bodies in various media. There the efficient causality invoked was the specific weight (*propria gravitas*) of the body in the medium. Here, however, there is an important difference, for the cause is no longer a natural agent. It is now a force impressed on a heavy body from without – precisely the type of agency treated in the *Quaestiones mechanicae* of the pseudo-Aristotle. Such a force apparently had more intuitive appeal to Galileo than has a natural power. In any event, his wording in *Le meccaniche* suggests a conviction on his part that his suppositions and his axiom here are true and indubitable, quite unlike the doubts he expressed when explaining acceleration in free fall in the *De motu antiquiora* [Sec. 6.2d].[14]

4. EXPERIMENTATION AT PADUA

The logical development of the mixed science of motion and mechanics sketched to this point has now brought us to the point where we began in the preceding chapter to treat the demonstrative methods used by Galileo in the mixed science of astronomy [Sec. 5.1]. The *Trattato della Sfera* discussed there overlapped to a considerable extent with *Le meccaniche*, for both seem to be vestiges of the instructional materials Galileo used at Padua in the first decade of his teaching there. Both works involved causal explanations and suppositions, although of different types as dictated by the subject matters they treated. The astronomical evidence was observational and so largely based on analyses of *perspectiva* or optical science; its suppositions related to the nature and properties of light rays and the truth of theorems provided by projective geometry. The causes it invoked were mainly formal causes based on the shapes and configurations of heavenly bodies, though obviously it presupposed the action of efficient causes for the production and transmission of light. Mechanical investigations, on the other hand, treated phenomena close at hand, where observation was not the unique source of information since it could be supplemented directly by experimentation. In this area efficient causes such as weight and force assumed greater importance, and suppositions focused mainly on incidental or extraneous factors that might impede their operations. Mathematical skill, particularly in geometry, was essential of course for both astronomy and mechanics. But in the case of mechanics more "hands on" experience was required if one were to make correct judgments about what was *insensibile* and so could be neglected, or was not and so had to be taken into account. Thus it is not surprising that about this time, in the early 1600's, Galileo embarked on an extensive program of experimentation that had no precedent among the mechanicians of his day. Two areas he investigated are noteworthy for our purposes, one relating to motions of pendulums and of bodies along inclines, the other to the paths followed by projectiles.

a. *Pendulums and Inclines.* With regard to the first, from evidence now available it seems clear that Galileo's experiments in the period from 1602 to 1604 were made mainly by rolling balls down planes inclined at small angles to the horizontal and by studying the swings of pendulums of various lengths. Stillman Drake has recently published a novel interpetation of the relevant data, arguing that the techniques Galileo developed in this period permitted him to make time measurements much more accurate than those attainable with the water clock he describes in the *Two New Sciences.*[15] Though Galileo worked with the six-foot pendulums he describes in a letter to Guidobaldo del Monte [GG10: 99], Drake conjectures that he also experimented with one some thirty feet long. From tests with this and possibly other pendulums, proposes Drake, Galileo was able to derive a pendulum law in mean proportional form "mathematically the same as our law that periods of pendulums are as the square roots of their lengths."[16] By then deflecting a standard pendulum a small arc from the vertical, and knowing how long it takes to return to the vertical, he had at hand a practical way of measuring small units of time. This he coupled with the skillful use of "timed beats" – a process of adjusting frets on an inclined plane at precise distances so as to coordinate, with a tune being sung, the faint bumping sounds a ball would make when rolling over them in its passage down the incline. Employing these procedures, by 1604 in Drake's accounting Galileo had experimentally established to a high degree of accuracy the times-squared law of free fall, that is, that a body falling vertically from rest traverses distances proportional to the square of the times through which it falls. He also had verified that the same law applies to the roll of a ball down an incline, and indeed that the speeds he had measured in each unit of time increased in the same ratio as the odd numbers 1, 3, 5, 7... [17]

Drake's findings make sense in terms of a notation made in Italian on folio 128 of MS 72, a folio that bears the same watermark as the cover sheet of a letter Galileo wrote to Paolo Sarpi on October 16, 1604. Toward the end of the notation Galileo states that "the distances traversed from the beginning of the motion are as the square of the times, and, by dividing, the spaces passed over in equal times are as the odd numbers from unity." This he corroborates by writing that his data "agree with what I have said all along and have observed in experiments (*esperienze*)" [GG8: 374]. The latter statement suggests that by October of 1604 Galileo felt he had good experimental confirmation of the times-squared law and the odd-number property of accelerated motion.

It also seems that by the time of his writing to Sarpi Galileo had abandoned the explanation proposed in his early writings on motion, wherein he characterized acceleration as a temporary phenomenon that lasted only until a *virtus impressa* or extraneous lightness had been used up [Sec. 6.2d]. Possibly because of an earlier preoccupation with measuring distances, Galileo first speculated that the speed of fall would go on increasing uniformly with distance of travel. Yet he knew that this was merely a supposition on his part, for in his letter to Sarpi he acknowledged that he "did not have a principle that was completely unquestionable and could serve as an axiom (*assioma*)." Lacking such a principle, he went on, he was forced to employ one "that has much of the natural and evident about it," namely, that the falling body goes on increasing its speed in proportion to the distances it traverses [GG10: 115]. This principle "being supposed (*questa supposta*)," Galileo hopes that it will suffice "for demonstrating the properties (*accidenti*)" of the motions he has observed [GG10: 115].

The project in which Galileo was then involved, using the wording of the letter and supplementing it by that of other notations on fol. 128, may be represented schematically as follows:

Supposition: A body falling naturally goes on continually increasing its speed according as the distance increases from the point from which it departs [GG8: 373; 10: 115].

Project: To demonstrate, from this supposition, the following experimentally determined properties of the motion:

1. That the distances traversed from the beginning of the motion are as the square of the times; and
2. That the spaces passed over in equal times are as the odd numbers from unity [GG8: 374; 10: 115].

These passages in the letter to Sarpi, taken in conjunction with the notations on fol. 128, have important methodological significance. They show that at this stage of his investigations Galileo was not employing a hypothetico-deductive methodology wherein the truth of his principles would be judged by the truth of the consequences he could derive from them. Although he claimed to know that the times-squared law and the odd-number rule were true, and even believed that he could demonstrate them from the principle he had assumed, such confirmation in his view was not sufficient to establish the truth of the assumed principle. To serve as a principle for a demonstrative science there would have to be independent evidence of its truth, either as *per se nota* in its own right or as demonstrated on other grounds. The way Galileo begins the notation on fol. 128 gives clear indication of his thinking in this regard. "I suppose," he writes, "and perhaps I shall be able to demonstrate this, that the naturally falling body goes on continually increasing its velocity according as the distance increases from the point from which it departed" [GG8: 373]. A supposition is not enough to ground a *scientia*; to do this, it would have to be either true or demonstrated. In 1604 Galileo was optimistic that he could produce such a demonstration but later found that he could not. By 1608 or 1609, however, through the use of other experiments he had discovered the correct principle, and these will be detailed in what follows.

b. *Projection Experiments.* Shortly after performing the experiments that established the times-squared law and the odd-number rule, Galileo seems to have gotten interested in tracing the paths of projectiles, for a number of the diagrams in MS 72 contain parabola-like figures that would be associated with such paths.[18] One folio (81r), dating from about 1605, shows parabolic paths that intersect a horizontal at various levels, together with measurements along these levels, suggesting experiments with bodies projected at different heights above a floor and then impacting on the floor at corresponding distances along the horizontal. Another folio (114v), from about the same period, shows a series of paths from the end of an incline and curving with different degrees of curvature to a horizontal plane a fixed distance below it. These suggest experiments with balls rolled down an inclined plane set on the top of a table and projected onto the floor beneath, hitting the floor at various distances from the end of the table depending on the distances (and thus the speeds) of their roll. Yet another folio (116v), dating from about 1608, suggests a more sophisticated experiment with a steeply inclined plane set back from the edge of the table and fitted with a deflector that could alter the path of the ball's free descent to the floor. When suitably adjusted the deflector would direct the ball horizontally; then it too would follow a series of parabolic trajectories in its fall to the floor, impacting at various distances from the end of the table depending on the heights above the table from which it was dropped. Other adjustments of the deflector would give the ball a slight rise as it was being projected from the table top, and in such tests it would follow yet different paths to the floor. Two additional folios (117r and 175v), dated prior to 116v, seem to provide mathematical calculations associated with these experiments that enabled Galileo finally to reject the supposition he enunciated in his letter to Sarpi. But in its place he apparently came to substitute another principle, namely, that a body falling naturally from rest goes on continually increasing its speed in proportion either to the time of fall or to the square root of the distances it traverses during fall – alternatives that in effect are mathematically equivalent.[19]

None of the folios just mentioned contains expository text, and thus it is difficult to know precisely how the diagrams and calculations they contain are to be interpreted. Various proposals have been made by Stillman Drake and his colleague, James MacLachlan, and more recently by David K. Hill and Ronald H. Naylor, all of whom have duplicated experimentally data recorded on the folios.[20] At this writing a complete consensus has not yet emerged. Yet it seems to be agreed by all that during the period from 1604 to 1608 Galileo was engaged in a research program of discovery as well as of proof, one that involved him in procedures of analysis as well as of synthesis.[21] Thus the evidence is strong that during this period Galileo followed, at least implicitly, the logical procedures of the demonstrative *regressus*. How he may have done so is the problem to which we now turn.

5. ACCELERATION IN FALLING MOTION

Precisely in what year Galileo discovered the correct acceleration principle on which to base a science of falling motion is difficult to ascertain. If David Hill's interpretation of folios 81r and 114v is correct, Galileo would have had experimental grounds to reject the principle that velocity increases with distance of fall by 1605, that is, within a year of his letter to Sarpi.[22] Around this time Galileo had a number of contacts with Jesuits at Padua, including Blancanus and Andreas Eudaemon-Ioannis and possibly even Vallius.[23] These contacts could have been the source of his new principle, namely, that falling motion is uniformly accelerated (*uniformiter difformis*) with respect to time (as opposed to distance) of fall, for this teaching, which derives from Domingo de Soto, was constant among the Jesuits from the time of Toletus onward.[24] Such a teaching would have given Galileo a clue to the correct principle, although it would not have supplied the demonstration for which he was then seeking. This apparently came some time later and is detectable in manuscript fragments wherein references are made to instantaneous speed and to the changes of speed that occur in free fall.

a. *Manuscript Evidence.* Some evidences of an evolution in Galileo's terminology when dealing with speed variations, possibly influenced by Collegio Romano teachings, is to be found in additional folios of MS 72. For example, on fol. 172, dating from 1603–1604, Galileo notes that changes in the speed of motion (*motus velocitas*) down an inclined plane will be proportional to changes in the moments of weight (*gravitatis momenta*), thus correlating speed with weight and using the term *momentum* to designate a particular value of weight at a given place and time. Again, on folio 85v, written in 1604, he describes speed as increasing with distance of fall in such a way that its value "increases continuously at every point along the line of descent" (*velocitas augetur consequenter in omnibus punctis lineae*) [GG8: 383]. Now the usual way of referring to a particular speed, generally adopted in the lectures at the Collegio Romano, was to designate this as a "degree of speed" (*gradus velocitatis*) – a terminology Galileo himself used regularly before 1605. But on fol. 179, which can be dated around 1605 or 1606, Galileo started to substitute for "degree of speed" the expression "moment of speed" (*momentum velocitatis*), and from then on used the latter expression to designate speed at a particular moment while the motion was being accelerated. Such a change, wherein the Latin *momentum*, which can have a wide variety of meanings, came to be used to designate an instantaneous speed, on the basis that the speed was changing continuously throughout the duration of motion, could be an indication that Galileo had already come to the notion of acceleration with respect to time by 1605 or 1606.[25]

However this may be, the earliest incontrovertible written evidence of Galileo's having a principle of uniform velocity increase with time of fall is found on two manuscript fragments that appear to date from 1609. The first of these, on fol. 91v of MS 72, states the principle explicitly, namely, that "in motion from rest the instantaneous speed increases in the same ratio as does the time of the motion" (*In motu ex quiete eadem ratione intenditur velocitatis momentum et tempus ipsius motus*) [GG8: 281n]. Galileo then offers a proof of the principle that he will use almost verbatim in one of his theorems on projectile motion in the *Two New Sciences*. This association gives reason to suspect that it was his experimentation with projectiles, especially those projected from a table top using the techniques described in the previous Section, that led him to the new principle and its demonstration.

The proof itself consists in taking a mean proportional between two distances along a vertical that measure the fall of a body, which distances are assumed to be in the same ratio as the speeds acquired by the body during the fall. By appropriate constructions along the horizontal from the points that mark off the two distances, and invoking the double-distance rule, namely, that a falling body acquires sufficient speed to travel horizontally double the distance of its fall from rest, Galileo is able to show that the ratio of the speed acquired during the fall through the shorter distance to that acquired during the fall through the longer stands in the same ratio as the times of fall through the two distances. This suffices to prove that the body's speed of fall is directly proportional to its time of fall [GG8: 281–282]. Galileo goes no further in the demonstration sketched on fol. 91v, but in its fuller elaboration in the *Two New Sciences*, after repeating the proof verbatim he proceeds to show that if the body is given a uniform horizontal motion during its entire time of fall, the path it then traverses will be that of a semi-parabola [GG8: 282–283]. The juxtaposition of the two proofs is strong evidence that the experiments Galileo recorded on folio 116v were thus the inspiration for his formulating the correct principle for uniform acceleration in free fall [cf. Sec. 6.8a].

The second piece of evidence is the celebrated *De motu accelerato* fragment now bound in MS 71 along with all the other materials of the *De motu antiquiora*, probably inserted there by Galileo himself to furnish the correct answer to the problem he had worked on at Pisa. This document is not so easy to date as is the first fragment, since arguments can be adduced for locating it as early as 1604, as did Favaro in the National Edition, or as late as 1630, as do Drake and Wisan on the basis of its relationship to the *Two New Sciences*.[26] Both Wohlwill and Koyré, however, date it in 1609, and the terminology Galileo employs, together with a few stylistic features of his writing, are compatible with that dating.[27] There seems little doubt that the fragment summarizes important experimental work done by Galileo, presumably at Padua, and is probably a first (or very early) draft of the new treatise on motion he mentions as requiring completion in May of 1610 [GG10: 351–352]. Thus it appears to belong to the end of his Paduan period – a fitting recapitulation of his efforts there to develop "a science of local motion."

b. *Manifesting a Definition.* The accelerated motion fragment ends with a definition of uniformly accelerated motion very similar to that given in the *Two New Sciences*. It reads as follows:

> **DEFINITION**
>
> A motion uniformly or equally accelerated I say is one whose moments or degrees of swiftness increase in fall from rest as does the increment in time itself from the first instant of the motion [GG2: 266; cf.GG8: 198].

Obviously, therefore, the whole intent of the fragment is to establish this definition as one of the principles on which Galileo can erect a new science of local motion. His procedure for doing so takes on particular interest in the context of MS 27, for there Galileo, following Aristotle, takes the position that it is not possible to demonstrate a definition or a quiddity, although it can be "shown" by a process that resembles a demonstration [D1.2.25, cf. Sec. 4.2d]. Now it is not uncommon in the present day for philosophers of science, noting Galileo's invoking of "physical experiments" in the fragment (and later in the *Two New Sciences*, where it seems to refer to the inclined plane experiments) as a confirmation of the definition, to see it as an instance of HD methodology [Sec. 1.2a]. On this account it may prove helpful here to locate the procedure followed in the fragment within the logical context with which Galileo himself was familiar at the time.

Actually the process used by Galileo to establish or manifest his definition is more akin to that of the demonstrative regress than it is to modern HD method. That this is so can be seen by taking the "physical experiments" of the fragment to refer not to the inclined plane experiments alone but rather to the whole range of Galileo's experimentation at Padua, and especially to the projection experiments described above. Since the definition may be seen as a causal definition, the schema of the regress may be used as heretofore to set out the reasoning process by which it was arrived at:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
The various properties of heavy bodies moving with a motion that is naturally accelerated	are caused by their falling at a speed directly proportional to their time of fall

Intermediate stage: *the work of the intellect to see if this really is the case, eliminating other possibilities.*

This is proved kinematically, because only a falling speed directly proportional to the time of fall can produce distances that satisfy the odd-number rule and the times-squared rule in vertical fall, the double-distance rule when the vertical speed is converted to horizontal speed, and the semi-parabolic path when free fall occurs after the vertical speed has been converted to horizontal speed – by geometrical demonstration, but under the physical supposition that all impediments such as friction, the resistance of the medium, and all other accidental factors have been removed.

It is also argued from physical considerations: for nature itself causes the falling motion of a heavy body, which is a natural motion, to increase in the simplest way: by adding equal increments to the speed in equal intervals of time.

It is also argued from disproof of the simplest alternative, since speed does not increase directly with the distance of fall but rather with the square root of that distance.

It is confirmed experimentally, for physical tests show that all these metrical properties are verified within degrees of accuracy that allow for slight departures owing to impediments and accidental causes.

Second progression: *from the cause, recognized formally as the cause, to its proper effect.*

Cause	**Effect**
A heavy body that is naturally accelerated in free fall at a speed that is directly proportional to its time of fall from rest	manifests metrical properties described by the odd-number, times-squared, and double-distance rules and by paths of semi-parabolic projection

The most important observation to be made about the above regressive argument concerns the type of causality it employs. Whereas the previous resolutions Galileo had attempted in the *De motu antiquiora* and *Le meccaniche* had relied on efficient causes such as weight and force, this resolution by-passes physical agency or dynamical factors entirely and concentrates on the formal kinematic relationships that are consequent on the operation of such factors, whatever they might be. Galileo explicitly reduces them to "nature" [GG2: 261], which in an Aristotelian physics is the first principle of motion and rest and thus is the ultimate explanatory factor one can come to, short of doing metaphysics. This is his master stroke, for it enables him to have, as he rightly claimed, a "new science" of local motion that could enjoy the same status as the geometrical optics whose demonstrations were explained in the preceding chapter. There too an efficient agency was presupposed, namely, whatever it is that produces light rays, but consequent on whose action there follow quantitative modalities that can be analyzed rigorously through the use of Euclidean geometry. Here Galileo has something similar, which enables him to apply geometry to the study of local motion in ways no one previously had succeeded in doing.

How Galileo's procedure relates to modern HD method may be illuminated by a remark he makes in the prologue to the definition briefly summarized above. There he contrasts what he is about to do with what has been done by some others (clearly with Archimedes in mind), namely, they arbitrarily make up some kind of motion, say helical or conchoidal, and calculate the properties that follow from it, demonstrating them suppositionally (*ex suppositione*) even though that kind of motion does not occur in nature. Their procedure is commendable, he goes on, but it is not what he has in mind [GG2: 261]. He is sure that nature employs a definite kind of acceleration and he wants to grasp its essential characteristics. To do so, as has been seen, Galileo also has to employ suppositions and thus it would seem that he too is here arguing *ex suppositione*. This is true, but the suppositions involved are very different in the two cases. As explained in an earlier chapter, at least ten kinds of supposition were known in Galileo's day [Sec. 4.2c]. Of the ten enumerated there, only the sixth type, that postulated for purposes of computation alone, applies to the Archimedean use, whereas Galileo's suppositions are much more sophisticated, involving elements that pertain to the third, fourth, eighth, and ninth types. One who is a mathematician alone can carry out the Archimedean type; to carry out the others, one must be a physicist with a good insight into nature's operations. Galileo's claim here is that he is more than a mathematician, that he is a mathematical physicist – with the accent more on the physics than on the mathematics [cf. GG1: 379, Sec. 6.2a] – and that this is what is revolutionary about his discovery.

The difficulty with the HD interpretation, then, is that it represents a logical view of suppositional reasoning, one seen on the basis of form of the suppositions (as in the *Prior Analytics*) without regard for their matter or content (as in the *Posterior Analytics*). Galileo's discourse is of the latter type, not the former. He is concerned not with a formal exercise, but with establishing a proper principle for the new science of local motion, and this is a difficult enterprise. The principle is not self-evident, but it can be discovered by effect-to-cause reasoning, by induction and experimentation, by hypothetical syllogism, and after much agitation of mind (*post diuturnas mentis agitationes*) [GG2: 261; cf. F2.4, Sec. 4.2d]. Galileo is confident that he has successfully concluded his search, so much so that he will use almost exactly the same formulation when he sets out his definitive exposition in the *Two New Sciences* of 1638 [GG8: 197].

6. HYDROSTATICS: THE DISCOURSE ON BODIES ON OR IN WATER

Immediately following the period of experimentation at Padua Galileo began his work with the telescope that would yield the demonstrations schematized in the preceding chapter [Secs. 5.2 and 5.3]. He then moved to Florence at the invitation of the Grand Duke, and shortly thereafter became involved in a controversy with some Aristotelians there, Ludovico delle Colombe among them, over a problem in hydrostatics. The problem goes back to Galileo's student days at Pisa, where the relative merits of Aristotle's and Archimedes' analyses of flotation were much argued, and its recurrence provides Galileo with an opportunity to strike back at his former teacher Buonamici, whose voluminous *De motu* had by then been published (Pisa, 1591). The Grand Duke encouraged Galileo to put his solution into print rather than enter into public debate about it, and he did so, publishing it in 1612 as the *Discourse on Bodies on or in Water* [GG4: 57–151]. Written in Italian, the *Discourse* is important for the methodological remarks with which it begins, reaffirming the demonstrative *regressus* Galileo has been using and showing how it can resolve the problem then being discussed.[28]

a. *Demonstrative Method.* The treatise begins by rejecting Aristotle's teaching about the behavior of bodies in water and aims to clarify "the true, intrinsic, and total cause" of such phenomena [GG4: 67]. This has been treated by Archimedes, Galileo acknowledges, but he seeks now to establish the same conclusion as did Archimedes "with a different method and by other means" [GG4: 67]. This observation is somewhat cryptic, although it resonates with an earlier proposed improvement on Archimedes signalled in the dialogue on motion [Sec. 6.2a] and with the implicit critique of him in the *De motu accelerato* fragment just analyzed [Sec. 6.5]. The improved method consists in "reducing the causes of such effects to more intrinsic and immediate principles," which will enable him to explain "marvelous and almost unbelievable effects" [GG4: 67]. In the reduction, moreover, he will employ "the demonstrative progression" referred to in his Treatise on Demonstration, D3.3.14. This requires him to define his terms and explain his basic propositions, so that he can subsequently use them "as true and manifest" [GG4: 67]. Among his definitions is one for specific gravity (now *grave in ispecie* in place of the *propria gravitas* of his earlier dialogue on motion, which he henceforth prefers as a technical term). His two principles are taken from "the science of mechanics," the first stating that equal weights (*pesi*) moving with the same speed (*velocità*) will exert the same force (*forza*) and moment (*momento*), and the second that the force and moment of a heaviness (*gravità*) increases with the speed of motion [GG4: 68]. The latter principle, he explains, is behind the operation of all machines and was basic to the Aristotelian *Quaestiones mechanicae* [GG4: 69] – an implicit admission that he is supplementing Archimedean statics with Aristotelian kinematics to reach a correct solution.

Precisely how this method differs from the Archimedean becomes clear from criticisms that were directed against the *Discourse* shortly after its publication. Generally Archimedes in his treatises proceeded as a mathematician, emulating the style of Euclid and setting out his teachings in the form of geometrical propositions that were capable of rigorous proof. The Aristotelians of Galileo's day expected him to be the mathematician also, and not to encroach on their preserve, that of natural philosophy. This is apparent in the attack of the "Unknown Academician" (*Academico incognito*) on Galileo's treatise: in his view, mathematical propositions and proofs are incapable of demonstrating "the forces and the true causes" behind the operations of nature [GG4: 165]. His criticism was followed by that of Vincenzio di Grazia, who attempted a more extensive refutation based on texts in Aristotle's *Physics* and *Posterior Analytics*. Each science, he wrote, has its own proper principles and causes, and from these it demonstrates properties of its proper subject; such being the case, it is improper to use the principles of one science to prove properties in another. This is particularly inappropriate where mathematics and physics are involved, since the proper concern of the physicist is motion and the subject of mathematics abstracts from all motion [GG4: 385]. Neither of these adversaries, it appears, was acquainted with the "mixed science" tradition in which Galileo was working, and thus both missed the point of his proposed demonstrations.

Similar interpretations of Aristotelian texts seem to have been much discussed in the Florentine court, and it is worth noting that Galileo gave his personal opinion of them in one of his draft versions of the *Discourse*. Those who argue in this way are apparently unaware that the truth is one, he states, as if geometry would prevent one from developing "a true philosophy." One can be a geometer and a philosopher as well, since the knowledge of geometry does not preclude that of physics, nor does being a geometer prevent one "from treating physical matters physically" [GG4: 49]. What is noteworthy about this statement is that it shows Galileo regarding himself both as a mathematician and as a philosopher who is perfectly capable of making judgments about sensible matter and knowing when quantitative considerations are appropriate or not. His work as an experimentalist had honed his skills in this regard, and thus his methods were superior to Archimedes'. The latter had to tie himself to the postulational techniques of the geometer; Galileo could go beyond them to employ the regressive method endorsed by Aristotle himself and thus discover, with the aid of mathematics, the true causes of natural phenomena. Such a view was consistent with what Clavius had been fostering at the Collegio Romano, and it is noteworthy that Clavius's students, Blancanus and Grienberger, both gave enthusiastic approval to the *Discourse* when it appeared.[29] The Aristotelianism being developed at the Collegio was thus quite different from that professed by Galileo's adversaries at Florence and their predecessors at Pisa and Padua.

b. *The Cause of Flotation.* The main issue being debated at Florence was whether a body floated on the surface of a fluid because its weight was not sufficient to cause it to submerge or because its shape was such that it could not divide the medium and so make its way to the bottom. The solution Galileo proffers at the outset of the *Discourse*, in accord with ideas he had begun to develop in the dialogue on motion [Sec. 6.2a], is that the specific weight of the body, that is, its weight compared to that of an equal volume of the medium, is the cause that determines whether it sinks or rises in that medium. "The cause by which some solid bodies descend to the bottom in water," he writes, "is the excess of their weight (*gravità*) over the weight of water, while conversely, the excess of weight of water over the weight of solid bodies is the cause that others do not descend, and even rise from the bottom and surmount the surface" [GG4: 67]. To show this, he makes use of the concept of specific weight and of geometrical demonstrations to calculate when bodies will sink in a particular medium, when they will rise in it, and, if they float, how much of their volume will protrude above its surface. As a result he concludes that "the true, intrinsic, and proper cause" of flotation phenomena is the differing excesses of the heaviness (*gravità*) of the heavy object and the media in which it is placed [GG4: 79]. In the course of his demonstrations, Galileo also investigates the problem of the amount of water or other medium required to raise a body of less specific weight than the medium, and comes to the surprising conclusion that a very small amount of water can support a weight a hundred or a thousand times heavier than itself. This is one of the "unbelievable effects" to which he had earlier alluded, which he illustrates with the hydraulic lift and then explains in terms of the principle of the balance [GG4: 77–79].

Why a thin plate of ebony floats on the surface of water whereas a ball of ebony sinks – the counter-experiment proposed by Galileo's adversaries – offers him difficulty. His reply to this is that one must consider not only the volume of water displaced by the plate but also the volume of air below the water's surface, assuming that the top of the plate is not wetted by the water, when calculating the factors keeping the plate afloat [GG4: 97–98]. When one does so, one will see that "the true and most proper cause" of the plate's going or not going to the bottom is its specific weight relative to the water [GG4: 108]. In the case where it stays afloat, one must proceed demonstratively (*dimostrativamente*) to uncover the particular accidents (*accidenti*) that cause these effects (*effetti*) [GG5: 109]. The air on top of an unwetted plate, that is, the portion of the air enclosed by ridges and below the water's surface, is the accidental cause or differential factor Galileo finally arrives at to explain the plate's flotation. To do so he invokes an interesting definition of cause, namely, "a cause is that which, being present, the effect is there, and being removed, the effect is taken away" [GG4: 112].[30]

To sum up, then, the demonstrative regress Galileo uses to settle the dispute over hydrostatics that had developed at Florence may be schematized as follows:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
Bodies either float in a medium such as water or they go to the bottom in it	because they have either less or more specific weight than the medium [GG4:67]

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

Hydrostatic principles, properly applied through geometrical analysis, reveal the proper cause of flotation, namely, the relative specific weights of the body and the medium in which it is placed.

The same principles are behind the operation of the hydraulic lift, explaining how a small amount of the medium can support a body many times heavier than itself.

A body's specific weight is thus the essential and immediate cause of its flotation; if its elemental composition is also a factor, this is only a remote and mediate cause.

A body's shape may affect the speed of its motion through a medium, but it is not a proper cause of its motion, as can be seen by experimenting with a mass of wax molded into various shapes.

The special case of a thin plate of ebony floating on water can be explained by an accidental cause: the volume of air enclosed by ridges and below the water's surface, joined to the unwetted top surface of the plate, adds to the plate's buoyancy and causes it to float [GG4: 67–120].

Second progression: *from the cause, recognized formally as the cause, to its proper effect.*

Cause	**Effect**
The true, intrinsic, and proper cause of flotation and submergence, excluding mediate and accidental causes, is the specific weight of a body relative to that of a medium	explaining why a body will float in a medium and others will not, how much will protrude above the surface when it does, and how a medium can support a weight much heavier than itself [GG4: 79]

Noteworthy in the above analysis again is the type of causality Galileo uses in his demonstrations. Although the proper cause he invokes is weight or force, which would seem to be an efficient cause, since he is analyzing an equilibrium condition in which these factors, though presupposed to the analysis, actually cancel out, the demonstrations are made through a formal cause, namely, the dimensive quantity of the bodies involved. This explains, of course, why his geometrical demonstrations can be effective in this case. Again, Galileo attempts to finesse what we now recognize as the surface tension problem through the use of a similar technique, that is, reducing it to an accidental cause tractable by volumetric considerations. Here, as in the early *De motu* and the tidal argument, his facile recourse to accidental or secondary causality to explain phenomena that resist explanation through his primary cause led him into serious difficulties, recognized not only by the philosophers but also by the mechanicians of his day.[31]

7. MECHANICS REVISITED: THE STRENGTH OF MATERIALS

These problems, along with his work on sunspots [Sec. 5.3], kept Galileo occupied well into 1613. He did not get back to his treatise on motion, which he apparently had laid aside in 1610, until 1618, when he took it up briefly only to be sidetracked again by the lengthy dispute with Grassi over the nature of comets [Sec. 5.4b]. Other short periods of work on it came in 1627 and 1631, intermingled with what had become his major preoccupation by that time, the *Dialogue* on the two chief world systems

[Secs. 5.6 and 5.7]. The penultimate sentence of that work, published in 1632, voices Galileo's expectation that a similar discussion can be arranged to lay bare the elements of his "new science" of local motion, both natural and violent – an indication that, despite the many controversies in which he was otherwise involved, the project was never far from his mind.

The opportunity came in a way quite unexpected after the tragic events of 1633. The state in which Galileo was left after the trial, together with the prohibition of any further publication on his part, did not augur well for the new treatise. Yet the archbishop of Siena, Ascanio Piccolomini, in whose residence Galileo was initially detained as part of his sentence, proved to be a gracious and stimulating host, and with his help Galileo was able to resume work on what was to prove to be his masterpiece. All of his manuscripts were preserved intact during his ordeal in Rome, as he was to find when he returned to Arcetri at the end of 1633, and there, with the help of his daughter, Sister Maria Celeste, he regained his composure and began to bring his sciences of mechanics and motion to their final form.

By mid-1635 Galileo had completed the first half of the new work, which would make up the first two days of the dialogue and would be devoted to a new science of mechanics. While patterned to some degree on the Aristotelian *Quaestiones mechanicae*, it was concerned with a more subtle inquisition into the strength of materials of which machines are composed. All are agreed that Galileo incorporated materials he had earlier written at Padua on these mechanical problems [Sec. 6.3], much of which he put into the second day of the dialogue to become the first of "the two new sciences" he announced in the title.

a. *The Cause of Cohesion.* The setting for the first day's conversations is the arsenal of Venice, where the participants in the dialogue are examining ships and weapons under construction. In the spirit of the earlier writings on mechanics, they are intent "on the investigation of the causes of effects that are not only striking, but are also hidden (*reconditi*) and almost unknowable (*quasi inopinabili*)" [GG8: 49]. Their starting point is the commonly accepted principle that "mechanics has its foundation in geometry" [GG8: 50], which seems to be at variance with the experience of artisans in the arsenal who note that, though the geometry of large and small structures is similar, the materials of which they are constructed make the small structure stronger than the large. The reason for the disparity would appear to lie in the forces (*le forze*) that hold materials together, and thus the discussion turns to finding out "what kind of glue keeps the parts of solids so tightly united" [GG8: 56]. Though not tied to any particular method, they are aware that "the moment the cause is discovered their surprise will vanish" [GG8: 53]. And although Aristotle has made a start in this type of investigation, he was not able to prove his results "with necessary demonstrations from their primary and certain foundations" [GG8: 54]. The Academician whose work they are studying (namely, Galileo) has done this, for all his results "have been geometrically demonstrated," and on this account his may truly be called "a new science" of mechanics [GG8: 54].

Despite these claims, by modern standards Galileo is not very successful in tracking down the cause he is seeking. Setting aside materials that have a fibrous structure, he speculates that the cohesion of the parts of solids can be explained in terms of two types of causes, the first being "the repugnance nature exhibits toward a vacuum" and the second "some viscous or pasty glue that firmly binds together the particles of which the body is composed" [GG8: 59]. The existence of the first type of cause is demonstrated "by clear experiments," otherwise well known in the sixteenth century,[32] and here illustrated by the placing together of two flat plates that slide freely over each other but strongly resist any effort to pull them apart [GG8: 59]. The second type proves more difficult to investigate, though Galileo is convinced that it exists in liquids as well as in solids, and in fact will serve to explain why siphons and suction pumps are unable to raise water to a height beyond eighteen Florentine cubits [GG8: 63]. Guided by the principle that "for one effect there must be a single true and optimal cause," he surmises that the gluey effect itself must be caused by minute vacua that hold together the smallest parts of material substances [GG8: 66]. This suggests a further speculation concerning how many such vacua can exist in a finite extent of matter, for the cumulative effect of a very large number would be required to generate any sensible cohesive force [GG8: 67]. The problem will then be solved if one can show that "within a finite continuous magnitude it is possible to discover an infinite number of vacua"; in the same stroke one will have the solution to the paradox presented by the "wheel of Aristotle" [GG8: 68]. The paradox was first discussed in the *Quaestiones mechanicae* and more recently by Bishop Guevara, though there are manuscript indications that Galileo was working on it as early as 1601.[33]

Elsewhere we have analyzed in detail Galileo's search for the "true cause" of cohesion as embodied in his study of Aristotle's wheel.[34] For purposes here only the main lines of his regress in this instance need be schematized. This may be done, as heretofore, as follows:

First progression: *from effect to cause; the cause is materially suspected but not yet recognized formally as the cause.*

Effect	**Cause**
The cohesion of solids, setting aside those with a fibrous structure,	is caused by minute vacua that by nature resist the separation of their parts

Intermediate stage: *the work of the intellect to see if this really is the cause, eliminating other possibilities.*

Such vacua are by definition insensible but their existence can be inferred by *a posteriori* reasoning based on the structure of the continuum.

Since nature abhors a vacuum and will exert force to fill it, the main problem of cohesion is finding a sufficient number of minute vacua in the structure of solids to exert, under nature's agency, a force sufficient to resist attempts to separate its parts.

The continuum is composed not only of quantified parts, which are finite in number, but also of unquantified parts, which may be infinite in number. Its ultimate components, however, are indivisibles that are not further divisible.

Analysis of the paradox posed by the "wheel of Aristotle," replacing the circular shape of the wheels by that of a regular polygon with a large number of sides, shows that the physical continuum contains not only quantified void spaces, which can make up for the excess of the distance traversed by the larger wheel over that traversed by the smaller, but also of an infinite number of unquantified vacua.

Thus there is a sufficient number of vacua to provide, under nature's agency, forces that resist the separation of the quantified parts of the solid, and this is the true cause of its cohesion [GG8: 68–96].

Second progression: *from the cause, recognized formally as the cause, to its proper effect.*

Cause	**Effect**
Nature's abhorrence of the vacua between the quantified parts of a	produces a force that accounts for the tensile strength of solids,

solid causes it to resist the separation of those parts and so	and possibly of liquids also

Galileo's proposed proof, it should be noted, was rejected by Cavalieri[35] – an indication that the student was more expert with indivisibles than his famous teacher. As we analyze it, in his use of regular polygons to substitute for Aristotle's circular wheels Galileo employs an invalid supposition, namely, that whatever is verified in the approach to a limit is also verified at the limit itself.[36] This yielded a true result when Galileo used it earlier in a hydrodynamic argument, for there he proposed that because bodies moving in water encounter less and less resistance the slower their motion, in the limiting case they encounter no resistance at all [GG4: 104]. It fails to do so in the present case, however, for when the interstices in the line being traversed get smaller and smaller as the number of sides of the polygon becomes larger and larger, at the limit now proposed, that is, the circle, they disappear altogether, and thus are ineffective for the task given them.[37]

b. *The New Science of Mechanics.* The second day resumes the treatment of the strength of materials, only now in more systematic fashion than on the first day. According to a prearranged plan, one of the spokesmen brings with him some papers in which "theorems and problems that propose and demonstrate various properties of this subject matter" are presented in rigorous fashion [GG8: 135]. The discussion they evoke is much briefer than that of the previous day, and it is evident that one of Galileo's earlier compositions is here being reworked in dialogue form.

The scope of the discussion is signalled at the outset: the previous dialogue was concerned mainly with the resistance bodies offer to fracture, and there the cause of their coherence was sought mainly in the vacuum [GG8: 151]. But bodies resist differently to a direct pull and to a bending force exerted on them, and since the first type of resistance has already been examined, the second type must now be considered. A beam that is mortised or cantilevered into a wall functions somewhat like a lever when forces are applied at the free end. Such are the cases Galileo proceeds to investigate, for their study enables him to take a variety of resistive phenomena and reduce them to problems that can be solved by a consistent application of the principle of the lever. And although the basis for such a study was laid by Aristotle, it was Archimedes who gave it a rigorous formulation and therefore will be Galileo's guide in its subsequent development [GG8: 152].

From this it is apparent that the program of the second day is to reduce the new science of mechanics to a branch of statics, an undertaking that will reinforce Galileo's thesis that geometry is the most powerful tool available for the solution of physical problems. The propositions and problems that make up this new discipline are not difficult to understand, once this aim is appreciated.

An example of what Galileo regards as a demonstration in this context may be seen in the proof he devises for his tenth proposition, which in reality is a problem that may be schematized as follows:

> **Problem:** Given a prism or cylinder and its weight, and the maximum weight it can sustain at one end, to find the maximum length beyond which the prism itself, if prolonged, will break of its own weight.
>
> **Solution:** Let the prism be AC and its weight D. Extend AC to AH, with AC and AH being in the same ratio as the weight of AC to the sum of the weight of AC and double the weight D. Then the length sought will be AG, where AG is the mean proportional between AC and AH.
>
> **Proof:** Since the downward moment of weight D at C is equal to the moment of a weight double that of D but placed at the middle of AC, which is the center of moment of prism AC, the moment of the resistance at A of prism AC is equivalent to the downward tendency of double the weight D plus the weight of AC, attached at the middle of AC. But the problem is to have this moment of combined weights (double D plus AC) stand in the same ratio to the moment of AC as length AH is to length AC. Now the mean proportional between AC and AH is AG; therefore the moment of double the weight D plus the moment of AC is to moment AC as the square of AG is to the square of AC. But the downward moment of prism AG is to the moment of AC as the square of AG is to the square of AC. Thus AG must be the length sought [GG8: 172].

It is not necessary to follow this reasoning in detail in order to see that the demonstration is geometrical throughout, with the causality involved being that of a quantitative form and not that of an efficient cause. And granted the physical suppositions that have been made about forces, resistances, moments, etc., the argument is just as rigorous as those that have earlier been schematized for the satellites of Jupiter or the phases of Venus, with the exception that these now pertain to the mixed science of mechanics rather than to that of geometrical optics.

Few are the methodological observations made in the course of the second day's conversations. One comment, however, has attracted some attention, and this is Galileo's comparison of geometry with logic as a tool of discovery in the sciences. After having worked through a goodly number of propositions, he has one of the discussants remark that geometry is the mind's most powerful instrument for sharpening its skills and for discoursing and theorizing effectively, thus reinforcing Plato's counsel to his students. Logic, on the other hand, while an excellent instrument for regulating discourse, cannot compare with geometry as a stimulus to invention [GG8: 175]. To put the matter even more clearly, logic enables one to evaluate whether or not arguments and demonstrations that have been already formulated are actually conclusive, but it does not teach one how to go about formulating them. Oddly enough, this is not exclusively a Platonic doctrine – it is in fact the teaching behind the *Analytics*, actually presupposed to the questions Galileo appropriated in MS 27 [F2.2.5; Sec. 2.8]. But he himself developed it in ways unforeseen by Aristotle, seeing in dimensive quantity a *topos* that would promote inquiry into nature in a totally unexpected fashion, and thus giving rise to what was to become a methodological revolution.

8. THE NEW SCIENCE OF MOTION

The third day begins directly with the reading of a Latin manuscript (the earlier papers having been in Italian) entitled "On local motion" (*De motu locali*), which purports to "set forth a very new science dealing with a very ancient subject" [GG8: 190]. The aim of the author in writing it is to report certain properties (*symptomata*) of local motion that are worth knowing but "hitherto have not been observed or demonstrated." Yet the author, clearly Galileo, does remark that some things are known about the subject, for example, that the natural motion of falling objects is continuously accelerated, but that "the ratios according to which the acceleration occurs have not thus far been determined" [GG8: 190]. The fact that Galileo does not claim to have himself discovered continuous acceleration but only its quantitative properties gives support to our earlier speculation that he learned about Soto's teaching from the Jesuits during his years at Padua [Sec. 6.5].

The treatise itself is divided into three books, the first concerned with uniform or equable motion (*De motu aequabili*), the second with naturally accelerated motion (*De motu naturaliter accelerato*), and the third, whose discussion is postponed until the fourth day, with the motion of projectiles (*De motu proiectorum*). The first book, dealing with equal or equable motion, is brief, axiomatic in structure, and sufficiently self-evident, in Galileo's mind, not to require discussion by the participants. Since it is unproblematic from the viewpoint of his logical methodology, we pass over it to deal with the more interesting problems of the second book.

a. *Naturally Accelerated Motion.* This begins with a passage that attempts to set forth, and justify, a definition of naturally accelerated motion that can form the basis for all the properties of such motion that are to be deduced. What is extraordinary about the passage is that it duplicates, with only minor changes, the draft of the first three paragraphs of the *De motu accelerato* fragment contained in MS 71, which have been analyzed above [Sec. 6.5b].

After some discussion of the definition by the participants,[38] Galileo proceeds with the task of systematically developing its consequences on the pattern of a Euclidean-Archimedean mathematical treatise. The new definition is added to the definition and axioms of the first book, and then a premise is explicitly assumed; other principles are introduced informally in the subsequent discussion, and still others are used but never stated in the work. From these Galileo deduces thirty-eight propositions and intersperses among them a number of corollaries and scholia. The propositions are identified as either theorems or problems, twenty-two being the former and sixteen the latter. Here Galileo follows the usual convention: if the proposition indicates something to be proved, it is a theorem, whereas if it states something to be done (usually requiring a construction), it is a problem.

The assumed premise, which Galileo refers to as a *supposto* or *postulato*, states that the degrees of speed acquired by one and the same body moving down planes of different inclines are equal when the heights of the planes are equal [GG8: 205]. He seems to have become convinced of the truth of this statement between 1600 and 1604, but he was unable to work out a satisfactory proof for it and so here presents it suppositionally.[39] Yet this does not prevent him from hinting at its obvious and self-evident character and noting that it "ought to be conceded without argument." Apart from its verisimilitude, moreover, he notes that an experiment can be adduced in its support that falls little short of a "necessary demonstration" [GG8: 205]. The experiment is one with which Galileo had long been fascinated, wherein a single pendulum is allowed to swing from a nail driven into a wall so that its plane of oscillation is parallel to the wall and only two digits in front of it. When a horizontal line is drawn on the wall from the point at which the bob is released, it is found that the bob practically reaches the same line on its upswing, with the very slight shortage that prevents it from getting there exactly (*precisamente*) being attributable to the resistance (*impedimento*) of the air and the string [GG8: 206]. Moreover, if the length of the pendulum is effectively shortened at the moment when it passes the vertical by having another nail driven in the wall below the point of suspension, and even below the horizontal line, the bob will "always terminate its rise exactly at that line" [GG8: 206]. This experiment "leaves little room to doubt the truth of the supposition," and yet some difficulty is posed by the pendulum traversing circular arcs rather than the straight lines of the inclined plane, and so Galileo agrees to take it as a postulate [GG8: 207]. Its absolute truth, he observes, will be established when consequences derived from it will be seen "to correspond with and agree exactly with experiment" [GG8: 208]. These observations suggest that Galileo regarded this particular supposition as pertaining to types 3, 4, or 5 of those listed in Sec. 4.2c above, and thus as capable of supporting the scientific conclusions he is about to adduce from it.

The first of these, sometimes referred to as the mean-speed theorem, states that the time in which any space is traversed by a uniformly accelerated body moving from rest is equal to the time in which the same body would traverse the same space when moving uniformly at a degree of speed one half that of the highest degree attained [GG8: 208]. The second is the famous times-squared theorem [GG8: 209], which Galileo already knew in 1604 but whose proof eluded him at that time. From this a number of interesting consequences follow, one of which Galileo presents as his first corollary, generally known as the odd-number rule, namely, that a body starting from rest will traverse, during equal intervals of time, distances that are related to each other as the odd numbers beginning with unity [GG8: 210–212].

These results again evoke a challenge from the discussants: such conclusions undoubtedly follow from the definition of accelerated motion that has been proposed, but one may still wonder if this is "the acceleration nature employs in the descent of falling bodies" [GG8: 212]. The request is made for some type of experimental proof, and Galileo sees this as most reasonable for sciences such as he is developing, that is, "sciences in which mathematical demonstrations are used to arrive at physical conclusions" [GG8: 212]. He then proceeds to explain his experiments with balls rolling down inclined planes, noting particularly how he went about measuring distances and times of travel, and concluding with the observation that the times of descent along different inclines maintained precisely (*esquisitamente*) the ratio he had "assigned and demonstrated" for them [GG8: 213]. It should be noted that the experiment is offered as direct proof of the times-squared law and not of the definition of naturally accelerated motion, but there seems little doubt that Galileo also regarded it as indirect proof of the latter.

So numerous are Galileo's theorems in Book 2 that it is not practical to list them all in sequence. Some idea of his development of a science of uniformly accelerated motion may be gained, however, from the following summary of the premises and theorems at the beginning and at the end of the book:

> **Definition:** A uniformly accelerated motion is one which, starting from rest, during equal intervals of time acquires equal moments of swiftness [GG8: 198].
>
> **Supposition:** The degrees of speed acquired by one and the same body moving down planes of different inclines are equal when the heights of the planes are equal [GG87: 205].
>
> **Theorems:**
> 1. The time in which any space is traversed by a body uniformly accelerated from rest is equal to the time in which the same body would traverse the same space when moving uniformly at a degree of speed one half that of the highest degree attained [GG8: 208].
> 2. A body descending from rest in uniformly accelerated motion traverses, in any times whatever, spaces that are as the squares of those times [GG8: 209].
> 3. The times of descent of a body starting from rest on an inclined plane and also along a vertical of the same height are to each other as the length of the plane and that of the vertical [GG8: 215].

4. The times of motion over equal planes, unequally inclined, are to each other inversely as the square root of the ratios of the lengths of those planes [GG8: 219].
5. The ratio of times of descent along planes differing in incline and length, and of unequal heights, is compounded from the ratio of the lengths of those planes and from the inverse ratio of the square roots of their heights [GG8: 220].
6. The times of descent along any inclined planes whatever drawn from the highest or the lowest point of a vertical circle to its circumference are equal [GG8: 221].

...

20. If a straight line is inclined at any angle to the horizontal and if, from any assigned point in the horizontal, a plane of quickest descent is to be drawn to the inclined line, that plane will be the one that bisects the angle contained between the two lines drawn from the given point, one perpendicular to the horizontal line, the other perpendicular to the inclined line [GG8: 251–252].
21. If in a horizontal line any two points are chosen and if through one of these points a line be drawn inclined towards the other, and if from this other point a straight line is drawn to the inclined line in such a direction that it cuts off from the inclined line a portion equal to the distance between the two chosen points on the horizontal line, then the times of descent along the line so drawn is less than along any other straight line drawn from the same point to the same inclined line. Along other lines that make equal angles on opposite sides of this line the times of descent are the same [GG8: 253].
22. If from the lowest point of a vertical circle an inclined plane is drawn subtending an arc not greater than a quadrant, and if from the two ends of this plane two other planes are drawn to any point on the arc, the time of descent along the other two planes will be shorter than along the first, and shorter also than along the lower of the other two planes [GG8: 261–262].

All of the above theorems are accompanied by diagrams, many quite elaborate, and are given strict geometrical demonstrations – signalled by the Q.E.D.'s with which they terminate. Lemmas are introduced from time to time to simplify the proofs, and corollaries and an occasional scholium are also introduced. As in the demonstrations of the first book, all of the proofs are kinematical and are made through formal causality alone, being based simply on the formal relationships that hold between space, time, and speed. As heretofore, the exercise of efficient causality is merely presupposed and does not enter into any of the proofs as such.

Some of Galileo's intermediate theorems not listed above, however, do have dynamical overtones in that they investigate the properties of motions that combine vertical fall with descent along an incline. These suggest the table-top experiments made during the Paduan period – not described in the text and relating more directly to the theorems to be discussed on the fourth day. A typical statement, repeating a theme found in Galileo's earlier writings, is found in a scholium following Problem IX. This is that any degree of velocity "will be by its nature indelibly impressed" on a moving body, "provided external causes of acceleration or retardation are removed"; this situation, however, "occurs only on a horizontal plane" and therefore "motion along the horizontal is also eternal" [GG8: 243]. The reason Galileo alleges for this is that if the plane slopes downward "a cause of acceleration" is present, and if upward "a cause of retardation"; if neither, the motion must be uniform or equable, and thus not weakened or diminished, much less taken away [GG8: 243]. Noteworthy here is not only the causal analysis but the result to which it leads, similar to that in the *De motu antiquiora* [Sec. 6.2c] and the *Letters on Sunspots* [Sec. 5.3], except that a horizontal plane is now substituted for a surface that remains always at the same distance from the universal center of gravity. Moreover, Galileo now maintains that any motion imparted to a body will be "connatural to it, indelible, and eternal," and so will "conserve itself permanently" until acted upon by subsequent causes [GG8: 243–244]. This prepares the way for a mathematical analysis of motions that are projected upward after downward descent and others with which he undoubtedly experimented during his Paduan period, and which provide the basis for his final book.

b. *Projectile Motion.* The fourth day, as already indicated, is devoted to the contents of the third and last book of *De motu locali*, concerned with the motion of projectiles. In it Galileo proposes to show how "certain principal properties," which he proposes "to establish by solid demonstrations," come to a body when its motion is compounded of two displacements, one uniform and the other naturally accelerated, such as is found in projectiles [GG8: 268]. With regard to the uniform component, Galileo illustrates this with a body projected along a horizontal plane, "mentally conceived, lacking all impediments"; such a body would have a motion that is uniform and perpetual if the plane were extended to infinity [GG8: 268]. If the plane is terminated, however, and set in an elevated position (and here Galileo seems to be thinking of a table top), a heavy body passing over its edge will acquire, in addition to its previous uniform motion, a downward propensity or component caused by its own weight. The motion that results, therefore, will be composed (*compositus*) of one that is horizontally uniform and another that is naturally accelerated downward, and it is this motion that he calls "projection" (*proiectio*) [GG8: 268].

The first of the properties of such a motion that Galileo proposes to demonstrate is stated as Theorem I of the third book, namely, that it follows the path of a semi-parabola [GG8: 269]. The proof requires some preliminary knowledge of conic sections, which is set forth in standard mathematical fashion, and when "we presuppose" (*suppogono*) such matters, the demonstration follows in a straight-forward manner [GG8: 272–273]. In the discussion that ensues it is noteworthy that these propositions taken from mathematics are not questioned at all; what is questioned, on the other hand, is the reasoning that enables them to be applied to the physical motion being investigated. So it is quickly pointed out that, although the argument is novel, ingenious, and conclusive, being made *ex suppositione*, it does "suppose" that transverse motion is always uniform, that downward motion is always accelerated according to the squared ratio of the times, that the resulting velocities can be added together without altering, disturbing, or impeding one another in any way, and that the path will not change ultimately into a different curve altogether [GG8: 273]. The last possibility seems inevitable on the basis of the projectile's tendency to seek the center of the earth after its fall, for if it begins its parabolic trajectory at some point directly above the earth's center, the farther it travels along the parabola the more it will depart from the earth's center, and this is contrary to its natural tendency [GG8: 274]. And the first supposition is questionable also, for to use it "we must presuppose (*noi supponghiamo*)" that every point on a horizontal plane is equidistant from the earth's center, which is not true, so that effectively motion along such a plane will "always be uphill." Moreover, "it is impossible to eliminate entirely the resistance of the medium." All of these difficulties thus make it highly improbable that the results demonstrated "from such unreliable suppositions (*con tali supposizioni inconstanti*) can ever be verified in actual experiments (*nelle praticate esperienze*)" [GG8: 274].

Since these criticisms are the most pointed that can be made against the new science of motion, it is instructive to see how Galileo handles them. He is quite ready to admit that conclusions demonstrated in the abstract are altered in the concrete, and in this sense can be falsified, but he notes that the same objection can be raised against Archimedes and other great men who employed suppositions of a like kind. In his demonstration of the law of the lever, for example, Archimedes supposed that the arm of a balance lies in a straight line equidistant at all points from the common center of gravity, and that the cords to which the weights are attached hang parallel to one another. Such licence (*licenza*) is permissible when one is dealing with distances that are very small compared to the enormous distance to the earth's center [GG8: 274–275]. One should recall, however, that Archimedes and others based their demonstrations on the suppositions that the balance should be regarded as "removed an infinite distance from the center of the earth." Granted this supposition, his results "are not falsified" but rather are drawn "with absolute proof" [GG8: 275]. Finally, the objection from the resistance of the medium must be handled in an analogous way. Galileo is quick to admit that there are so many factors affecting the motions of bodies that it is almost impossible to have "a firm science" of them. But if one wishes to treat such matters "scientifically," one "needs to abstract" from impediments of this type. One must find and demonstrate conclusions "in abstraction from such impediments," and then be able to use them in practical situations "under limitations that can be learned through experimentation" [GG8: 276]. In this way the resistance of the medium can be minimized, and, if proper apparatus is employed, "external and accidental impediments" can be reduced to the point where they are hardly noticeable (*pochissimo notabili*) [GG8: 276].

In effect, what Galileo has done throughout this passage is distinguish various types of supposition that must be employed to develop a mathematical physics, a full enumeration of which has been given above in Sec. 4.2c. First there are terms and definitions taken over from the abstract science of mathematics, which generally pose no difficulty (type 2). Then there are other suppositions that are made when applying such abstract definitions to the geometry actually found in physical situations, and here some adjustments must be made if simplifications are introduced; these are legitimate if one is aware of the orders of magnitude involved, and if the results demonstrated *ex suppositione* depart only in an insignificant way from those that would be deduced without the simplifications (type 9). Finally there are suppositions that look to the impediments that are omnipresent in nature and that seem to preclude the scientific treatment of phenomena as complex as motion (type 8). These are more difficult to formulate, but if one is sufficiently ingenious in techniques of experimentation, one will learn how to abstract even from these extrinsic and accidental factors and so arrive at the essences of the motions being investigated. In the last case, no less than in the second, one cannot expect a perfect fit between nature and mathematics. All that is required, however, is close enough agreement so that experimentally verified results do not depart significantly from those calculated on the basis of the suppositions.

The remainder of Book Three consists of only fourteen propositions, some of which are theorems and others problems, the last nine of which describe the main mathematical properties of parabolic paths and directions for the computation of trajectories based on them. The second proposition, stated as a theorem, supplies the rule of vector addition for the composition of velocities [GG8: 280], which manuscript evidence shows was known to Galileo by 1609. The same can be said of the third proposition, also stated as a theorem [GG8: 281–283], a draft of whose proof was written by 1609 and is contained on fol. 91v of MS 72 [cf. Sec. 6.5a]. Since this draft contains a proof for the definition of naturally accelerated motion based on parameters that can be measured accurately, there is good reason to believe that it was these experiments with parabolic paths, rather than the inclined-plane experiments recounted on the third day, that convinced Galileo of the truth of his definition. Such a proof would, of course, be *a posteriori*, for it would reason from the quantitative effects of such motion to its nature or essential characteristics, as these were set out early in the third day.

As in the case of uniformly accelerated motion, it would be impractical to summarize Galileo's demonstrations of the properties of projectile motion in their entirety. The basic lines of his development, however, may be gathered from the seven theorems he proves, which may be schematized as follows:

> **Definition:** Projection is the motion that results when a heavy body, moving with uniform horizontal motion to the edge of a plane, begins to be naturally accelerated as a result of the downward tendency from its heaviness being added to its previous equable and indelible motion [GG8: 268].

Theorems:

1. A projectile that is carried by a uniform horizontal motion compounded with a naturally accelerated downward motion describes a semi-parabolic line in its movement [GG8: 269].
2. If a body is equably moved in double motion, that is, horizontal and vertical, the square of the resultant momentum is equal to the sum of the squares of the two component momentums [GG8: 280].
3. If a body accelerates from rest along line AB, and if its speed at some point C intermediate between A and B is AC, then its speed at B is to its speed at C as AS is to AC, where AS is the mean proportional between AB and AC [GG8: 281].
4. If projectiles describe semi-parabolas of the same amplitude, the impetus required to describe the one whose amplitude is double its altitude is less than that required for any other [GG8: 294].
5. The amplitudes of two parabolas described by projectiles fired with the same impetus, but at angles of elevation which exceed and fall short of half a right angle by equal amounts, are equal to each other [GG8: 297].
6. The amplitudes of parabolas are equal when their altitudes and sublimities are inversely proportional [GG8: 298].
7. The impetus or momentum of fall through any semi-parabola is equal to the momentum of natural vertical fall through a distance equal to the sum of the sublimity and the altitude of the semi-parabola [GG8: 299].

The demonstration of each of these theorems again requires a complex diagram, plus the understanding of various lemmata describing the properties of conic sections. As heretofore, the action of efficient agents is presupposed and does not enter explicitly into the proofs, all of which are made through formal causality, that is, through various relationships holding between kinematic factors such as distance, time, and speed (or momentum, or impetus) of travel.

c. *Suppositions Rejoined.* Of the remainder of Galileo's writings, three letters are of interest for the reflections they contain on his methods and his use of suppositions in formulating his demonstrations. The first was written to Pierre de Carcavy at Paris on June 5, 1637, while the *Two New Sciences* was still in press, and contains Galileo's answer to a query from the French mathematician, Pierre Fermat, concerning a passage in the

Dialogue of 1632 [GG7: 190–193]. In his reply Galileo affirms that in the passage criticized he is arguing *ex suppositione*, postulating a motion that departs from rest and increases its velocity in the same ratio as the time increases, and of this motion he "conclusively demonstrates many properties" [GG17: 90]. He notes further that if experiment (*esperienza*) were to show that these were the same properties as were observed in the motions of bodies falling naturally, he could affirm that the latter was the motion he had "defined and supposed"; even if not, however, his demonstrations would have been just as valid as those of Archimedes concerning motions along spiral lines, which do not occur in nature [GG17: 90]. But in the case of the motion he has supposed, writes Galileo, "it has happened (*e accaduto*) that all the properties demonstrated are verified in the motion of bodies falling naturally." They are verified, he emphasizes, in this way, "that howsoever we perform experiments (*esperienze*) on the earth's surface, at heights and distances that are practical there, we do not encounter a single sensible difference," even though he is aware that such a difference would not only be sensible but very great if one were to perform them closer to the center of the earth [GG17: 91]. The reply is important not only for the information it provides about the precision of Galileo's measurements but also for its implication that the cumulative effect of all his experiments, and not merely one or another, is what assures him of the truth of the definition he has supposed.

The second letter, written by Galileo to Giovanni Battista Baliani in Genoa on January 7, 1639, after the publication of the *Two New Sciences*, repeats in summary form the information contained in the letter to Carcavy. Here too Galileo is clear that his argument is made *ex suppositione* and that, like Archimedes' demonstration, his would be valid whether or not the motion such as he had defined it actually corresponded to that found in natural fall. "But in this matter," he goes on, "I have been lucky, so to speak, for the motion of heavy bodies, and the properties thereof, correspond exactly (*respondono punctualmente*) to the properties demonstrated by me of the motion as I have defined it" [GG18: 12–13]. Here, and in the surrounding passages, Galileo makes the point that it is a large number of precise experimental confirmations that assure him that the definition of naturally accelerated motion he has supposed accurately portrays the one nature employs. And his acknowledgement to Baliani that he was "lucky" in obtaining these confirmations indicates that these experimental investigations caused him much trouble, and that it was not without considerable "agitation of mind" (as he put it in the *Two New*

Sciences [GG8: 197] and even earlier in the *De motu accelerato* [GG2: 261]) that he was able to assure himself that his supposition was ultimately justified.

Galileo's final reference to his "suppositions," it would appear, occurs in yet another letter he wrote, this time to Fortunio Liceti on September 14, 1640. This is of particular importance for our study, for in it Galileo sets out in more general terms what it means to be a true follower of Aristotle and then allows, surprisingly, that in this sense he is really an Aristotelian himself. The passage reads as follows:

> I consider (and I believe you do too) that to be truly a peripatetic – that is, an Aristotelian philosopher – consists principally in philosophizing according to Aristotelian teachings, proceeding from those methods and those true suppositions and principles on which scientific discourse is founded, supposing the kind of general knowledge from which one cannot deviate without great disadvantage. Among these suppositions is everything that Aristotle teaches us in his logic, pertaining to care in avoiding fallacies in discourse, using reason well so as to syllogize properly and deduce from the conceded premises the necessary conclusion, and all this teaching relating to the form of arguing correctly. As to this part, I believe that I have learned sureness of demonstration from the innumerable advances made by pure mathematicians, never fallacious, for if not never, then at least very rarely, have I fallen into mistakes in my argumentation. In this matter, therefore, I am a peripatetic [GG18: 248].

Needless to say, the confidence Galileo here expresses in his ability to use Aristotelian logic, both in its material and its formal aspects, would be almost impossible to understand if MS 27, with his appropriated treatises on the *Posterior Analytics*, had not survived.

But now that these treatises, their antecedents, and their sources have been made available, and now that we are aware that all three make adequate allowance for the conformity of mathematical and mixed sciences to Aristotelian canons, we can appreciate the project in which Galileo was involved throughout his life. This was simply that of combining mathematics and physics in a way never attempted before to unveil the hidden causes behind nature's operations. Some have questioned his sincerity in thus expressing himself to Liceti only sixteen months before his death, but Stillman Drake argues that "at Galileo's advanced age, especially in private letters, he had no reason to dissemble."[40] In Drake's view Galileo's correspondence with Liceti "remains his last will and testament on the relation of science as he saw it with philosophy as it was practiced, and is as deserving of study as anything else he ever wrote."[41]

Perhaps it is not too much to expect that the worth of this volume will be judged on the extent to which it permits Galileo's testament to be taken on face value.

NOTES

[1] On Buonamici, see Helbing, *Buonamici*, 86–97, 352–396; on Fantoni, see Essay 10 of Schmitt's *Studies in Renaissance Philosophy and Science* as well as his entry on Girolamo Borro in the *Dictionary of Scientific Biography*, ed. C.C. Gillispie, 16 vols., New York: Charles Scribner's Sons, 1970–1980, 15: 45. Additional details concerning all three are given in the last section of the Introduction to *Galileo's Logical Treatises*. For a related discussion of Galileo's Archimedean beginnings in the context of the logical teachings contained in MS 27, see *Galileo and His Sources*, 219–230.

[2] *Galileo and His Sources*, 91–92, 225; see also the following note.

[3] The contents of MS 71, as well as the background to the *Theoremata*, are explained in fuller detail in the Introduction to *Galileo's Logical Treatises*.

[4] Helbing makes this point in his *Buonamici*, 361; see also his earlier discussion of Buonamici's project in writing *De motu*, 28–38, 64–86.

[5] See the Introduction to *Galileo's Logical Treatises*; additional methodological background is provided in *Galileo and His Sources*, 230–248.

[6] Mazzoni, a distinguished humanist who in 1597 published a scholarly work on Dante, had joined the faculty at Pisa slightly before Galileo and was soon to become his close friend. Mazzoni entered the philosophy cycle at a point where he would be teaching *De caelo* in 1589, along with Buonamici. That Galileo and Mazzoni were collaborating is clear from a letter written by Galileo to his father on November 15, 1590 [GG10: 44–45]; see *Prelude to Galileo*, 227. Possible influences of Mazzoni on Galileo, and particularly their joint study of Benedetti's work, are discussed in *Galileo and His Sources*, 225–230. Additional comments will again be found in the last section of the Introduction to *Galileo's Logical Treatises*.

[7] For a detailed analysis of Galileo's reasoning and why it is invalid in this context, see our "The Problem of Apodictic Proof in Early Seventeenth-Century Mechanics: Galileo, Guevara, and the Jesuits," *Science in Context* 3 (1989), 67–87.

[8] See Drabkin's translation of the *De motu* in Galileo Galilei, *On Motion and On Mechanics*, Madison: The University of Wisconsin, 1960, 35 n. 18.

[9] *Galileo and His Sources*, 162–163, 240.

[10] This is described generally by Christopher Clavius in the introduction to his second edition of Euclid's *Elements* (Rome: Apud Bartholomaeum Grassium, 1589), and in more detail by Clavius's student, Joseph Blancanus, in his *Apparatus ad mathematicas addiscendas et promovendas* (Bologna: Typis Sebastiani Bonomii, 1620). A yet fuller exposition will be found in Ioannis de Guevara, *In Aristotelis Mechanicas Commentarii, una cum additionibus quibusdam ad eandem materiam pertinentibus* (Rome: Apud Iacobum Mascardum, 1627). For an overview, see *Galileo and His Sources*, 136–148, 206–216.

[11] This work is not found in the National Edition but has been transcribed by Stillman Drake in his "Galileo Gleanings V: The Earliest Version of Galileo's *Mechanics*," *Osiris* 13 (1958), 262–290. The text is found on 270, and is cited here and hereafter with the letters GM to differentiate it from references to the National Edition.

[12] Further reflections on Galileo's first treatise on mechanics are offered in *Galileo and His Sources*, 248–251.

[13] *On Motion and On Mechanics*, 171 n. 26.

[14] See also the analysis of *Le meccaniche* in *Galileo and His Sources*, 251–254.

[15] *Galileo: Pioneer Scientist*, Toronto: University of Toronto Press, 1990, 11–15.

[16] *Galileo: Pioneer Scientist*, 21.

[17] *Galileo: Pioneer Scientist*, 9–31.

[18] Most of these diagrams are contained on various folios of MS 72, especially 81r, 114v, 116v, 117r, and 175v, as described in what follows. Photoreproductions of these folios, together with various interpretations of them, will be found in the articles cited in n. 20 below. For the full collection see *Galileo's Notes on Motion*, arranged in probable order of composition and presented in reduced facsimile by Stillman Drake, Florence: Istituto e Museo di Storia della Scienza, 1979.

[19] The first following the interpretation of Ronald H. Naylor, the second that of David K. Hill. See the most recent publications of these authors cited in the following note.

[20] The more significant work thus far reported in the literature includes the following: Stillman Drake, "Galileo's Experimental Confirmation of Horizontal Inertia: Unpublished Manuscripts," *Isis* 64 (1973), 291–305; Drake and J.H. MacLachlen, "Galileo's Discovery of the Parabolic Trajectory," *Scientific American* 232 (1975), 102–110; R.H. Naylor, "The Evolution of an Experiment: Guidobaldo del Monte and Galileo's *Discorsi* Demonstration of the Parabolic Trajectory," *Physis* 17 (1974), 323–346; Naylor, "Galileo: The Search for the Parabolic Trajectory," *Annals of Science* 33 (1976), 153–172; Naylor, "Galileo's Theory of Projectile Motion," *Isis* 71 (1980), 550–570; Naylor, "Galileo's Method of Analyis and Synthesis," *Isis* 81 (1990), 695–707; D.K. Hill, "A Note on a Galilean Worksheet," *Isis* 70 (1979), 269–271; Hill, "Galileo's Work on 116v: A New Analysis," *Isis* 77 (1986), 283–291; and Hill, "Dissecting Trajectories: Galileo's Early Experiments on Projectile Motion and the Law of Fall," *Isis* 79 (1988), 646–668.

[21] Such a procedure is explicitly stated in the title of Naylor's 1990 essay, "Galileo's Method of Analysis and Synthesis."

[22] See his "Dissecting Trajectories...," 661–662.

[23] Eudaemon-Ioannis had taught logic and natural philosophy at the Collegio Romano in 1596–1597 and 1597–1598 respectively, after which he was sent to teach at Padua. According to the testimony of Mario Guiducci, it was to Eudaemon that Galileo recounted the details of an experiment he had performed in which a body was let drop perpendicularly from the mast of a ship and was found to fall at the foot of the mast whether the ship was in motion or at rest [GG13: 205]. On Blancanus's and Eudaemon's contacts with Galileo at Padua, see *Galileo and His Sources*, 269–272. Since publishing that account I have heard from Mario Biagioli, in a personal communication dated November 8, 1989, that while working in the Roman Archives of the Society of Jesus he uncovered a letter indicating that Vallius had also been at Padua teaching natural philosophy up to 1601. Corroborating details are provided by Ugo Baldini, also based on work in the Roman Archives, in his extended review of *Galileo and His Sources* in the *Archives internationales d'histoire des sciences* 39 (1989), 357–367.

[24] See our "The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," *Isis* 59 (1968), 384–401, and "The Early Jesuits and the Heritage of Domingo de Soto," *History and Technology* 4 (1987), 301–320; also *Galileo and His Sources*, 269-272.

[25] For a complete analysis of Galileo's use of the term *momentum*, see Paolo Galluzzi, *Momento: Studi galileiani*, Rome: Edizioni dell'Ateneo & Bizzarri, 1979. On 136–137 Galluzzi cites the texts in which St. Thomas Aquinas uses *momentum* for the indivisible of motion, which would be consistent with Galileo's usage and might indicate yet another Jesuit influence on his thought.

[26] Drake in his *Galileo at Work*, 315; Wisan in her "The New Science of Motion: A Study of Galileo's *De motu locali*," *Archive for History of Exact Sciences* 13 (1974), 277–278, nn. 2–4. For a methodological analysis of the fragment see *Galileo and His Sources*, 272–276.

[27] See *Galileo and His Sources*, 273 n. 105.

[28] Fuller background is provided in *Galileo and His Sources*, 284–288.

[29] Blancanus in his *De natura mathematicarum scientiarum*, Bologna: Apud Bartholomeum Cochium, 1615, 62–64; Grienberger in a letter reprinted in the National Edition, GG11: 477.

[30] Drake sees this definition as a novel contribution to scientific method, arguing that it describes causes in a way that makes them specially apt for scientific inquiry; see his introduction to *Cause, Experiment and Science*, Chicago-London: The University of Chicago Press, 1981, xxv–xxvii. It is interesting to note that Galileo's teacher at Pisa, Buonamici, gives essentially the same definition in his *De motu*, 495; on this, see Helbing, *Buonamici*, 83–85. Actually there is nothing novel about it, since Galileo's aim is to find the intrinsic cause of flotation [GG4: 67], and intrinsic causes are always coexistent with their effects; extrinsic causes, on the other hand, may coexist with their effects or they may precede or follow them. Galileo touches on this teaching in his MS 46; see our *Galileo's Early Notebooks*, 50.

[31] For more particulars see W.R. Shea, *Galileo's Intellectual Revolution: Middle Period, 1610–1632*, New York: Science History Publications, 1972, 14–27.

[32] See Schmitt, *Studies in Renaissance Philosophy and Science*, Essay 7; entitled "Experimental Evidence for and against a Void: The Sixteenth-Century Arguments," which first appeared in *Isis* 58 (1967), 352–366.

[33] Drake, *Galileo: Pioneer Scientist*, xiii, 85.

[34] In "The Problem of Apodictic Proof" cited in n. 7 above.

[35] In a letter to Galileo of October 2, 1634 [GG16: 136–137], as noted by W.R. Shea, "Descartes as Critic of Galileo," *New Perspectives on Galileo*, 154, n. 46.

[36] "The Problem of Apodictic Proof," 79.

[37] A fuller presentation of the first day's discussion is in *Galileo and His Sources*, 299–303.

[38] For a summary, see *Galileo and His Sources*, 323–327.

[39] Drake notes that an attempted demonstration of this postulate was added at Galileo's request to editions after 1638, *Two New Sciences*, 162 n. 19. He includes the addition in his translation, 171–175.

[40] *Galileo at Work*, 410.

[41] *Galileo at Work*, 410.

EPILOGUE

The outcome of these *prolegomena* to Galileo's appropriated treatises on Aristotle's *Posterior Analytics* may surprise historians and philosophers of science. After all, its central finding is quite simple: that Galileo employed the demonstrative regress of the Paduan Aristotelians, transmitted to him via the lecture notes of Jesuits at the Collegio Romano, to give causal explanations of phenomena he was the first to discover in the emerging sciences of astronomy and mechanics. Galileo felt that these explanations yielded true and certain knowledge on a par with the truth and certitude one attains in ordinary experience of the world of nature. There was, for him, nothing esoteric or deeply metaphysical about his newly discovered truths. For the most part they were established by a straightforward combination of mathematical and physical reasoning in the "mixed science" tradition of his day. If one could understand how sense observation of the phases of the moon, carefully analyzed, can yield apodictic knowledge that a remote object like the moon is a sphere, one would have no difficulty assenting to the startling explanations Galileo gave for what he had seen through his telescope: mountains on that moon, other moons revolving about Jupiter, Venus revolving around the sun. And the same geometrical-physical type of proof, based now on suppositions of the Archimedean type that fitted into his model of the *regressus*, enabled him to move beyond the ancient statics to a new kinematics or dynamics, one that demonstrated unexpected properties of bodies in local motion as well as at rest.

The very simplicity of this analysis of MS 27 and the insight it offers into Galileo's logical methodology clears up a problem that has bothered historians of science from Viviani to the present. Puzzles are cryptic for those who lack the key; invariably the solvers create complexity in their attempt to find it. In Galileo's case we have seen those who characterize him as an empiricist but overlook rationalist elements in his thought; others who do the opposite, making him a Platonist while disregarding his pervasive use of observation and experiment; yet others who see him pursuing an erratic course from rationalism to empiricism to skepticism and back to rationalism again; still others who try to fit him into a Kantian mold; and finally others who accuse him of ambiguity and inconsistency because he doggedly adhered to an ideal of science they feel is forever beyond man's reach. Galileo would not have seen himself in any of these characterizations. His logical methodology was set out in straightforward fashion in one of the first notebooks he wrote, and, what is even more remarkable, was clearly reaffirmed in one of the last letters he wrote. When the missing piece of the puzzle is supplied, in this case MS 27, his life's work becomes clear, and so does his place in the history of scientific thought.

The many arguments among historians of science over continuity in their discipline have never yielded a satisfactory solution. The few who work in Babylonian and Egyptian science may see themselves as quite remote from their colleagues, and so perhaps may those who specialize in Greek science. But the rebirth of medieval and Renaissance science in the latter half of our century awakened expectations that seem now unfulfilled, so much so that one wonders about the inclusion of all these subdisciplines within the history of science itself. The sticking point is first of all Aristotle, and after him Galileo. Those who work in the history of Greek, medieval, and Renaissance science know how dominated it is by the thought of Aristotle. And Galileo is invariably presented by historians as the watershed, the person who succeeded almost single-handed in overturning Aristotle and setting science on a new course that would bring it down to the present day. Despite the continuity of name, for it is all labelled "science," it seems universally recognized that the enterprise the word is used to describe suffered a clean break, a discontinuity, in the person of Galileo.

Discontinuities, of course, present a challenge to historians, for what is discontinuous in the broad view often displays elements of continuity when its fine structure has been revealed. Now the analysis of MS 27 offered in this study really concerns the fine-structure, and precisely because it does it can be helpful for sorting out where both discontinuity and continuity may be found. There is no doubt that Galileo made a clean break with Aristote's cosmology, with the false dichotomy the Stagyrite had introduced between the heavens and the earth. On this historians have been in agreement for centuries. Where they have been thrown off is in the assessments they have given of the logical methods Galileo used to make the break. One can point to various factors that explain this: a too-narrow reading of Aristotle's *Organon*, particularly the *Posterior Analytics*; a related preoccupation with formal and extensional logics; a further neglect of the theory of knowledge that lies behind the *Analytics*. But by far the greatest oversight has been the avenue through which Galileo came to learn his Aristotle: not from a Greek text newly translated, but from a long tradition of development that paid attention to precisely the factors modern historians tend to overlook. When this tradition is understood one can appreciate the confidence with which Galileo would take on the Aristotelians of the day and claim that, were Aristotle himself to see the evidence only recently unveiled, he too would have broken with the cosmology enshrined in his texts.

The continuity, then, is not in the brand of physics set out in Aristotle's *De caelo*, as has been universally recognized, but rather in the method Galileo would use to argue for the truth of his own results, to demonstrate them, as he claimed, following Aristotelian canons and thereby creating a new science of the heavens. The essentials of that method are laid out in MS 27, and therein one sees its great value. But the essentials of the method are far from being the whole story. The commentary Galileo appropriated clearly makes allowance for inductive methods, for careful observation and definition of natural phenomena, for mathematical reasoning based on quantitative attributes, for the use of suppositions in proofs, particularly those of the subalternated sciences, for the removal of impediments when these seem to make nature unintelligible or opaque to human investigation. Yet the commentary makes no explicit mention of experimentation and approximation techniques, and it here that the discontinuity begins to manifest itself. Galileo was a pioneer scientist, as Drake has shown, but his pioneering was done not as a hypothetico-deductivist but as an experimentalist, one who could work within an established methodological context, devise ingenious ways of posing questions to nature, and then quantify, even more cleverly, the answers nature would in turn supply.

It is when MS 27 and its sources are studied in this setting, not merely as a static *logica docens* but as a dynamic *logica utens*, a logic whose virtualities would be worked out over Galileo's entire lifetime, that its value for resolving the continuity-discontinuity question is best seen. Aristotle may bear the title "Father of Science in the West," but Galileo has no less a claim to the title "Father of Modern Science." The element of continuity is the ideal of science, of true and certain knowledge about the world of nature, embodied in the *Organon* generally and more particularly in the *Posterior Analytics*. The discontinuity comes with Galileo's questioning whether that ideal had been realized in the

Aristotelian corpus, with the mathematization of nature he was able to effect through use of experimental techniques, with the mathematical physics he would have emerge from the subalternated science of his predecessors. A break took place here, one almost too subtle to be recognized. His precedessors had seen the subalternated science as somewhat deficient, a *scientia secundum quid*, science in a qualified sense. Galileo saw his quantitative physics quite differently, as superior, not inferior, to the qualitative reasoning that had dominated Aristotelian physics from its outset. On that ground he was not prepared to accept for his new science the second-class citizenship it would be accorded in the mixed-science tradition of the Aristotelians. His was a more robust science, better equipped than Aristotle's to attain truth and certitude in its renewed study of the world of nature.

The interpretation of the *Posterior Analytics* found in MS 27, and in the source materials from which it derives, makes adequate provision for the use Galileo would make of that work in elaborating his new science. As should be clear from the manuscript and the teachings requisite to its understanding, it incorporates an epistemology quite different from those expounded by many philosophers in the present day. That epistemology may be characterized as a type of common-sense realism. Its details have been explained in the early sections of Chap. 2, but these need not be rehearsed here. What is important about the epistemology is that it disposed Galileo to have what has been called "a natural ontological attitude." He thought that a world of nature exists independently of his thinking about it, that objects and events in nature are real, that as presented in sense experience they can be known, and that the natural light of the intellect is adequate to the task of knowing them as they are. Because he was possessed of that attitude he had the courage to make the strong knowledge claims he did about mountains on the moon, satellites of Jupiter, phases of Venus, flotation phenomena, pendulums and inclines, speeds of fall, paths of projectiles. These could not be for him vain imaginings, his own projections on reality, as some of his contemporaries wished him to maintain. For him they *were* the reality. And that conviction enabled him to persevere against all odds, to set science on a new course, to begin forging mechanics into the discipline it has become today.

As we move into the twenty-first century this Galilean ideal of science, no less than the Aristotelian ideal from which it derives, has come under vigorous attack. One now hears more of the fallibility of science than of its certitude, truth is no longer seen as its goal, and a sense of what is "really real" has been lost in ongoing debates between realists and antirealists. In such an atmosphere one may suggest that something is to be gained by turning back to a simpler period in science's history, bracketing for a while arguments over black holes and quarks, over Heisenberg's uncertainty principle and Bell's theorem, to address once again the problems that engaged Galileo and his times. At a minimum it will be seen that a questioning of the frontiers of knowledge evoked as much muddled thinking in his day as in ours. More than that, a return to the problems faced by the "Father of Modern Science," along with a recovery of the logic he used to solve them, will prompt scientists to examine anew their epistemic roots. Perhaps it will also give them renewed hope and confidence in their discipline, as they prepare to face the space-age challenges in the decades that lie ahead.

BIBLIOGRAPHY

The bibliography provided herein is divided into two sections, the first listing sources and the second literature. The sources listed are only those to which explicit reference has been made in this volume. For a complete listing of the bibliographical sources on which Galileo's MS 27 is based either proximately or remotely, together with biographical sketches of their authors, the reader should consult the Bibliographical and Biographical Register at the end of its companion volume, Galileo's Logical Treatises.

SOURCES

Aquinas, St. Thomas, 1955: *In Aristotelis libros Peri hermeneias et Posteriorum Analyticorum Expositio*, ed. R.M. Spiazzi. Turin: Marietti.

______, 1964–1976: *Summa theologiae*. Latin text and English translation, Introductions, Notes, Appendices and Glossaries, ed. and trs. Thomas Gilby et al., 60 vols. New York: McGraw-Hill and London: Eyre & Spottiswoode.

______, 1968: *Human Intelligence*, ed. and trs. P.T. Durbin, Vol. 12 of *Summa theologiae*.

______, 1970: *Commentary on the Posterior Analytics of Aristotle*, trs. F.R. Larcher. Albany: Magi Books.

Blancanus, Iosephus, 1615: *De natura mathematicarum scientiarum tractatio*. Bologna: Apud Cochium.

______, 1620: *Apparatus ad mathematicas addiscendas et promovendas*. Bologna: Typis Bonomii.

Carbone, Ludovico, 1588: *Introductio in dialecticam Aristotelis per Magistrum Franciscum Toletum Sacerdotem Societatis Iesu, Philosophiae in Romano Societatis Collegio Professorem. Additis in eandem introductionem praeludiis, eiusdem introductionis Tabulis*. Venice: Apud Meiettum.

______, 1589a: *De oratoria et dialectica inventione vel de locis communibus*. Venice: Apud Zenarum.

______, 1589b: *De arte dicendi libri duo*. Venice: Ex officina Zenarii.

______, 1595: *Divinus orator, vel De rhetorica divina libri septem*. Venice: Apud Societatem Minimam.

______, 1597a: *Additamenta ad commentaria D. Francisci Toleti in Logicam Aristotelis: Praeludia in libros Priores Analyticos; Tractatio de Syllogismo; de Instrumentis Sciendi; et de Praecognitionibus, atque Praecognitis*. Venice: Apud Angellerium.

______, 1597b: *Introductio in logicam sive totius logicae compendii absolutissimi libri sex*. Venice: Apud Sessam.

______, 1599: *Introductio in universam philosophiam libri quatuor*. Venice: Apud Zalterium.

Cesalpino, Andreas, 1571: *Peripateticae quaestiones libri V*. Venice: Apud Iuntas.

Clavius, Christopher, 1581: *In sphaeram Ioannis de Sacro Bosco commentarius, nunc* *iterum ab ipso auctore recognitis et multis ac variis locis locupletatus*. Rome: Ex officina Basae.

______, 1589: *Euclidis Elementorum libri XV, accessit XVI De solidorum regularium cuiuslibet intra quodlibet comparatione, omnes perspicuis demonstrationibus, accuratis scholiis illustrati*. Rome: Apud Grassium.

Galilei, Galileo, 1890–1909: *Le opere di Galileo Galilei*, ed. Antonio Favaro, 20 vols. in 21. Florence: G. Barbèra Editrice.

______, 1958: "The Earliest Version of Galileo's *Mechanics*," ed. Stillman Drake, *Osiris* 13: 262–290.

______, 1988: *Tractatio de praecognitionibus et praecognitis and Tractatio de demonstratione*. Transcribed from the Latin autograph by William F. Edwards, with an Introduction, Notes, and Commentary by William A. Wallace. Padua: Editrice Antenore.

Ghetaldus, Marinus, 1630: *De resolutione et compositione mathematica*. Rome: Ex Typographia Reverendae Camerae Apostolicae.

Guevara, Ioannis de, 1627: *In Aristotelis Mechanicas Commentarii, una cum additionibus quibusdam ad eandem materiam pertinentibus*. Rome: Apud Mascardum.

Lorinus, Ioannes, 1584: *Ioannis Laurini [= Lorini] Societatis Iesu Logica*. Rome: Biblioteca Apostolica Vaticana, Cod. Urb. Lat. 1471.

______, 1620: *In universam Aristotelis logicam commentarii, cum annexis disputationibus Romae ab eodem olim praelecti*. Cologne: Sumptibus Cholini.

Riccobonus, Antonius, 1579: *De natura rhetoricae*. Venice: Apud Meiettum.

Rugerius, Ludovicus, 1589: *Commentariorum et quaestionum in Aristotelis Logicam tomus*. Bamberg: Staatsbibliothek, Msc. Class. Cod. 62–1, 62–2.

Toletus, Franciscus, 1575: *Commentaria una cum questionibus in III libros de anima*. Venice: Apud Iuntas.

Vallius, Paulus, 1622: *Logica Pauli Vallii Romani ex Societate Iesu, duobus tomis distincta*. Lyons: Sumptibus Prost haeredibus Rouille.

Zabarella, Iacobus, 1597: *Opera logica*, 3d ed. Cologne: Sumptibus Zetzneri.

LITERATURE

Baldini, Ugo, 1985: "Una fonte poco utilizzata per la storia intellettuale: Le 'censurae librorum' e 'opinionum' nell'antica Compagnia di Gesù," *Annali dell'Istituto Storico Italo-Germanico in Trento*, 11: 19–67.

______, 1989: Review of *Galileo and His Sources* by W.A. Wallace, in *Archives internationales d'histoire des sciences*, 39: 357–367.

Barnes, Jonathan, 1975: "Aristotle's Theory of Demonstration," *Articles on Aristotle: 1. Science*, ed. J. Barnes et al. London: Duckworth, 65–87.

Berti, Enrico, ed., 1981: *Aristotle on Science: The "Posterior Analytics"*. Padua: Editrice Antenore.

______, 1983: "Differenza tra il metodo risolutivo degli aristotelici e la 'resolutio' dei matematici," *Aristotelismo veneto e scienza moderna*, 2 vols., ed. Luigi Olivieri. Padua: Editrice Antenore, 1: 453–457.

Brody, Baruch, and Grandy, Richard, eds., 1989: *Readings in the Philosophy of Science*, 2d ed. Englewood Cliffs, N.J.: Prentice Hall.

Butts, R.E., 1978: "Some Tactics in Galileo's Propaganda for the Mathematization of Scientific Experience," *New Perspectives on Galileo*, eds. R.E. Butts and J.C. Pitt. Dordrecht-Boston: D. Reidel Publishing Co., 59–85.

Butts, R.E., and Pitt, J.C., eds., 1978: *New Perspectives on Galileo*. Dordrecht-Boston: D. Reidel Publishing Co.

Carugo, Adriano, and Crombie, A.C., 1983: "The Jesuits and Galileo's Ideas of Science and of Nature," *Annali dell'Istituto e Museo di Storia della Scienza di Firenze*, 8.2: 3–68.

Clavelin, Maurice, 1974: *The Natural Philosophy of Galileo*, Essays on the Origins and Formation of Classical Mechanics, trs. A.J. Pomerans. Cambridge, Mass.: The M.I.T. Press.

Crombie, A.C., 1975: "Sources of Galileo's Early Natural Philosophy," *Reason, Experiment, and Mysticism in the Scientific Revolution*, eds. M.L. Righini Bonelli and W.R. Shea. New York: Science History Publications, 157–175, 304–305.

———, 1977: "Mathematics and Platonism in the Sixteenth-Century Italian Universities and in Jesuit Educational Policy," *Prismata: Naturwissenschaftsgeschichtliche Studien*, eds. Y. Maeyama and W.G. Salzer. Wiesbaden: Franz Steiner, 63–94.

De Jong, Everard, 1989: *Galileo Galilei's Logical Treatises (MS 27) and Giacomo Zabarella's Opera Logica: A Comparison*, Ph.D. Dissertation. Washington, D.C.: The Catholic University of America.

Dollo, Corrado, 1989: "L'Uso di Platone in Galileo," *Siculorum Gymnasium*, 42: 115–157.

Drake, Stillman, 1957: *Discoveries and Opinions of Galileo*. Garden City, N.Y.: Doubleday Anchor Books.

———, 1972: "On the Probable Order of Galileo's Notes on Motion," *Physis*, 14: 55–68.

———, 1973: "Galileo's Experimental Confirmation of Horizontal Inertia: Unpublished Manuscripts," *Isis*, 64: 291–305.

———, 1975: "Galileo's New Science of Motion," *Reason, Experiment, and Mysticism in the Scientific Revolution*, eds. M.L. Righini Bonelli and W.R. Shea. New York: Science History Publications, 131–156.

———, 1978: *Galileo at Work: His Scientific Biography*. Chicago: The University of Chicago Press.

———, 1979: *Galileo's Notes on Motion*. Arranged in probable order of composition and presented in reduced facsimile, by Stillman Drake. Florence: Istituto e Museo di Storia della Scienza.

———, 1981: *Cause, Experiment and Science*. A Galilean dialogue incorporating a new English translation of Galileo's "Bodies That Stay atop Water or Move in it." Chicago and London: The University of Chicago Press.

———, 1983: *Telescopes, Tides, and Tactics*. A Galilean Dialogue about the *Starry Messenger* and Systems of the World. Chicago: The University of Chicago Press.

———, 1986: "Galileo's Pre-Paduan Writings: Years, Sources, Motivations," *Studies in History and Philosophy of Science*, 17: 429–448.

———, 1990: *Galileo: Pioneer Scientist*. Toronto: University of Toronto Press.

Drake, Stillman, and Drabkin, I.E., 1960: *Galileo Galilei: On Motion and On Mechanics*. Madison, Wis.: The University of Wisconsin Press.

———, 1969: *Mechanics in Sixteenth-Century Italy*. Madison, Wis.: The University of Wisconsin Press.

Drake, Stillman, and MacLachlen, J.H., 1975: "Galileo's Discovery of the Parabolic Trajectory," *Scientific American*, 232: 102–110.

Edwards, W.F., 1961: *The Logic of Jacopo Zabarella* (1533–1589). Ph.D. Dissertation, Columbia University. Ann Arbor: University Microfilms.

______: "Randall on the Development of Scientific Method in the School of Padua," *Naturalism and Historical Understanding: Essays on the Philosophy of John Herman Randall, Jr.*, ed. J.P. Anton. Albany: State University of New York, 53–68.

______, 1983: "Paduan Aristotelianism and the Origin of Modern Theories of Method," *Aristotelismo veneto e scienza moderna*, 2 vols., ed. Luigi Olivieri. Padua: Editrice Antenore, 1: 206–220.

Favaro, Antonio, ed., 1890–1909: *Le opere di Galileo Galilei*, 20 vols. in 21. Florence: G. Barbèra Editrice.

Fine, Arthur, 1986: *The Shaky Game: Einstein, Realism, and the Quantum Theory*. Chicago and London: The University of Chicago Press.

Finocchiaro, M.A., 1980: *Galileo and the Art of Reasoning: Rhetorical Foundations of Logic and Scientific Method*, Boston Studies in the Philosophy of Science, 61. Dordrecht-Boston: D. Reidel Publishing Co.

______, 1989: *The Galileo Affair: A Documentary History*. Berkeley: University of California Press.

Galilei, Galileo, 1974: *Two New Sciences*. A New Translation, with Introduction and Notes, by Stillman Drake. Madison, Wis.: The University of Wisconsin Press.

______, 1989: *Sidereus Nuncius, or The Sidereal Messenger*. Translated with introduction, conclusion, and notes by Albert van Helden. Chicago and London: The University of Chicago Press.

Galluzzi, Paolo, 1973: "Il 'Platonismo' del tardo Cinquecento e la filosofia di Galileo," *Ricerche sulla cultura dell'Italia moderna*, ed. Paola Zambelli. Bari: Laterza, 37–79.

______, 1976: "Evangelista Torricelli. Concezione della matematica e segreto degli occhiali," *Annali dell'Istituto e Museo di Storia della Scienza*, 1: 71–95.

______, 1979: *Momento: Studi galileiani*, Lessico Intellettuale Europeo, 19. Rome: Edizioni dell'Ateneo e Bizzarri.

Gilbert, N.W., 1960: *Renaissance Concepts of Method*. New York and London: Columbia University Press.

______, 1963: "Galileo and the School of Padua," *Journal of the History of Philosophy*, 1: 223-231.

Green-Pedersen, N.J., 1984: *The Tradition of the Topics in the Middle Ages: The Commentaries on Aristotle's and Boethius's "Topics."* Munich-Vienna: Philosophia Verlag.

Helbing, M.O., 1989: *La Filosofia di Francesco Buonamici, professore di Galileo a Pisa*. Pisa: Nistri-Lischi Editori.

Hill, D.K., 1979: "A Note on a Galilean Worksheet," *Isis*, 70: 269–271.

______, 1986: "Galileo's Work on 116v: A New Analysis," *Isis*, 77: 283–291.

______, 1988: "Dissecting Trajectories: Galileo's Early Experiments on Projectile Motion and the Law of Fall," *Isis*, 79: 646–668.

Hintikka, J., and Remes, U., 1974: *The Method of Analysis: Its Geometrical Origin and Its General Significance*. Dordrecht-Boston: D. Reidel Publishing Co.

Hutchinson, Keith, 1990: "Sunspots, Galileo, and the Orbit of the Earth," *Isis*, 81: 68–74.

Jardine, Nicholas, 1976: "Galileo's Road to Truth and the Demonstrative Regress," *Studies in History and Philosophy of Science*, 7: 277–318.

______, 1984: *The Birth of the History and Philosophy of Science: Kepler's A Defense of Tycho Against Ursus, With Essays on Its Provenance and Significance.* Cambridge: Cambridge University Press.

Kessler, Eckhard, 1988: "The Intellective Soul," *The Cambridge History of Renaissance Philosophy*, eds. C.B. Schmitt et al. Cambridge: Cambridge University Press, 485–534.

Koyré, Alexandre, 1978: *Galileo Studies*, trs. John Mepham. Atlantic Highlands, N.J.: Humanities Press.

Kristeller, P.O., 1962: *La tradizione aristotelica nel Rinascimento*. Padua: Editrice Antenore.

______, "Aristotelismo e sincretismo nel pensiero di Pietro Pomponazzi," *Aristotelismo veneto e scienza moderna*, 2 vols., ed. Luigi Olivieri. Padua: Editrice Antenore, 2: 1077–1099.

Levi, Eugenia, 1968: *Indice delle fonti dell'edizione nazionale delle opere di Galileo Galilei.* Florence: G. Barbèra Editrice.

Lewis, Christopher, 1980: *The Merton Tradition and Kinematics in Late Sixteenth and Early Seventeenth Century Italy.* Padua: Editrice Antenore.

Mach, Ernst, 1960: *The Science of Mechanics: A Critical and Historical Account of Its Development*, 6th ed. Chicago: The Open Court Publishing Co.

Machamer, Peter, 1978: "Galileo and the Causes," *New Perspectives on Galileo*, eds. R.E. Butts and J.C. Pitt. Dordrecht-Boston: D. Reidel Publishing Co., 161–180.

Mahoney, M.S., 1968–1969: "Another Look at Greek Geometrical Analysis," *Archive for History of Exact Sciences*, 5: 318–348.

McMullin, Ernan, 1978: "The Conception of Science in Galileo's Work," *New Perspectives on Galileo*, eds. R.E. Butts and J.C. Pitt. Dordrecht-Boston: D. Reidel Publishing Co., 209–257.

______, 1983: Review of *Prelude to Galileo* by W.A. Wallace, *Philosophy of Science*, 50: 171–173.

Mertz, D.W., 1980: "On Galileo's Method of Causal Proportionality," *Studies in History and Philosophy of Science*, 11: 229–242.

Modrak, D.K.W., 1987: *Aristotle: The Power of Perception*. Chicago: The University of Chicago Press.

Moss, J.D., 1983a: "Galileo's *Letter to Christina*: Some Rhetorical Considerations," *Renaissance Quarterly*, 36: 547–576.

______, 1983b: "Galileo's Rhetorical Strategies in Defense of Copernicanism," *Novità celesti e crisi del sapere*, ed. Paolo Galluzzi. Florence: Istituto e Museo di Storia della Scienza, 95–103.

______, 1986a: "The Rhetoric Course at the Collegio Romano in the Latter Half of the Sixteenth Century," *Rhetorica*, 4.2: 137–151.

______, 1986b: "The Rhetoric of Proof in Galileo's Writings on the Copernican System," *Reinterpreting Galileo*, ed. W.A. Wallace, Studies in Philosophy and History of Philosophy, 15. Washington, D.C.: The Catholic University of America Press, 179–204.

______, 1987a: "Aristotle's Four Causes: Forgotten *Topoi* of the Italian Renaissance," *Rhetoric Society Quarterly*, 17.1: 71–88.

______, 1987b: "Rhetorical Invention in the Italian Renaissance," *Visions of Rhetoric: History, Theory and Criticism*, ed. C.W. Kneupper. Arlington, Texas: Rhetoric Society of America, 30–41.

______, 1990a: "Dialectic and Rhetoric: Questions and Answers in the Copernican Revolution," *Argumentation*, 5: 17–37.

______, 1990b: "Ludovico Carbone's Commentary on Aristotle's *De caelo*," *Nature and Scientific Method*, Essays in Honor of William A. Wallace, ed. D.O. Dahlstrom, Studies in Philosophy and the History of Philosophy, 20. Washington, D.C.: The Catholic University of America Press, 169–192.

______, 1993: *Novelties in the Heavens: Rhetoric and Science in the Copernican Controversy*. Chicago: University of Chicago Press.

Naylor, R.H., 1974: "The Evolution of an Experiment: Guidobaldo del Monte and Galileo's *Discorsi* Demonstration of the Parabolic Trajectory," *Physis*, 16: 323–346.

______, 1976: "Galileo: The Search for the Parabolic Trajectory," *Annals of Science*, 33: 153–172.

______, 1980: "Galileo's Theory of Projectile Motion," *Isis*, 71: 550–570.

______, 1990: "Galileo's Method of Analysis and Synthesis," *Isis*, 81: 695–707.

Olivieri, Luigi, 1983a: *Certezza e gerarchia del sapere. Crisi dell'idea di scientificità nell'aristotelismo del secolo XVI*. Padua: Editrice Antenore.

______, ed., 1983b: *Aristotelismo veneto e scienza moderna*, 2 vols. Padua: Editrice Antenore.

______, 1988: *Pietro d'Abano e il pensiero neolatino*. Padua: Editrice Antenore.

Papuli, Giovanni, 1983: "La theoria del *regressu* come metodo scientifico negli autori della Scuola di Padova," *Aristotelismo veneto e scienza moderna*, 2 vols., ed. Luigi Olivieri. Padua: Editrice Antenore.

Poinsot, John [of St. Thomas], 1955: *The Material Logic of John of St. Thomas*, trs. Y.R. Simon, J.J. Glanville, and G.D. Hollenhorst. Chicago: The University of Chicago Press.

______, 1985: *Tractatus de signis: The Semiotic of John Poinsot*, ed. and trs. J.N. Deely. Berkeley, Cal.: University of California Press.

Poppi, Antonino, 1970: *Introduzione all'aristotelismo padovano*. Padua: Editrice Antenore.

______, 1972: *La dottrina della scienza in Giacomo Zabarella*. Padua: Editrice Antenore.

Procissi, Angiolo, 1959: *Indici e cataloghi, Nuova serie V*. La collezione galileiana della Biblioteca Nazionale di Firenze, Vol. 1. Florence: Isituto Poligrafico dello Stato, Libreria dello Stato.

Purnell, Frederick, 1972: "Jacopo Mazzoni and Galileo," *Physis*, 3: 273–294.

Randall, J.H., Jr., 1940: "The Development of Scientific Method in the School of Padua," *Journal of the History of Ideas*, 1: 177–206.

______, 1961: *The School of Padua and the Emergence of Modern Science*. Padua: Editrice Antenore.

______, 1876: "Paduan Aristotelianism Reconsidered," *Philosophy and Humanism: Renaissance Essays in Honor of Paul Oskar Kristeller*, ed. E.P. Mahoney. New York: Columbia University Press, 275–282.

Rolih, M.S., 1985: *Code Magliabechiane: Un gruppo di manoscritti della Biblioteca Nazionale Centrale di Firenze fuori inventario*. Florence: La Nuova Italia Editrice.

Schmitt, C.B., 1971: *Critical Survey and Bibliography of Studies on Renaissance Aristotelianism*, 1958–1969. Padua: Editrice Antenore.

______, 1978: "Borro, Girolamo," *Dictionary of Scientific Biography*, ed. C.C. Gillispie, 16 vols. New York: Charles Scribner's Sons, 15: 44–46.

______, 1981: *Studies in Renaissance Philosophy and Science*. London: Variorum Reprints.

______, 1983a: *Aristotle and the Renaissance*. Cambridge, Mass.: Harvard University Press.

______, 1983b: "Aristotelianism in the Veneto and the Origins of Modern Science," *Aristotelismo veneto e scienza moderna*, 2 vols., ed. Luigi Olivieri. Padua: Editrice Antenore, 1: 104–123.

Segre, Michael, 1989: "Viviani's Life of Galileo," *Isis*, 80: 207–231.

Settle, T.B., 1971: "Ostilio Ricci, a Bridge between Alberti and Galileo," *XIIe Congres internationale d'histoire des sciences*, Paris, Tome IIIB: 121–126.

Shea, W.R., 1972: *Galileo's Intellectual Revolution: Middle Period*, 1610–1632. New York: Science History Publications.

______, 1978: "Descartes as Critic of Galileo," *New Perspectives on Galileo*, eds. R.E. Butts and J.C. Pitt. Dordrecht-Boston: D. Reidel Publishing Co., 139–159.

Smith, A. Mark, 1985: "Galileo's Proof for the Earth's Motion from the Movement of Sunspots," *Isis*, 76: 543–551.

Sommervogel, Carlos, et al., 1890–1932: *Bibliothèque de la Compagnie de Jésus*, 11 vols. Brussels-Paris: Alphonse Picard.

Struve, Otto, 1959: *Elementary Astronomy*, rev. ed. New York: Oxford University Press.

Stump, Eleonore, 1978: *Boethius's De topicis differentiis*. Translated, with Notes and Essays on the Text. Ithaca and London: Cornell University Press.

Veatch, Henry, 1952: *Intentional Logic: A Logic Based on Philosophical Realism*. New Haven: Yale University Press.

______, 1969: *Two Logics: The Conflict Between Classical and Neo-Analytic Philosophy*. Evanston, Ill.: Northwestern University Press.

Villoslada, R.G., 1954: *Storia del Collegio Romano dal suo inizio (1551) alla soppressione della Compagnia di Gesù* (1773). Rome: Gregorian University Press.

Wallace, W.A., 1968: "The Enigma of Domingo de Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," *Isis*, 59: 384–401.

______, 1972–1974: *Causality and Scientific Explanation*, 2 vols. Ann Arbor: The University of Michigan Press.

______, 1974: "Aquinas on the Temporal Relation Between Cause and Effect," *The Review of Metaphysics*, 27: 569–584.

______, 1977: *Galileo's Early Notebooks: The Physical Questions*. A Translation from the Latin, with Historical and Paleographical Commentary. Notre Dame: The University of Notre Dame Press.

______, 1978: "Galileo Galilei and the *Doctores Parisienses*," *New Perpectives on Galileo*, eds. R.E. Butts and J.C. Pitt. Dordrecht-Boston: D. Reidel Publishing Co.

______, 1981a: *Prelude to Galileo: Essays on Medieval and Sixteenth-Century Sources of Galileo's Thought*. Boston Studies in the Philosophy of Science, 62. Dordrecht-Boston: D. Reidel Publishing Co.

______, 1981b: "Aristotle and Galileo: The Uses of *Hupothesis* (*Suppositio*) in Scientific Reasoning," *Studies in Aristotle*, ed. D.J. O'Meara. Studies in Philosophy and History of Philosophy, 9. Washington, D.C.: The Catholic University of America Press.

______, 1982: Review of *Galileo and the Art of Reasoning* by M.A. Finocchiaro, *Journal of the History of Philosophy*, 20: 307–309.

______, 1983a: "Aristotelian Influences on Galileo's Thought," *Aristotelismo veneto e scienza moderna*, 2 vols., ed. Luigi Olivieri, 1: 349–378.

______, 1983b: "The Problem of Causality in Galileo's Science," *Review of Metaphysics*, 36: 607–632.

______, 1983c: "Galileo's Early Arguments for Geocentrism and His Later Rejection of Them," *Novità Celesti e Crisi del Sapere*, ed. Paolo Galluzzi. Florence: Istituto e Museo di Storia della Scienza, 31–40.

______, 1983d: "Galileo and Aristotle in the *Dialogo*," *Angelicum*, 60: 311–332.

______, 1983e: "Galilée et les professeurs jésuites du Collège romain à la fin du XVI siècle," *Galileo Galilei: 350 ans d'histoire, 1633–1983*, ed. Paul Poupard. Tournai: Desclée International, 75–97.

______, 1983f: "Galileo's Science and the Trial of 1633," *The Wilson Quarterly*, 7.3: 154–164.

______, 1984a: *Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science*. Princeton: Princeton University Press.

______, 1984b: "Galileo and the Continuity Thesis" [Reply to Ernan McMullin], *Philosophy of Science*, 51: 504–510.

______, ed., 1986a: *Reinterpreting Galileo*, Studies in Philosophy and History of Philosophy, 15. Washington, D.C.: The Catholic University of America Press. "Introduction," vii–x; "Reinterpreting Galileo on the Basis of His Latin Manuscripts," 3–28.

______, 1986b: "Galileo's Sources: Manuscripts or Printed Works?" *Print and Culture in the Renaissance: Essays on the Advent of Printing in Europe*, eds. G.B. Tyson and Sylvia Wagonheim. Newark, Del.: University of Delaware Press, 45–54.

______, 1986c: "Galileo and His Sources" [Reply to A.C. Crombie], *The Times Literary Supplement*, 3 January (no. 4318), 13, 23.

______, 1987a: "The Early Jesuits and the Heritage of Domingo de Soto," *History and Technology*, 4: 301–320.

______, 1987b: "Science and Philosophy at the Collegio Romano in the Time of Benedetti," *Atti Convegno Internazionale di Studio "Giovan Battista Benedetti e il suo tempo."* Venice: Istituto Veneto di Scienze, Lettere ed Arti, 113–126.

______, 1988: "Randall *Redivivus*: Galileo and the Paduan Aristotelians," *Journal of the History of Ideas*, 48.1: 133–149.

______, 1989: "The Problem of Apodictic Proof in Early Seventeenth-Century Mechanics: Galileo, Guevara, and the Jesuits," *Science in Context*, 3: 67–87.

______, 1990a: "The Dating and Significance of Galileo's Pisan Manuscripts," *Nature, Experiment, and the Sciences*, Essays on Galileo and the History of Science in Honour of Stillman Drake, eds. T.H. Levere and W.R. Shea. Boston Studies in the Philosophy of Science, 120. Dordrecht: Kluwer Academic Publishers, 3–50.

______, 1990b: "Duhem and Koyré on Domingo de Soto," *Synthese*, 83: 239–260.

______, 1992: "Zabarella and Galileo: The Transmission of Paduan Methodology," *Giacomo Zabarella tra filosofia e scienza*. Padua: Editrice Antenore, forthcoming.

Weisheipl, J.A., 1985: *Nature and Motion in the Middle Ages*, ed. W.E. Carroll. Studies in Philosophy and the History of Philosophy, 11. Washington, D.C.: The Catholic University of America Press.

Whewell, William, 1967: *Philosophy of the Inductive Sciences*, 2 vols. London: J.W. Parker, 1847, repr. 1967.

Wisan, W.L., 1974: "The New Science of Motion: A Study of Galileo's *De motu locali*," *Archive for History of Exact Sciences*, 13: 103–306.

——, 1978: "Galileo's Scientific Method: A Reexamination," *New Perspectives on Galileo*, eds. R.E. Butts and J.C. Pitt. Dordrecht-Boston: D. Reidel Publishing Co., 1–57.

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