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iii

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Transversal: International Journal for the Historiography of Science

2 (June 2017)

ISSN 2526-2270

www.historiographyofscience.org

Transversal: International Journal for the Historiography of Science is a semiannual online journal published by the Graduate Program in History (Science and Culture in History) of Federal University of Minas Gerais (Universidade Federal de Minas Gerais).

Transversal: International Journal for the Historiography of Science promotes scholarly research in the historiography of science and chronicles its history and criticism. Although historiography of science is a sub-discipline of History, we construe this subject broadly to include analysis of the historiography of science produced by history of science, philosophy of science and related disciplines. By focusing its analysis on the different historical, social and epistemological implications of science, historiography of science is a transversal knowledge with respect to the production of science, hence the name of this journal. In order to accomplish its purpose, *Transversal: International Journal for the Historiography of Science* discusses historical, theoretical, conceptual and methodological aspects of the different themes, works and authors present in this tradition, as well as the new approaches in the recent historiography of science.

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iv

Open topic articles may deal with a variety of themes in historiography of science as long as they are within the scope of the journal. All papers must be results of original research. Papers previously published are not eligible for submission, and will not be considered.

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Publication Frequency

Transversal: International Journal for the Historiography of Science is a semiannual online journal (issues in June and December).

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v

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Contents

From the Editors

Historiography of Science: The Link between History and Philosophy in Understanding of Science Mauro L. Condé Marlon Salomon	01
---	----

Dossier Pierre Duhem

Pierre Duhem's Philosophy and History of Science

Introduction Fábio Rodrigo Leite Jean-François Stoffel	03
Duhem's Analysis of Newtonian Method and the Logical Priority of Physics over Metaphysics Eduardo Salles O. Barra Ricardo Batista dos Santos	07
The French Roots of Duhem's early Historiography and Epistemology Stefano Bordoni	20
Duhem's Critical Analysis of Mechanicism and his Defense of a Formal Conception of Theoretical Physics José R. N. Chiappin Cássio Costa Laranjeiras	36
Anti-Scepticism and Epistemic Humility in Pierre Duhem's Philosophy of Science Marie Gueguen Stathis Psillos	54
Duhem: Images of Science, Historical Continuity, and the First Crisis in Physics Michael Liston	73
Duhem in Pre-War Italian Philosophy: The Reasons of an Absence Roberto Maiocchi	85

Was Pierre Duhem an <i>Esprit de Finesse</i> ? Víctor Manuel Hernández Márquez	93
Was Duhem Justified in not Distinguishing between Physical and Chemical Atomism? Paul Needham	108
<i>Bon Sens</i> and <i>Noûs</i> Roberto Estrada Olguin	112
Duhem's Legacy for the Change in the Historiography of Science: an Analysis Based on Kuhn's Writings Amélia J. Oliveira	127
Poincaré and Duhem: Resonances in their First Epistemological Reflections João Príncipe	140

Dossier Pierre Duhem – Book Review

Víctor Manuel Hernández Márquez (Ed.) <i>Pierre Duhem: Entre Física y Metafísica</i> (<i>Pierre Duhem: Between Physics and Metaphysics</i>) Damián Islas Mondragon	157
Duhem, Pierre. <i>La Théorie Physique: son Objet, sa Structure</i> (The New French Edition of Pierre Duhem's <i>The Aim and Structure</i> <i>of the Physical Theory</i>) Jean-François Stoffel	160
Bordoni, Stefano. <i>When Historiography Met Epistemology: Sophisticated Histories and Philosophies of Science in French-speaking Countries in the Second Half of the Nineteenth Century</i> Jean-François Stoffel	163

vii

Articles

A Development of the <i>Principle of Virtual Laws</i> and its Conceptual Framework in Mechanics as Fundamental Relationship between Physics and Mathematics Raffaele Pisano	166
Michael Scot and the Four Rainbows Tony Scott	204
Galileo and the Medici: Post-Renaissance Patronage or Post-Modern Historiography? Michael Segre	226



Interview: Helge Kragh 233
Interviewed by Gustavo Rodrigues Rocha

Book review

The New French Edition of Erwin Panofsky's
Galileo as a Critic of the Arts (Galilée Critique d'Art) 238
Halhane Machado

A Contribution to the Newtonian Scholarship:
The "Jesuit Edition" of Isaac Newton's *Principia*, a Research in Progress
by Paolo Bussotti and Raffaele Pisano 242
Gustavo Rodrigues Rocha

Transversal: International Journal for the Historiography of Science, 2 (2017) 01-02

ISSN 2526-2270

www.historiographyofscience.org

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From the Editors

Historiography of Science: The Link between History and Philosophy in Understanding Science

Mauro L. Condé¹

Marlon Salomon²

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.01>

It is with great pleasure that we launch this second number of the *Transversal: International Journal for the historiography of Science* (June 2017 Edition). For this edition we have been able to count on the collaboration of 23 authors from 10 different countries and therefore believe we are on the right path to having a periodical that is as frontier-less as it could possibly be. This strong adherence of authors from different places also shows us that our editorial proposal has been well received by this international community of academics interested in the historiography of science; we are certainly very glad about that. We are well aware that the path of consolidation is a long one, but the first two numbers have strongly motivated us to forge ahead.

In seeking to foster research and academic exchanges in the historiography of science, narrating its history and critique, we have elected that our editorial focus should not only be on authors and themes but also on the different scientific disciplines and their specific historiographies. While the first two numbers have concentrated on authors who are important for the historiography of science, in the near future we will have dossiers on themes and disciplines that are equally worthy to be featured on the editorial agenda of the *Transversal: International Journal for the Historiography of Science*. Similarly, the articles section is open to receive collaborations that address historiographic perspectives of different disciplines and the most varied themes and authors.

Although it is considered to be a sub-discipline of history, the historiography of science is in fact a point of confluence of science, history and philosophy. It can undertake the important task of establishing the analysis and registration of the different narratives of the history of science but also, in a philosophical perspective, of questioning the parameters, outreach and possibilities of different historiographic models constructed by those historical narratives of science. Thus, the historiography of science is somewhat like a delta in which the waters of science, history and philosophy, and, albeit contemplated to a lesser extent in this tradition, those of the disciplines of sociology and anthropology, flow through together. In short, the historiography of science brings together bodies of knowledge that are quite distinct and that have equally distinct trajectories, but they interweave and imbricate to the point that their waters become almost indistinguishable from one another.

In that sense, the editorial stance of this periodical holds that, among the different and common concerns and interests present in science, history and philosophy, the historiography of science constitutes

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a link that unites those fields of knowledge in an integrated manner. For some time now we have known of the need to imbricate history and philosophy to achieve an understanding of science. That has been imparted by authors like Ludwik Fleck, Georges Canguilhem and Imre Lakatos. To Fleck, “any theory of knowledge without historical or comparative studies will remain a mere hollow play of words, an imaginary epistemology (*Epistemologia imaginabilis*)” (Fleck 1979 [1935]). In 1966, Canguilhem stated that “without having recourse to epistemology, a theory of knowledge would be a meditation on emptiness and without any relation to the history of the sciences, an epistemology would be a perfectly superfluous double of the very science whose history it intended to discourse on” (Canguilhem 1983 [1966]). In turn, years later, inspired by Kant, Imre Lakatos asserted that “the philosophy of science without the history of science is empty; the history of science without the philosophy of science is blind” (Lakatos 1998). We are convinced that the historiography of science can be precisely that terrain of integration. In other words, even if the history of science and the philosophy of science in their aspects as distinct disciplines that address the same object but each with its own methodology, governed by different intentions – as Kuhn showed in his text on the History of Science and the Philosophy of Science (Kuhn 1977) – even if the final result of both bodies of knowledge, those of history as much as those of philosophy, are quite distinct from one another, it is still difficult to deny the vast terrain that they share in their historiographic preoccupations.

Indeed considering the historiography of science to be a sub-discipline of history seems to be more to meet the demands of our classificatory natures or academic policy that compartmentalizes everything into its departments. From the epistemological point of view, there is no reason not to associate the historiography of science as knowledge common to and shared by science, history and philosophy.

This edition honors the figure of Pierre Duhem, who is not only a classic example in the affirmation of the connections among science, history and philosophy but also a highly important figure for the historiography of science, especially in the light of his fruitful historiographic conception and his important archival discoveries which, it is well to remember, brought mediaeval science to life. Thus that erudite French intellectual cannot be left outside the scope of our editorial focus. We hereby pay homage to him and his rich legacy of thought.

We must also underscore the important editorial work undertaken by Fábio Rodrigo Leite and Jean-François Stoffel in the production of the dossier on Pierre Duhem. Were it not for the two organizers' profound knowledge of Duhem's works – allied to their tremendous capability in articulating an editorial process that involved authors, evaluators and editors – this special number would never have existed. Thus we register not only our acknowledgement of their labors but also our great debt of gratitude to them. Particularly, Fábio Leite for his leadership in this process.

Mauro L. Condé – UFMG
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Transversal: International Journal for the Historiography of Science, 2 (2017) 03-06

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Pierre Duhem's Philosophy and History of Science

Introduction

Fábio Rodrigo Leite¹

Jean-François Stoffel²

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.02>

We are pleased to present in this issue a tribute to the thought of Pierre Duhem, on the occasion of the centenary of his death that occurred in 2016. Among articles and book reviews, the dossier contains 14 contributions of scholars from different places across the world, from Europe (Belgium, Greece, Italy, Portugal and Sweden) to the Americas (Brazil, Canada, Mexico and the United States). And this is something that attests to the increasing scope of influence exerted by the French physicist, philosopher and historian.

It is quite true that since his passing, Duhem has been remembered in the writings of many of those who knew him directly. However, with very few exceptions (Manville et al. 1927), the comments devoted to him exhibited clear biographical and hagiographic characteristics of a generalist nature (see Jordan 1917; Picard 1921; Menétré 1922a; 1922b; Humbert 1932; Pierre-Duhem 1936; Ocagne et al. 1937). From the 1950s onwards, when the studies on his philosophical work resumed, the thought of the Professor from Bordeaux acquired an irrevocable importance, so that references to *La théorie physique: Son objet et sa structure* became a common place in the literature of the area. As we know, this recovery was a consequence of the prominence attributed, firstly, to the notorious Duhem-Quine thesis in the English-speaking world, and secondly to the sparse and biased comments made by Popper that generated an avalanche of revaluations of the Popperian “instrumentalist interpretation”. The constant references Duhem received from Philipp Frank, translator of *L'évolution de la mécanique* into German as early as 1912, certainly cannot be disregarded (see Duhem 1912 [1903]). As it happened, the reception of Duhem's ideas conditioned the subsequent debate on the prevailing preferences in the English-speaking world, namely, the thesis of underdetermination of theories by data, the merely representative value of theories, the criticism of the inductive method, and, especially, the holism and criticism of the crucial experiment, culminating in the volume edited by Sandra Harding (1976).

The case of Duhem shows that the value of original reprints and translations cannot have their impact overlooked. *La théorie physique* was translated into English by Philip P. Wiener in 1954, and it was not until 1981 (and therefore exactly 67 years after the previous one) that it received, thanks to the efforts of Paul Brouzeng, its first reprint, made from the second edition in French (in the following year, Brouzeng

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introduced the facsimile publication of the original edition of *Σύζειν τὰ φαινόμενα: Essai sur la notion de théorie physique de Platon à Galilée*). It is difficult to define whether this reprint was the cause or a simple epiphenomenon of a larger and steady growing of interest that would once and for all consolidate the studies on Duhem, henceforth concentrated in large publications devoted entirely to him. This point of inflection resulted from researches of an easily identifiable group of interpreters. Among the main publications are those by Stanley L. Jaki (1984, 1991), Roberto Maiocchi (1985), Paul Brouzeng (1987), Alfredo Marcos (1988), Anastasios Brenner (1990) and Russell N. D. Martin (1991), in which should be added some special issues of journals (Ariew and Barker 1990a; 1990b; Brenner et al. 1992). The interpretations of Duhem's work have become thereafter more *balanced*, since the realistic aspects of Duhemian philosophy have gained appreciation, in addition they have become more complete, as the religious and political motivations and consequences of his thought have begun to receive unprecedented attention. To this new context we must recognize two essential features of the work of the French author – its *complexity* and *unity*.

If almost all the publications aforementioned persist as a reference source, in the present century a new generation (still diffuse) of scholars, equipped with a bibliography already advanced (Stoffel 1996) and sources previously little explored, has imposed interpretive restrictions on the previous generation. Inspired by Martin's style and indirectly by Harry W. Paul (1979), Jean-François Stoffel (2002) has emphasized the apologetic aspects imbricated in the production of the French philosopher and, largely supported by Duhemian correspondence, has outlined more clearly the personal relationships nourished by Duhem in an academic environment that was admittedly unfavorable. Paul Needham, in an extensive series of articles (see, for example, 1996; 2002; 2008), and Stefano Bordoni (2012) have devoted mainly, but not exclusively, to the scientific works of Duhem, concerning chemistry and thermodynamics respectively. Needham is also responsible for translating Duhem's important scientific works into English (Duhem 2002; 2011).

It seems to us that two thorny issues have stood out among the experts in recent decades. One of them, more widespread, concerns the determination of the place due to Duhem in the debate on scientific realism: in this case, it is the constant attempts to conciliate realistic and instrumentalist theses in his philosophy, and among those who incline for the realistic interpretation, of the exact definition of its alleged realism. The second, concerning the historical links of philosophy embraced by the French philosopher, refers to his distant methodological affiliation, identified almost always alternately, now in Aristotle, sometimes in Pascal. In addition, this preoccupation is associated with another, that is, of his theological option, which makes him to be inserted among the neo-Thomists or among the modernists, or even excluded from both categories, given the peculiarity of his Catholicism. The present issue of *Transversal* contemplates and prolongs the questions posed by this new generation of scholars.

The reader will soon note that some of the articles presented here instigate new reflections because they have a critical tone, such as the one written by Marie Gueguen and Stathis Psillos, which call into question the Duhemian distinction between theoretical representation and explanation, essential for a second distinction, this time between physics and metaphysics, and for the establishment of his historical continuity. In the same vein, Michael Liston criticizes Duhem's attempt to use evolutionary standards derived from the history of physics as an expedient for the justification of methodological judgments, since, he argues, such patterns are always easy to find. Other articles are in charge of doing justice to some aspects of Duhemian thought. Paul Needham's contribution seeks to demonstrate the reasonableness of our physicist's position by criticizing the atomism of his time, on which a "general skepticism" would stand, and by not distinguishing physical atomism from chemist, deriving this indistinction from the non-methodological unificationism of Duhem. Víctor Manuel Hernández Márquez makes a thorough analysis of the roles that the *finesse* and geometry minds play in the Duhemian style, and insists at the same time that the scope of the second is greater than one thinks and, conversely, that the Pascalian influence on the formation of the author's thought is less than some interpreters suspect. In turn, Eduardo Barra and Ricardo Santos argue that Duhem's critical exam of the Newtonian method, in spite of the replications and amendments received, still remains generally valid, and that, after all, Newton and Duhem tried to defend, with different terminologies, the autonomy of physics in the face of metaphysics. A similar spirit stirs up the article by Amélia Oliveira, when she tries to acknowledge contemporaneity, never sufficiently recognized – particularly by Thomas Kuhn –, of the historical methodology defended and practiced by Duhem, that would approach the so called "new historiography of science." More neutral analyzes are made by José Chiappin and Cássio Laranjeiras, who focus on the question of methodological constraints (such as the refusal of mechanism,

the demands of theoretical testability and continuity) demanded by Duhem for theories to evolve according to an acceptable standard of rationality. For his part, Roberto Olguin, in a suggestive way, examines the historical and conceptual clusters between the very important Duhemian notion of *bon sens*, which links the philosopher directly to Pascal, and the Aristotelian notion of *noûs*, supposedly situated at the root of the other, when apprehending the first principles. Reading Duhem with “Duhemian eyes,” Stefano Bordoni seeks to link Duhem to a tradition whose philosophical and historical sophistication, in which scientific practice would be taken seriously, would go back to Cournot, Naville, and Paul Tannery. Following this same path of contextualization, João Príncipe elaborates an analysis of the crisscrossing genesis of Poincaré and Duhem philosophies, relying on the thesis of the existence of a consensus among philosophers of the late nineteenth century about the hypothetical nature of theories. Roberto Maiocchi compiles the references to Duhem in the period before the First World War in the Italian academic sphere, marked by idealism, and reveals to us the indifference or hostility with which his epistemology was received. Finally, three book reviews close the number: Damián Islas Mondragon presents *Pierre Duhem: Between physics and metaphysics*, a collection of texts edited by Víctor Hernández Márquez in Spanish in 2016, with the participation of experts in Duhemian thought from Latin America. Jean-François Stoffel analyzes the new electronic edition of *La théorie physique* edited by Sophie Roux. Stoffel also signed the book review of Stefano Bordoni’s book *When historiography met epistemology: Sophisticated histories and philosophies of science in French-speaking countries*. This book deepens many of Bordoni’s ideas contained in his article mentioned above.

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Dossier Pierre Duhem – Introduction

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Pierre Maurice Marie Duhem (1861-1916)

Transversal: International Journal for the Historiography of Science, 2 (2017) 07-19

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Duhem's Analysis of Newtonian Method and the Logical Priority of Physics over Metaphysics

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Abstract:

This article offers a discussion of Duhemian analysis of Newton's method in the *Principia* considering both the traditional response to this analysis (Popper *et alii*) and the more recent ones (Harper *et alii*). It is argued that in General Scholium to the *Principia*, Newton is not advocating what Duhem suggests in his best-known criticism, but he is proposing something very close to the establishment of a logical priority of physics over metaphysics, a familiar thesis defended by the French physicist himself.

Keywords:

Pierre Duhem; Newton; universal gravitation; Newtonian method; *Principia*

Received: 02 April 2017. Reviewed: 11 May 2017. Accepted: 30 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.03>

7

Introduction

Pierre Duhem is often remembered as a critic of the Newtonian method. He was one of the first to undertake a more detailed logical analysis of Isaac Newton's masterpiece, *Mathematical Principles of Natural Philosophy* (1687), and to point out some inconsistencies in the claims of the famous scientist about his method. Since then, Duhem's criticism was followed by several well-known philosophers of science, such as Karl Popper, Imre Lakatos, Paul Feyerabend, among others. But it would be a mistake to think that Duhemian analysis of the *Principia*'s method is limited to these more critical considerations. Duhem also carried out a *positive* analysis of this treatise, arguing that the Newtonian physical system unveiled a new and fruitful way of conceiving physical theories, inspired by the desideratum of independence of physics from metaphysics. This "positive analysis", however, was often overshadowed by his critical statements.

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Thus, the objective of this paper is to highlight the positive Duhemian analysis of Newton's method from the confrontation with some historical studies on the methodology of the *Principia*. More specifically, the chief aim of this paper is to show that, although Duhem's criticisms of the Newtonian method are challenged by current studies, such disputes have little impact in his positive analysis; on the contrary, in some cases they even reinforce it. In order to achieve the goal here set out, we first will present an overview of the *Principia* and Duhem's critique, especially those aimed at Newton's pronouncements in the General Scholium. Next, we will present objections to the Duhemian critique from four different authors. Finally, we will offer the aforementioned positive analysis, in conjunction with recent studies of the methodology of the *Principia*. As we intend to show, recent interpretations of Newton's methodological pronouncements in the General Scholium suggest a reorientation of the interpretation of this text in the direction of the positive conclusions of Duhem.

The *Principia* and Duhem's Critique to the Newtonian Method

In the *Principia*, Isaac Newton presents one of the most celebrated and successful physical theories ever conceived, the theory of universal gravitation. This theory emerged with the goal of solving the famous *Two chief world systems* problem, that is, the problem of deciding between the geocentric and the heliocentric world systems (Harper 2011, 2). Astronomical observations at that time reported that planets moved in oval orbits obeying some well-established regularities; however, these observations did not provide any conclusive empirical evidence to determine whether the Earth or the Sun should be considered the real center of these motions. Kepler had suggested that identifying the cause of planetary motions could put an end to this old controversy and, therefore, one of the objectives of the *Principia* was to determine this cause (Harper 2011, 11).

Several hypotheses had already been formulated at the time. Of course, there was the Aristotelian hypothesis of the celestial orbs. Yet most thinkers agreed that this hypothesis did not fit the parameters of the new science. Alternatively, there was the hypothesis of a magnetic force exerted by the Sun, espoused by Kepler; there was Borelli's attractive hypothesis; there was also the hypothesis of mechanical waves, suggested by Hooke; and there was the popular hypothesis of Descartes, by which the planets would be immersed in an ether that moves like a vortex of matter, driving them in its motion.

In the *Principia*, Newton did not ignore these varied hypotheses. Nevertheless, he sought to determine the cause of planetary motions from a rigorous and, as far as possible, non-speculative procedure. Based exclusively on the phenomena established by experience, he concluded that the cause of these motions was an "attractive force" among all matter; a force that manifested itself mainly among celestial bodies of great mass (Prop. 7, Book III). Newton thus concludes that the force of gravity we observe on the Earth's surface is a universal phenomenon, and manifests itself among all bodies and particles. It would be thanks to this force that planets are continually deflected from their rectilinear motions and obey the regularities observed by astronomers. The cause of planetary motion, therefore, is the "universal gravity." And since Newton showed this force to be proportional to the quantity of matter of bodies and inversely proportional to the square of the distances between them, it follows that the center of the system of the world is very close to the center of the Sun. Therefore, the most adequate representation of the system of the world is the heliocentric and not the geocentric system (Prop. 12, Book III).

From the brief introduction of the *Principia* outlined above, it is possible to see that Newton, at one time, rejected the popular cartesian vortices hypothesis, and offered an unprecedented solution to the problem of the system of the world. However, perhaps even more significant, he presented a new mode of inquiry, a mode followed by a rigorous procedure from "effects" to "causes", or from "phenomena" to theory. Both in his preface to the first edition of the *Principia* and in Roger Cotes's preface to the second edition, as well as in the acclaimed General Scholium, one can see the purpose of Newton in highlighting this new mode of inquiry by which his theory is constructed. In the first preface, he manifests the hope that "the principles set down here will shed some light on either this mode of philosophing or some truer one" (Newton 1999, 383). In the preface to the second edition, the editor Roger Cotes called Newton's method an "incomparably best way of philosophizing" (Newton 1999, 386). And in the General Scholium, Newton provides a brief description of this "incomparably best" method. It is worth transcribing this memorable passage:

I have not as yet been able to deduce from phenomena the reason for this properties of gravity, and I do not feign hypothesis. For whatever is not deduced from phenomena must be called a hypothesis; and hypothesis, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction. The impenetrability, mobility, and impetus of bodies, and the laws of motion and the law of gravity have been found by this method. And it is enough that gravity really exists and acts according to the laws that we have set forth and is sufficient to explain all the motion of the heavenly bodies and of our sea. (Newton 1999, 943)

The brand new method Newton used in the *Principia* could be summarized, *prima facie*, in the famous statement "I do not feign hypothesis" (*hypotheses non fingo*), and in the comment that in this philosophy "propositions are deduced from the phenomena and are made general by induction". In fact, Newton established here an opposition between "feigning hypothesis" and "deducing propositions from phenomena". He points out that "whatever is not deduced from phenomena must be called a hypothesis". Feigning hypotheses and deducing from phenomena are two opposed approaches. The latter would characterize the new way of inquiry proposed in the *Principia* — a way whose effectiveness would be attested by its successful application in solving the two difficult problems mentioned above. Furthermore, the reception of the resolution of these problems in the following centuries strengthened the idea that, in the *Principia*, Newton had in fact launched a secure method in natural philosophy, one which deserved to be imitated.

Duhem did not disagree with this general historical perspective about the *Principia*. However, he was responsible for one of the most influential criticisms of the argument of universal gravitation, especially as described by Newton in the General Scholium.

According to the French physicist, Newton believed he had "deduced" his theory of universal gravity from "phenomena". These "phenomena" would be Kepler's observational laws of planetary motion, enunciated at the beginning of Book III of the *Principia* under this exact denomination ("Phaenomena"). However, Duhem argued that it is not possible to derive the principle of universal gravitation from Kepler's laws, neither by deduction nor by any inductive generalization. The conclusion of the argument (universal gravity) is simply not consistent with the premises (Kepler's laws) (Duhem 1991, 193). Let us look at Duhem's argument. He begins describing what follows from each of Kepler's laws:

This first law of Kepler's, "The radial vector from the sun to a planet sweeps out an area proportional to the time during which the planet's motion is observed," did, in fact, teach Newton that each planet is constantly subjected to a force directed toward the sun.

The second law of Kepler's, "The orbit of each planet is an ellipse having the sun at one focus," taught him that the force attracting a given planet varies with the distance of this planet from the sun, and that it is in an inverse ratio to the square of this distance.

The third law of Kepler's, "The squares of the periods of revolution of the various planets are proportional to the cubes of the major axes of their orbits," showed him that different planets would, if they were brought to the same distance from the sun, undergo in relation to it attractions proportional to their respective masses. (Duhem 1991, 191)

According to Duhem, Newton infers the centripetal force toward the Sun from Kepler's law of ellipses (now Kepler's first law); he infers the proportion of the inverse square of the distance from the law of the areas (today Kepler's second law); and he infers the proportionality between the centripetal force and the mass of the planets from the harmonic law (now Kepler's third law). Nevertheless, the theory of universal gravity does not merely states there is an attractive force toward the Sun proportional to the mass of the planets and proportional to the inverse square of the distance from its center. It states there is a *mutual attraction* between all the planets, something that cannot be taken from the Keplerian premises. Moreover, there is a mutual attraction not only between all the planets, but also between all bodies and all matter.

For Duhem, the result of mutual attraction between all bodies can definitely not be derived from the Kepler's laws. From these laws it follows at most that there is an attraction of the planets towards the Sun,

and from the satellites towards the planets, proportional to their quantities of matter and inversely proportional to the square of the distance between their centers. Further, no consistent inductive generalization towards universal gravity can be provided from this result, for such generalization implies the prior recognition of a mutual attraction between the celestial bodies. Moreover, and even more surprising, from the universal gravity, Newton draws the conclusion that Kepler's laws themselves are wrong. Proposition 13 of Book III of the *Principia* shows that, taking the theory of gravitation as the foundation, the Sun does not precisely occupy the focus of the planetary orbits, and the planets do not obey the harmonic and area law in relation to the Sun (as Kepler's Laws dictate). Duhem states: "The principle of universal gravity, very far from being derivable by generalization and induction from the observational laws of Kepler, formally contradicts these laws. If Newton's theory is correct, Kepler's laws are necessarily false" (Duhem 1991, 193).

Newton claims to have deduced the universal gravitation from Kepler's phenomena, but his own theory shows that these phenomena are false. Therefore, either the theory of gravitation is false because it contradicts the premises on which it is based, or the theory of gravitation is true, but it was not really deduced from phenomena, as Newton argued. Apparently, Duhem follows the latter option. He considers Newton's methodological observations to be misleading, and seeks an alternative interpretation for the formulation of universal gravity.

What alternative interpretation would this be? Precisely the one that was derived from his own conception of the nature of physical theories.

In his work, Pierre Duhem analyzed the nature of physical theory in particular. One of his main theses establishes that the purpose of physical theories is not to explain natural phenomena, but to construct a "natural classification" of experimental laws (Duhem 1991, chap.1-2). In other words, the role of theories is simply the systematic coordination of experimental laws, or to make the experience more easily assimilable and manipulable without the pretense of increasing to some degree the content of truth that experience provides (Duhem 1991, 327). As the author himself admits, this understanding finds parallels in Mach's work, which related the goal of physical theory to a principle of "economy of thought". The purpose of physical theory would be to "replace experience with the shortest possible operations" or to provide a synthesis of a large set of experimental laws in a unique and economical formulation (Duhem 1991, 327).

For Duhem, therefore, Newton could not even have deduced the universal gravity from Kepler's phenomena, for that is not how physical science works. What Newton effectively did was to construct a theory that is the synthesis of a significant number of experimental laws, like Kepler's laws, the Galilean law of the falling bodies, the laws of motion, pendulums, motions of the Moon, and so on. The theory of universal gravitation would be a "natural classification of experimental laws," and not a rigorous deductive or inductive result from Kepler's laws, as Newton suggested in the General Scholium.

Objections to Duhem's Criticisms to the Newtonian Method

Duhem's criticism of Newtonian method became influential among important twentieth-century philosophers of science. Karl Popper, for example, stated "it is impossible to derive Newton's theory from either Galileo's or Kepler's, or both, whether by deduction or by induction. For neither a deductive nor an inductive inference can ever proceed from consistent premises to a conclusion that formally contradicts these premises" (Popper 1983, 140). Similarly, Imre Lakatos stated that "Newton's compartmentalized mind cannot be better characterized by contrasting Newton, the methodologist, who claimed that he derived his laws from Kepler's 'phenomena', with Newton, the scientist, who knew very well that his laws directly contradicted these phenomena" (Lakatos 1978, 210). Similar comments can be found in Feyrabend (1970, 164, note 11).

However, in the face of the more detailed historical studies of Newtonian science in recent times, Duhem's analysis has begun to receive severe criticism – starting with the rather sketchy historical details on which he based his analysis. For example, the presentation of the three fundamental dependencies of gravitational theory from Kepler's three laws deviate significantly from the details of the *Principia*'s argument. Although it is permissible to deduce the proportionality between the centripetal force and the mass of the planets from harmonic law (Kepler's third law), Newton did not deduce the centripetal force from the law of ellipses (Kepler's first law), nor did he deduce the inverse square law from the law of areas (Kepler's second law). Considering Duhem's observations, it is clear that he did not study the *Principia* in-depth, or rely on a secondary source. Even a simple reading of the *Principia* is sufficient to realize that Newton rejected the

admission of the law of ellipses as a "phenomenon" in Book III, and deduced the inverse square rule from the harmonic law, not from the area law. The area law, in turn, was used to deduce the centripetal force.

The above observations, however, are not the most significant criticisms directed at Duhem's analysis. More substantial objections can be drawn from the studies of George Smith, Nicolas Maxwell, Zev Bechler, and William Harper. Initially, we will focus on the objections of Maxwell, Smith and Bechler; later, we will discuss those of Harper.

According to Maxwell, Duhem's criticism can be summarized as a claim that "the whole argument of the *Principia* is nothing more than a *reductio ad absurdum*." He exemplifies: "Assume *A* (the laws of motion and Kepler's law); Derive *B* (Kepler's laws are false). *B* contradicts *A*, therefore *A* is false. Newton's great argument is reduced to the demonstration that his initial assumptions amount to a contradiction – not what Newton claimed to prove at all" (Maxwell 2014, 3-4). However, the author recognizes that when we analyze Duhem's critique, we have the strong impression it does not do justice to the sophistication of Newtonian argument. Moreover, many should find that the fact that the conclusion of universal gravitation led to corrections in Kepler's premises, "far from sabotaging the whole validity of the argument, is actually one of the great strengths of the argument, and provides additional strong grounds for holding that the law of gravitation should be accepted" (Maxwell 2014, 4). In other words, this peculiarity, far from being a weakness of the *Principia*, is "the clinching argument". Maxwell, therefore, seeks to show that the conclusion of gravitation is not inconsistent with its premises. According to the author, Newton's argument is a succession of steps, some deductive and others inductive. And although the conclusion of gravitation corrects Kepler's initial assumptions, it does not invalidate the whole argument. He argues that if we supply Kepler's observations with additional phenomena, constructing a more precise version of these premises, "and run the whole argument as before, deductive stages plus inductive stages, with these new corrected premises, no Duhem inconsistency between premises and conclusion emerges at all. The whole argument becomes self-consistent"(Maxwell 2014, 4). In short, Duhem failed in his analysis of the Newtonian method.

George Smith also challenges Duhem's analysis. He argues that when Newton declares he deduced his theory from Kepler's phenomena, he would not be referring to strictly Keplerian motions, but to approximate Keplerian motions. Following Cohen's interpretation of *Newtonian style* (Cohen 1980), Smith assumes the procedure developed by Newton in the *Principia* as essentially approximate. It starts from a simplified mathematical model that involves a single body of unit mass under the action of an attractive force toward a mathematical point. He draws some conclusions from this model and adds, in the following steps, more bodies, specific forms, non-negligible quantities of matter and other accidents. Thus, at each step of the process, the constructed model is refined and becomes closer to its expression in the world of nature. This interpretation of the Newtonian method has repercussions on Duhem's criticism because it would reveal that Newton has never attempted to deduce the universal gravitation from Kepler's laws in the sense that the French physicist understood, nor would he be suggesting that in the General Scholium. In Smith's words:

[Newton] is using "if, then" statements that have been shown in Book 1 to hold in "if (...) *quam proxime*, then (...) *quam proxime*" form to infer conclusions from premises that hold at least *quam proxime* over a restricted period of time. Of course, this means that the deduction shows only that the conclusions, most notably the law of gravity, hold *quam proxime* over the restricted period of time for which the premises hold. The Rules of Reasoning then license the conclusion to be taken exactly, without restriction of space or time. The conclusions, so taken, do indeed then show that the premises hold only *quam proxime*, and not exactly. This conclusion in no way contradicts the premises. (Smith 2008)

In the General Scholium, Newton would have mentioned a "deduction from phenomena" only in a generic form. The meaning of this expression would be that the inferences that culminated in the universal gravitation were undertaken on the grounds of Kepler's phenomena. As revealed in Book III of the *Principia*, for Newton both Kepler's phenomena and the conclusion of universal gravitation were approximate. In Smith's analysis, Duhem would have ignored this essentially approximate character of the *Principia*, a character already evidenced by Cohen's work (Cohen 1980).

Bechler's interpretations converge with that of Smith. After informing that Newton himself perceived and explicitly expressed the above-mentioned "inconsistency", and that he did not seem surprised or worried about it at all, the author proceeds to an enlightening synthesis that reveals Newton's conclusion does not

contradict his premises. It is worth looking at Bechler's arguments in detail.

Abbreviating Kepler's laws of ellipses, areas, and period by KI, KII, and KIII, respectively, Bechler draws a distinction between "Keplerian motion" and "non-Keplerian motion" in a way that the former is the one that obeys at least two laws of Kepler, namely, KI and KII or KII and KIII. In addition, he distinguishes a "strictly Keplerian motion" (St.Kp.) and a "non strictly Keplerian motion" (N.St.Kp.), and an "observationally Keplerian motion" (Ob.Kp) and an "observationally non-keplerian motion" (Ob.N.Kp.). It is not difficult to realize that a St.Kp. will be an Ob.Kp., but an Ob.Kp. can be both St.Kp. and N.St.Kp; moreover, it is not difficult to realize that an Ob.N.Kp. is also a N.St.K; but a N.St.K. can be both Ob.N.Kp. as Ob.Kp (Bechler 1991, 401).

From the above definitions, Bechler divides the argument of the *Principia* into four stages. In the first stage, Newton considers a simplified mathematical model and derives the inverse square rule for central forces from St. Kp motions. In the second stage, he shows that planetary motions are Ob.Kp. motions, and from this he builds his proof that planets attract themselves with a force proportional to their masses and to the inverse square of their distances. In the third stage, there is the inductive jump to universal gravitation. And in the fourth stage, assuming universal gravitation, he shows that the true planetary motions are N.St.Kp. Therefore, there is no inconsistency between the conclusion and the premises of universal gravitation. For – as Bechler asserts – "even though universal gravitation would imply N.St.Kp. Motions, these still could well be Ob.Kp." (Bechler 1991, 402). In short, the premise of universal gravitation is not the St.Kp., but the Ob.Kp one.

Harper's Critique of the Thesis of Underdetermination

Harper's remarks on Duhem's criticism partly diverge from the previous analyses. He clarifies that Duhem was well aware that the experimental laws and, in particular, Kepler's laws, were approximate. That would not be the main point of his criticism. The main point would be that these laws, as long as they are approximate, to be useful in the inference of the theory, have to receive a specific interpretation among many others possible. In other words, Newton used a "symbolic" translation of Kepler's laws, and this translation implies the assumption of a whole previous theoretical body. It implies the assumption of a significant number of hypotheses. Harper states:

According to Duhem: Kepler's laws are empirical approximations that succeed in reducing Tycho Brahe's observations to law; but, they needed to be translated into symbolic laws to be useful for constructing physical theory, and such translation presupposed a whole group of hypotheses. (Harper 2012, 130)

Clearly, Harper is referring to Duhem's famous holistic thesis, also known as Duhem-Quine thesis.

In his analysis of physical theories, Duhem realized that once experimental laws are approximated, there would never be a single hypothesis compatible with this law. In fact, there would be an infinity of hypotheses whose consequences could fit the experience. Thus, in order to be able to compare the consequences of a hypothesis with experience, the physicist would first have to make a "symbolic transformation" in the experimental laws. This transformation involves the translation of these laws into a single mathematical version that can then be compared with theoretical results. However, this procedure comprises the prior admission of a set of hypotheses and additional theories that support the scientist's choice.

In the light of the foregoing, Duhem argues that there are two basic characteristics in the laws used by theoretical physics: they are "symbolic" and "approximate". Physical theory is fundamentally a symbolic construction representing laws of experimental origin. As Duhem said, physicists do not "conceive an experimental fact without simultaneously making it correspond to the abstract and schematic expression that theory gives it" (Duhem 1996 [1894], 80). But what is the form of expression capable of providing abstract and symbolic representations that replace the data of experience? The French thinker states that this language of theoretical physics is mathematical analysis. Analysis taken not as an end, but as an instrument to derive hypotheses through which theory must be subjected to the control of experience. On the other hand, mathematics also allows one to represent, through an algebraic or geometric quantity, the most immediate properties of the corresponding physical notions. Thus, for example, the mathematical

properties of the physical notion of "temperature" represent the experimental properties of the notion of "heat." Mathematical analysis relates the experimental laws to each other, using these symbolic representations of the physical notions.

In the case of gravitation, in order for Newton's theory to suggest the principle of universal gravitation, it is necessary that the laws collected by Kepler from astronomical observations be transformed in a way such that properties of the force exerted on the planets by the Sun become objects of a mathematical analysis. Such a symbolic transformation of Kepler's observational statements involves the physical notions of mass and force, whose meaning derive from dynamic laws. Thus, the translation of Kepler's laws into symbolic laws, the only ones useful to theory, presuppose "prior adherence to a whole set of hypotheses" (Duhem 1991, 195). The requirement for a "holistic consistency" imposes itself again to show that "no experimental law is useful to the theorist before he has been subjected to an interpretation that transforms it into a symbolic law" (Duhem 1991, 196).

Of course, the reflection set out above has wide consequences. For our purposes, it will suffice mentioning only three of them.

The first consequence is that every experimental proposition is *theory-laden*, since in this proposition a series of principles converge drawn from various theories, and the result of the experiment will have consequences for the whole set. The second consequence is that no particular experiment is able to confirm or falsify an isolated hypothesis. An experiment that contradicts a hypothesis actually contradicts the set of assumptions presupposed in the situation for experimentation, so that it is not possible to locate precisely the incorrect hypothesis. In Duhem's words: "When the experiment is in disagreement with its predictions, what he [the physicist] learns is that one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed" (Duhem 1991, 187). The third consequence is that experimental laws are not established by being verified one by one. This kind of verification is not possible in science. In fact, the comparison of a hypothesis with experience necessarily involves the testing of the whole set of hypotheses, which are inseparable. Referring to the case of universal gravitation, Duhem states:

Such a comparison will not only bear on this or that part of the Newtonian principle, but will involve all its parts at the same time; with those it will also involve all the principles of dynamics; besides, it will call in the aid of all propositions of optics, the statics of gases, and the theory of heat, which are necessary to justify the properties of telescopes in their construction, regulation, and correction, and in the elimination of the errors caused by diurnal or annual aberration and by atmospheric refraction. (Duhem 1991, 194)

The three consequences we posit above – that every experimental proposition is theory-laden; that it is not possible to confirm or falsify an isolated hypothesis; and that the experimental laws are not established one by one – evidently give rise to a more rigorous critique of Newton's observations in the General Scholium. Since there are an infinite number of equally possible symbolic translations for the same set of experimental laws, Newton's choice of a given dynamic translation of Kepler's laws among infinite possible ones was logically underdetermined by these same laws. Further, experiments cannot help him in this choice, since experience itself is not possible without the theory that underlies it. The appeal to experience in this case would be circular. Therefore, the main problem with Newton's claim about deducing the gravitation from Kepler's phenomena would not be the supposed inconsistency between the conclusion and the premises. The problem would instead be that even if planetary motions were conceived as approximate, that is, as *Ob.Kp*, it would still mean only that Newton made a certain symbolic translation of these motions among many possible ones, which presupposes his previous adherence to a certain number of theoretical hypotheses. On the face of it, his famous pronouncement *hypotheses non fingo* would be clearly in check.

As we presented earlier, in the General Scholium Newton described the "deduction from phenomena" as opposed to "feigning hypothesis". He stated thusly: "I have not as yet been able to deduce from phenomena the reason for this properties of gravity, and I do not feign hypothesis. For whatever is not deduced from phenomena must be called a hypothesis". However, the symbolic transformation he had to make in Kepler's experimental laws implies the prior association of many hypotheses.

One could think this point to be a demerit of the theory of gravitation in particular. Instead, it would

be only the recognition of the usual operation in construction of physical theories. In other terms, it would not be a problem of the argument of gravitation specifically, but rather, at most, a philosophical problem requiring justification of why, in spite of this, inferences that culminate in physical theories still deserve to be considered rational. In part, it is to justify this question that Duhem formulates his conception of the nature of physical theories. In any case, constructing a theory without assuming or feigning hypotheses, or constructing a theory that is perfectly "deduced from phenomena," whatever the meaning might be given to that expression, would be impractical, not only in the *Principia*, but also in the general framework of the scientific enterprise. In short, in one way or another, Newton's description of his method in the General Scholium would be mistaken.

In contrast to what Newton suggested in the scholium, Duhem argues that among the many possible translations, the physicist "has to choose one which will provide him with a fruitful hypothesis" (Duhem 1991, 199). Consequently, "the motives that guides their choice will have neither the same nature nor the same imperious necessity as those which require the preference of truth over error" (Duhem 1996 [1894], 104). For example, the fecundity Newton observed in the principle of universal gravitation derive from the goals he drew for his theory, namely, the natural classification of celestial motions. As we have seen above, Duhem considers the representational character and the continuous progress the main attributes of the physical theories constructed according to a natural classification of experimental laws. If Newton indeed accomplished such objectives with his theory, the fecundity he found in the principle of universal gravitation is due to the guarantee it offered to realize such attributes of physical theories.

Harper realized that the main point of Duhem's criticism on Newtonian method would be the reflections outlined above. As described by Newton in the General Scholium, the method of the *Principia* is impracticable. Another interpretation should be provided. Not only for the method of this particular treatise, but for science in general. Nevertheless, Harper also raises an objection to this second criticism of the French thinker. Indeed, a good part of his book substantiates this objection.

The core of Harper's study in his book is to show that the method that best characterizes Newton's *Principia*, and more broadly science until today, is not the *hypothetical-deductive method* (HD), presupposed in Duhem's analysis. Harper warns that, like many interpreters of science before and after him, Duhem is committed to a HD version of science, a version in which prediction of phenomena is the requirement for confirmation of theories. And this being the ideal of confirmation of science, on account of the non-accuracy of the empirical data and the experimental laws, the holistic thesis of Duhem immediately follows.

However, it happens that for Harper, Newton's method is not purely HD. From a detailed study of the *Principia*, he sought to show that the method of this famous treatise constitutes a model of confirmation that is supported by "systematic dependencies" of phenomena based on "theory-mediated measurements". Obviously, it is not possible to go deeper into the details of his discussion here, but it will be sufficient to say that this model envisages a systematic mathematical study of independent phenomena from which we draw consequences that, once converging onto the same result, produce a confirmation that excludes other possible alternatives (Harper 2011). One example will suffice.

In the hypothesis of the centripetal force acting on the planets being proportional to the inverse square of the distance, the confirmation of this hypothesis is not only due to its consequences from Kepler's harmonic law. Since this law is only observed approximately for the planets, based only on this inference we do not have any guarantee that the force law acting on these planets is an inverse square type. Other options might be possible. However, in the *Principia* Newton also investigates the consequences of others force laws with varied powers of distance: force law proportional to the distance, inversely proportional to the distance, inverse cube of the distance, in addition to a whole class of force laws proportional to any integer power of the distance (D^n) (cor. 7, 8 of Prop. 4, Book I). It is from this group of studies that Newton concluded that none of the powers of distance besides the inverse square could generate a motion similar to that observed in the planets.

Even so Newton was not satisfied. What about small deviations from the law of the square inverse? What about fractional powers between 2 and 3? Are these alternatives also excluded? From another series of studies (Section 9 of Book I), Newton concluded that the smallest deviation in the inverse square law would lead to a precession motion in the planetary orbits, which could be clearly distinguished in the astronomical observations. As no similar motion is observed for the planets around the Sun, all alternatives to the inverse square law were excluded, and the study of systematic dependencies converged onto a single hypothesis. For Harper, it was this set of systematic dependencies studies that allowed Newton to effectively

confirm the inverse square law. Therefore, Newton's method is not merely HD. Likewise, the same can be said of current scientific method. The expression "deduction from phenomena" would be only a generic reference to this inference procedure that makes use of the analysis of systematic dependencies from independent phenomena. Newton made no mistake in describing his procedure in the *Principia*; Duhem is the one who misunderstood him.

The Autonomy of Physics from Metaphysics and the Newtonian Physical System

Even though Harper's critique of underdetermination is controversial, one might wonder whether, if this thesis were accepted, there would still be some breath to the Duhemian analysis of the Newton's method. In the last two sections of this article we intend to show that there is. As we mentioned in the introduction, Duhem's analysis of the *Principia*'s method is not limited to the negative observations presented earlier. These observations focus more on Newton's description of his method than on the method itself. In analyzing the *Principia* itself, Duhem adopts a distinct discourse. For him, this treatise would be a watershed in the history of scientific thought.

As we mentioned earlier, Pierre Duhem analyzed the nature of physical theory in his work, and concluded that the purpose of these theories is not to explain natural phenomena, but to construct a "natural classification" of experimental laws. This conception substantiates his criticism concerning the use of "mechanical models" in science. Duhem was a severe critic of the use of these models in the construction of physical theories. The problem with this approach lies precisely in the pretension of explaining the reality of phenomena and things. Since physical theory is barred from offering such explanations, the application of mechanical models goes beyond the limits of the content physics can legitimately sustain.

The so-called "mechanicism" system, which became popular in the sixteenth and seventeenth centuries, is used by Duhem as an example of transgression of these limits. In Duhem's words, this system would be a kind of "false ideal" that physicists had long pursued. The problem with this system - as with any other that attempts to explain phenomena with mechanical models - is that it makes physical theories dependent on metaphysics. Since there is no possibility of justification of these explanations in the empirical plane, its bases rest necessarily in metaphysics. For Duhem, this is not admissible, since what should characterize physical theory is precisely its autonomy in the face of metaphysics.

For the French physicist, the autonomy of physics over metaphysics is first established through the experimental method. Without this experimental element, physics could not "constitute itself through a proper method independent of any metaphysics" (Duhem 1996, 34). The scientist can make legitimate use of the experimental method without certain notions (body, laws of physics, etc.) and principles (the axioms of geometry and kinematics, the existence of laws regulating phenomena) have been fully understood. What these notions and principles have of evident in themselves is what is necessary and sufficient in Physics (Duhem 1996 [1893], 35). Besides this autonomy in the "experimental phase" (the observations of facts and their reduction to laws), physics would also be independent of metaphysics in the "theoretical phase". By incorporating them into physical theories, experimental laws have the same meaning as they did before in isolation. Physical theory explains nothing "about the *raison d'être* of these laws and about the nature of the phenomena they rule" since it serves primarily for practical rather than metaphysical purposes. Within its own limits, it is therefore absurd to seek among the truths of metaphysics either the confirmation or refutation of a physical theory, at least to the extent that it remains confined to its proper domain (Duhem 1996 [1893], 36).

Duhem believes that perhaps it was Descartes who most contributed to breaking the barrier between the methods of physics and the method of metaphysics, confounding his domains (Duhem 1996 [1893], 44). The long deductive chains by which the mechanical explanations of physical phenomena are derived from metaphysical principles of matter and knowledge, make the distinction between physics and metaphysics devoid of any foundation. Descartes's method, from Duhem's point of view, suppressed the autonomy that Peripatetics had granted to physics in the face of metaphysics. Descartes, starting from the definition of matter as an extension, associated with the principles of figure (geometry) and motion (kinematics), intended to "construct the world" deducing explanations of all physical phenomena. But subjugated by the method of

metaphysics, mechanical theory has incorporated elements beyond the realm of the physical method.

Despite Descartes' great influence on later physicists, Duhem argues that mechanistic tradition found strong resistance in Newton's physical system, whose work launched a new way of conceiving the nature of physical theory. Duhem interpreted the appearance of the Newtonian system in this manner, with an emphasis on the opposition he offered to Cartesianism (Duhem 1996 [1893], 46). And the main achievement of this approach was to restore physics to its full value, as far as it restored its full autonomy again.

Duhem points out, for example, that the use of the term "attraction" to name the force by which bodies tend toward each other does not imply any compromise with the cause or nature of that force. On the contrary. The force can remain perfectly as a symbol whose physical applicability does not depend on knowing the realities that they intend to represent. In the final comment to Definition 8, Newton's position is clear when he states he uses the term attraction to "any sort of propensity toward a center, considering these forces not from a physical but only from a mathematical point of view", so that he is not "defining a species or mode of action or a physical cause or reason (...)" (Newton 1999, 408). Newton is not committed to explaining the causes of this force that produces gravity. This can also be seen at the beginning of the passage quoted from the General Scholium, when he admits he did not establish the "cause of gravity". This aspect introduces the *Principia's* method into the autonomy Duhem understands as one of the most fundamental features of physical theory.

In addition, Duhem also emphasizes that in Newton's understanding the theory of gravitation retains its full value whatever the result of the investigation of the cause of that attraction. This is what is presented in the General Scholium itself. Newton states that although he did not establish the cause of gravity, "it is enough that gravity really exists and acts according to the laws that we have set forth and is sufficient to explain all the motion of the heavenly bodies and of our sea" (Newton 1999, 943). Put differently, gravitational theory would not be obliged to make claims regarding the ultimate cause of the mechanism of attraction. The theory is fully valid, despite the unawareness of this cause.

Notwithstanding these remarks, it should be mentioned that although Newton did not explain gravity, it is evident that at least he has the expectation to explain. He said he have not "yet" assigned the cause of gravity. This is important. For Duhem, it is not a question of saying that physical theory does not care or is not interested in the explanations of natural phenomena. In fact, what it does is to establish a necessary course towards explanations. The theory will move toward explanations "going from effects to causes" – to use Newton's terminology. That is to say, there would be a "logical priority" of physics over metaphysics. As Duhem asserts, "any metaphysical investigation concerning brute matter cannot be made logically before one acquires some understanding of physics" (Duhem 1996 [1893], 32). Thus, Newton did not exclude any metaphysical pretension of his system of investigation. What he did was to distinguish physics from metaphysics first, then establish an order of inquiry, that is, from physics to metaphysics.

This "pretension" of the Newtonian system to proceed methodically toward the ultimate causes of phenomena is not understood by Duhem as foreign to the scientific enterprise. While avoiding mechanical models, which confer real value to the laws, Duhem also rejects the positivist and conventionalist position, which relegates the goal of theoretical physics exclusively to the development of an economic summary or an *artificial* classification of experimental laws. For the French thinker, what must be preserved is the epistemic value of physical theory – transcendent to its practical utility and always within the reach of the physical method. Thus, it is expected from physical theory that by becoming a natural classification, it can establish a logical coordination among the various experimental laws that is the image and reflection of the true order according to which the realities that escape us are organized (Duhem 1996 [1893], 31). Physical theory therefore moves toward a final explanation; one could say, a "metaphysical" explanation. But it does not occur by means of a deduction of physics from metaphysics. Even though it is theoretically possible, it is not possible in a practical manner. (Duhem 1996 [1893], 34). Given this limitation of epistemological order, the only secure methodological resource we have would be the physical method. The task of revealing the epistemic value of physical theory rests exclusively in the physical method – not directly, for these realities escape from physics – but through an analogy that imposes itself on the mind of the physicist (Duhem 1990, 186).

Therefore, Newton's *Principia* would have established one of the most fundamental characteristics of the physical method. He would strictly follow an epistemological order of physics toward metaphysics, and never the opposite. For Duhem, scientists such as Euler, Lagrange, Laplace, Gauss, Fresnel, Poisson, Ampère, and Cauchy, among others, can see the fecundity of this understanding in the significant progress

made by this approach in the research of diverse phenomena, such as electricity, capillary, elasticity and heat theories. After Newton's abstract notion of "attraction" disconnected from metaphysical meanings, molecular attraction has become a potent tool for constructing synthetic representations of physical phenomena. As a result, there has been a long period of continuous progress in these "attractionist" theories, which have remained loyal to the limits established by the Newtonian system. This trend, according to Duhem, was prevalent until the middle of the nineteenth century, when the rapid development of thermodynamics, which corroborated Descartes' assumptions about the nature of heat, triggered a new trend of explanatory theories. However, in his own time Duhem diagnosed that the contradictions and failures of this new trend were gradually led the physicists "back to the sound doctrines Newton had expressed so forcefully" (Duhem 1991, 53).

Hypotheses non Fingo and the Logical Priority of Physics over Metaphysics

From what was discussed in the previous section, it is possible to see that for Duhem, Newton's system reestablished the autonomy of physics in relation to metaphysics. And it occurred largely on account of the specific Newtonian approach that gave primacy to the symbolic abstractions of forces, rather than metaphysical speculations on causes. First, it is important to say that this interpretation of the French physicist finds echoes in the historical studies of I. B. Cohen, who argued that one of the most fundamental aspects of the Newtonian method is the priority it set for the mathematical level of analysis over the physical level (Cohen 1980). It is worth remembering here that in Newton's terminology, what Duhem understands as "physics" (i.e., mathematical-physics), Newton understood simply as "mathematics"; and what Duhem understands as "metaphysics", in Newton's context was part of the "natural philosophy" (which was also called "physics"). Thus, the priority of the mathematical level of the analysis over the physical level, to which Cohen refers, does not differ significantly from what Duhem understands as the priority of physics over metaphysics.

But secondly, it must also be said that in the light of even more recent studies on the *Principia* it is possible to conclude that not only was Newton himself aware of this methodological prioritization, this would in fact be the most fundamental meaning of his General Scholium³

Duhem's misunderstanding of Newton's words in the General Scholium was apparently deeper than previous criticisms suggest. Duhem understood that Newton's purpose in the famous scholium was to clarify or summarize the argument that led to universal gravitation. However, far from it, the context of the General Scholium is to clarify the method of "experimental philosophy". Written 26 years after the first publication of the *Principia*, the focus of this conclusive text is to respond to later polemics, and especially those around the tension between the method of "experimental philosophy" and the so-called "speculative" or "hypothetical philosophy". That said, what Newton seeks to reinforce in his scholium is that in "experimental philosophy" (not necessarily in the *Principia*'s argument of gravitation), "propositions" (not "gravitation theory") are "deduced from phenomena and made general by induction." Newton is not saying that "universal gravitation" was deduced from "Kepler's phenomena." He is asserting that by the method of "experimental philosophy" all and every "proposition", in order to be accepted, ought to be "deduced from phenomena".

The "phenomena" to which Newton refers are not only the Kepler's laws enunciated at the beginning of Book III. They are every and any result obtained from experience. The very argument of gravitation in Book III presents several empirical results beyond Kepler's laws: magnetic experiments, collisions between bodies, data on the precession of the Moon, and Newton considers them all as "phenomena." Therefore, to assert that in experimental philosophy the propositions must be "deduced from the phenomena and made general by induction" is merely to affirm that the method on which the *Principia* is based is an essentially empirical method. Statements that do not have solid empirical support are not accepted. Moreover, since the *Principia* are constructed following the axiomatic model of the demonstrative sciences, the verb "deduce" does not cause great surprise either. Any proposition that is built upon a phenomenal statement must be writable in the form of a syllogism from that statement.

Universal gravitation is offered in the General Scholium only as an example of the procedure of "the

³ The next remarks were developed in the author's (Ricardo Santos) PhD Dissertation, *The science methodology of Newton's Principia*, yet to be published.

experimental philosophy." For Newton, the laws of motion were also constructed in the same way; simple empirical concepts such as "impenetrability" and "mobility", which could hardly be considered in the same sense that Duhem attributes to a "physical theory", were also constructed in the same way. In short, at no point did Newton intend to assert that universal gravitation was obtained by means of a strict deduction or induction from the laws of Kepler. Instead, he sought to clarify that gravitation was obtained by means of an argument whose inferences were made based on "propositions" taken — all of them — rigorously from experience, by means of a rigorous empirical-mathematical method.

Undoubtedly, the understanding summarized above leads the motto *hypotheses non fingo* to another interpretation. As was said earlier, "feigning hypothesis" is the opposite procedure to "deduce from phenomena". And this procedure implies an extrapolation of the strict limits of the empirical science is being proposed in the *Principia*. In other words, to frame hypotheses implies assuming statements of any kind that are not empirical; but in particular, metaphysical statements. For this reason, Newton adds in the scholium: "For whatever is not deduced from phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy" (Newton 1999, 943).

Any statement that is not "deduced from phenomena", *i. e.*, that cannot be considered as attested empirically, must be deemed a "hypothesis"; as such, it should not be accepted in experimental philosophy. Thus, the "hypotheses" Newton mentions in the scholium have little relation with those of the problem of underdetermination mentioned above. What the English mathematician is seeking to establish in the General Scholium – ironic as it may seem – is something very close to what Duhem himself understood as one of the greatest benefits brought by the Newtonian system: the autonomy of physics over metaphysics. Or an epistemological priority of empirical and mathematical statements over those of traditional speculative natural philosophy.

In the end, Duhem was not so mistaken in his analysis of Newton's method as some might suppose. And Newton was not such a bad methodologist as Duhem supposed.

Acknowledgements

The authors would like to thank Dr. Ron Martinez and the Academic Publishing Advisory Center (*Centro de Assessoria de Publicação Acadêmica, CAPA* - www.capa.ufpr.br) of the Federal University of Paraná for assistance with English language editing.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 20-35

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

The French Roots of Duhem's early Historiography and Epistemology

Stefano Bordoni¹

Abstract:

Pierre Duhem can be looked upon as one of the heirs of a tradition of historical and philosophical researches that flourished in the second half of the nineteenth century. This tradition opposed the naïve historiography and epistemology of the positivist school. Beside the positivists of different leanings such as Littré, Laffitte, Wyruboff, and Berthelot, we find Cournot, Naville, and Tannery, who developed sophisticated histories and philosophies of science focusing on the real scientific practice and its history. They unfolded elements of continuity and discontinuity in the history of science, and enlightened the complex relationship among experimental, mathematical, logical and philosophical components in scientific practice. In Pierre Duhem we find a systematic and vivid interpretation of these meta-theoretical issues, and a meaningful development of a cultural tradition that re-emerged in the second half of the twentieth century.

20

Keywords:

Pierre Duhem; historiography; epistemology; experimental practice; theoretical practice; continuism - discontinuism

Received: 27 March 2017. Reviewed: 26 May 2017. Accepted: 31 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.04>

The originality of Duhem's meta-theoretical researches consisted in the interconnection between historical inquiry and philosophical analysis. The complexity of the natural world and the complexity of scientific practice urged him to go beyond the naïve historiography and epistemology of the positivist tradition. In reality, another tradition had already emerged alongside positivism in the second half of the nineteenth century. Some mathematicians had inquired into the actual scientific practice and its history, and had discovered a plurality of theoretical streams, and stagnations or regressions over time. In their philosophy of science, the positivistic rhetoric of relentless progress, and the cult of a simplified and idealised scientific practice gave way to a mature awareness of inescapable shortcomings and dramatic detours in scientific method and its history.

I would like to focus on the positivist and the *critical* traditions, and then on some historiographical

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and epistemological issues Duhem developed in the early stages of his research. He can be looked upon as the direct heir of the second tradition, which had already put forward a critical historiography and epistemology. Although Duhem has been considered as one of the founding fathers of the modern philosophy of science, I would like to interpret him as the most outstanding follower of an already existing tradition. In some specific issues he discussed between 1892 and 1896, we can find some traces of the previous *critical* tradition. Nevertheless, in the papers he published in this time span we do not find explicit references to scientists and philosophers who had previously put forward a critical analysis of scientific practice.² This seems really surprising when we notice that Duhem always mentioned the scientists who had contributed to the establishment of the mathematical thermodynamics he developed in the same years. The fact is that his interests in history and philosophy of science had emerged from his researches in theoretical physics rather than from an autonomous philosophical research on the track of a specific philosophical tradition. Duhem was objectively in debt to some previous scientists and philosophers, but this debt did not correspond to a direct influence. The content of his scientific researches on thermodynamics led him to the rediscovery and reinterpretation of the Aristotelian tradition, whereas his actual scientific practice led him to the rediscovery and reinterpretation of a subtle epistemological tradition that can be traced back to Pascal (Bordoni 2017, 240-241).³

I find that the historiographical thesis that places the emergence of a mature philosophy of science in France in the last years of the nineteenth century should be updated (Brenner 2003, 1, 2, 4-5, and 7-8; Chimisso 2008, 1-2, and 5-6; Knight 2008, vii; Rheinberger 2010, 1 and 3-4; *MPIWG* 2012, 7; Braunstein 2012, 33; Brenner (ed.) 2015, 5-6). A sophisticated philosophy of science emerged quite earlier. Poincaré, Duhem, and Milhaud were the heirs of a tradition that did not manage to produce any institutional effect in the last decades of the nineteenth century but left long-lasting traces in French intellectual environment. We do not find a direct filiation or an explicitly acknowledged line of descent but a conceptual stream that flowed through an adverse cultural context.

The Context

21

The last decades of the nineteenth century saw an “industrial and social revolution,” and the spread of new technologies (Barraclough 1964, 17, 36-8, and 40). A process of professionalization of scientific practice, and a process of specialization, took place in those decades.⁴ Both an optimistic and a pessimistic scientism emerged: science represented the suitable solution for solving technological problems, fostering social progress, and “slowing down the deterioration of the human species” (Bowler and Morus 2005, 147-148 and 150; Olson 2008, 253, 274, 277, and 293).

The intellectual trend that can be qualified as *scientism* rested upon two pillars: the unavoidability of human progress, and the close link between scientific and social progress. In the French context, the most radical scientism can be traced back to the six volumes of the *Cours de philosophie positive* that Auguste Comte published between 1830 and 1842.⁵ He coined the expression “*philosophie positive*” in order to qualify his intellectual commitment. His philosophical system was a “*philosophy of sciences*” that encompassed “every kind of phenomena”, social ones included, because all sciences had to be submitted “to a single method”. At least three strong metaphysical commitments supported his ambitious design: first, the rejection of any question that did not deal with a scientific approach to reality, second, the methodological unification among the different sciences, and third, the faith in human progress (Comte 1830, VII-VIII).

² However, it should be specified that the most complex and questionable of Duhem’s concepts, namely the idea that scientific practice aimed at a *natural classification* of material phenomena stemmed from both traditions.

³ On the relationship between Duhem’s thermodynamics and Aristotle’s natural philosophy, see Bordoni 2012b and the tenth chapter of Bordoni 2012c. Bas van Fraassen pointed out the importance of Pascal’s “underground epistemology” in the history of science (van Fraassen 1989, 151). The deep influence of Pascal on Duhem has been stressed by Jean-François Stoffel (Stoffel 2007, 299 and 301).

⁴ The establishment of definite boundaries between science and philosophy was one of the achievements of scientific practice in the late nineteenth century (Ross 1962, 66, and Morus 2005, 3, 6-7, 20, and 53).

⁵ For the polysemy of the word *scientism*, and its connection with the equally plural meaning of the word *positivism*, see Paul 1968, 299, footnote 2. For the origin of the word in the French context, see Schöttler 2012, 253-254.

Comte looked upon his philosophical system as the last stage in the history of civilisation. The first stage corresponded to the dawn of human civilisation, when mankind relied on magic and religion: it was “the theological stage”. The second one, “the metaphysical stage”, corresponded to the emergence and development of philosophy, logic, mathematics, and rational practices in general. The last stage was the *positive* one, and corresponded to a widespread scientific development. Comte ventured to qualify his historiographical framework as a law: “direct observation” proved “the exactness of this law”, and rational considerations suggested the necessity of that law or “positive theory” (Comte 1830, 3-8).

He offered a simplified and idealised account of scientific practice based on the possibility of a sharp separation between science and metaphysics, and on the structural difference between the third stage of science and the previous stages of religion and metaphysics. He underestimated the fact that the founding fathers of modern science pursued metaphysical agendas, and modern science was based on explicit and implicit meta-theoretical beliefs. However *positivism* was both a specific philosophy, which can be traced back to Comte’s *Cours de philosophie positive*, and a broader cultural mood. Moreover, Comte’s philosophy went through different stages, as the historian of philosophy Isaac Benrubi pointed out many decades ago. Comte crossed in reverse order the three stages that would describe the development of Humanity: although he had started from what he considered as the positive stage of mankind, he then “advanced or retrograded to the metaphysical and religious stages” that corresponded to the religion of Humanity (Benrubi 1926, 16-17; Bordoni 2017, 11).⁶

Comte’s *Cours de philosophie positive* had an enduring influence in French-speaking countries and abroad. Even when his positivistic philosophy was judged too radical and dogmatic, the enthusiasm for scientific methods could rely on a wide consensus. We find a milder scientism in the book the British philosopher William Whewell published in 1840, *The Philosophy of Inductive Sciences founded upon their History*. He insisted on scientific progress and the paradigmatic role of physical sciences but paid more attention to history and metaphysics. He found that Comte’s reduction of science “to the mere expression of the laws of phenomena, expressed in formulae of space, time, and number” was “historically false”. To exclude any inquiry into the nature of scientific phenomena would have led us “to secure ourselves from the poison of error by abstaining from the banquet of truth”.⁷ Going beyond Comte’s naïve scientism, Whewell explored the essential tension between the structures of thought and sensorial experiences (Whewell 1847a, v-x, 1, 7, and 14; Whewell 1847b, 321-322, 324, 326, and 329).

In the French context, the expression *philosophy of science* had already been used by the mathematician and natural philosopher André Marie Ampère, and the corresponding meaning was not so different from Comte’s. In the *Essai sur la philosophie des sciences* he published in 1834, Ampère specified that his work dealt with “the analytical exposition of a natural classification of all human knowledge”. The adjective *natural* involved the connection among “the objects of our knowledge”, the essential features of the human mind, and the history of cultural development (Ampère 1834, v-vi, xiii-xiv, xix-xx, xxxi, xxxvi, and xl-xlix).

Ampère was not satisfied with the classification of the *Encyclopaedists*, and followed Comte’s hierarchy that started from mathematics and led to “philosophical, moral, and social sciences” through sciences dealing with inorganic matter and life sciences. At the same time, we find a new, dynamic conception of classification: the progress of science involved a continuous rearrangement of old classifications (Ampère 1834, 2-3, 9-10, 13-15, and 18). The second volume of the *Essai* was published after Ampère’s death. The most eminent of Comte’s followers, the physician, lexicographer, and philosopher Émile Littré, added a celebratory scientific biography, but reminded readers that Ampère himself had regretted not having managed to achieve a more ambitious target, namely a detailed account of foundations and methods of sciences, and a critical analysis of competing theories (Ampère 1843, ix, xiii, lxxxi-vii, and xcii).

In 1848, the English philosopher and logician John Stuart Mill published a long and demanding book,

⁶ In 1930, the historian of science Hélène Metzger remarked that *Positivism* was something more than a mere philosophical school: it was rather one of the essential components of “an atmosphere” or a broader intellectual attitude that branched out in different directions in the nineteenth century (Metzger 1987 [1930], 113).

⁷ Whewell acknowledged that sometimes the borderline between facts and interpretations was vague, and scientists were committed to “*interpreting* the phenomena” rather than merely reporting them (Whewell 1847a, 37, 39-41, 44, 48, and 50).

A System of Logic, Ratiocinative and Inductive, where he put forward a philosophy of science that might be looked upon as an intermediate philosophical approach between Comte and Whewell. In Comte’s “admirable speculations” he found the explicit awareness that the causes of natural phenomena were beyond the understanding of scientists and philosophers: only empirical and mathematical laws were attainable. Nevertheless, he did not agree with Comte on the uselessness of “those scientific hypotheses ... which are unsusceptible of being ultimately brought to the test of actual induction” such as the two hypotheses on the nature of light. (Mill 1848, 172, 209-210, 336, 339, and 433).

Mill’s confidence in the empirical foundation of knowledge put him in contact with Comte and distanced him from Whewell. Where Whewell saw “a conception of the mind, which did not exist in the facts themselves”, or “a principle of connexion”, Mill found that our conceptions were always “conceptions of something which really is in the facts” (Mill 1848, 178-179, 390, 561, 576, and 586).

Two Different Traditions

In 1851, the first outline of a more sophisticated philosophy of science appeared in Paris. The author had gained a reputation as a mathematician who had put forward a daring mathematization of economics.⁸ In 1838, he had published a short book, *Recherches sur les principes mathématiques de la théorie des richesses*, which dealt with “applications of mathematical analysis to the theory of wealth” (Cournot 1838, V, VII-VIII). In 1843, he had published a longer book on statistics and probability, wherein he paid attention to philosophical and scientific foundations (Cournot 1843, III-V, 84, 181-184, and 205-206).⁹

After eight years, in the book *Essai sur les fondements de nos connaissances et sur les caractères de la critique philosophique*, he attempted to go beyond Comte and Ampère’s horizon, and undertook a new and sophisticated analysis of scientific practice. Statistics and probability appeared as the most meaningful link between the formal structures of mathematics and the complexity of phenomena: they could encompass both natural and human sciences (Cournot 1851, tome 1, 18-19, 48, 62-65, 82, and 418-419; Faure 1905, 409-410).¹⁰

Cournot also paid attention to tacit meta-theoretical issues that guided scientific research: the confidence in the permanence of scientific laws over time, the confidence in analogy and induction, and the confidence in the simplicity, unity, symmetry, and beauty of scientific laws. Probability was also at stake in this context, because these issues could not share the certainty of logical deductions and empirical experiences, but could only rely on a certain degree of probability. It was “a *philosophical probability*” that allowed scientists to synthetically grasp “the order and reason of things” (Cournot 1851, tome 1, 92-100, 294 and 308; tome 2, 247-248).

He frequently stressed the difference between science and philosophy, and, at the same time the necessity of a fruitful alliance. He found that science was a progressive practice whereas philosophy inquired into a set of problems that persisted over time.¹¹ According to Cournot, a specific philosophical practice stood beside logical, computational and empirical practices in science. The philosophical component spanned both the interpretation of experiments and debates on scientific methods: in some way, it involved both theoretical and meta-theoretical issues. A purely *positive* science, in the sense of a merely empirical practice, could hardly exist (Cournot 1851, tome 2, 216-217, 228, 234-237, 244, 252-253, 255).

The following year, Comte published what he considered the achievement of his human experience

⁸ On Poisson’s recommendation, Cournot was appointed to a chair of mathematical analysis in Lyon in 1834, and in Grenoble the following year. After becoming Dean in Grenoble, in 1838 he replaced Ampère as General Inspector of Public Education (Moore 1905, 528-535; Martin 2017, 3).

⁹ He had also published *Traité élémentaire de la théorie des fonctions et du calcul infinitésimal* in 1841, and *De l’origine et des limites de la correspondance entre l’algèbre et la géométrie* in 1847. For Cournot’s biography, see Moore 1905, 521-543.

¹⁰ In 1905, Fernand Faure, politician, professor of law, and then professor of statistics in Paris, remarked that Cournot’s researches on statistics passed almost unnoticed because they were “too philosophical for statisticians and too statistical for philosophers” (Faure 1905, 396).

¹¹ Cournot warned against “the overconfidence in the possibility of solving typical philosophical problems inside the scientific context” (Cournot 1851, tome 2, 404).

and intellectual enterprise, the *Catéchisme positiviste*; the same pathway that had led him from a sound scientific practice to “a sane philosophy” was leading him from the latter to “the universal religion”. Both philosophical and political issues were at stake: he opposed his “proven religion” to “an anarchic democracy and a retrograde aristocracy”. Political commitment merged with religious inspiration: the new mankind would have marched towards “the conciliation of order and progress” (Comte 1891 [1852], 1, 4-6, 11, 15-17, 21, 26 and 29).¹²

In the same year, Émile Littré, the most sophisticated philosopher of Comte’s entourage, pointed out the necessity of a social order that could assure wealth, justice, order, and stability. The new positivist *dogma* or “spiritual order” required a new regime or a new social order.¹³ Only “the positive philosophy” could help discover the scientific deterministic laws of human societies in order to inspire, encourage, and accompany social progress (Littré 1852, VI-IX, XXX-XXXII, 15-6, 35-36, 48, 311-312, and 327).

Nevertheless, after Comte’s death (1857), Littré progressively distanced himself from Comte’s *religion of Humanity*. In a long book he published in 1863, Littré undertook the extremely demanding task of making use of “Comte’s method to judge Comte himself”, and rejected Comte’s religious commitment (Littré 1863, III-IV, VI, IX, 667-8, 674, and 677-8). Mill put forward the same criticism in 1865, and regretted that Comte had transformed into “the High Priest of the Religion of Humanity”. He agreed with Littré on the necessity of separating the sound foundations of Positivism from the subsequent mystic drift (Mill 1865, 5, 9, and 125-128).¹⁴

Another English philosopher, Herbert Spencer, opposed Comte’s dogmatism, and focused on scientific knowledge in a more pragmatic way than Mill and Whewell: he was interested in human beliefs from the sociological rather than the logical point of view. A sophisticated criticism and relativism led him to inquire into “tacit assumptions” that many different beliefs had in common. Unlike Comte, Spencer saw “a fundamental harmony” between science and religion: both of them were “constituents of the same mind” that corresponded to “different aspects of the same Universe”. Both science and religion tacitly acknowledged that the comprehension of the world required the continuous effort of going beyond common experiences and appearances (Spencer 1862, 10, 17, 21, and 24).

In Spencer’s text, the key words and the key concepts were *change* and “the relativity of all knowledge”: the mind was moulded by the world, and the comprehension of the world was continuously transformed by the mind. Cycles of evolutions and dissolutions led to a “universal” and “omnipresent” metamorphosis (Spencer 1862, 66-8, 96-97, 122-123, 440, and 489-491).

Naïve versus Sophisticated Philosophies of Science

In the meantime, Cournot had published a book on methods and practices in mathematics, physical sciences, natural sciences, and human sciences, *Traité de l’enchaînement des idées fondamentales dans les sciences et dans l’histoire*. He put forward a detailed analysis of the conceptual structure of *positive* science, in order to make its hidden philosophical foundations emerge. He was also interested in understanding how the emergence of modern science had changed our patterns of explanation. Modern science had emerged when time and history had come into play, when Copernicus and Tycho’s purely geometrical models were transformed into physical models. Recent developments had highlighted the differences among “contents, principles, and methods” of the various sciences, and the pivotal role of life sciences (Cournot 1861, II-VII and 118-122).

General principles were interpretations of present and future experiences rather than necessary consequences of experiences. Scientific concepts involved both science and metaphysics, or rather, “a

¹² Comte frequently insisted on the essential contribution of women and proletarians to social progress, and on them as privileged recipients of his catechism. In the conclusion of the *Foreword*, the references to the most important women of his life are intertwined with the evocation of the Supreme Being (Comte 1891 [1852], 11, 16, 21, 24, 26, 28, and 30).

¹³ See Littré 1852, XXXII: “Voilà un dogme, voilà un régime, voilà un culte qu’il s’agit de développer, de propager, de prouver, d’éclaircir !”

¹⁴ Mill qualified Littré as “the only thinker of established reputation” in French positivist environment (Mill 1865, 126)

shared land” where scientific principles had their natural seat.¹⁵ *Philosophy of science* was the name of the borderland between philosophy and science (Cournot 1861, 179 and 181-183). This third component of scientific practice stood besides mathematics and experiments, and was not submitted to “an experimental proof or mathematical demonstration” (Cournot 1861, 189-190).

Chemistry could not be reduced to physics, and life sciences could not be reduced to physics and chemistry. Chemistry was a science of transformations, and chemical transformations could be violent or marked by a sharp discontinuity (Cournot 1861, 191-2, 208, 210-212, and 214).¹⁶ In a living structure, the action of one part on another was affected by the systematic link with “the structure and the functions of the system” as a whole. Moreover, living species had appeared and then disappeared over time: *Nature* was not compelled “to act always in the same way in the same situations”, and “time was involved in an intrinsic way in the laws ruling Nature” (Cournot 1861, 223, 272-273, 277, and 284).

In 1867, Littré and the mineralogist Gregoire Wyruboff¹⁷ published a booklet on the philosophical relationship between Comte and Mill. Littré insisted on the validity of Comte’s intellectual enterprise, and separated Comte’s philosophical core from his later intellectual decay, which was inconsistent with “his principles and his methods”. Definitely less refined than Littré from the philosophical point of view, Wyruboff defended “the new dogma” of Positivism, and defended Comte against Mill’s criticism: he found that Mill was too pragmatic and not prone to ideological scientism and sharp reductionism (Littré 1867, 5; Wyruboff 1867, 59-62, 68, and 84).¹⁸

The following year, Littré insisted on the concept of “positive science”, wherein every *a priori* was excluded. This naïve epistemology led to a likewise naïve historiography and sociology: the disappearance of metaphysics was looked upon as tightly linked to the disappearance of war and the emergence of industry in the history of mankind (Littré 1868, 11, 31, 39-40, 49-50, and 74).

Nevertheless, it seems that Littré was not enough radical as a positivist. The most radical among them, who had followed Comte even in the late mystic drift, sharply criticised Littré. Jean François Robinet, Comte’s former physician and one of his literary executors, charged Littré with having belittled and betrayed Comte. A political controversy was also involved: Littré was blamed for having endorsed “parliamentarism and plutocracy” (Robinet 1871, 3-4 and 10-14).

Cournot’s anti-reductionist attitude was restated in 1872, in the book *Considérations sur la marche des idées et des événements dans les temps modernes*. It is worth remarking that, in the same year, the renowned German physiologist Emile Du Bois-Reymond claimed that scientific knowledge consisted in “reducing all transformations taking place in the material world to atomic motions”. A strict reductionism led him to a strict determinism: the universe was ruled by mechanical necessity (Du Bois-Reymond 1872, 441-444 and 446).

Cournot’s concept of *chance*, and adjectives such as *accidental* and *contingent* were at the core of his philosophy of history. He stressed that chance did not mean ignorance or unreliability: chance had its laws, and those laws were no less reliable than the laws of physics and astronomy.¹⁹ His probabilistic turn

¹⁵ In 1858, in the book *La métaphysique et la science ou principes de métaphysique positive*, the French philosopher Étienne Vacherot had attempted to revive metaphysics as a free practice of “analysis and critics”, which allowed philosophers to protect themselves against “unreasonable dogmatism and regrettable scepticism”. Metaphysics needed to be updated, and he ventured to “reconcile metaphysics with science” (Vacherot 1858, V-VI, XV, XXXV, 52, and 94).

¹⁶ Different kinds of discontinuity emerged from chemistry: discontinuity in the sense of abrupt and energetic transformations, discontinuity in the sense of qualitative transformations, and discontinuity in the sense of rearrangements of chemical substances in accordance with integer ratios between their weights (Cournot 1861, 215).

¹⁷ Wyruboff, a scholar of Russian origin, got in touch with Littré when attending Comte’s lectures, and in 1867 they founded the journal *La Philosophie Positive*, which was published until 1883.

¹⁸ Wyruboff’s philosophical naivety was also displayed in a booklet he published in 1865. He stated that “every philosophical debate can be reduced to a matter of fact”, and scientific laws were nothing else but a collection of facts that continuously occurred “in the same way under the same conditions” (Wyruboff 1865, 1).

¹⁹ In 1812 the mathematician Pierre Simon de Laplace had published *Théorie analytique des probabilités*, and two years later a less demanding *Essai philosophique sur les probabilités*. In the latter he had claimed that “the most important problems of life” dealt with probability: in his words, “l’induction et l’analogie se fondent sur les probabilités” (Laplace 1825, 1-2). Comte considered the theory of probability as “false sciences”: in general, he did not rely on the application of mathematics to social sciences.

encompassed both necessity and contingency. History could be overturned by sudden revolutions: his historiographical framework rested upon the continuity of ordinary processes and the discontinuity of extraordinary processes or revolutions. During revolutions, apparently meaningless contingencies could lead to long-term effects (Cournot 1872, 1-6).

In reality, Cournot’s historiographical framework involved two different traditions. On the one hand he saw systematic sciences, which had been systematic since the age of Greek civilisation, and had experienced “a revolutionary crisis” in the sixteenth and seventeenth centuries. On the other hand, he saw scattered bodies of knowledge, such as “theories of heat, magnetism, and electricity”, that had preserved their “childish condition” of semi-empirical sciences throughout the seventeenth century and even afterwards (Cournot 1872, 292-294). This historiographical and epistemological perspective was not in tune with positivist historiography and epistemology.²⁰

On the positivist shore, Littré criticised Spencer for having dared to replace the Comtian hierarchical order of sciences with a more pragmatic interconnection, and remarked that ancient civilisations were aware and proud of their past but less interested in their future, whereas “modern civilisations” focused mainly on their future. Actually, it was just Littré and other positivists that celebrated the cult of progress, and underestimated the complex dynamics of historical transformations (Littré 1873a, 13; Littré 1873b, IV-VII).

Although the adjectives *naïve* and *sophisticated* cannot be formally defined, it seems to me that they can help us understand what really happened at the dawn of modern philosophy of science in the second half of the nineteenth century. Naïve approaches can be associated with a simplified account of scientific practice, the celebration of a simplified scientific method, and the uncritical mythology of scientific progress. Sophisticated approaches managed to grasp the complexity of scientific practice, the complex interaction among rational, empirical, and intuitive components in scientific research, the plurality and pliability of scientific methods, and the existence of different scientific traditions which had developed throughout history. However, I must warn against any dogmatic application of my tentative, dichotomic classification: *naïve* and *sophisticated* are only provisional labels that help us interpret the emergence, the history, and further developments of modern history and philosophy of science (Bordoni 2017, 189).

Further Debates

In 1874, the young philosopher Émile Boutroux published his doctoral dissertation, *De la contingence des lois de la nature*, wherein he focused on the relationship between scientific theories and experiences. The emergence and development of natural philosophy had involved the transition from “a purely descriptive science” to “an explicative knowledge”. According to Boutroux, this development had widened the gap between the disorder of experiences and the order of laws that should explain those experiences (Boutroux 1874, 2-4). In Boutroux’ radical anti-reductionism, variability and instability replaced invariance and stability of mathematical laws. Boutroux’ philosophy was not antiscientific but was based on the assumption that sharp reductionism and determinism were not necessary foundations for a natural science. He did not despise science, and kept abreast of recent scientific achievements but he firmly opposed the positivistic trend.²¹

Boutroux’ theses were put forward in a context where positivism was hegemonic; nevertheless, the following year, Cournot published another book where his anti-reductionist attitude was further developed. Nevertheless, his philosophy of science was more cautious and pliable than Boutroux’s. The development of life sciences required a new epistemology: statistics and probability allowed scientists to replace certainty with probability. Cournot pursued a new alliance between determinism and contingency, between the stability of laws and the contingency of facts. In the late 1870s we find that some physiologists and physicians rejected reductionism (Egger 1877, 193-196, 197-198, 200-201, 209-211) but the majority was less cautious than Cournot, and put forward a radical reduction of psychology, anthropology, and sociology

²⁰ It is worth remarking that, in the twentieth century, Thomas Kuhn inquired extensively into the two traditions (Kuhn 1976, 4-22).

²¹ Benrubi stressed that Boutroux’ contingency should not be confused with chance: it was close to the idea of natural freedom, in the sense of free and unpredictable unfolding of natural laws. In other words, contingency occupied the intermediate place between chance and necessity (Benrubi 1926, 154-157).

to brain physiology (Luys 1876, VIII-XI; Boëns 1878, 345-347, 349-354, and 359-360; Boëns 1879, 5-9, and 14-15).

Cournot pointed out the impossibility of getting rid of meta-theoretical practices that “*positivist* philosophers” had discarded together with metaphysics (Cournot 1875, 371 and 373-5). His words echoed what Whewell had written some decades before: he stressed that “a body of purely empirical knowledge is not a real science” (Cournot 1875, 371-376). On the other hand, Pierre Laffitte, one of Comte’s most passionate followers, and the head of the community that accepted the whole of Comte’s doctrine, insisted on empiricism in scientific practice, determinism in history, and the “universal religion” as the necessary achievement of “*western evolution*” (Laffitte 1876, 1, 13-14, 18, and 30)

In the late 1870s, the debates on reductionism were accompanied by debates on determinism, and in both cases the problematic link among mechanics, life sciences, and philosophy was at stake. The mathematician Cournot put forward a sophisticated approach to reductionism, and another mathematician put forward a sophisticated approach to determinism. In 1878, Joseph Boussinesq published the booklet *Conciliation du véritable déterminisme mécanique avec l’existence de la vie et de la liberté morale*. His commitment was mathematical, physical, and philosophical: some differential equations led to “*branch points (points de bifurcation)*”, and a material system could evolve towards unpredictable directions (Janet 1878, 3 and 12-13).

Boussinesq reminded readers that mathematicians and engineers had inquired into concepts such as guiding principle in life sciences. In 1861, the mathematician Cournot had spoken of “a principle of harmonic unity, global direction, and homogeneity”, whereas in 1877 the mathematician and engineer Adhémar Barré de Saint-Venant had introduced a vanishing “trigger action (*travail décrochant*)”, which was not so different from the small amount of force required to pull a gun trigger. Boussinesq specified that a guiding principle was not in need of a corresponding mechanical force, however negligible it might be. Those “bifurcations in the integrals of the equations of motion” offered a structural analogy and a mathematical model for physical instabilities and complex systems such as living structures (Cournot 1861, 364, 370, and 374; Saint-Venant 1877, 421-422; Boussinesq 1878, 31-33; Bordoni 2015, 28-29).²²

It seems that the first mathematician who raised the question of *determinism* in connection with singular solutions of differential equations was really Boussinesq himself, in a brief *Note* he published in the *Comptes Rendus* of the *Académie des sciences* in 1877. In reality, in 1872 Cournot had briefly discussed the instability stemming from a cone in equilibrium upon its top (Cournot 1872, 276). In 1875 Cournot envisaged a more general kind of determinism where both deterministic and non-deterministic processes were submitted to the normative role of mathematics (Cournot 1875, 113-120 and 128). This is exactly the pathway that Boussinesq followed two years later.

Cournot first reflections on foundations and methods of scientific practice were put forward in the 1860s, in an adverse intellectual environment.²³ Still in 1881, after Cournot’s death, in a summary of Comte and Laffitte’s doctrines, Robinet insisted on a naïve philosophy of science which was based on a strict empiricism, and on a naïve historiographical framework wherein Positivism was looked upon as the crowning achievement of “a mental revolution triggered off by Thales and Pythagoras” (Robinet 1881, 6-7 and 10). However, in the same decade, Cournot’s theses reappeared and found new implementations. In 1883, the philosopher and theologian Ernest Naville put forward a sophisticated conception of science as a dynamic body of knowledge rather than a naïve accumulation of empirical procedures and rational truths. On the track of Cournot, whom Naville mentioned only occasionally, we find an epistemology marked by fallibility

²² During the nineteenth century, singular integrals had sometimes attracted the attention of mathematicians. Boussinesq mentioned Siméon Denis Poisson, Jean-Marie Constant Duhamel and Cournot’s, and briefly commented on their texts (Boussinesq 1878, 121-130). In 1841, in the second volume of his *Traité élémentaire de la théorie des fonctions et du calcul infinitésimal*, Cournot had devoted a whole chapter to the subject (Cournot 1841, II vol., 271-292).

²³ According to the historian of philosophy Isaac Benrubi, Cournot’s philosophy eluded any attempt to describe it by means of “a definite summary and a sharp classification” (Cournot 1851, 1 vol., 171-172; Benrubi 1926, 89-90). According to the philosopher François Mentré, Cournot’s *discreetness* was both a personal leaning and an epistemological commitment (Mentré 1905, 483; Mentré 1908, 644 and 646). See Martin 2017, 18: “le style de sa pensée, la prudence et la rigueur avec lesquelles il construit ses analyses n’ont pas la puissance séductrice des grands systèmes de pensée de la tradition occidentale”.

and probability, and intertwined with a dynamic historiographical framework (Navelle 1883, 28, 32-35, 41-47, and 50-55).

According to Navelle, theories represented the pivotal stage in scientific practice: they occupied the “intermediate region” between experimental laws and general principles: that intermediate position was consistent with their nature of “changeable and provisional” entities. Physical laws corresponded to an empirical necessity, and guiding principles corresponded to a rational necessity, whereas theories could only rely on a problematic correspondence between the empirical and rational domains. The dynamic process of emergence, development, and replacement of physical theories was the essential feature of scientific progress: it was just the caducity of theories that protected science from involution and decadence (Navelle 1883, 54-55).

A completely different meta-theoretical attitude can be found in the book the authoritative chemist and influential politician Marcelin Berthelot published in 1886. He focused on “positive science”, which started from facts, and connected them by means of “immediate relations”. Science had led mankind to “the explanation of a huge number of phenomena” merely on the basis of “the coarsest facts”. We find here both confidence in a continuous scientific progress and in a simplified empiricism. Not only could “physics and chemistry be reduced to mechanics”, but also the process of reduction was an empirical necessity rather than a rational option. (Berthelot 1886, V, VII, 4-5, and 9-11).²⁴ The previous year he had published a history of the ancient alchemy. He saw a continuous line of descent that led from antiquity to the late Renaissance. Broad and simplified analogies also emerged: Heraclitus’ world-view was associated with modern “transformation of forces and the mechanical theory of heat” (Berthelot 1885, 78-9, 248, 250, 252, 262-265, 267, 271, and 275).

After two years the engineer and mathematician Paul Tannery published a very different history of ancient science, *Pour l’histoire de la science Hellène*: he could rely on mathematical competence, philosophical sensitivity, and the study of ancient languages (Duhem 1905, 216). The accuracy of his historical reconstructions, the careful and detached analysis of original texts, and the presence of a cautious but definite historiographical perspective, makes his history of science a milestone in the intellectual landscape of the late nineteenth century. He did not manage to gain an academic position in France even though he was acknowledged as one of the most competent European scholars in the history of ancient science.²⁵ He contributed to the establishment of a modern history of science, where the adjective *modern* means a history of science that is not pursued from the point of view of present-day scientific theories and beliefs, and does not confine itself to a list of cumulative successes (Brenner 2003, 184-185).

He stressed the necessity of separating “philosophical history” from “scientific history”. Historians of philosophy had naturally leant towards an abstract classification of theories in accordance with modern linguistic and conceptual standards: they had tacitly assumed a sort of ideal continuity between different contexts in order to safeguard the unitary structure of philosophy. On the contrary, a history of science required the analysis of both historical filiations and discontinuities. Another question involved the sources: many original texts had been lost, and the history of subsequent loans and influences had to be analysed. In reality Tannery’s history was both a history of original ideas and a history of historical reconstructions and misunderstandings (Tannery 1887a, 10-11, 14, and 18-19).²⁶

²⁴ I find that a short passage deserves to be quoted: “Une généralisation progressive, déduite des faits antérieurs et vérifiée sans cesse par de nouvelles observations, conduit ainsi notre connaissance depuis les phénomènes vulgaires et particuliers jusqu’aux lois naturelles les plus abstraites et les plus étendues.” (Berthelot 1886, 10).

²⁵ For a reconstruction of the events that prevented him from being appointed to a Chair of “Histoire Générale des Sciences” at the *Collège de France*, see Milhaud 1906, 14, Sarton 1948, 30, GUSDORF 1966, 43-4, 62, 98-101, and 104-6, Canguilhem 1979, 63, Stoffel 1996, 416, Brenner 2003, 5 and 101, and Chimisso 2008, 85, fn 1. That chair was looked upon as “a fiefdom of the positivist school (or church)” (Sarton 1938, 690). It is worth remarking that Berthelot had supported Tannery’s nomination.

²⁶ On the influence of German history of philosophy on Tannery, and more specifically on the philosophical background of the conception “of history of science as *complementary* to history of philosophy”, see Catana 2011, 517-523. In the same year Tannery also published a book on Greek geometry wherein he stressed that a reliable history of mathematics had to account for “*the events and the causes*” that had led to stages of “*past decadence*” (Tannery 1887b, V-VI, 4, and 8-9).

Mature Historiographies and Epistemologies

The cult of progress, historiographical and epistemological simplifications, and even positivist religion did not fade away. As late as 1891, the editor of a new reprint of Comte’s *positive* catechism paid tribute to “the saint Father” of the new cult, and insisted on the necessity of putting “the cult before the dogma”, namely the religious commitment before the philosophical one. He regretted that some former followers disdained the cult, and confined themselves to a purely philosophical commitment. Not only did he address his criticism to Littré but also to Laffitte, who was charged with having relinquished Comte’s religious and political preaching (Lagarigue 1891, V and VII-XI)

However, Tannery’s style of research left a meaningful heritage, and inspired the mathematician Gaston Milhaud.²⁷ The book he published in 1893, *Leçons sur les origines de la science grecque*, consciously pursued the setting up of a tradition of research that could rely on Tannery’s innovative and authoritative researches (Milhaud 1893, 3-5 and 8-9).²⁸ He stressed the creative power of the human mind: scientific progress consisted of “a linguistic evolution”, or in other terms, “a new explanation of the same phenomena”. This epistemological and historiographical perspective was in conflict with Comte’s positivistic faith in the mighty pressure of *facts* (Milhaud 1893, 11-3, 16-8, and 21-28).

In the late 1880s and early 1890s, we find three different histories of science. At first we find Berthelot’s positivistic history wherein both the march of scientific progress and the naivety of ancient science were emphasised. Tannery’s histories offered a different intellectual landscape: the historiographical reference frame included regressive stages and centuries of stagnation besides progressive trends. Milhaud’s histories were embedded in more explicit historiographical and epistemological frameworks. In contrast with the empiricism of the Comtian tradition, he insisted on scientific practice as an act of mathematisation and linguistic reinterpretation. He attempted to merge history of science and philosophy of science into each other in order to establish a new sophisticated discipline.

Berthelot, Tannery, and Milhaud had been trained in science, engineering or mathematics: their historical and philosophical interests stemmed from a scientific background. This is also true for the younger physicist Pierre Duhem. Since the late 1880s he envisaged a unified mathematical framework for mechanics, thermodynamics, and chemistry, which was based on analytical mechanics, and founded this unified theory on the two principles of thermodynamics. His *Energetics* was different from Georg Helm and Wilhelm Ostwald’s *Energetics*: the latter mainly focused on the universality of the principle of the conservation of energy, whereas Duhem also developed a sophisticated mathematical theory.²⁹

Struggling against the old physics of qualities, modern science had set aside the complexity of the physical world, and put forward a simplified geometrical world. Duhem found that, at the end of the nineteenth century, that complexity, and more specifically dissipative effects, could scientifically be addressed. In terms of the ancient Aristotelian natural philosophy, Duhem’s unified theory could go beyond *local motion* in order to describe all kinds of transformations.³⁰

In the meantime, in the early 1890s, he published the first paper explicitly devoted to meta-theoretical issues.³¹ On the track of Cournot and Naville, he stressed that theoretical physics was something more than the mere alliance between “experience and mathematical analysis”, and allowed scientists to go beyond “the confused and inextricable accumulation” of laws derived by experience (Duhem 1892 (1987), 175). He

²⁷ Milhaud set up “a programme of study in philosophy of science” at Montpellier University in the 1890s. A Chair of *History of Philosophy in its Relation to Science* was then created for him at the Sorbonne in 1909. He was one of the first scholars of Jewish origin to be appointed to a Chair in Paris (Chimisso 2008, 25-26; Brenner and Gayon 2009b, 5).

²⁸ In 1906, after Tannery’s death, Milhaud underscored the deep influence exerted by Tannery (Milhaud 1906, 4).

²⁹ On Duhem’s design of a rational thermodynamics, see Bordoni 2012a, 2012b, 2012c, and 2013. The warm relationship between Duhem and Ostwald cannot be interpreted as an agreement on the meaning of *Energetics*. On their friendship, see Brouzeng 1981, vol. 2, 226-228.

³⁰ Duhem’s theoretical and meta-theoretical design was explicitly unfolded in a book he published in 1903, *L’évolution de la mécanique* (Duhem 1992 [1903]), 199 and 218-219).

³¹ At that time, Duhem was “maitre de conférences” at Lille University: for further biographical details, see Brouzeng 1987, 54.

pursued a critical overview of scientific practice that was not so different from Poincaré’s: I find that the “important differences” between them that some historians and philosophers of science have pointed out should not be overestimated (Brenner 1996, 389-390).³² They criticised the mechanistic view, and were aware of the intrinsic historicity of scientific achievements.

In 1893, Duhem devised a four-level scientific practice that, starting from phenomena, led to mathematical laws, theories, and then a plurality of metaphysical foundations. The four levels were mutually independent: a plurality of theories could stem from a definite set of laws, and a plurality of metaphysical assumptions could stem from a theory or set of theories (Duhem 1893a, 65-66 and 68-71). Once more, we find here a meta-theoretical analysis of scientific theories that had much in common with what Cournot and Naville had previously put forward.

In the same year, he published another paper wherein he introduced the concept of “*natural classification of laws*” that had already appeared in the tradition of French philosophy of science. He qualified the concept as a “perfect and ideal theory” or a “complete and appropriate metaphysical explanation of the nature of material things”. Actual physical theories had to “strive for perfection”, even though perfection could not be attained. This commitment dealt with practices that eluded any definite definition, but Duhem insisted on further, fruitless philosophical specifications (Duhem 1893b (1987), 136-137).³³

Unlike Comte, Duhem’s concept of natural classification made reference to the essential features of a scientific theory. Comte’s natural classification was a relationship among different bodies of knowledge that was in tune with logical and historical genealogies (Comte 1830, 60-61, 76-8, and 86). Cournot’s concept was not so different from Comte’s: a natural classification could grasp some essential features of reality even though he specified that every classification was provisional and incomplete (Cournot 1861, 423 and 425-426). Boutroux rejected the concept itself: every classification was intrinsically artificial (Boutroux 1874, 46).

The following year Duhem focused on experimental practice. Every experiment involved a wider body of knowledge that dealt with general assumptions and concepts, and specific laws: when a science progressed, the role played by theory increased progressively. He put forward the fundamental thesis that “*a physical experiment can never condemn an isolated hypothesis, but only a theoretical system*”. When an expected prediction did not take place, a definite source of the mismatch could not be found. The complex relationship between theory and experiment required a specific sensitivity or some kind of flair that involved “*the esprit de finesse rather than the esprit de géométrie*”. (Duhem 1894a, 153-155, 157, 179 and 188).³⁴

In a following paper, Duhem stressed the extra-logical concept of the *fruitfulness* of a physical theory. Philosophers and scientists have traditionally focused on the concepts of truth or falsity, but truth was the outcome of a historical process, and therefore it was a provisional value. On the contrary, the fruitfulness of a theory was a permanent value (Duhem 1894b, 124-5). On the track of Cournot and Naville, Duhem outlined a complex historiography where both linear progress and cyclical processes were at stake.³⁵ Theories emerged, were successfully upheld, suffered a dogmatic drift, then they were overwhelmed by their flaws, and were eventually replaced by new theories. This process left behind a permanent and valuable heritage: the long-term progress of key concepts, mathematical structures, and empirical laws (Duhem 1894b, 122 and 125).³⁶ A striking metaphor followed:

³² According to Milena Ivanova, both Duhem and Poincaré “expressed a form of structuralism”, namely structural realism, but they adopted different epistemological views with regard to “how knowledge of the structure of the world is reached” (Ivanova 2015, 88). I stress the sterility of philosophical labels when complex research programmes are involved, and when they are applied to historical contexts quite different from the context that has generated those labels. Can we find essential differences between what might be labelled as structural realism or pragmatism in the late nineteenth century?

³³ Going beyond the debates on Duhem as an instrumentalist or a realist, I agree with Sindhuja Bhakthavatsalam that Duhem’s concept of natural classification deals with “the pragmatic rationality of a physicist” (Bhakthavatsalam 2015, 11 and 21).

³⁴ This fundamental thesis is known as Duhem’s holistic thesis: it has been widely discussed and criticised under the misleading label “Duhem-Quine thesis”. I have discussed it in Bordoni 2017, 292-300.

³⁵ Obviously, this is only a brief outline of the emergence of Duhem’s historiographical and epistemological frameworks. A more detailed analysis can be found in my recent book (Bordoni 2017, chapters 8 and 9).

³⁶ The original passage deserves to be quoted: “Ainsi, sous les théories qui ne s’élèvent que pour être abattues; sous

When waves go towards the beach, a water layer ripples and swarms into the dry sand before retreating from the beach giving up its conquest. Waves fade away and let the sand dry up before new waves come one after the other. This superposition of waves that rise and then collapse seems a shallow effort of the sea, an idle combination of foam and noise. Nevertheless, two hours later, the beach that had been trodden by our footsteps is now sleeping under deep water: during the relentless oscillations of water back and forth, the Ocean tide has really gone up (Duhem 1894b, 125).³⁷

Concluding Remarks

Duhem has been looked upon as one of the founding fathers or ancestors of twentieth-century history and philosophy of science in France. My thesis is that Duhem accomplished an intellectual stream that had emerged considerably earlier. The awareness of the complexity of scientific practice and scientific tradition can be found in some historical and philosophical studies from Cournot to Duhem through Naville and Tannery. They had analysed the superposition of cyclic and linear processes, and the persistence of structural continuities throughout scientific transformations.

Cournot, Boussinesq, and Duhem put forward bold mathematisations of new research fields such as economic processes, physical and chemical instabilities, and thermodynamics of irreversible processes. Cournot, Tannery, and Duhem attempted to cast light on the plurality of scientific methods and their histories. Suspicious of every rigid philosophical system, they were in search of a third way between scepticism and dogmatism. The dissemination of Cournot, Naville, Tannery, and then Duhem and Milhaud’s researches in the history and philosophy of science contributed to the professionalisation of the field in the French context, and the establishment of a research tradition that is known as *historical epistemology*.³⁸

Duhem always mentioned the scientists who had contributed to the development of his research field, and put forward a historical reconstruction of those researches. On the contrary, in his historical and philosophical papers we do not find explicit references to previous researches. The fact is that his interests in history and philosophy of science stemmed from his practice in theoretical physics rather than from the study of previous literature. Duhem was objectively in debt to Cournot and other scholars, but a direct influence is questionable. His scientific researches led him to the rediscovery and reinterpretation of Aristotle, and another influence was exerted by Pascal’s epistemology.³⁹ Duhem found in Aristotle the awareness of the complexity of natural processes, and in Pascal the awareness of the complexity of scientific practice.

les hypothèses qu’un siècle contemple comme le mécanisme secret et le ressort caché de l’Univers, et que le siècle suivant brise comme des jouets d’enfant, se poursuit le progrès lent, mais incessant, de la physique mathématique” (Duhem 1894b, 125).

³⁷ Stoffel pointed out the striking analogy between Duhem’s passage and one of Pascal’s *Pensées* on cyclic, historical processes (Pascal 1951, 417; Stoffel 2007, 292-293). I point out the analogy with Naville’s passage on the slow, scientific progress underlying the appearance and disappearance of theories: “Les théories passent, la science demeure: ... à un système détruit en succède un autre dont les conceptions sont plus solides et plus vastes” (Naville 1883, 55).

³⁸ The emergence of this tradition has frequently been associated with Gaston Bachelard and Georges Canguilhem. I find in them some scientist nuances that Cournot, Tannery, Milhaud, and Duhem would not have endorsed. Canguilhem agreed with Bachelard on the existence of “two kinds of history of science”, namely the history of the out-of-date scientific knowledge, and the history of *legitimate* science that satisfied contemporary standards (Canguilhem 1979, 13 and 20-21). At the same time, I must acknowledge that Canguilhem suitably criticised Cournot and Duhem’s unhistorical concept of *precursor*, which had already been criticized by Hélène Metzger in 1939 (Metzger 1987 [1939], 79 and 83).

³⁹ As already pointed out in fn. 2, van Fraassen stressed the role played by Pascal’s epistemology (van Fraassen 1989, 151). Other scholars have stressed the deep influence of Pascal on Duhem [Picard 1922, CXXX and CXXXV-CXXXVII; Paul 1979, 3 and 159; Maiocchi 1985, 13; Martin 1991, 68, 90 and 115; Stoffel 2002, 196 and 345; Stoffel 2007, 299 and 301).

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Transversal: International Journal for the Historiography of Science, 2 (2017) 36-53

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Duhem's Critical Analysis of Mechanicism and his Defense of a Formal Conception of Theoretical Physics

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Abstract:

The aim of this paper is to present Duhem's critical view of the dynamical development of mechanics according to two principles of his theory of the development of physics: the continuous and the rational development of physics. These two principles impose a formal conception of physics that aims at demarcating physics from the metaphysical view on the one hand and the pragmatist/conventionalist view on the other hand. Duhem pursues an intermediary conception of physics, a representational system of empirical laws based upon formal principles. This formal conception of physics will adjust to his idea of scientific progress in the form of a sequence of representational systems as structures of increasing comprehensiveness of empirical laws, which leads him to defend a convergent structural realism pointing to an ideal physical theory.

36

Keywords:

Pierre Duhem; mechanicism; theoretical physics; synthetical / analytical method; structural realism

Received: 07 April. Reviewed: 31 May 2017. Accepted: 12 June 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.05>

Introduction

The aim of this paper is to present Duhem's critical view of the development of mechanics according to two principles of his dynamical theory of the development of physics: the continuous and the rational development of physics (Duhem 1980, 188). These two principles impose a formal conception of physics that aims at demarcating physics from the metaphysical view which searches for causal explanation of physical phenomena, on the one hand and the pragmatist/conventionalist view, with its defense of the principle of undertermination of theories by data on the other hand (Chiappin 1989, 131, 93; Duhem 91, 330; Poincaré 1901, vi). Duhem pursues a formal conception of physics that he defines as a representational system of empirical laws based upon formal principles (Duhem 1974, 19; 1902b, 5), a middle way between

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these two conceptions (Chiappin 1989, iv, 92-93, 121, 243). He constructs this formal conception in such a way that he ends up with an idea of scientific progress in the form of a sequence of representational systems as structures of increasing comprehensiveness of empirical laws, which leads him to defend a convergent structural realism toward an ideal physical theory (Chiappin 1989, 198, 110-116, 198-210).

Duhem rejects a conception of physics that searches for causal explanation, which he deems metaphysical and whose origins, as he points out, lie in the emergent mechanicism of Descartes's rationalistic program. For Duhem, mechanicism – understood as a kind of large-scale mechanism – was a strategy of scientists such as Descartes and Galileo to mathematize nature. Despite rejecting the mechanistic approach to physics, Duhem values the framework of the emergent rationalist program that contains it, together with its demand for the principle of unity and the formal or mathematical organization of science (Duhem 1892, 170).

However, his critical analysis of the mechanistic point of view of physics is more complex than it seems at first sight, since it envisages the dynamical development of mechanics according to two approaches, the synthetic and the analytical. Each one of them has its own vices and virtues, according to Duhem's principles of the continuous and rational development of physics (Duhem 1974, 270, 296; 1980, 188, Chiappin 1989, 77, 80, 91). Both principles are the coordinating principles of a proposal to construct a dynamical conception of physics made of a sequence of more encompassing theories that move systematically toward an ideal conception of physics which pretends to mirror the structural relations between the empirical laws (Duhem 1893, 298, 368-369; 1917, 157; Chiappin 1989, 198, 106-113, 243).

Duhem's Description of Mechanical Theory and the Mechanistic Program

Duhem's critical analysis of the evolution of mechanics and of the conception of physics associated with it is, obviously, made from a historical point of view. Besides allowing him to find a possible tendency in the development of physics, it also enables him to construct a tradition and a historical support for his own formal conception of physics.

In his historical-critical analysis of mechanicism (Chiappin 1989, 18-19), Duhem provides the following general description of the nature of mechanical theory:

Let us seek to account exactly for the nature of what one calls a mechanical theory. In a mechanical theory one imposes [besides symbolization] on all physical magnitudes on which rely the laws that one has to tie with one another the condition that they be composed by means of the geometrical and mechanical elements of a certain fictitious system; one imposes on all hypotheses that they be the propositions [*énoncés*] of the dynamical properties of this system (Duhem 1892, 154).

This means in mechanical theory that: *i*) the physical concepts used in the empirical laws must be defined in terms of geometrical and mechanical properties of a material system (for example, particles in motion in Descartes's view, matter and repulsive/attractive forces in the mechanical-molecular tradition of Laplace and Poisson, the continuous medium of Fresnel's and Maxwell's ether); *ii*) there is an additional supposition that these mechanical concepts are restricted to mass, size, motion and/or force. Duhem spells out this additional condition as follows: "When we propose to construct a mechanical theory, we impose upon ourselves another obligation which consists in putting, into these definitions and hypotheses, only a very restricted number of notions of a determined nature [mass, size, motion and/or force]" (Duhem 1892, 156).

These two conditions define the core of the physical content of mechanical theory. The basic mechanical concepts of mass, motion, size and/or force are mechanical properties of a mechanical system. The kinetic, or Cartesian view, excludes force from the definition of mechanical explanation. It views change of motion as the result of collisions. The dynamical or Newtonian view of mechanical theory includes force as a primitive concept. These constitute two general and competing programs to explain physical phenomena mechanically.

The mechanistic program is characterized by additional principles: *iii*) commitment to a set of propositions (in the form of equations) by means of which the general features of physical phenomena are described (these propositions are the fundamental laws of mechanics); *iv*) commitment to the application of

the principle of logical unity in physics (Duhem 1974, 91; Chiappin 1989, 178, 240-241), which requires a single theoretical system to account for physical phenomena; v) commitment to make mechanical theory the unifying framework of physics, which means that all physical concepts, entities, hypotheses and laws of physical theories must be composed of these restricted mechanical concepts and the fundamental theoretical principles (Duhem 1892, 155); vi) the assumption that physics is not mathematical unless it is first mechanical. Duhem clearly states this methodological principle: "A branch of physics cannot be transformed into a mathematical theory unless it becomes a mechanical theory. For a century, this principle has guided the efforts of the physicist geometricians" (Duhem 1901, 131). Duhem disputes this principle which, for him, conflicts with the true principle that guides the construction of mathematical physics. He replaces it with what he understands to be the correct principle to make theoretical physics mathematical, the measurement theory; vii) an implicit commitment to physics as a rational system and method of constructing physical theory. This method, as noted above, requires the use of well-established concepts and postulates from physical theory and of rational arguments (in the form of mathematical deductions) to obtain empirical laws from its theoretical basis.

The Mechanical Theory and its Two Versions: Synthetic and Analytic

Duhem's analysis of mechanicism is organized around his criticism of two methods for applying mechanical theory to explain physical phenomena: the "synthetic method" and the "analytic method". Duhem states such an idea clearly when he says:

The attempts made at explicating mechanically the physical phenomena that the universe presents fall into two categories. The attempts in the first category are carried out according to a method that can aptly be named the synthetic method [...]; in the eyes of the majority of physicists, the synthetic method no longer seems capable of giving a mechanical and complete explication of natural phenomena; it is, then, the analytic method that one requires today for such an explication (Duhem 1980, 95; 1905, 180-181).

These are two different views of the mechanical method of constructing physical theories. Duhem's criticism of mechanicism is the criticism of these two methods of constructing mechanical theories. Each of these mechanical methods of construction leads to different views of what mechanical explanation and theoretical physics mean and aim for. Duhem critically examines the problems and difficulties affecting both approaches, evaluating them critically regarding their capacities to provide a general and unifying mechanical explanation of physics and to make the development of physics continuous and rational. Further, they are evaluated on the basis of his own view of physical theory as a rational system, that is to say a system of physical laws represented by a few formal principles.

The Synthetic Method and its Two Versions: The English School and the Classical Rationalism / Continental School

Duhem argues, from the beginning, that the use of mechanical theories is not sufficient to distinguish between the English School and the Continental School or the metaphysical view of physics. He writes: "This predilection for explanatory and mechanical theories is, of course, not a sufficient basis to distinguish English doctrines from the scientific traditions thriving in other countries" (Duhem 1974, 72; Chiappin 1989, 38-58). Both schools follow the synthetic approach to the mechanistic program. However, they differ in their ways of interpreting what theoretical physics and mechanical explanation are, how to represent their conceptual bases, and how to connect the conceptual and the empirical bases. The sharp distinctions between these schools can also be traced to distinctions between the metaphysical/rationalist and the pragmatist/empiricist view of the mechanistic program. These two views of scientific knowledge strongly influenced the various ways of interpreting the synthetic approach to mechanical theory. Undoubtedly, both views seek to explain physical phenomena according to the synthetic method. Both seek to elaborate

definite, specific and detailed models of matter and motion to explain physical phenomena mechanically, the characteristic feature of the synthetic method.

However, one can reconstruct the common core of these two versions of the synthetic method to explicate physical phenomena mechanically as described by Duhem:

In this method one begins by constructing a mechanism from all pieces; one says what are the bodies that compose it, what are their shapes, sizes and masses, and what forces act upon it, and from these data one draws the laws according to which the mechanism moves; then, by comparing these laws with the experimental laws it seeks to explain, one judges whether there is sufficient agreement between them (Duhem 1980, 95).

At the root of the synthetic method of mechanical theory to explain physical phenomena mechanically is an attempt to define specific and detailed mechanisms, or mechanical models based on definite hypotheses about the shape of atoms and molecules, their size and their arrangement. For each category of physical phenomena the synthetic method defines a mechanical explanation based upon a specific number of bodies with a specific arrangement of shapes and definite motions and mass.

These arrangements are supposed to express the causal explanation of, or to imitate and simulate, physical phenomena, or, "in the words of English physicists, [to be] a model" (Duhem 1980, 102). These mechanical models or mechanisms may further be made of dimensional and real elements, such as fluids or corpuscles with definite sizes, shapes and masses. When these models are made of plastic or wood, or drawn, they are called scale models. The use of real and concrete models is a trademark of the English School. Although this synthetic approach is not the only means of applying mechanical theory, it prevailed over every other and is practiced by most of those working on mechanistic research programs. According to Duhem, Poincaré was one of its representatives and introduced the English School (pragmatism) in France (Duhem 1974, 319, 328; Chiappin 1989, 130-134; 136-150).

As a result, most mechanical explanations are based on this method of constructing physical theory. The examples abound. Duhem cites Descartes's theory of magnetic attractions and repulsions, Descartes's explanation of weight by vortex action, and Kelvin's gyrostatic ether (Duhem 1980, 96). Other instances are Maxwell's thermodynamical surfaces used to describe Gibbs's phase rules, Descartes's mechanical explanation of the properties of light, Laplace's physical theory, and, most outstanding, Maxwell's cellular constructions, with which he attempts to account for electromagnetic actions. Maxwell's description can be found in his memoir entitled "On Physical Lines of Force" (Maxwell 1952). This model simulates a mechanism put forward to explain (mechanically) electrostatic and electromagnetic effects.

An understanding of the differences between these two views is central to understanding Duhem's view of a conception of physical theory as an intermediary or a middle way (*tertium*) between them.

The Continental School

Besides the characteristic features of the synthetic method, i.e. the use of a restricted number of mechanical concepts applied through the definite properties of a specific material system, the Continental School (Duhem 1893, 352; Chiappin 1989, 40) requires that mechanical explanations "be subject to certain logical requirements" (Duhem 1974, 78; 1893, 358). For example, all mechanical concepts and empirical laws must be organized into a single, rigid axiomatic system, made of well-established concepts and principles, and all empirical laws must be mathematically deduced within this axiomatic system. The Continental School follows the rationalist tradition as to the nature of physical theory, meaning that physical theory is modeled on the ideal of the rational system of Euclidean geometry. Physical theory is a rigorous axiomatic system that logically unites its definitions and postulates with their testable consequences. Duhem says, "For a Frenchman or a German, a physical theory is essentially a logical system. Perfectly rigorous deductions unite the hypotheses at the base of a theory to the consequences which are derivable from it and are to be compared with experimental laws" (Duhem 1974, 78).

The Continental School requires the basic concepts of physical theory to be quantitative in order that algebraic language may be used. Basic concepts are mechanical concepts because they are quantitative. Mechanical theory is thus both a rational and algebraic system. The Continental School also follows the

metaphysical view associated with this rationalist tradition, which defines the nature of physical theory as aiming at the causal explanation of physical phenomena. Duhem describes how “The French geometers who composed the first theories of mathematical physics had a tendency to see theories as true explanations in the metaphysical sense. They assumed that they had reached the reality of things and the true causes of phenomena. This tendency begun by Descartes was evident in the work of Laplace and Poisson, of Fresnel, Cauchy and Ampère” (Duhem 1893, 358).

The metaphysical commitment of the Continental School to the principle “of the identity of the real and the intelligible” (Duhem 1974, 320) requires that basic mechanical concepts represent the real causes in nature. The content of physical theory is given by assuming that the primary concepts (mass, shape, motion and/or force) and principles which are composed of these concepts represent real mechanical properties of a material system and the laws of nature governing masses in motion. They assume that there are causal principles for all physical phenomena. All remaining physical properties must be reduced to these basic mechanical concepts. According to this view, physical theories are explanatory (Duhem 1974, 80).

As mentioned, it was Descartes who developed the ontology and epistemology to legitimate this view and define methodologically the characteristic features of its physical theory and method of construction. Both demands, rationalist and metaphysical, establish fundamental differences between the Continental School's view of physical theory and the English School's view. The rationalist tradition of the Continental School constructs physical theory according to rules of geometrically inspired principles such as abstraction, simplicity, coherence, and logical unity. These principles are also metaphysical principles, in that they are assumed to be properties in nature (as described by Descartes) (Duhem 1893, 352).

Among the rationalist virtues with which the Continental School expects physical theory to comply, the logical unity of physical theory is foremost. This principle rejects any contradiction and incompatibility within as well as between theoretical systems. It requires that the laws be not contradictory, that they be independent and mutually consistent. This principle pervades all the different physical theories of the Continental School.

Mechanical theory, for the Continental School, is the unifying framework of physics, and most of its members make the mechanical theory of matter the means by which the mechanistic program seeks to unify the whole of physics. From Descartes to Poisson, the principle of logical unity takes different forms, depending on the details of the theory of matter, but it stands as an unquestionable category to shape physical theory. The principle of unification in Descartes's view is matter and motion and its set of fundamental mechanical principles. They are the basis for causal explanation for every physical phenomenon: gravity, light, magnetic attractions and repulsions (Duhem 1980, 472-473).

Laplace grounds the principle of unity in his mechanical-molecular model of matter with central forces and Newton's vectorial mechanics. Lagrange differs from Laplace in that he rests the principle of logical unity not precisely on a theory of matter but rather on a system of generalized coordinates, generalized forces, the principle of virtual velocities (Duhem 1905, 42) and D'Alembert's principle (Duhem 1905, 62). Duhem rejects the unification of physical theory by a theory of matter. He is committed to the goal of unification by means of mathematical properties: stability and equilibrium imposed by thermodynamics. Duhem makes use of Lagrange's method, which is called energetic mechanics – his method to construct the energy of the system –, and makes the potential of the system its main concept.

A theoretical physics developed according to Lagrange's method of potential and without a theory of matter is present in Gibbs's formulation of thermodynamics. The Gibbsian view of thermodynamics is the model of unification according to Duhem. For the Continental School, the principle of logical unity is the leading principle for the construction of physical theory. In accordance with this principle, “for a mathematician of the school of Laplace or Ampère, it would be absurd to give two distinct theoretical explanations for the same law, and to maintain that these two explanations are equally valid” (Duhem 1974, 81).

The principle of logical unity is understood by the rationalist tradition as a logical principle whose violation is against the laws of reason and is therefore absurd. This principle demands that incompatibilities between theories/laws and concepts be eradicated from physical theory, and that all physical phenomena be deduced from the concepts and fundamental principles of mechanics.

The English School

The nature and character of the English School's mechanical explanation of physical phenomena (Chiappin 1989,43) is spelled out by Lord Kelvin when he says that

It seems to me that the true meaning of the question "What do we understand or not understand by a particular subject in physics?" is this: Can we make a corresponding mechanical model? [...] I am never satisfied as long as I have been unable to make a mechanical model for the object; if I can make a mechanical model, I understand; for as long as I cannot make a mechanical model, I do not understand (in Duhem 1980, 102).

Therefore, model-building is the method of construction of physical theories, and physical theory is identified with the constructions of models. The synthetic application of mechanical theory in the English School's view is influenced by the empiricist tradition stemming from Bacon, Locke, Hume and Newton. Particularly, the experimentalist conception of physical theory is taken from Newton's optics. At the root of this experimental/empirical conception of physics is the methodological principle of the separability and testability of isolated hypotheses, of crucial experiments, and of the inductive method. This principle can also be associated with Newton's basic view of mechanics as applying to isolated particles. The fundamental laws of mechanics as stated by Newton hold in the first instance for a single particle only (Lanczos 1986, 4).

The English School took from the empiricist tradition the determination to emphasize the empirical/practical aspects of physics over the theoretical. Moreover, for the English School, empirical adequacy should be the relevant criterion to accept mechanical explanations over rationalist criteria. Most of the English scientific community in the 19th century did not take formal and rationalist values as meaningful criteria to construct physical theory. Logical unity and other rationalist virtues such as axiomatization, consistency and simplicity were considered of no great importance to elaborate a mechanical explanation of physical phenomena.

For this school, as opposed to the Continental School, a mechanical explanation is not given by a well-constructed system of propositions logically chained from its definitions and postulates to its testable consequences, but rather by means of a sequence of disparate, concrete and figurative models. The English School does not require rational agreement between the mechanical system and the empirical laws which it is supposed to explain. Physical theory is identified with mechanical models made of concrete and real elements. The English School defends the methodological right to construct for each category of physical phenomena one or more mechanical models instead of a single mechanical model. Underdetermination of models by data is a methodological resource to construct physical theories. Maxwell and Lord Kelvin are outstanding representatives of this school, and their physical theories are a sequence of disparate models. Duhem states: "for a physicist of the school of Thomson or Maxwell, there is no contradiction in the fact that the same law can be represented by two different models" (Duhem 1974, 81; 1893, 81, 361).

In other words, they replace the principle of logical unity with the principle of the underdetermination of theory by data. Incompatibilities and contradictions between models are not violations of any logical principles, but instead they are methodological resources for the construction of mechanical explanations. The English School also does not demand rational agreement between the conceptual basis of mechanical theory and its empirical basis of empirical laws. This connection is made by mechanical models that simulate or show resemblances to these physical laws.

The relevant criterion to accept mechanical models as a mechanical explanation is that of "resemblance" between the model and the physical phenomena thereby represented. Empirical adequacy is interpreted as a pictorial way of simulating physical phenomena by models rather than a logical agreement between experimental laws and mechanical principles. The model-building method to construct physical theories is not concerned with logically rigorous hypothetical-deductive derivation of the empirical laws from the conceptual system of mechanics (Duhem 1893, 353-354).

Thus, the fundamental differences between the rationalist-oriented Continental School and the pragmatist/empiricist-oriented English School can be summed up as follows: the definition of the nature of

physical theory; the use of the principle of logical unity in physical theory; the method of the construction of physical theory; and the cognitive value of mechanical theory.

The Continental School follows the rational method while the English School uses the piecemeal method of model building. While for the Continental School "it would be absurd to give two distinct explanations for the same law and to maintain that these two explanations can be true at the same time, for an English physicist there would be no contradiction in the fact that the same law can be figured out in two different manners by two different models" (Duhem 1893, 360).

The Continental School applies the principle of logical unity while the English School applies the principle of the underdetermination of theory by data. In the wake of its rejection of the principle of logical unity, the English School demolishes another much-loved rationalist value, namely the notion of physical theory as a rational system. Duhem says: "Theory is, for [the English School], neither an explanation nor a rational classification of physical laws, but a model of these laws, a model built not to satisfy reason, but to please the imagination" (Duhem 1974, 81; 1893, 361).

Duhem stresses the fact that the model-building method is a non-rational method to construct physical theory, and that physical theories constructed in this manner are non-rational systems, unable to provide a rational classification of empirical laws. They must, therefore, be rejected. This is one of his harsh criticisms of the English School. "From there, these discrepancies, these incoherencies, these contradictions generated by English theories, that we tend to judge severely because we are looking for a rational system when the author only intends to give us a work of imagination" (Duhem 1893, 361).

Duhem's defense of the rational method of construction of physical theory is essential because it reflects the true nature of his conception; his rationalist commitment gives rise to a demarcation from the conventionalist/instrumentalist interpretation of physical theory. His rationalist commitment is what explains his commitment to applying the principle of logical unity in physical theory. Further, it is his view of physical theory as a rational system that explains his criticism of the English School for manipulating theoretical systems as algebraic models. Conventionalism and instrumentalism are committed to the principle of the underdetermination of physical theory by data.

The Continental School follows a metaphysical view of the nature of physical theory regarding its cognitive status, meaning that physical theory aims at explicating the real causes of physical phenomena. Its main representatives, Descartes, Laplace and Poisson, believe that mathematical physics provides real and causal mechanical explanations. Two concepts of truth underlie this view: the theory of truth as correspondence, which is applied to the conceptual basis of mechanical systems, and the theory of truth as coherence, which guarantees that mathematical principles and logical principles lead from truth to truth. The theory of correspondence is the rationalist principle of "identity of the real and the intelligible" (Duhem 1974, 320; 1893, 358).

The English School rejects the application of both concepts of truth because they are incompatible with the underdetermination of models. The underdetermination of models does not seem methodologically compatible with the idea that the mechanical elements in the models can represent the mechanical cause of physical phenomena. The principle of underdetermination is inherently an anti-realist methodological rule. The model-building method methodologically expresses its purposes: physical theories are to be considered solely as convenient instruments for experimental research. According to Duhem, the English School is concerned only with the utilitarian value of physical theory rather than with theoretical knowledge (Duhem 1974, 319).

The English School does not regard mechanical models as solutions for the problem of the identity of the real and the intelligible. According to Duhem, mechanical models are used as solutions to the problem of providing convenient instruments for experimental research, and are means to act on nature rather than to know nature. The underdetermination principle is the primary anti-realist argument. Consistent with its methodology, the English School assumes a pragmatist/empiricist view of the value of physical theory in which models have the status of a recipe, a practical and instrumental value. The pragmatist/empiricist view of the cognitive value of physical theory is reinforced by the fact that the metaphysical/aprioristic approach to the theory of matter which underlies mechanical theory suffered various theoretical as well as experimental setbacks.

The English School also questions the idea that the experimental method is a means to decide the truth value of scientific propositions. It is a method to construct empirical laws and models resembling these laws. These models and laws are interpreted as guides to act on nature rather than means to know nature

(in its metaphysical sense). According to Duhem, Poincaré not only introduces the English School view in France, but further develops its epistemological and methodological assumptions (Duhem 1974, 86-93, 149, 251, 319; Chiappin 1989, 320, 130-134;). Duhem's goal is to avoid this conception of physical theory (Duhem 1974, 149; Chiappin 1989, 320). He must pursue this goal without committing himself to the metaphysical aspect of the rationalist view of physical theory. His proposal pursues a middle way between the metaphysical view and the English (pragmatist/conventionalist) views, retaining from the pragmatist/empiricist view its criticism of the causal explanation of physical phenomena.

The problem for him is how to construct a view of the nature of physical theory which methodologically rejects the search for causes without becoming, at the same time, a pragmatical view. The English School's assumption of the principle of underdetermination is a natural methodological resource against such an explanatory view of physical theory. Duhem's problem is to find a similar methodological resource which naturally rejects the causal view of physical theory (its realist component, the entities) without giving up the application of the principle of logical unity in physical theory, for logical unity is the core of Duhem's view of physical theory as a rational system and of his view of scientific growth as rational and continuous. The principle of logical unity presses the search for more and more comprehensive physical theories.

Therefore, despite the epistemological and methodological differences between these two views of the synthetic method of constructing physical theory – one associated with the metaphysical/rationalist and the other with the pragmatic/empiricist view –, Duhem shows that neither can comply with the principle of the continuity of scientific development, nor with the aim of a unifying framework for the whole of physics (Duhem 1917, 152). In particular, he shows that the model-building method is not a rational method of constructing physical theory, and, accordingly, that any physical theory constructed by this method is not rational.

One can sum up his criticisms of the synthetic method by saying that this method does not make scientific progress continuous. A physicist, says Duhem, "Will see [the physical theories constructed by the mechanistic method] constantly being reborn, but constantly aborted; [...] it will clearly appear to him that the physics of atomism, condemned to perpetual fresh starts, does not tend through continued progress to the ideal form of physical theory" (Duhem 1974, 304). Duhem also argues that it cannot comply with the principle of the logical unity of physics. He says that the attempt of the synthetic method to provide a unification of all empirical laws makes its explanation overburdened with arbitrary and bizarre combinations (Duhem 1974, 304).

The Analytic Method: Description and Problems

According to Duhem, the second attempt at explicating physical phenomena mechanically is carried out by the analytic approach (Duhem 1980, 96; Chiappin 1989, 57-80). This new attempt arose in the English School when the aforementioned difficulties began to emerge from the synthetic approach to mechanical theory. Maxwell himself was disappointed with the results of his attempt at a mechanical explanation according to the synthetic approach. Duhem says: "Undoubtedly, therefore, Maxwell found little satisfaction in the mechanism he had thought of, for he soon abandoned it to set out upon a completely different path toward the mechanical explication of electric phenomena" (Duhem 1980, 68-69). Thus Duhem and even Poincaré point out that the complicated and bizarre forms taken by the mechanical models are some of Maxwell's reasons for such a disappointment. Poincaré says: "The strangeness and complication of the hypotheses that he had been compelled to make had led him to give them up" (Poincaré 1901, ix).

Certainly Poincaré does not care about the problems of the synthetic method in regard to the principle of logical unity and the principle of historical continuity because he is not methodologically committed to these principles. Maxwell seeks to develop, in order to avoid the difficulties associated with the use of mechanical models, a mechanistic method that will provide a more abstract approach to constructing physical theory. By this time mechanics already had an abstract method to construct mechanical theories, namely the Lagrangian/analytic mechanics. Maxwell adopts analytic mechanics to explain physical phenomena, giving rise to a new view of mechanical explanations. The application of analytic mechanics to the construction of mechanical theories constitutes a new version of the mechanistic program. The core of these new mechanical explanations consist of mechanisms of masses and motions.

The epistemological and methodological consequences of this analytic method are far-reaching. One of them is that the arbitrariness and indeterminacy of these invisible mechanisms of hidden masses and motions (Maxwell 1867, 50) make any empirical law susceptible to a mechanical explanation. In our view Poincaré develops the appropriate epistemological and methodological details of this view of the nature of physical theory: a pragmatist/conventionalist view.

Duhem proposes to disenfranchise Poincaré's generalization of the consequences of his critical analysis of this method applied in mechanical theory to physics in general. This generalization would be legitimate if mechanical theory were the unifying framework for the whole of physics, or if it were based on purely mathematical terms. Duhem questions Poincaré's view because it is based upon strategies appropriate to mechanical theories and not to theoretical physics such as mathematical physics.

Maxwell's methodological viewpoint on the mechanistic program (like many of his contemporaries') uncritically presupposes a generalized thesis about the relation between Lagrange's formulation of mechanics and the mechanical interpretation of physical phenomena. This thesis identifies the Lagrangian method with the mechanical interpretation of physical phenomena, constituting the main methodological principle of the analytical approach to mechanical explanation.

Duhem begins his critical examination of the analytic approach to mechanical theory by questioning this identification, and in particular its sufficient condition. Duhem separates the mathematical structure of Lagrange's method from its mechanical interpretation (Chiappin 1989, 62-68). The mechanical interpretation is, for him, one of the many possible interpretations of the Lagrangian system of equations and generalized coordinates. His criticism illuminates the generality of the mathematical formulation of Lagrangian mechanics by pointing out that its applications go beyond the application in mechanical theory (for example, Maxwell's use of it to organize his electromagnetism theory). The axiomatization of thermodynamics by Gibbs was based upon thermodynamical potential instead of Carnot's cycle. The model of this axiomatization of thermodynamics is the Lagrangian method and constitutes Duhem's ideal of theoretical physics. The Lagrangian method constitutes Duhem's ideal of mathematical structure for the organization of empirical laws.

Duhem is also particularly concerned with the unfalsifiable character of physical theories produced by the application of the analytical method, and with its strategy to avoid the falsifiability of physical theories (Duhem 1980, 78-79; Chiappin 1989, 74-80). This comes from the identification of Lagrange's method of mechanics (Lagrange 1997) with the theory of matter (Chiappin 1989, 64). Unfalsifiable theories do not meet Duhem's view of scientific growth as rational and continuous.

The key element of the analytical approach to mechanical explanation is analytic mechanics according to Lagrange's and Hertz's formulations. The Lagrangian formulation of mechanics corresponds to the foundations of mechanics according to the dynamical current of mechanics (Newton), while Hertz's formulation corresponds to the foundations of mechanics according to the kinetic current of mechanics (Descartes). Whatever its formulation, the analytic approach to the mechanistic program aims at establishing the possible conditions to explain a physical phenomenon mechanically, rather than effectively construct a mechanical explanation, as with the synthetic approach. This new approach seeks, instead of building up a mechanical model (like that of honeycomb or idle wheel particles proposed by Maxwell to explain currents and fields) (Duhem 1980, 68), to construct an algebraic equation for the kinetic energy (T) and potential energy (U) of the physical system. The Lagrangian formula is defined as $L = T - U$. Lagrange's equation

is given by $\left(\frac{dL}{dq}\right) - \frac{d}{dt}\left(\frac{dL}{dq'}\right) = 0$, where (q, q') describes its generalized coordinates. From this

equation, one can mathematically derive through Lagrange's equations the empirical laws of physical phenomena discovered by the experimental method, and then search for mechanical interpretations or analogies.

The mathematical equations and the transformation laws involved in the analytical approach to mechanics enable a strategy to defend a unified mechanical view of physics. Maxwell himself explicitly makes this commitment to the application of the analytic approach to mechanics in his *Treatise on Electricity and Magnetism* (Maxwell 1954). There are now two new logical goals brought by the application of analytical mechanics to electromagnetism. First, to apply the analytic approach, which is concerned with measurements and mathematical relations concerning physical quantities; and second, to be committed to

the establishment of the connections and analogies between this analytic approach and the mechanical explanation of physical phenomena.

Poincaré synthesizes, in his analysis of Maxwell's electromagnetism, these conditions according to the analytical method to make physical phenomena susceptible to mechanical explanation. This is precisely a definition of the analytic approach to mechanicism:

It is easy to understand now what Maxwell's fundamental idea is. To demonstrate the possibility of a mechanical explanation of electricity, we did not need to worry about finding the explanation itself, it was enough to know the expression of the two (T) and (U) functions, which are the two parts of the energy, to form with these two functions Lagrange's equations, and then compare these equations with the experimental laws (Poincaré 1901, viii).

There is a meaningful assumption underlying this entire discussion, namely the thesis of the identification between Lagrangian representation and mechanical theory. This thesis is supposed to guarantee the existence of a mechanical explanation as soon as one constructs the Lagrangian function of the system. Thanks to this thesis, Poincaré shows that the analytic approach to the mechanistic program brought into evidence this methodological truth: "if a phenomena has a complete mechanical explanation, it will have an infinity of others which give an equally good account for all the particularities revealed by experiment" (Poincaré 1901, viii). This methodological conclusion contains the core of Poincaré's pragmatist/conventionalist view, the principle of the underdetermination of physical theory by data (Pareto 1909, 31-36), which Duhem's theory of science strives to make illegitimate.

So what one must understand by a mechanical explanation of physical phenomena is the possibility of constructing an independent system of equations made up of kinetic (T) energy and potential (U) energy and certain numbers of parameters (generalized coordinates). Assuming that one can construct such equations, one can always, according to Poincaré's analysis of Maxwell's analytic method, determine masses (hidden or visible) and their generalized coordinates in such a way that the kinetic and potential energy of this system of masses is equal to that of the kinetic (T) and potential (U) energy appearing in Lagrange's equations (Poincaré 1901, viii).

From these conditions and from Lagrange's equation one draws the equations for the motion of the system. If these equations are identical with the empirical laws constructed by the experimental method, then, according to Maxwell, "we shall have proved that electromagnetic phenomena are capable of a mechanical explication" (Duhem 1980, 70). Duhem questions that these conditions prove that a physical phenomenon is susceptible to a mechanical explication. All one has here is a mechanical interpretation or analogy.

Duhem's Analysis and Objections to the Analytic Approach to Mechanicism

Duhem begins his criticism of the analytic approach by questioning the identity between analytic mechanics and mechanical explanation. First, though, it should be recalled that, with this approach, attempts are made to avoid the aforementioned difficulties arising from the synthetic approach. Those difficulties associated with the complicated and bizarre form of mechanical models (Duhem 1980, 68-69; Poincaré 1901, ix; Chiappin 57-58) arise from the demand of making conjectures about, or simulating, the particular mechanisms underlying the physical phenomena being studied. Nothing similar is done in the analytic approach: there is no demand to construct a mechanical model. The task is to construct the algebraic equations of the kinetic and potential energy of the system and to apply them to Lagrange's equations.

The construction of these equations employs the measurable quantities of observable physical phenomena. The agreement between the empirical laws of physical phenomena discovered by the experimental method and the equations deduced (through Lagrange's equations) from the kinetic energy, potential energy, and virtual work constructed from these same measurable quantities is then assumed to guarantee a mechanical explanation for the phenomena. Since these conditions are fulfilled, one can construct, in principle, a mechanical system of masses in motion with the same kinetic energy and potential energy of the physical phenomena studied. Therefore, according to Maxwell, a mechanical correspondence between the two sets of quantities involved is warranted, which is Maxwell's concept of the reduction of physical theories. This reduction is guaranteed by three assumptions: the thesis of equivalence, the

construction of Lagrange's equations, and the agreement between the constructed laws and the empirical laws.

According to Duhem, the significant mistake (confusion) here is the identification between the Lagrangian method and mechanical interpretation (Chiappin 1989, 61-79). Underlying this criticism, one finds his view that theoretical physics is a mathematical structure with a physical interpretation. He understands Lagrangian mechanics as an interpreted calculus (variational calculus), whose mathematical structure can be applied to different domains of physics.

Duhem questions the thesis of the equivalence between analytic mechanics and mechanical interpretation and its condition of sufficiency. The first major objection to the analytic approach to the mechanistic program is that the formulation of a physical problem in Lagrangian mechanics is not a sufficient condition to guarantee its mechanical explanation (Duhem 1980, 71).

Duhem agrees that it is a necessary condition, but disagrees that it is a sufficient condition. He questions the thesis that from this condition one can conclude with certainty that there exists a certain group of masses and forces, a certain mechanism, admitting such potential, and, above all, such kinetic energy (Duhem 1980, 7).

The proposed mechanical explanations were to be interpreted as illustrations, which, at most, imitate or simulate in their laws of motion the equations that are being discussed, and are not true mechanical explanations. Duhem says:

It seems imprudent to dismiss similar difficulties with a stroke of the pen. What has been found to be best, up to now, for clearing objections of this nature, is to imagine extremely simple mechanisms whose internal potential and kinetic energy offer, in their various particularities, a more or less direct analogy with the potential and kinetic energy that it is proposed to study; in a word, this is to construct models which imitate in their laws of motion the equations that are discussed. Aided by the theory of monocyclic systems, Boltzmann has illustrated the views of Maxwell on the analogy between Lagrange's equations and the laws of electrodynamics within such models (Duhem 1980, 72).

Duhem draws attention to the difference between a truly mechanical explanation, which would be the real causal explanation of the physical phenomena, and mechanical illustrations/models, which are mechanisms with the similar potential and kinetic energy of the system studied. Further, we would like to point out that Duhem's view of the generality and extendibility of the Lagrangian method underlies his criticism of the thesis of equivalence.

Duhem considers that the Lagrangian method is a mathematical structure that can be applied to different domains of physical quantities and kinds of forces where the generalized system of coordinates receives different physical interpretations. Mechanical interpretation would be only one particular interpretation.

Duhem himself applies Lagrangian formalism to make thermodynamics an axiomatic system, without mechanically interpreting the phenomenon of heat. The conditions of the extendibility of the Lagrangian method is the subject of his article "Sur quelques extensions récentes de la statique et de la dynamique" (Duhem 1901, 130-157) and is also his preoccupation in the second part of his book *The Evolution of Mechanics* (Duhem 1980, 105-189; 1905). His conception of physical theory arises from his understanding of this generality of Lagrangian mechanics. His view of the scientific development as pursuing more comprehensive abstract theories is based upon the transition from Newton's vectorial mechanics to Lagrangian mechanics.

This objection is not, however, the only objection that he raises against the mechanistic interpretation of the analytic approach. Duhem points out a methodological problem in this method of constructing physical theory. The analytic method constructs unfalsifiable mechanistic theories, and leads the mechanistic method to a process of infinite regression. The analytic approach to the mechanistic method, which aims to make mechanical theory a unifying principle of physics, gives rise to the following question "can all physical laws be put into the form of Lagrange's equations?" (Duhem 1980, 73). In other words, can the analytic approach to the mechanistic program provide the means to accomplish the aim of unifying the whole of physics? There are two answers to the above question.

The first is pursued by some physicists, like Poincaré, who state that "There exists a radical incompatibility between Lagrange's mechanics and the laws of physics" (Duhem 1980, 73; Poincaré 1892,

xviii). These physicists point out the difficulty in reducing the second law of thermodynamics (and all irreversible phenomena) to mechanics as evidence of this fact. They reason that if, on the one hand, an analysis of Lagrange's mechanics shows that "all motions controlled by the dynamics of D'Alembert and Lagrange are reversible motions" and if, on the other hand, the examples given by the experimental method and the facts show that "natural motions are not reversible" (Duhem 1980, 75), then there is an evident incompatibility between the facts and the mechanical theory. What follows from this incompatibility? Is it that one should stick to mechanical theory as the unifying framework of physics and attempt to modify its basic principles, such as D'Alembert's principle (Duhem 1980, 75-76)? Let us not go too fast here.

According to Duhem, a more detailed logical analysis of the analytic approach to mechanical theory, for example the one carried out by Helmholtz's analysis of the mechanical program, shows that there is no possible justification for such an incompatibility. Based upon Helmholtz's analysis, Duhem opposes the thesis of the incompatibility between the analytic approach to the mechanistic method of constructing physical theories and natural motions. According to Duhem, to show that such incompatibility is only apparent from the viewpoint of the foundations of analytic mechanics is enough to admit the hypothesis of the atomistic nature of matter (Duhem 1980, 78).

As mentioned the arbitrariness and indeterminacy of these invisible mechanisms of hidden masses and motions (Maxwell 1867, 50) make possible to construct a mechanical explanation for any empirical law. Duhem says: "Whatever may be the form of the mathematical laws to which experimental inference subjects physical phenomena, it is always permissible to pretend that these phenomena are the effects of motions, perceptible or hidden, subject to the dynamics of Lagrange" (Duhem 1980, 78). The indeterminacy and arbitrariness of the invisible mechanism of hidden masses in motion make theoretically possible the finding of mechanical explanations for any possible discrepancies with empirical facts (Duhem 1980, 77-78).

Is this appeal to invisible mechanism repugnant to a physicist? Not at all. It is exactly what physicists have been doing, and examples in physics are abundant. This is the case in the work of Helmholtz, Boltzmann, Clausius, and Maxwell (Laranjeiras 2002; Laranjeiras and Chiappin 2006). Helmholtz employed the mechanistic program according to the analytic method, where the hypothesis of a system of hidden masses in motion assumes the form of monocyclic systems, generated by Lagrange's equations, to build up mechanical illustrations, for instance of the second law of thermodynamics (Laranjeiras and Chiappin 2008). Helmholtz's work was the source for Boltzmann, Clausius, and Gibbs. Therefore, for Duhem, physicists such as Poincaré methodologically interpret incorrectly the incompatibility between mechanism and facts. There is no possible incompatibility between mechanism and experimental facts. The mechanistic method based on Lagrange's mechanics and on the indeterminacy and arbitrariness of the system of hidden masses in motion is not falsifiable in any way (Duhem 1905, 182). This, for Duhem, is the correct interpretation of the analytic approach to the mechanistic method. Thus, he points out that the consequence of using this hypothesis will be the unfalsifiable character of mechanical theories constructed by the analytic approach. He seeks then to preserve the mathematical formulation of D'Alembert's principle. He argues that, from the algebraic viewpoint, modifications and generalizations of the equations of dynamics by introducing the term of first degree in the velocities are easy to notice (Duhem 1980, 76).

Thus, Duhem preserves the basic principles of Lagrangian mechanics and redirects the difficulties and obstacles raised against the mechanistic program to its two presuppositions: the atomistic hypothesis of matter and its ultimate demand that physical concepts be reduced to a restricted number of primitive mechanical magnitudes such as mass, motion, shape and/or force (Duhem 1903, 270).

The goal of unifying the whole of physics (the principle of logical unity) by the analytic approach to the mechanistic program, with its restricted mechanical conceptual basis (mass, motion, forces), is attained at the cost of a large indeterminacy of its parameters of masses and motions. And the consequence of the large indeterminacy and arbitrariness of this system of hidden masses in motion is the unfalsifiability of mechanical theories. In the analytic approach, mass and motion are mere parameters in mathematical equations, without the realistic or figurative connotations they have in the synthetic approach. The synthetic approach to mechanical explanation involves specifications and determinations of the masses, motions, sizes and forces.

Why does Duhem see the unfalsifiability of the mechanistic method as a problem and why is it a source for his criticism? Is not this approach a legitimate heuristic approach as defended by Poincaré? Duhem does not think that the aim of theoretical physics is to promote the discovery of new laws. Such is the task of experimental physics.

Duhem pursues a rational explanation of the evolution of theoretical physics. From this viewpoint, he evaluates the methodological characteristics of the analytic approach to mechanistic theories. He does not think that hypotheses and methods that construct unfalsifiable physical theories make scientific growth rational and continuous. For him, rationality is controllability, and continuity means that the new theory encompasses everything already accounted for. Duhem's method of constructing physical theory with his rules for the formation of physical concepts establishes that the introduction of concepts in physics must be controlled by measurable conditions, and that mechanical models are to be expelled from physical theories.

As we have seen, these systems of hidden masses in motion are totally indeterminate and arbitrary, since nothing limits the nature and number of these masses in motion; this makes mechanical theories unfalsifiable (Duhem 1980, 78-79). Consequently, physical theories constructed by a mechanistic method, according to Hertz's and Lagrange's mechanics, are not subject to the control of empirical facts. This means that the application of the empiricist principle of the testability of physical theory is made meaningless by the mechanistic method of constructing physical theory. For Duhem, a rational method of constructing physical theory (Chiappin 1989, 282-310) must optimize the empirical testability of physical theories.

Therefore, from Duhem's viewpoint, the ongoing polemic between Lagrange's formulation of classical mechanics, which takes force as a primitive concept representing a real cause, and Hertz's formulation, which rejects force as a primitive concept, does not make any difference with respect to his criticism of the mechanistic method. For him, both formulations generate unfalsifiable physical theories (Duhem 1980, 97; 1905, 183).

Hence, the analytic approach and the atomistic hypothesis can always provide compatibility between a mechanical theory and facts making it unfalsifiable (Duhem 1905, 182). This thesis, the compatibility between mechanical program and facts, means, for Duhem, that a mechanistic program cannot effectively comply with the empiricist principle of empirical testability, or with its associated methodological principle. Duhem defines this principle as follows: "In physics, one criterion alone allows the rejection as false of a judgment which does not imply a logical contradiction: the record of a flagrant disagreement between this judgment and the facts of experience" (Duhem 1980, 97).

The application of this principle which governs the empirical testability of physical theory in the test of mechanistic theories illuminates Duhem's conclusion that the proposition which states "all physical phenomena are explained mechanically" transcends the physical method. By physical method Duhem understands the experimental method by which one discovers empirical laws, while the mechanistic method is a method to construct physical theories which explain or represent these laws. Thus, Duhem concludes that "It is impossible for anyone who holds to the processes of the experimental method to declare as true this proposition: 'All physical phenomena are explained mechanically'. It is also impossible to declare it false. This proposition transcends the physical method" (Duhem 1980, 97-98; 1905, 183-184).

So, Duhem's analysis of the status of the mechanistic program has far-reaching methodological consequences for his conception of physical theory. One can be cited: that the decision about the mechanistic method of construction goes beyond the experimental method. Duhem says: "If, in regard to this proposition [stating the transcendent character of the mechanistic method], one wishes to depart from a state of mind where every decision remains suspended, one will have to resort to arguments unknown to experimental method" (Duhem 1980, 98).

In summary, according to Duhem, neither the metaphysical method – the foundationalist version (Duhem 1980, 98) – nor the experimental method are able to decide the truth value of mechanistic theories. Where can the answer come from? Duhem suggests: "The degree of suitability of a method in fact is essentially a matter of personal appreciation; the particular turn of each thinker, the education received, the traditions immersed in, the customs of the environment in which he lives, all influence this appreciation to a high degree; these influences vary in the extreme from one physics to another" (Duhem 1980, 99). It is difficult to avoid thinking that Duhem is the source of Popper's assertion that conceptions of science are a methodological and conventional matter. The important aspect of this discussion about the analytic method of constructing mechanical theories is Duhem's awareness of the unfalsifiable character of this method, and that the decision to reject it goes beyond the experimental method (Duhem 1974, 293-294; 1893, 366; Chiappin 1989, 134). Further, for Duhem, the analytic approach to the mechanistic method of constructing physical theory is the source of its unfalsifiability. Poincaré sticks to the mechanistic method to analyze physical theories and reinterprets the experimental method as unable to decide conclusively the truth value of scientific propositions.

Duhem defends the position that the experimental method can reject (well-constructed) physical theories, and he turns down the use of methods and hypotheses that introduce indeterminacy and arbitrariness into physical theory (Duhem 1980, 97). Duhem defines a method of constructing physical theories which makes the experimental method effective in refuting theories (Duhem 1974, 78; Chiappin 1989, 287).

Duhem does not affirm that the unfalsifiability of physical theory is a characteristic of any method of constructing physical theory. For him, the experimental method cannot reject physical theories constructed by the mechanistic method. But it is not true that the experimental method cannot aid in the rejection of any theory. The refutability of physical theories depends on their method of construction. He defends the idea that we can control the process of constructing physical theories. And from the logical viewpoint, well-constructed physical theories can be refuted (Chiappin 1989, 62-80). If theories are not falsifiable, it is our own fault. It is essential, for Duhem's view of scientific progress, that the experimental method can refute and reject physical theories. This is also essential to construct physical theories that satisfy the principle of empirical testability. This is so because Duhem is committed to the principle of continuity. From his arguments against mechanicism one can see, now, that Duhem is strongly committed to a view of scientific growth as rational and continuous. Therefore, the true methodological verdict of his historical-critical analysis of the analytic approach to the mechanistic program is that one cannot, due to its use of the atomistic hypothesis, scientifically decide whether to accept or reject it in a conclusive manner. (Chiappin 1989, 77-82)

Once it is accepted that the decision about the mechanistic program goes beyond the experimental method, one requires extra-empirical rules to legitimate this option. The notion of rational method for Duhem acquires a meaning beyond mathematical consistency and experiment. It requires extra-empirical rules.

Duhem rejects Poincaré's as well as the English School's pragmatical conception of physical theory (Duhem 1974, 149; Chiappin 1989, 51-57). He also rejects the metaphysical view of classical rationalism (Chiappin 1989, 40-42). His strategy is to reject the metaphysical view, as well as the model-building view of the nature of physical theory, and to rescue the ideal (from classical rationalism) of physical theory as a rational system based upon a very small set of formal principles in the style of the principle of least action or the potential functions of thermodynamics.

To conclude our appraisal of Duhem's position, for him the mechanistic program faces an unsolvable dilemma: if the mechanistic program wants to use the mechanical theory according to the synthetic method, then it must give up the principle of logical unity. Consequently, the rejection of the mechanistic program is, ultimately, a methodological/epistemological decision and not a scientific one.

If the mechanists want to use the mechanical theory according to the analytical method, containing the atomistic hypothesis of matter, rescuing thereby in principle its purpose of providing a unifying principle for the whole of physics, then they must give up the aim of providing a mechanical explanation proper, i.e. a picture or model of physical phenomena. Duhem states this situation clearly:

Hence the analytic method, which alone seems capable of providing from the laws of physics a logically constructed mechanical explanation, seems incapable of satisfying the requirements of imaginative physicists, that is to say, of the very ones who required a mechanical interpretation of phenomena. If these physicists want, at any price, to picture the qualities of bodies in shapes suitable for geometric intuition, in shapes simple enough to be depicted in a table clearly understandable to the eyes and the imagination, they will have to renounce the hope of uniting all these representations into a coherent system, into a logically ordered science (Duhem 1980, 101).

Duhem's Formal Conception of Theoretical Physics and the Principle of Rational and Continuous Progress

This section will outline Duhem's view of the rational and continuous progress of physical theory.

As concluded before, for Duhem neither the metaphysical view of physics nor the model-building method can comply with the principle of continuity. Besides that, the model-building method cannot comply with the rational method, which rejects the resource to contradictory models in physics. Duhem preserves the mathematical formalism of the analytic approach, namely the Lagrangian formalism, and focuses his

blame for the problems of this method on the atomistic hypothesis of matter. He points out that the atomistic hypothesis gives rise to strategies which make physical theories unfalsifiable and thereby traps them in a process of infinite regression. For him, the unfalsifiable character of physical theories and the process of infinite regression are real obstacles to seeing scientific progress as rational and continuous.

The continuous development of physical theory is shown, for Duhem, by the development of abstract theories. Abstract theories are mathematical structures (Lagrangian, Hamiltonian formalism) which form the mold to systematize and organize the empirical laws discovered by the experimental method.

Duhem clearly states his predisposition for the abstract aspect of physical theory as an element of continuity between physical theories. At the same time, he blames the use of atomistic theories of matter as unifying frameworks for the discontinuity of physical theory. He says:

He [the physicist who is not content with knowing physics through the gossip of the moment] will see abstract theory, matured through patient labor, take possession of the new lands the experimenters have explored, organize these conquests, annex them to its old domains, and make a perfectly coordinated empire of their union. It will appear clearly to him that the physics of atomism, condemned to perpetual fresh starts, does not tend by continued progress to the ideal form of physical theory (Duhem 1974, 304).

Thus, the continuity of scientific progress is accounted for by viewing the nature of physical theory as representational structures (Chiappin 1989). The historical continuity of scientific development is shown by the increasing generalization and abstraction of these mathematical structures with which we organize our set of empirical laws. This view of the nature of physical theories operationalizes the idea of progress as the increasing comprehensiveness of physical theories, where the idea of increasing comprehensiveness accounts for the idea of continuity. The principle of continuity states that new theories contain the acquired knowledge and are systematized by the old theories.

Continuity, in Duhem's view, is identified with comprehensiveness. This view of progress as the increasing generalization and abstraction of physical theories, and therefore of increasing comprehensiveness, accounts for the continuity between Descartes, Galileo, and Newton; and between Newton and Lagrange. Further, this view accounts for the continuity between Lagrange and energetics. To make this clearer: from Descartes and Galileo to Newton we go from a set of disconnected general laws, such as inertial law, the law of fall, Kepler's laws, and the collision laws, to a more general and abstract structure forming a rigid axiomatic system of concepts and principles that encompass these laws.

This axiomatic structure is vectorial mechanics, that accounts for all these laws, which means that it provides the unification of the terrestrial and celestial laws. Vectorial mechanics is based upon the idea that bodies are composed of isolated mass points, two vectors, force and momentum, four laws, Euclidean geometry, and the parallelogram rule. From Newton to Lagrange, we move from vectorial mechanics to a mechanics based upon energy, generalized coordinates, which is applied to a system of bodies instead of isolated mass points. From the Lagrangian method to the energetic method, Duhem wants to move from local motions (velocity) to general motions (e.g. chemical reactions).

For Duhem, the continuity of scientific progress between the mechanistic and energetic methods (new mechanics) is obtained by using, at least as an analogy, the Lagrangian formalism to construct axiomatic thermodynamics. This task is undertaken by Gibbs, by applying thermodynamics as the unifying framework for physics. Thermodynamics, so constructed, provides the foundations of chemistry-physics, not mechanics. This successful unification gives rise to a promising program to implement thermodynamics as the new unifying framework.

Duhem interprets Lagrangian formalism, with its principle of D'Alembert and its principle of virtual work, as a powerful mathematical instrument to be applied in any physical domain without turning it into mechanical physics. The development toward the generalization and abstraction of the Newtonian structure of mechanics into Lagrangian mechanics is described in Duhem's book *The evolution of mechanics* (Duhem 1980, 22-46).

As a result of this generalization and abstraction, one gets rational mechanics as constructed by Lagrange and to which Bernoulli, D'Alembert and Euler contributed (Duhem 1980, 23). This mechanics reduces all laws of equilibrium and motion to a single principle (the principle of virtual velocities) and to a single method of calculation (variational calculus).

The mathematical structure furnished by Lagrangian formalism is quite simply formed of two scalar quantities: the “kinetic energy” and the “potential energy,” along with the Lagrangian equations. Further, the problem-solving theory embodied by this method is, in general, more comprehensive and simpler than the theory of vectorial mechanics. The relationship between its theoretical elements, potential function and kinetic energy, and the empirical laws discovered by the experimental method seems to be more cohesive than in vectorial mechanics.

Indeed, there is almost a routine, already described by Poincaré, for solving problems in this analytic approach: the kinetic energy and potential function must be constructed in generalized coordinates, the Lagrangian function L formed from them and substituted into the Lagrangian equations in order to obtain the equations of motion. This routine furnishes Duhem with the ideal of a rational method to construct physical theory (Duhem 1892, 146; Chiappin 1989, 110)

There are two more features in this formalism that are of fundamental importance for Duhem's methodology. The first one, introduced by the Lagrangian approach to mechanics, is that it focuses on the system of particles instead of an individual particle, as in Newtonian mechanics (Lanczos 1970, 4). The second one is that it can be entirely derived from a single principle, namely the principle of least action. We have here a truly unifying principle for all sciences to which the Lagrangian formalism applies. These two features of the Lagrangian formalism fit in well with Duhem's two major methodological elements, namely the D-thesis and the principle of logical unity (Duhem 1974, 91; Chiappin 1989, 178, 240).

The next step was a new mathematical structure, whose essential scalar quantities are H , q and p , Hamiltonian mechanics. This Duhemian view of the continuous progress of theoretical physics by increasing the abstraction and generalization of mathematical structures receives substantial support from Arnold's book *Mathematical Method of Classical Mechanics* (Arnold 1980; Chiappin 1989, 86-87). Arnold describes Lagrangian and Hamiltonian mechanics with set theory. He demonstrates the generality of these two mathematical structures and the relation of inclusion between them. In the first part of his book (Arnold 1980, 1-52) he describes Newtonian mechanics as studying the motion of a system of point masses in three-dimensional Euclidean space.

In the next part (Arnold 1980, 53-159) he discusses Lagrangian mechanics, which is described as a mechanical system “given by a manifold (‘configuration space’) and a function on its tangent bundle (the ‘Lagrangian function’)” (Arnold 1980, 53). Commenting on the relation of the Lagrangian mechanical system and the Newtonian mechanical system, he says that: “A Newtonian potential system is a particular case of a Lagrangian system (the configuration space in this case is Euclidean, and the Lagrangian function is the difference between the kinetic and potential energies)” (Arnold 1980, 53).

In the following part of his book (Arnold 1980, 161-300) he discusses Hamiltonian mechanics, explaining that a “Hamiltonian mechanical system is given by an even-dimensional manifold (the ‘phase space’), a symplectic structure on it (the ‘Poincaré integral invariant’) and a function on it (the ‘Hamiltonian function’). Every one-parameter group of symplectic diffeomorphism of the phase space preserving the Hamiltonian function is associated to a first integral of the equations of motion” (Arnold 1980, 161). With respect to the relation between them, he says: “Lagrangian mechanics is contained in Hamiltonian mechanics as a special case (the phase space in this case is the cotangent bundle of the configuration space, and the Hamiltonian function is the Legendre transform of the Lagrangian function)” (Arnold 1980, 161).

If Duhem could have known Arnold's book, he would have seen it as the true expression of the continuous progress of theoretical physics. We assume that Arnold's view of the axiomatic foundations of mechanics (Chiappin 1989, 86-87), with its use of set theory, in terms of larger and more abstract mathematical structures, can be used to define Duhem's view of the progress of the order in which physical theory organizes empirical laws. However, there are increasing evidences for the role of the structures in the characterization of the physical phenomena, mainly, in statistical mechanics with the phase transition phenomena (Chiappin 2005, 11-15; Chiappin 1979, 134, 140, 169; Pettini, Franzosi and Spinelli 2000; Franzosi, Pettini and Spinelli 2014). This notion of progress corresponds to the epistemic component of Duhem's conception of physics. It has a value of knowledge.

There is another view of scientific progress in Duhem. This view is concerned with the subject-matter of physical theory and not with the mathematical structure which provides the mold for these laws. This other view defines progress as the accumulation of empirical laws with a special relation to the theory that resembles an algorithm. The development of physics can be understood as the search for a physical theory

which provides a method of establishing a tight connection between the mathematical structure of the theory and the empirical laws provided by the experimental method. The method of thermodynamical potential provided by Gibbsian thermodynamics comes closer to this ideal of method, and Duhem contributed to it with the famous Duhem-Gibbs relation. This view of progress corresponds to the practical component of his conception of physics.

In summary, we have argued that a complete point of view of Duhem's continuous and rational development of physics can be condensed as his defense of a conception of physics according to which physics should be a representational system with very few formal principles coordinating the set of empirical laws (Duhem 1974, 19; 1902b, 5) which works as an intermediary or a middle way between the metaphysical and a pragmatist/conventionalist conception of theoretical physics (Chiappin 1989, iv, 92; Duhem 1917, 157). He constructs this formal conception of physics in such a way that he ends up with an idea of scientific progress in the form of a sequence of representational systems as structures of increasing comprehensiveness of empirical laws (Duhem 1974, 304, Chiappin 1989, 86-87;). This leads him to defend a convergent structural realism toward an ideal physical theory (Chiappin 1989, 198). Duhem's conception of the ideal physical theory is a natural classification of laws (Duhem 1974, 298; Chiappin 1989, 106-114). This convergent structural realism allows him to demarcate his conception, on the one hand, from the conception of metaphysical foundationalism, associated with classical rationalism (mainly Descartes), and, on the other hand, from the conception of pragmatism/conventionalism, associated with the English School (mainly Poincaré).

Conclusions

We have argued that a point of view of Duhem's continuous and rational development of physics requires a formal conception of physics that he defines as a representational system of empirical laws based upon formal principles (Duhem 1974, 304; Chiappin 1989, 260) This is a middle way between two conceptions to physics the metaphysical view and, on the other hand, the pragmatist/conventionalist view (Chiappin 1989, 243-247). He constructs this formal conception of theoretical physics in such a way that he ends up with an idea of scientific progress in the form of a sequence of representational systems as structures of increasing comprehensiveness of empirical laws, which leads him to defend a convergent structural realism (Chiappin 1989, 198) toward an ideal physical theory, a natural classification of empirical laws (Duhem 1893, 368-369; 1902a, 206, 1974, 270). The combination of a historical-critical approach to the study of physics with a formal conception allows him to develop this kind of an intermediary strategy with the construction of a dynamical theory of theoretical physics. It is this dynamical theory that allows him to demarcate his conception, on the one hand, from the metaphysical conception, associated with classical rationalism (mainly Descartes), and, on the other hand, from the conception of pragmatism/conventionalism, associated with the English School (mainly Poincaré).

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Transversal: International Journal for the Historiography of Science, 2 (2017) 54-72

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Anti-Scepticism and Epistemic Humility in Pierre Duhem's Philosophy of Science

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Abstract:

Duhem's philosophy of science is difficult to classify according to more contemporary categories like instrumentalism and realism. On the one hand, he presents an account of scientific methodology which renders theories as mere instruments. On the other hand, he acknowledges that theories with particular theoretical virtues (e.g., unity, simplicity, novel predictions) offer a classification of experimental laws that "corresponds to real affinities among the things themselves." In this paper, we argue that Duhem's philosophy of science was motivated by an anti-sceptical tendency, according to which we can confidently assert that our theories reveal truths about nature while, at the same time, admitting that anti-scepticism should be moderated by epistemic humility. Understanding Duhem's epistemological position, which was unique amongst French philosophers of science in the beginning of the 20th century, requires a careful examination of his accounts of representation, explanation, and of their interrelation.

54

Keywords:

Pierre Duhem; representation; explanation; natural classification; holism; epistemic humility; realism

Received: 09 April 2017. Reviewed: 24 May 2017. Accepted: 31 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.06>

Introduction

Duhem's philosophy of science has justifiably attracted a great deal of attention. His *The Aim and Structure of Physical Theory*, initially published in 1906, offers a comprehensive account of science, its method and its value, which is difficult to classify according to standard categories such as instrumentalism and realism. The key interpretative difficulty relates to the fact that Duhem himself appears to be ambivalent. On the one

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hand, he presents an account of the method of science which renders theories mere instruments for classification of experimental laws and for predictions (cf. 1908a, 333). On the other hand, he acknowledges that theories meeting various conditions (unity, simplicity, novel predictions) are in the process of offering natural classifications of experimental laws, where a classification is natural if it “corresponds to real affinities among the *things themselves* (*les choses elles-mêmes*)” (1906, 26). This ambivalence appears in his partly autobiographical essay “Physics of a Believer” (1905). There he says quite clearly that a theory “constitutes a kind of synoptic or schematic sketch suited to summarize and classify the laws of observation” (1905, 277) and that “physical theory through its successive advances tends to arrange experimental laws in an order more and more analogous to the transcendental order according to which the realities are classified” (1905, 297). Claims such as the above might be hard to reconcile and this had led many commentators to argue that Duhem’s views are in some kind of tension.³ Or that Duhem was trying to canvass a middle position between realism and instrumentalism. Or that he was a ‘structural realist’.⁴

In this paper, we will argue for the unity of Duhem’s thought. From his early papers dating from the early 1890s to the *magnum opus* of 1906, Duhem occupied a special epistemological position which can be best understood in light of the debates about science, its method and its value, that were taking place among French philosophers of science. Seen in this light, Duhem wanted to achieve two things at the same time. He wanted to show that physical theories have “value as knowledge outside their practical utility” (1908a, 319) and that this very assertion can only be justified if the scientist breaks with positivism and endorses a conception of rational judgement that goes beyond the strict confines of logic.⁵ At the same time, he wanted to show that the cognitive value of physical theory is not tied to its offering (mechanical) explanations of the experimental laws. It is, nonetheless, tied to its being responsive, in some way, to the reality behind the phenomena it aims to represent.

A key point of this paper is that Duhem’s philosophy of science was motivated by an anti-sceptical tendency, admitting at the same time that anti-scepticism should be moderated by epistemic humility: it should come to terms with the history of science and the fact that “it reminds [us] that the most attractive systems are only provisional representations, and not definitive explanations” (1906, 270).⁴

Understanding Duhem’s philosophy of science requires a careful examination of his accounts of representation and explanation as well as of their relation. Duhem’s account of science rests decisively on the claim that the explanatory parts and the representative parts of theories can be sharply distinguished. We shall argue that they cannot be. The repercussions of this failure for Duhem’s account of science will then be explored.

Here is the roadmap. Section “Representation vs Explanation” will discuss Duhem’s account of representation, focusing on his novel use of abstraction principles to introduce physical magnitudes and his account of explanation, focusing on his anti-Cartesianism. Then, in section “Disentangling the two Parts of the Theory”, we will examine Duhem’s two arguments for the existence of a sharp distinction between the representative and the explanatory parts of the theory and we will show, summoning historical evidence available to Duhem himself, that this distinction is shaky and problematic. Section “Re-assessing Duhem’s holism” will then offer a new reading of Duhem’s anti-atomism (*aka* holism). In section “Natural Classification”, we will offer the main argument for the unity of Duhem’s thought in light of his views on natural classifications. Finally, in section “Are Relations the Epistemic Limit?” we shall briefly discuss some motivations of Duhem’s relationism.

³ For a useful discussion see Needham (2011).

⁴ Fábio Leite (2017) offers a nice summary of the various interpretative accounts of Duhem’s philosophy of science. For an account of Duhem as canvassing a middle way between realism and instrumentalism, see Psillos (1999, 34-37).

⁵ This point has been emphasized by Maiocchi (1990, 398).

⁴ Our notion of “epistemic humility” must be distinguished from the notion discussed by Kidd in his (2011); and more generally, from its use in recent debates concerning whether Duhem was a virtue epistemologist. Kidd talks of “intellectual humility” as virtue and takes it to refer to some inherent limitation of “the scope of human mind”, circumscribing “the proper acknowledgment of the cognitive capacities appropriate to human beings, and of the proper epistemic ambitions open to us”. We take ‘humility’ to capture the epistemic modesty concerning the reach and extent of scientific knowledge, which is driven mostly from patterns of theory-change in the history of science.

Representation vs Explanation

'Representation' is a technical term for Duhem. It captures a specific method of constructing physical theories, which is based on abstraction. Roughly speaking, a theory is said to be representative if it is constructed in such a way that its basic concepts are abstracted out of experimental facts and its basic principles—which connect the basic concepts of the theory—symbolize a set of experimental laws. The very idea of 'representation' for Duhem is connected with symbolism: a representation is always symbolic.

To be more precise, Duhem took it that there are four fundamental operations in a physical theory which renders it a *representation* (of a set of experimental laws):

- (1) the definition and measurement of physical magnitudes;
- (2) the selection of hypotheses;
- (3) the mathematical development of the theory;
- (4) the comparison of the theory with experiment.

Let's take a closer look at the four steps.

Abstraction Principles

The first step consists in determining the physical concepts which will stand for basic physical properties, viz., the simple elements from which every other physical property can be derived. These properties are extracted from a set of experimental laws, corresponding to the range of phenomena that the theory aims to describe (1892a, 6). As such, the fundamental building blocks of the theory must be closely connected to observations or experiments. The physical concepts are then translated into mathematical symbols, whose adequacy depends only on the features that the empirical property exhibits and that the physicist wants to capture.

The key method Duhem introduces for the specification of the basic concepts is abstraction (1892a, 3-4). Here is his example. Warmth is an empirical property of bodies. Bodies can be as warm as others or more or less warm than others. These features, however, though "essential to the concept of *warmth*, do not permit the measurement of the object of this concept—that is, to regard it as a *magnitude*". And yet, the relation of being *as warm as*, which holds between actual physical bodies given in experience, has the properties of being reflexive, symmetric, and transitive. It is, in modern terminology, an equivalence relation. Duhem observes that a more rigorous physical concept (and a corresponding magnitude) can be introduced on the basis of this equivalence relation, viz., temperature. As he (1892a, 3) put it:

We make two equal values of temperature correspond to two points that are as warm as each other.
We make two unequal values of temperature correspond to two points that are not equally warm,
and in such a manner that the higher value of temperature corresponds to the warmer point.

This move allows, among other things, the transition from a qualitative property to a quantitative one. Even though it makes sense to say that body A is as warm as, or warmer than, body B, it does not make sense to assert that the warmth of the body C is equal to the warmth of body A plus the warmth of body B. Not so for temperature, since this is a magnitude which is additive. In fact, the concept of temperature has excess content over the concept of warmth, since the concept of temperature involves an extra quantitative assumption, according to which to each point of a body can be assigned a definite value of temperature – an assumption that permits additivity.

The concept of temperature, then, is introduced on the basis of an *abstraction principle* over a set of physical bodies and an equivalence relation among them (*being as warm as*). Duhem is clear that this procedure is essentially generalizable. What was said about temperature "could be repeated—at least in its essentials—about all definitions of magnitude that we find at the beginning of any physical theory whatsoever" (1892a, 4). Note that this is a far-reaching approach. It shows that Duhem was appealing to

what came to be known as abstraction principles in order to introduce new physical concepts and corresponding magnitudes.⁶

This definition of physical magnitudes by abstraction out of equivalence relations among the properties of empirical bodies makes possible the statement of mathematical-quantitative physical laws about the magnitudes thus defined. In particular, it makes possible the symbolic representation of empirical laws, even if these laws do not bear strictly speaking on the same empirical properties as the empirical laws did. For Duhem, abstraction is indispensable because the magnitudes to which the theoretical hypotheses of a physical theory apply must be mathematized, so that these hypotheses state mathematically precise relations (laws) among magnitudes.

Hence, abstraction makes the mathematisation of nature possible. As he put it in (1893b, 58),

It is abstraction that furnishes the notions of number, line, surface, angle, mass, force, temperature and quantity of heat or electricity. It is abstraction, or philosophical analysis, that separates and makes precise the fundamental properties of these various notions and enunciates axioms and postulates.

But abstraction, when used within a physical theory, also implies that, as Duhem (1906, 128-131) explains, a physical magnitude need only possess the relevant mathematical properties (to be expressed by a number and to be additive) and to obey the relevant mathematical laws (commutativity and associativity). Hence, different symbols could be chosen for a given physical property, as long as the mathematical features of interest of the latter are captured by the symbol. Apart from this requirement, the definition of the physical magnitude is arbitrary, like a sign is arbitrarily chosen to represent the thing it signifies.

Hypotheses and Beyond

The second step in theory construction consists in relating these symbols to each other through hypotheses. Here again, there is absolutely no constraint whatsoever on the choice of hypotheses provided that, taken as a whole, they represent the totality of the experimental laws. From his early work *Some Reflections on the Subject of Physical Theories*, published in 1892, to *The Aim and the Structure of Physical Theory*, Duhem kept defining the 'ideal' method for choosing the hypotheses as "accepting no hypotheses except the symbolic translation, in mathematical language, of some of the experimental laws from the group (...) [the physicist] wished to represent" (1892a, 6, see also 1906, 190-191). Concerning the recourse to hypotheses, Duhem made clear that their formulation should be as little restricted from above by metaphysics as possible, the only logical constraint on their adoption being the principle of non-contradiction.

Enter the third step. From these hypotheses, the theoretical physicist proceeds via mathematical deduction to derive the (mathematical) consequences of the chosen hypotheses. This mathematical development of the theory is committed only to the rules of logic: the physical world is put in brackets at this stage of the process. Finally, however (and this is the fourth step), the consequences of the mathematical deduction will be translated back into claims about observational and testable predictions, allowing the physicist to submit his theory to the verdict of experimentation. As famously put by Duhem, the agreement with the experiment is the only criterion of truth for a theory. If the consequences of the deduction tally with the experimental laws, the theory will have fulfilled its aim: allowing the physicist to substitute the multiplicity of experimental laws with a few number of principles from which the laws can be reconstructed, i.e., to provide a compact representation of a vast set of experimental facts. Such a successful representation would offer a condensed symbolic representation of the laws, but also a classification of them:

Between a set of experimental laws taken as experimentation has brought them to light and the same set of laws connected by a theory, there is the same difference as that between a mass of documents

⁶ Abstraction principles were characterized as such by Bertrand Russell in his (1903) and were used by him and earlier on by Frege (1884, § 63-67) to define the concept of cardinal number. It is significant that the main idea was employed by Duhem.

heaped in confusion and the same documents carefully classified in a methodical collection. They are the same documents; they say exactly the same thing and in the same way. But in the first case, their disorder makes them useless, for one is never sure of recovering the document one needs at the moment one needs it; similarly, in the second case, the documents are made fruitful by a methodological grouping which places the desired document surely and without effort in the hands of the researcher (1893a, 36).

Indeed, *qua* representation, the goal of a theory is merely to recover the experimental laws, only *simplified* and *better ordered*. *Qua* representation, then, the physical theory is seen as an economy of thought.

Explanation

Famously, Duhem contrasts representation to explanation. Now, for Duhem explanation proceeds with positing unobservable entities and structures and consists in reducing the behaviour of observable entities (the empirical laws) to these invisible entities, their properties and their own laws of behaviour. Being not given in experience these entities (and the laws they are supposed to obey) are deemed, by default, metaphysical. Hence, explanation is taken to be characteristic of metaphysics, thereby falling outside the scope and bounds of science. As he put it, science “is not an explanation. It is a system of *mathematical propositions deduced from a small number of principles, which aim to represent as simply as completely and as exactly as possible a set of experimental laws*” (1906, 19, our emphasis). Given how Duhem defines an explanation as an attempt to “strip the reality of the appearances covering it like a veil, in order to see the bare reality itself” (1906, 7), the physicist aiming to explain “the appearances” has to accept that there is some distinct ‘reality’ behind them and that the task of science is to reveal it. Outside such a framework, as Duhem puts it, “the search for a physical explanation could not be conceived” (1906, 9). Thus, the success of an explanation can only be assessed on the prior adoption of a given metaphysics. To explain is therefore to step outside of physics, and to subordinate a physical theory to a metaphysics that alone can deliver the standards of evaluation of a successful explanation. For Duhem, not only does this way of proceeding misrepresent the aim and object of physical theories, but it also ruins the possibility for an autonomous physics.

Notably, he blames Descartes for having breached “the barrier between physics and metaphysics” (1893a, 44). The Cartesian project to reconstruct the whole edifice of knowledge on secure and indubitable principles implies that physics rests on hypotheses not obtained through scientific methods and as such not belonging to science, but to metaphysics. But his criticism of Descartes bears as much on his extreme hypothetico-deductivism as on his use of mechanical hypotheses. Instead of merely translating into mathematical symbols the physical concepts appearing in experimental laws, the Cartesian physicist adds constraints on the choice of properties and admits no simple property other than motion, size, shape. Duhem argues that the imposition of these conditions on the physical theory will result in an extraordinary complexity:

The (...) inconvenience of such a method is that in restricting the number of elements that may be used in constructing the representation of a group of laws, physicists are left with no other resource than to complicate the combinations they make with these elements in order to respond to all of the demands of experimentation (1892a, 13).

Disentangling the two Parts of the Theory

Explanations should be banned from ideal physical theories, according to Duhem. In fact, he found energetics, *qua* a rival of atomism, to be as close to this ideal as possible. Far from looking for the “revelation of the true nature of matter”, energetics was taken to operate by general principles (like the principle of conservation of energy) under which experimental laws are subsumed. These principles, as Duhem put it, were “pure postulates or arbitrary decrees of reason” (1913, 233).

But even a cursory look at the history of science suggests that many, if not most, of actual theories were far from meeting the above ideal standard. Actual theories were such that explanation and representation were intermingled. How can they be disentangled? And if they can be disentangled, what is the argument against theories that have an explanatory part?

Let us address these questions by noting that to motivate the distinction between two types (or two parts) of theory, Duhem (1906, 52) took a cue from Rankine's (1855) distinction between "abstractive" and "hypothetical theories". According to Rankine (1855, 209), the difference between the two kinds of theory stems from the first operation we described in section "Abstraction Principles", i.e., the definition of properties. An abstractive theory will consider as its fundamental properties only those that can be "perceived by the senses": properties are introduced in the physical theory only by means of an abstraction. A hypothetical theory, on the contrary, will accept properties "not apparent to the senses" through conjectures about the underlying nature of the perceived objects or phenomena. Thus, explanatory hypotheses will be introduced as soon as the relevant properties have been chosen, bearing consequences on all of the edifice afterwards. However, unlike Duhem, Rankine did not devalue hypothetical theories. He took them as an indispensable "preliminary step" for the reduction of "the expression of the phenomena to simplicity and order, before it is possible to make any progress in framing an abstractive theory" (1855, 213). For him, the contrast between the two types of theory was a contrast between two different modes of unification, of the "tendency (...) to combine all branches of physics into one system". One way is to rely on the axioms of mechanics as "the first principles of the laws of all phenomena—an object for the attainment of which an earnest wish was expressed by Newton", the other being to rely on "propositions comprehending as particular cases the laws of the particular classes of phenomena comprehended under the more extensive classes".

Hence, from Rankine's distinction it does not follow that an explanatory theory should be devalued. Is there any other reason that Duhem summons? For Duhem, explanatory hypotheses are actually "the germs that kill all mechanical theories", inasmuch as these hypotheses are not derived from any experimentation but from arbitrary *restrictions* added by the physicist. It might seem that the explanatory part of the theory is simply an extension of the representative part beyond the realm of the senses. Not so, for Duhem: the explanatory part is a *restriction* on theories. For him, as noted already, explanatory hypotheses consist of restrictions imposed on the construction of the theory. Drawing on his criticism of Descartes, he stresses that mechanical hypotheses add extra constraints on the choice of properties, since every physical property admitted within the theory must eventually be reduced to motion, size or shape. Hence, they limit the admissible properties to those which can have a mechanical grounding.

To see more clearly what Duhem has in mind, as well as to highlight the problem with his view, let us take a brief look at the difference between Newton and Descartes concerning the causes of gravity. Unlike Descartes and later Huygens, Newton avoided the mechanist demand to explain any phenomena "in terms of the arrangement and motion of minute, insensible particles of matter, each of which is characterized exclusively by certain fundamental and irreducible properties—shape, size, and impenetrability" (Nadler, 2000, 520), and did not try to account for the phenomena of attraction on the basis of mechanical principles:

I use the word "attraction" here in a general sense for any endeavor whatever of bodies to approach one other, whether that endeavor occurs as a result of the action of the bodies either drawn toward one another or acting on one another by means of spirits emitted or whether it arises from the action of ether or of air or of any medium whatsoever – whether corporeal or incorporeal – in any way impelling toward one another the bodies floating therein. (Newton, 1999, 548-549)

Newton deliberately avoided hypotheses about the reduction of the phenomena of attraction to mechanisms and relied only on these principles which allow for the phenomena of attraction to be treated as a mathematically expressed natural law. It is arguable that Newton did this not by a mere desire to stay neutral with respect to these hypotheses, but because he was actually trying to "identify and so to isolate, precisely those presuppositions that, apart from their bearing on metaphysical questions, are also necessary presuppositions of the physics that he and his contemporaries practiced" (DiSalle, 2013, 449-450), that is, the presuppositions of the study of any dynamical system. This quest soon made him realize that the "common methodological ground between himself and his philosophical opponents" was constituted of those mathematical principles that allow the mathematical treatment of empirical concepts, like that of attraction,

thus strengthening the distinction between mechanical and mathematical principles. Pushing this line further, if those mathematical principles were to be understood as the necessary and unavoidable presuppositions of any dynamical theory, it would also make sense to consider them as fundamental principles, which, as such, need not (and should not) be reduced to any more fundamental mechanism. This kind of stance, viz., taking the law of gravity as a fundamental principle, would have certainly made it easier to identify the forces holding planets in their orbits with the force attracting bodies towards the Earth. On the contrary, such an extension was not easily accessible to those who thought that gravity should be offered a mechanical explanation, simply because, as George Smith (2002, 141) has noted, “no hypothetical contact mechanism seems even imaginable to effect ‘attractive’ forces among particles of matter generally”. Far from extending the theory, the quest for a mechanical account of gravity would restrict it since it could not ground the universality of the law of gravity. Huygens (1690, 160) himself, after reading Newton’s *Principia*, admitted that his defense of the Cartesian theory of vortices had prevented him from extending the action of gravity to large distances:

I had already thought, a long time ago, that the spherical shape of the Sun could have been produced in a similar way to the one which, according to me, produced the spherical shape of the Earth; but I never thought of extending the action of gravity to such large distances, from the Sun to the Planets, or from the Earth to the Moon; as my thoughts were obstructed by the vortices of Mr. Des Cartes, which in the past seemed to me so plausible and which I still had in my mind (M. G. translation).

This kind of case might bring home Duhem’s point that explanatory hypotheses are not an *extension* of the theory to a realm inaccessible to sense, but chains by which a metaphysical system forced upon a theory, preventing its full growth. By the same token, however, this kind of case shows that representation and explanation need not be as far apart as Duhem thinks, if explanation is taken to be unification of diverse empirical laws under a theoretical scheme. Newton’s law of gravity, as Newton himself admitted, is explanatory of a vast array of phenomena, even if the cause of gravity is not explained; or even if the very demand for an explanation of the cause of gravity is deflated.

It is fair to say that Duhem never conceded that explanation should be the aim of science. No matter how intricate the relation between representation and explanation can be, as the Newton case shows, Duhem thought of them as corresponding to distinct and separable parts of the theory.⁵ In support of this view, he offers two arguments.

The *first* is an historical argument, expressed in the famous *continuity thesis* stated by Duhem in (1906, 32-33):

When the progress of experimental physics goes counter to a theory and compels it to be modified or transformed, the purely representative part enters nearly whole in the new theory, bringing to it

⁵ In the original French text of *The Aim and the Structure*, Duhem translated Newton’s famous words from the Optics, Query 31: “To tell us that every species of things is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing: but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a very great step in philosophy, though the causes of those principles were not yet discovered”. Duhem’s own translation into French is this: “*Expliquer* chaque propriété des choses en les douant d’une qualité spécifique occulte par laquelle seraient engendrés et produits les effets qui se manifestent à nous, c’est ne rien *expliquer* du tout. Mais tirer des phénomènes deux ou trois principes généraux de mouvement, *expliquer* ensuite toutes les propriétés et les actions des corps au moyen de ces principes clairs, c’est vraiment, en Philosophie, un grand progrès, lors même que les causes de ces principes ne seraient pas découvertes” (2007, 81—emphasis added). Note that Newton’s “to tell us” is translated (thrice) by Duhem into “expliquer” [to explain]. In the first two occurrences, ‘to explain’ refers to explanation by means of occult qualities, which Duhem says ‘explains nothing’. But in the third instance, ‘to explain’ refers to the explanation of all properties by clear principles. This double use of ‘explanation’ seems to suggest that Duhem may well allow an *explanation* that does not rely on any hidden causes, but aims at the kind of unification Newton offers by his laws. However, in the subsequent paragraph he seems to equate this sense of ‘explanation’ with geometric representation (*représentation géométrique*). Thanks to an anonymous reader for bringing this subtle point to our attention.

the inheritance of all the valuable possessions of the old theory, whereas the explanatory part falls out in order to give way to another explanation. Thus, by virtue of a continuous tradition, each theory passes on to the one that follows it a share of the natural classification it was able to construct, as in certain ancient games each runner handed on the lighted torch to the courier ahead of him, and this continuous tradition assures a perpetuity of life and progress for science. This continuity of tradition is not visible to the superficial observer due to the constant breaking-out of explanations which arise only to be quelled.

The metaphor of a relay in which the lighted torch is passed on from one runner to the next is very vivid. It highlights Duhem's belief in continuity; in the existence of a pattern of retention in the history of science. But the retention is limited to the representative parts of theories; the explanatory parts are supposed to have been abandoned and replaced by new explanations. For Duhem, the lighted torch of science is representation and not explanation.

His favorite example was the theory of light-refraction. In Descartes's own work, the representative part was entirely subsumed under one law: the Snell-Descartes law, which asserts the constancy of the ratio between the sine of the angle of incidence and the sine of the angle of refraction of a light ray. This law is still accepted and can nowadays be found in any optics textbook. However, this representation of the phenomena of refraction was accompanied by an explanation of this phenomenon, where light was analyzed as being caused by a "pressure engendered by the rapid motion of incandescent bodies within a "subtle" matter penetrating all bodies" (1906, 33). This explanation has a history of its own: it was replaced by the emissionist theory under Newton's influence, and was resuscitated by Young and Fresnel one century later. But according to Duhem it has never been genuinely related to the representative part of the theory of refraction. Duhem provides two considerations to support this idea of a mere juxtaposition of the two parts rather than a genuine relation between them: first, when trying to explain why light travels faster in denser than in rarer medium, Descartes appealed to a mechanical analogy with balls which is more suitable to the emissionist hypothesis than to the wave theory of light. Second, Descartes was convinced that the infinite speed of light was a necessary consequence of his explanation of light. As a result, Römer's experiment showing the finite speed of light led to the demise of Descartes's explanatory theory. Nonetheless, the Snell-Descartes law was never jeopardized by this experiment and has been retained through all the successors of the Cartesian theory of light.

An obvious worry with this kind of historical argument is that it might well be the case that the alleged distinction between the two parts of the theory is *ex post facto*: the representative parts are those that have been retained in theory change and the explanatory parts are those that have been abandoned. Is there an independent reason to draw this distinction for an arbitrary theory? Is there an argument why those *and only those* theoretical parts that have been abandoned are explanatory?

It is in order to address this kind of worry, that Duhem aims to make a case for the *predictive dispensability* of the explanatory parts. On his view, the only part of the theory that possesses empirical content that can lead to predictions is the representative part. This is supposed to be illustrated by the wave theory of light. Huygens, Duhem argues, despite being one of the most ardent defenders of the mechanist philosophy and being the one who actually unraveled the consequences of a wave theory, did not use mechanical hypotheses to extend Descartes's laws of refraction to the phenomena of double refraction. He simply extended the representative theory already available to a new range of phenomena. The only hypotheses on which his reasoning was grounded were "a comparison between the propagation of sound and the propagation of light, the experimental fact that one of the two refracted rays followed Descartes' law while the other did not obey it, a felicitous and bold hypothesis about the form of the surface of the optical wave in media of crystals" (1906, 35). Hence, on Duhem's view, the credit for Huygens's account of double refraction goes only to the representative part, whereas the wave hypothesis did not contribute at all to this predictive success.

Things, however, are more complicated. Duhem himself showed this when he described how Huygens used the Iceland Spar to study double refraction. Huygens observed, as Bartholin did before him, that two images of one and the same line could be produced by placing a Spar on a piece of paper. Moreover, one of these images would rotate if the Spar itself was being rotated. One of these images, that Huygens called "ordinary", obeys Descartes's law and stays fixed when the Spar is rotated. The rotating image, called "extraordinary", does not satisfy the law of refraction. Upon having explained the phenomena of reflection

and refraction based on a wave hypothesis and the propagation of light through spherical waves, Huygens postulated that the two images were the results of two different kinds of wave propagation, corresponding respectively to the light propagation in the aether contained in the Spar and to the light propagation in the particles constituting the Spar itself. While the former waves are spherical, the latter are ellipsoidal, thus explaining the phenomena of double refraction. Based on these explanatory hypotheses which yielded the ellipsoidal model, Huygens thus constructed a geometrical model that succeeded in representing the phenomena of double refraction and thus extended Descartes's theory of refraction.

The point here that Huygens did *use* the wave hypothesis to construct his theory of double refraction. It was the wave hypothesis that made possible the ellipsoidal model, since as he (1690b, 73) put it: "upon having explained the refraction of transparent ordinary bodies, by means of the spherical emanations of light, (...), I went back to examining the nature of this crystal, about which I could not discover anything before" (M. G. translation). An ellipsoidal propagation as the one described would not have made sense outside the wave theory. Hence, that we may, *ex post facto*, deem a hypothesis 'explanatory' does not imply that, in the reasoning that led to this model, it did not play a role.

Duhem, indeed, grants that Huygens's theory "*represents* at the same time the laws of simple refraction, the object of Descartes' works, and the laws of double refraction" (1906, 35, our emphasis). But what does 'represent' mean here? There are only two ways to determine whether something is a representation according to Duhem: a part of a theory is said to be representative a) if it follows the four-step method of construction of a theory and aim only at summarizing and classifying experimental facts; and b) if it is retained over time. We noted already that the second way needs independent support and that Duhem's argument we have been examining, viz., that the explanatory part does not contribute to the predictive power of the theory, was meant to offer this independent support. Huygens's case shows that Huygens's theory was not representative in the sense of being constructed in the way suggested by Duhem's four-step method. It was nonetheless involved in extending the law of light-refraction to cover the phenomena of double refraction.

To sum up. The continuity thesis (viz., the retention of representative parts in theory-change), if it's not merely an *ex post facto*, and hence *ad hoc*, way to identify the representative parts of theories, has to be supported by an independent criterion for taking a principle to be representative as opposed to explanatory. This criterion is meant to be offered by the predictive impotence argument, viz., that explanatory principles do not contribute to the predictive success of the theory. But this alleged impotence cannot be substantiated by the very cases that Duhem considers. It turns out that the supposed sharp distinction between two parts of the theory (the explanatory and the representative) is either *ad hoc* or unsupported by historical evidence. To say the least, there is no cogent argument to the effect that the explanatory part is attached to the representative part "like a parasite" to a "fully formed organism" (1906, 32).

Re-assessing Duhem's Holism

It might be thought that Duhem cannot have it both ways. He cannot be a holist and at the same time accept that only the representative part of the theory gets any credit from the empirical successes of the theory. Wouldn't commitment to holism imply that the explanatory part (assuming that we can draw such a distinction) also gets some credit by getting some of the empirical support of the theory? In this section, we shall argue that appearances to the contrary, Duhem was not committed to a radical version of holism and that he used his anti-atomism as a weapon against the empirical support of the explanatory hypotheses of the theory.⁷

Duhem wanted to make the strong point that it is a "chimera" to try to isolate a(n) explanatory hypothesis and subject it to empirical test on its own (cf. 1906, 200). We call this view anti-atomism, since the emphasis is meant to be on showing that isolated hypotheses—that is, hypotheses which are not part

⁷ At least part of Duhem's motivation for holism is his anti-inductivism. In his (1906, 190-194) he argues extensively against the claim that Newton's laws are inductive generalizations from experience. For Duhem laws are not justified "one by one", by observation and made general by induction. Rather, testing them "is a matter of comparing the corollaries of a whole group of hypotheses to a whole group of facts" (1906, 194).

of a theoretical system—do not have their own empirical content; hence, they cannot be tested atomistically, viz., independently of a theoretical system in which they feature.⁸

Suppose there is a dispute about a theoretical hypothesis H. Ideally, there must be some prediction “of an experimental fact” drawn from H. The experiment is then performed and if the “fact is not produced”, the hypothesis H “will be irrevocably condemned” (1894, 82). Duhem shows that this account of testing is an illusion, since no hypothesis taken in isolation from a theoretical system implies *any* predictions. As he put it: “The prediction of the phenomenon whose nonproduction will cut off the debate does not derive from the disputed proposition taken in isolation but from the disputed proposition joined to this whole group of theories”. Hence, when the prediction is not brought out “it is the whole theoretical scaffolding used by the physicists” which is “shown to be wanting” (1894, 82). But where does the error lie? The experiment cannot pinpoint the culprit among the parts of the whole theoretical system. Duhem’s conclusion is that how the blame is distributed among these parts is not a matter of “logical necessity” (1894, 83).

All this is rather well-known. What is not typically perceived is that Duhem uses this kind of argument to show that explanatory hypotheses do not have empirical content of their own. Immediately after the logical argument, he offers an illustration by means of Newton’s emission theory of light, which he took it to be a typical case of an explanatory theory. On this theory, light is formed of very small particles emitted with great velocities by light sources. These particles (the projectiles, as Duhem calls them) are subjected to distance-dependent attractive and repulsive forces and permeate all bodies. This set of “essential hypotheses”, linked (and only linked) with many others entail that light travels faster in water than in the air. This prediction, noted by François Arago, was tested by Léon Foucault in a famous experiment. Duhem was quick to point out that the negative result of the experiment (viz., that light travels faster in air than in water) does not tell where the error in Newton’s emission theory lies. Hence, not only has Newton’s explanatory hypothesis no empirical content in isolation of a theoretical system, but given a conflict between the theoretical system and experience, Newton’s hypothesis can be saved from refutation. Hence, because of anti-atomism, an isolated explanatory hypothesis is not genuinely testable.

What is more, Foucault’s experiment is far from crucial. It does not prove the opposite theoretical hypothesis, viz., that light consists of waves. On this hypothesis, defended by Huygens, Young and Fresnel, light consists of waves which are propagated through an elastic luminiferous medium. This alternative explanatory theory yields the prediction that light travels faster in air than in water. One might have expected that we are faced here with a crucial experiment. It might be thought that we have two competing hypotheses H and H’, H being that light consists of particles and H’ being that light consists of waves, and a decisive experiment among them since H entails e and H’ entails not-e. But this is the wrong way to think of the matter and anti-atomism brings out what’s wrong with it. It is not two competing explanatory hypotheses that are being tested but two “theoretical groups or systems, each taken as a whole”: Newton’s optics and Huygens’s optics (1894, 86). Given this, no experiment can decide between two explanatory hypotheses, viz., that light is a body and light is a vibration in a medium. These two explanatory hypotheses do not have their own empirical content.

Still, could it be that under *favourable* circumstances a theoretical group or system ends up being well supported by the evidence given that its rival is taken to be disconfirmed by the evidence? Holism is compatible with this scenario: one theoretical system might be more supported by the evidence than another. Though Duhem does allow that a theoretical system might be condemned by empirical evidence and abandoned, he was firm in claiming that a theoretical system T is supported by the evidence e *only if* we are certain that there are no other theoretical systems (hitherto unconceived) such that were they available, they would entail e. Hence, he couples his anti-atomism with a radical view of empirical support. Hence, assuming the empirical failure of the emission theory of light is not proof of the *truth* of the alternative wave theory of light because it does not have to be the case that light is either a body or that light is a wave. We will never be able “to affirm that no other hypothesis is imaginable” (1894, 87). And because of this we can “never be certain that we have exhausted all the imaginable hypotheses concerning a group of phenomena” (1894, 87).

This line of thought has become standard in arguments against a realist understanding of science: no theory T is confirmed by the evidence unless it is shown that no other theory exists or could be conceived

⁸ It should be noted that Duhem was an anti-atomist too in that he opposed the atomic theory of matter. The two senses of anti-atomism should not be confused.

such that it is empirically equivalent to T. But it should be noted that Duhem was rather careful. The point he wanted to make is that no theory can be proved to be *true*. He noted this explicitly in his (1908b, 110):

Grant that the hypotheses of Copernicus manage to save all the known phenomena; that these hypotheses may be true is a warranted conclusion, not that they are assuredly true. Justification of this last proposition would require that one prove that no other set of hypotheses could possibly be conjured up that would do as well at saving the phenomena. The latter proof has never been given.

But doesn't his anti-atomism extend to the representative part of the theory. The though here might well be that unless representative principles are sharply distinguished from explanatory ones by independent means, it seems that Duhem's anti-atomism does contribute to the undermining of the very distinction between two parts of the theory. Don't representative principles face the 'tribunal of experience' no less holistically than explanatory ones? Is there a way out for Duhem?

Duhem's way out was the restriction of his anti-atomism to the explanatory part of the theory. As we stressed, he used anti-atomism as an argument against the testability of explanatory hypotheses. But we saw already in section "Representation vs Explanation", that representative propositions were taken to be close to the experimental facts because they, in effect, replace these facts "with abstract and symbolic representations". This replacement is what Duhem calls "interpretation" (1894, 88). But interpretation does involve theories, since the very idea of abstract and symbolic representation implies the "transportation" of a fact into a theory. Though the experimental facts are interpreted by the theory that "physicists regard as established" (1894, 95), Duhem is adamant that the representative part of the theory is independent from the explanatory part, the reason being that it can be interpreted within alternative theoretical systems. Hence, an explanatory theory T may 'interpret' a certain set of empirical laws and facts according to its own conceptual resources, but *the very same laws* — being abstract and symbolic representations — can be "translated into the language" of an alternative theory T'. Once abstraction principles have allowed scientists to treat empirical properties as physical magnitudes, the symbols, *qua* symbols, admit of different interpretations. When transported into an explanatory framework, the symbols and the relations they stand to each other, are interpreted in light of the relevant theory. But precisely because they can be interpreted by different theories, theories can share representative parts (1894, 96). Hence it is possible to make "elements of the new theory correspond to elements of the old theory at certain points": a representative proposition, though interpreted within a physical theory, is interpretable within alternative physical theories too. All this requires that, though interpretable by a given theory T, the representative part has empirical content of its own, independently of T. Hence, it acquires its content, as it were, atomistically. It is this atomistic empirical significance of the representative propositions that makes them interpretable in alternative theories. It is this that makes possible the establishment of a "correspondence" between the symbols that represent the results of experiments in theory T and the symbols that represent the same results in theory T' (cf. 1894, 96).

It seems plausible that Duhem drew the distinction between the two parts of the theory in terms of their distinct modes of testability: the explanatory hypotheses are tested non-atomistically, whereas the representative propositions are tested atomistically. But there is a drawback. This way to draw the distinction between the two parts of the theory very much depends on whether or not an explanatory hypothesis contributes to the empirical content of the theory. If it does, then it should certainly get some credit from the predictive and empirical success of the theory, even if Duhem is right in claiming that it is tested as part of a whole (that is, anti-atomistically). When we discussed Huygens's account of double refraction in the previous section, we noted that explanatory hypotheses contributed to predicting the distinct geometrical forms of the two ways, viz., the spherical and the ellipsoidal. That Huygens's wave hypothesis gets no credit from this requires the aforementioned controversial assumption that empirical support accrues to the explanatory part of the theory *only if* it is shown that there can be no other explanation of the same phenomena available. And though Duhem rightly noted that unless such an assumption is granted, no theory can be *proved* to be true, it's important to distinguish between proving the truth of a theory and allowing the evidence to confirm it. The problem with Duhem's radical view of empirical support is that it makes confirmation simply impossible: no evidence can bear on a theory.

Duhem used the vivid metaphor of an organism to talk about physics. Physics, he says, is not "a machine that let's itself be taken apart"; it is "an organism that must be taken as a whole" (1894, 85). This

might be taken to imply that explanatory hypotheses, being part of the body of physics, can be subjected to test by subjecting to the test the whole body of physics. We have argued that for Duhem the organism metaphor, his holism, is meant precisely to show that what “is commonly thought”, viz. that each of the explanatory hypotheses “may be taken in isolation, submitted to the control of experiment, and then, when varied and numerous proofs have established its validity, put in place in an almost definitive manner in the totality of science”, is wrong: explanatory hypotheses lose their significance and “no longer represent anything” (1894, 88) if they are cut off from a system. Still, it does not follow that theoretical systems within which explanatory hypotheses are embedded can never be supported by empirical evidence.⁹

Natural Classification

Duhem had always been adamant that the aim of physical theory is classification. As he explains in the Introduction to his essay *The Electric Theories of J. Clerk Maxwell: A Historical and Critical Study*, for him theoretical physics “is only a schematic representation of reality. Using mathematical symbols, it classifies and directs the laws that experience has revealed; it condenses these laws into a small number of hypotheses; but the knowledge it gives us from the outside world is neither more penetrating nor of a different nature than the knowledge provided by experience” (1902a, 8).

But classification is always relative to a scheme of classification and there can be different and competing schemes. These schemes are “the free decree of our understanding” (1906, 286) and the only constraint in using these schemes is that they should not be mixed up. Using an example from biology, Duhem notes that a naturalist can classify some animals according to the structure of their nervous systems and some other group of animals according to the circulatory system. Similarly, the physicist can use the hypothesis that matter is continuous to classify some laws and the hypothesis that matter is atomic in another classification (1893b, 66). This no-mixing up condition, Duhem attributes to Poincaré. In fact, he finds it in Poincaré's *Électricité et Optique*, where Poincaré notes that “Two conflicting theories can, indeed—provided they do not mix and that are not seeking the bottom of things—be both useful instruments of research, and perhaps reading Maxwell would be less suggestive if it had not opened both new and divergent pathways” (1890, v; cited by Duhem 1902a, 8). Duhem himself is happy with this condition, since when it comes to the logical examination of theories, the only constraint he has put forward is logical consistency. Otherwise, a theorist is free to represent “different sets of laws, or even a single group of laws, by several irreconcilable theories” (1893b, 66). Logic imposes only one “obligation on physicists, and that is not to mix theory different procedures of classification” (1893b, 66.). To put the point bluntly, a theorist can use scheme A on Mondays, Wednesdays and Fridays and scheme B of Tuesdays, Thursdays and Saturdays. This way she avoids incoherence by avoiding to combine “a major premise” obtained by theory A with a “minor premise” obtained by theory B (1906, 294). The key rationale for this attitude is that the genuine content of a physical theory is taken to be the set of the empirical laws classified. As he says: “The systematic classification that theory gives [the empirical laws] does not add or take away anything concerning theory truth, their certainty or their objective scope” (1906, 285).

But this is half of the story. Duhem attributed to Poincaré and Édouard Le Roy the view that the no-mixing up principle is the only condition on scientific rationality and let himself occupy a different, more nuanced, position. He takes it that theoretical physics “deserves the name of science on the condition of being *rational*” (1902a, 8). But being rational, that is responding to reasons, is not confined to following strictly and exclusively the principle of non-contradiction and the rules of logic. A scientist may be free to choose any hypothesis she pleases “provided that these hypotheses are not redundant or contradictory”,

⁹ It is noteworthy that if Duhem was an anti-atomist, he was an anti-conventionalist too. He resisted the thought that just because hypotheses are not atomistically tested, they can be held on come what may. Taking distances from Poincaré and Le Roy, he argued against the view that some principles are elevated to conventions thereby acquiring a status of being “universally adopted” (1906, 212). For him, no principle (or hypotheses) is immune to revision; hence no principle can be held on come what may. As he put it: “The history of science shows us that very often the human mind has been led to overthrow such principles completely, though they have been regarded by common consent for centuries as inviolable axioms” (1906, 212). Duhem's opposition to conventionalism has been thoroughly discussed by Maiocchi (1990).

but a unified theory is preferable to “a junk heap of irreconcilable theories” (1906, 295). This “single physical theory which, from the smallest possible number of compatible hypotheses between them, would derive, by impeccable reasoning, all known experimental laws is obviously an ideal perfection which the human mind will never reach; but if it cannot reach this limit, it must constantly be directed” (1902a, 8). In fact, Duhem notes that representing by theories “unconnected with each other, or even by theories that contradict each other when they meet in a common domain” is a “transitory evil”. Unity should be what physics should aim for.

It might seem that in Duhem's writings, two tendencies are always fighting against each other: on one hand, his own project of proceeding to the strict logical examination of physical theories—“we shall in this book offer a simple logical analysis of the method by which physical science makes progress” (1906, 3); and, on the other hand, his desire to support some theories over others. In the first case, Duhem insists on the value of the physical theory as an economy of thought, as condensing a multitude of facts and laws. In the second case, the emphasis is put on the theory as a *natural* classification, reflecting the true order of the world.

We shall argue that Duhem went beyond the positivist trend in French epistemology of science by bringing into it the thought that some kind of contact between theory and an underlying reality is necessary for taking a theoretical classification to be “a satisfactory representation” of experimental laws (1906, 298). His chief point, addressed to his fellow epistemologists, is that the ideal form of a scientific theory is achieved when a theory is a “natural classification” of experimental laws and that looking for this ideal form, even though it is a limiting condition, is reasonable and warranted (by the history of science). Its justification, however, exceeds the confines of the positivist method they (and he himself) were prone to follow. All this was thrown into sharp relief after Abel Rey published a sympathetic but critical essay of Duhem's views in 1904 and Duhem replied in 1905. But it was already there in Duhem's very early writings on the epistemology of science. Let us go into this matter in some more detail.

Already in 1893, Duhem assumed the idea of “the best classification” of experimental laws as this classification which would follow from a “detailed metaphysical knowledge of the essence of material things” (1893a, 37). This is because this kind of classification would map the order (viz., relations) there is (are) among things in the world, where this order would “result from their nature itself”. He's careful to add that even if this knowledge were available, the physicist would still have the right to adopt another theory, “to connect physical laws in a different order, to accept another mode of representation of physical phenomena” (1893a, 37). But he adds that this attitude, though fully consistent with logic, would be “unreasonable” since “in every order of things we should choose what excels”. The very assumption he started with is “purely ideal”. But its conceivability is used by Duhem to show that the sceptics—those who deny the very “principles on which experimental science logically depends” (1893a, 38)—can be blocked only if we go beyond the method of physics and look for its justification. He meaningfully dissociated himself from positivism insofar as the latter asserts that “there is no logical method other than the method of positive sciences”. Hence, there is more to justification than the method of science (understood as being constrained only by the principle of non-contradiction).

In another piece he published in 1893, he introduced the idea of ‘natural classification’ in connection with a perfect theory. Here he noted that “considerations of pure logic are not the only ones that reasonably direct out judgements (1893b, 67). Take the following rule:

In physical theory, we must avoid logical incoherence BECAUSE IT INJURES THE PERFECTION OF SCIENCE (1893b, 67).

This is not a principle of logic. Yet Duhem thinks it's reasonable (legitimate). Perfection is a matter of degree, but ideally a perfect theory (or the “true theory”, as he put it) would be “the complete and adequate metaphysical explanation of material things” (1893b, 68). The perfect theory would classify experimental laws in a natural way:

an order which would be the very expression of the metaphysical relations that the essences that cause the laws have among themselves. [A perfect theory] would give us, in the true sense of the world, a natural classification of laws (ibid.)

Although Duhem talks of metaphysical relations between essences, what he really refers to are relations among unobservable entities—the minute constituents of material objects. Recall that for him the atomic hypothesis (as well as any other hypothesis which refers to unobservable entities) was a “metaphysical” hypothesis. A perfect theory would pertain to the true theory, and a “natural classification” is the one issued by a *true* theory. Avoiding contradictions and unifying the empirical laws into a single system of hypotheses is for Duhem the road to perfection. In effect, Duhem argues that if science aims at a natural classification, then unification is the most natural thing to look for. A natural classification cannot possibly be “an incoherent collection of incompatible theories” (cf. 1893b, 67)—even though each and every theory may save some phenomena. Unification is then seen as a way to remove inconsistencies and to approach what Duhem calls the “perfect theory”.

Note that a natural classification is still a *classification* and not an explanation. What makes it natural is that the scheme for the classification, far from being arbitrary, is the one that nature itself uses, so to speak, to classify “the relations” that the causes of the empirical laws have among themselves. This, again, is an ideal form. Achieving it “infinitely surpasses the scope of human mind”. But this does not mean that it does not exist; that is, there are true relations among the “essences” whose manifestation are the relations among the phenomena (the empirical laws). Given this ideal theory, it makes sense to aim to remove the contradictions among existing physical theories since the relations that there are among the causes of the phenomena are “neither indeterminate nor contradictory” (1893b, 68).

This is the justification for the *Unification Principle (UP)*:

Physical theory has to try to represent the whole group of natural laws by a single system all of whose parts are logically compatible with one another (1906, 293).

This principle was for him perfectly reasonable, though it could be denied without contradiction. So the key point we want to make is that the very idea of natural classification is part and parcel of a broader conception of reasonableness that Duhem endorsed in order to distinguish his view from the positivist ones in vogue in France in his time.

As noted already, this reaction to positivism was thrown into sharp relief in his exchange with Rey. He actually compared Duhem to most of his contemporaries (notably, Rankine, Helmholtz, Dubois-Reymond, Ostwald, Poincaré and Milhaud). They take it to be the case that science explains nothing and that looking for causes is a venture into metaphysics. For them, Rey (1904, 703, M. G. translation) says, “Sciences merely record relations among phenomena, connections that are convenient to achieve an exact description of these relations, a description that allows to some extent to predict”. For Rey, Duhem stresses the indispensability of theoretical physics and claims that “purely experimental physics is a chimera” (1904, 704). Still, theories are arbitrary; they are “formal”; “They are a set of relations between numerical values, between quantities; They do not at all worry about the real content which enters relations, the objective properties evaluated by these quantities” (1904, 718). The comparison with reality is done at the end, when the theory is tested empirically. “But at the end, this game gains meaning thanks to a set of measures, that allow to detect reality; our formula must then give us results that coincide as fully as possible with this real detection” (1904, 722).

The issue Rey concentrated on was the value and objectivity of theory, if all there is to it is a scheme of classification. He credited Duhem with showing that empiricism (which was based on the claim that science is “a simple summary of experimental observations”) is a fiction. But he took him too to distance himself from the claim that science is an arbitrary conventional classification. The theory “has a relation that is certain with reality, i.e., with the experimental records – the fact that the experiment must eventually intervene to confirm it or refute it proves it. What is arbitrary is everything that at first sight will allow us to make the order of our thoughts correspond to the real order. What is not arbitrary anymore is the correspondence itself” (1904, 728). In fact, according to Rey, Duhem argued against the “neo-sceptics” that it’s not the case that every theoretical path possibly taken is fruitful. Instead, “there will be a theoretical development, which, better than any other, will correspond to the order of the phenomena which we wish to describe. There will thus be a set of theories which will impose themselves at least in general lines, to the exclusion of any other. It will constitute theoretical physics; This will be determined and one, not arbitrary and multiple” (1907, 133).

In the end, Rey called Duhem's view the "physics of a believer" (1904, 744). This was a charge that Duhem tried to dispel in his reply. But the key feature of his reply was his insistence on the claim that natural classification is the aim of science. He noted that the natural classification of experimental laws is a "limiting form" that the theory tends to achieve, through "its successive advancements" (1906, 297). He insisted that if a scientist is not an "intransigent positivist", he or she will come to see that "physical theory advances gradually toward its limiting form" (ibid.). Now, one point that Rey insisted on was that Duhem's resistance to neo-scepticism will come to nothing if the history of science showed that attempts to unify the theoretical image of the world had been a failure. Duhem (1906, 295) responded to this criticism by acknowledging it and by saying that, ultimately, the issue at stake is empirical:

it is up to the history of science [...] to tell us whether men, ever since physics took on a scientific form, have exhausted themselves in vain efforts to unite into a coordinated system the innumerable laws discovered by experimenters; or else, on the other hand, whether these efforts through slow and continuous progress have contributed to fusing together pieces of theory, which were isolated at first, in order to produce an increasingly unified and ampler theory.

Duhem's verdict was that the history of science has tilted the balance towards unity: "diversity fusing into a constantly more comprehensive and more perfect unity, that is the great fact summarizing the whole history of physical doctrines" (1906, 296).

What's also important to stress is that there is a contingent mark for a classification being natural, viz., the ability of theory to yield novel predictions; that is, the ability of theories to anticipate experiment, establishing novel predictions like prophets would reveal the future:

The highest test, therefore, of our holding a classification as a natural one is to ask it to indicate in advance things which the future alone will reveal. And when the experiment is made and confirms the predictions obtained from our theory, we feel strengthened in our conviction that the relations established by our reason among abstract notions truly correspond to relations among things (1906, 28).

Still, the very idea that a theory is (or tends to be) a natural classification cannot be justified by the narrow positivist method that Duhem himself canvassed. All the more so for his anti-scepticism. Far from yielding to scepticism Duhem relied on a broader conception of justification which, we might say, relies on explanatory considerations: it is truth that explains perfection and it is perfection that explains why unification should be aimed at, or at least why it is reasonable to strive for it. The fact that *natural* classification will always remain an unjustifiable claim on the positivist method does not make it unjustifiable:

Physical theory confers on us a certain knowledge of the external world which is irreducible to merely empirical knowledge; this knowledge comes neither from experiment nor from the mathematical procedures employed by the theory, so that the merely logical dissection of theory cannot discover the fissure through which this knowledge is introduced into the structure of physics; through an avenue whose reality the physicist cannot deny, any more that he can describe its course, this knowledge derives from a truth other than the truth apt to be possessed by our instruments; the order in which theory arranges the results of observation does not find its adequate and complete justification in its practical or aesthetic characteristics; we surmise, in addition, that it is or tends to be a natural classification; through an analogy whose nature escapes the confines of physics but whose existence is imposed as certain on the mind of the physicist, we surmise that it corresponds to a certain supremely eminent order (1906, 334-335).

Are Relations the Epistemic Limit?

A natural question at this point is this: why does Duhem insist that knowledge can extend only up to relations among “hidden realities whose essence cannot be grasped” (1906, 297). The answer to this lies, by and large, with Duhem’s account of representation and the role of hypotheses in science.

At one point Duhem notes that “an intelligence that see essences” would classify the laws according to the “natural order” (1893b, 68). But we humans do not see “essences”. And that’s the problem for Duhem. If we (have to) rely only on representations of “essences”, then we can never have knowledge of them. Despite his criticism of empiricism, Duhem was wedded to the view that knowledge of entities requires that they are given to us in experience. Not so for knowledge of relations, though. Let us see why.

We noted already that Duhem’s talk of essences is meant to capture the unobservable causes or constituents of the phenomena. In his (1902b, 117), he put the point thus: “Contemporary physics is not metaphysics. It does not propose to penetrate behind our perceptions and come to know the essence and intimate nature of the objects of these perceptions”. The “essence and intimate nature” of perceived object were the particles posited by theories as their micro-structure, e.g., “viewing the rapid movement of particles as constituting the essence of heat (1902b, 39). The only possible access to them is via hypotheses, but hypotheses are beyond the limits of experience. Hypotheses might well be indispensable in doing science but they are never a means to empirical knowledge. As he put it in an early piece: “let us never trust hypotheses for an instant, and in particular let us never attribute a body and a reality to the abstractions that the weakness of our nature imposes on us” (1892b, 177).

How then can *relations* be knowable? For a start relations are captured by mathematical equations, which are constructed in such a way as to represent the formal properties of the empirical entities under investigation. For another, because of this formal character, resemblance is not required. It is worth repeating that for Duhem, theoretical physics starts with empirical objects and aims to “represent” their properties. But “in order to represent these properties, theoretical physics defines certain algebraic and geometric magnitudes and then establishes relations between these magnitudes which symbolize physical laws to which the system is subjected” (1892c, 39). These relations are among magnitudes which bear no resemblance to the actual physical properties; they are symbolic and abstract representations of these properties and as Duhem notes, stand in “no relation to [the] nature” of the properties they represent: “But we can put this non-quantitative property into correspondence with an algebraic magnitude which, without standing in any relation to its nature, will be a representation of it” (1892c, 47).

Representation, then, cannot cut through relations. But of this representation something more can be said, if it meets the requirements noted above (viz., unity, simplicity and novel predictions): that it is (tends to be) natural. That the mathematical relations among the physical magnitudes express real relations among “hidden realities”, of which “the essence”—what they are intrinsically so to speak—cannot be known. This kind of ‘relationist’ approach to knowledge is not far from the one that Poincaré developed at roughly the same time in an attempt to defend the objectivity and value of science. In fact, Duhem himself spoke approvingly of Poincaré’s attitude when he wrote:

The logical analysis that he had made with a pitiless rigor ineluctably led M. Henri Poincaré to the following fully pragmatic conclusion: theoretical physics is a mere collection of recipes. Against this proposition, he felt a sort of revolt, and he loudly proclaimed that a physical theory gives us something else than the mere knowledge of the facts, that it makes us discover the real relations among things (2007, 446, M. G. translation).

One of us has discussed Poincaré’s relationism in detail elsewhere (Psillos, 2014). The relevant point here is that for Duhem too relationism is the limit of objective knowledge and at the same time his resting point against scepticism. As noted already, a key argument for this relationist approach to theoretical knowledge comes from the pattern of retention in theory-change in science. But it should be added that in making a case for theoretical knowledge of relations Duhem had to rely on explanatory considerations of the very sort that he thought were illegitimate as part of science.

Far from being an instrumentalist, Duhem took it as fully legitimate for a scientist to accept that science does offer some substantial theoretical knowledge of the world. A scientist who would stick to a strict positivist account of rational judgement in science would

at once recognize that all his most powerful and deepest aspirations have been disappointed by the despairing results of his analysis. [For he] cannot make up his mind to see in physical theory merely a set of practical procedures and a rack filled with tools.... [H]e cannot believe that it merely classifies information accumulated by empirical science without transforming in any way the nature of these facts or without impressing on them a character which experiment alone would not have engraved on it. If there were in physical theory only what his own criticism made him discover in it, he would stop devoting his time and efforts to a work of such a meagre importance (1906, 334).

And he immediately added: "The study of the method of physics is powerless to disclose to the physicist the reason leading him to construct a physical theory".

Conclusions

Duhem's philosophy of science could be described as (increasingly) anti-instrumentalist. Yet, his anti-instrumentalism did not amount to endorsement of realism. If we were to identify the realist view of science with the atomic theory of matter and with the mechanistic view of the world, then Duhem was clearly not a realist, since he resisted both of them till the very end. But, to his credit, Duhem distinguished emphatically between two questions: "Does physical theory have the value of knowledge or not?" and "Should physical theory be mechanistic or not?" (1906, 320). He answered negatively the second but positively the first. And it is the first question that is deeply philosophical. Duhem's positive answer was meant to distinguish his views from what he took them to be purely positivist accounts of scientific method and of the rationality of science. The proper appraisal of the epistemic credentials of scientific theories requires adopting substantive principles such as the Principle of Simplicity, the Unification Principle and the Principle of Novel Predictions, which, though not forced on scientists by the scientific method, strictly understood, are reasonable and are required for taking science to offer some knowledge of the world. But this knowledge has a limit: it can only reach up to the relations there are behind the 'hidden essences' of the observable entities and the laws they obey. This limit (which captures what we have called 'epistemic humility') is licensed by the pattern of retention in theory change, as exemplified in the history of science.

Duhem's view, then, is not anti-realist either. He readily admitted that there is a natural order in the world which can be fathomed by theories which possess the marks of natural classification, viz., simplicity, unity and novelty. It's just that justifiably endorsing this kind of anti-sceptical view requires a broader conception of justification, which takes it to be the case that there is more to rational judgement than experiment and logic.

Accordingly, Duhem occupied a rather unique philosophical position which can be characterized by a combination of anti-scepticism about scientific knowledge with epistemic humility concerning its extent.

Acknowledgement

We would like to thank Robert DiSalle for many useful comments and discussions. Thanks are also due to two anonymous readers of this journal for encouragement and useful comments.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 73-84

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Duhem: Images of Science, Historical Continuity, and the First Crisis in Physics¹

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Abstract:

Duhem used historical arguments to draw philosophical conclusions about the aim and structure of physical theory. He argued against explanatory theories and in favor of theories that provide natural classifications of the phenomena. This paper presents those arguments and, with the benefit of hindsight, uses them as a test case for the prevalent contemporary use of historical arguments to draw philosophical conclusions about science. It argues that Duhem provides us with an illuminating example of philosophy of science developing as a contingent, though natural, response to problems arising in a particular scientific context and under a particular understanding of the history of science in that context. It concludes that the history of science provides little support for interesting theses about the present or future state of science.

73

Keywords:

Pierre Duhem; historical induction; scientific realism; antirealism; natural classification; energetics

Received: 30 March 2017. Reviewed: 13 May 2017. Accepted: 30 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.07>

Introduction

Debates between scientific realists and antirealists have persisted in the philosophy of science literature of the past half century following the demise of logical positivism. Realists view scientific theories as largely faithful representations of reality; antirealists do not. On the realist image of science, scientific theories reveal the reality that underlies and causally explains the phenomena. On the antirealist image, in contrast, scientific theories are representational fictions constructed by human beings to solve problems that seem pressing at a particular time, to save the phenomena, etc.³ A good deal of the dialectic between the two

¹ I thank two anonymous referees whose comments significantly improved this paper. I also thank both the National Science Foundation for a grant (NSF SES-0726051) and the University of Wisconsin-Milwaukee for a sabbatical award that supported this and other research on Duhem.

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³ Thus drawn, the distinction between realist and antirealist images of science is broad-brush and generic. The variety of specific proposals is large: for example, there is realism/antirealism about the existence of unobservables and about the truth of fundamental laws; there is selective realism about the causes of phenomena and about structures; there are disputes about the intelligibility and/or point of the debates. Here I focus only on the debates insofar as they rely

relies on meta-level inductions on the history of science. Realists apply an optimistic induction on the history of science. They see it as the continuous, progressive evolution, guided by reality, of increasingly correct theories and increasingly reliable methods that put us in closer contact with reality: the history of modern science, they argue, is a history of increasing instrumental success; such a history would be a miracle if scientific theories were not successively tracking reality more closely with the passage of time – if they weren't progressively converging on the truth (Putnam 1975, 1978). Antirealists press a pessimistic induction. They see a graveyard of past theories that enjoyed significant empirical success but turned out to be false and were discarded – phlogiston theory, caloric theory, and Fresnel's aether theory of optics, for example (Kuhn 1970; Laudan 1981). Though few are nowadays persuaded either by the optimistic path to realism or by the pessimistic path to antirealism, qualified versions of these debates continue today with little resolution.⁴

These contemporary philosophical debates bear striking similarities to debates about physics that took place in the late 19th century. The contemporary philosophical literature makes passing references to the 19th century debates. For example, Poincaré is correctly considered to be a precursor of contemporary structuralism (Worrall 1989); far more controversially Duhem is variously considered to be a "paradigm antirealist" (Van Fraassen 1980), an antirealist "who rejects theoretical laws" (Cartwright 1983), a convergent realist (Lugg 1990), and a structural realist (Psillos 1999). However, this engagement is unsystematic and consists mainly in selectively drawing examples that support one's position or in citing some 19th century figure as authority for that position. This is unfortunate, because the 19th century debates provide an ideal study of the use of historical arguments to support realist and antirealist views about science. The principals of these debates – Duhem, Helmholtz, Hertz, Kelvin, Mach, Maxwell, and Poincaré – were primarily reflective physicists (philosopher-physicists, as I think of them).⁵ Their primary motivation traces to their concerns as historically informed, working physicists attempting to make sense of their enterprise based on their reflections on the history of science and the state of extant theories. They wondered, as our contemporaries do, about the relationship between physics and metaphysics, the aims and methods of science, the content of physical theories, and the extent to which the progress of science, understood as a series of attempts to fathom the depth and/or extent of the universe, is a "bankrupt" history. Most importantly in the context of using them as a test case, some of them made pronouncements about the future state and proper aim of science based on their historical extrapolations, pronouncements that we can now assess with the benefit of 100 years of hindsight. In this paper I will focus on some of Duhem's historical arguments as such a test case. Duhem was *the* historical expert among this historically informed group – not only did he write a number of histories devoted to special branches of physics; he is widely acknowledged to be the founder of the history of medieval science.⁶ As such, one would expect him to get the projectable historical patterns right if anyone could. In this, I will argue, he was only partly successful. His story provides us with an illuminating example of philosophy of science developing as a contingent, though natural, response to problems arising in a particular scientific context and under a particular understanding of the history of science in that context. I will conclude that the history of science provides little support for interesting theses about the proper aim or future state of science.

on historical arguments. For a comprehensive account of the twists and turns the debates (including their historical dimensions) have taken from the late 19th century to the present, see (Liston 2016).

⁴ Thus, for example, it is now commonplace to argue that we should be (selectively) realist about those parts (and only those parts) of scientific theories that explain their instrumental success and are preserved in successor theories, where those parts are variously understood to be structures (Worrall 1989), working posits (Kitcher 1993), core causal descriptions (Psillos 1999), detection property clusters (Chakravartty 2007). (Stanford 2006) challenges all such selective realist strategies.

⁵ Their separation into realists and antirealists is complicated, but Helmholtz, Hertz, Kelvin, and Maxwell had realist sympathies and Duhem, Mach, and Poincaré had antirealist doubts. Though Duhem, Kelvin, Mach, and Poincaré were active into the 20th century, they are 19th century thinkers.

⁶ Mach, of course, also authored important critical histories of mechanics and of theories of heat. However, while Mach's histories were selective, pursued primarily as an illustrative guide to the present, and largely relied on secondary sources, Duhem's histories, especially after 1903, increasingly became excavations of primary sources, particularly texts of the Middle Ages. This is convincingly argued in (Martin 1991).

19th Century Images of Science, Duhem's Historical Arguments

Duhem begins his major philosophical work (Duhem 1991 [1906], 7) with two images of physical theory, characterized in terms of aims:⁷

R: A physical theory ... has for its object the explanation of a group of laws experimentally established [the stripping of reality of the appearances covering it]

AR: A physical theory ... is an abstract system whose aim is to summarize and classify logically a group of experimental laws without claiming to explain these laws.

He argues that **R** makes physics subordinate to metaphysical systems since, at any given time, it requires that our physical theories be guided by a metaphysical picture that shapes the explanatory fit. But this is bad, he thinks, for two reasons. First, the history of modern metaphysics is one of irresolvable disputes (action-at-a-distance versus contact action, atoms versus continuous matter-stuff, etc.) with the disputants accusing each other of positing absurd or occult causes. Second, a metaphysical system at best provides directions for constructing models showing that physical theories are consistent with it, but no metaphysical system suffices to derive a physical system from it. The Cartesians and Leibnizeans, for example, argued about what quantity of motion was conserved in the universe as a result of God's immutability – momentum (mv) or vis viva (mv^2) – each was consistent with their fundamental metaphysical systems, but only experiment determined (or could determine) that Leibniz was right. Given the sorry track record of metaphysics guiding physics, Duhem concludes that **R** hinders the progress of physics: physics must be autonomous; i.e., not be hostage to substantive metaphysical or cosmological hypotheses.

However, Duhem does not quite conclude, as Mach does, that **AR** is correct, that a physical theory is merely an economical instrument for organizing the phenomena (Mach 1960 [1893], 577-595). While physics must not be subordinate to substantive metaphysics, it is ultimately grounded in a general metaphysical conviction: that nature is orderly: "the belief in an order transcending physics is the sole justification of physical theory" (Duhem 1991 [1908], 335) While we can't prove the existence of an ontological order, the metaphysical truth that nature is orderly is presupposed and displayed in our scientific activities and expectations (our urge to generalize and unify expecting success); we have an instinctive belief in this metaphysical truth that cannot be shaken by philosophical doubt. Accordingly, Duhem introduces a third image (Duhem 1991 [1906], 30).

NC: A physical theory is a natural classification.

For Duhem a natural classification is a mathematical physical hierarchical organization of the phenomena which, as it becomes more complete, is the reflection of an ontological order. Newton's great achievement in *Principia*, he argues, is a natural classification that unites heavenly and terrestrial motions so that Neptune's existence was predicted and subsequently discovered. As a theory makes novel predictions, we can't but feel it is providing a natural classification – no merely contrived artifice should be expected to "be a prophet for us". By making physics depend on metaphysical speculations **R** puts the cart before the horse; instead we need to use the natural classifications of the phenomena provided by physics to tentatively guide metaphysical investigation, since they allow us to see dimly the cosmological/metaphysical realities behind them.

Duhem's sympathy for **NC/AR** and antipathy to **R** is motivated by an induction from the past and current state of physics to the conclusion that proponents of causal explanatory theories guided by **R** allow metaphysical preconceptions to influence their physical theorizing and end up with theories that fail to satisfy the requirements expected of an explanation – and thereby fail to achieve their defining explanatory goal. By contrast, the proponents of abstract representative theories guided by **NC** have made slow but steady progress in fulfilling their aims.

⁷ Compare with the contemporary pithy "aims" characterization in (van Fraassen 1980, 6-9): science aims to give us a literally true story (realism) or an empirically adequate story (antirealism).

Most of (Duhem 1991 [1906], Part I) is an historical argument designed to show that the abstract representative approach guided by **NC** is the progressive path to a perfected physics that reflects an ontological order in contrast with the problem-strewn random path of the causal-explanatory approach.⁸ Versions of this historical argument can be found throughout Duhem's writings. His *Theories of Heat* (1895) is a brief for thermodynamics contrasted with kinetic theories; his *Mixture and Chemical Combination* (1902) advocates physical chemistry based on thermodynamic and thermochemical foundations contrasted with chemistry based on atomism; his *The Evolution of Mechanics* (1903) is similarly a brief for energetics as opposed to mechanics – though the settings are different, the central theme and argumentative structure are the same. Key themes are: first, causal-explanatory hypotheses (atoms, ether, etc.) wax and wane, while the history of abstract theories displays steady cumulative growth with later theories preserving earlier theories as special cases; second, unification is achieved by the mathematical organization of experimental data rather than by the search for deep explanatory mechanisms; third, divergences of opinion about mechanisms are irresolvable, while disagreements about abstract theories are eventually settled.

The First Crisis in Physics: Synthetic Physics of Mechanism or Analytic Physics of Principles

But the primary motivation for this comparative exercise lies less in Duhem's historical analysis than in his reflections on the state of science and the extant theories of his day, including the theories he was working on as a practicing physicist, and they provide the best insight to his view. By the 1880-s it had become apparent to working physicists that classical mechanics lacked both the conceptual and mathematical tools to properly describe a host of phenomena. This sense of dissatisfaction with classical mechanics is elegantly expressed in the writings of both Poincaré and Duhem. Poincaré describes the grand, majestic conception of a physics inspired by Newton's and Laplace's treatment of the heavens, a physics of central forces acting between material points attracting or repelling each other with inverse forces, a physics that attempted "to penetrate into the detail of the structure of the universe, to isolate the pieces of this vast mechanism, [and] to analyze one by one the forces which put them in motion".⁹ And he continues, "Nevertheless, a day arrived when the conception of central forces no longer appeared sufficient" and calls this the "first crisis" of physics (Poincaré 1913, 299). The old physics of mechanisms guided by **R** was failing, and it was time for a new physics of principles.

Duhem similarly distinguishes two types of methods, synthetic and analytic (Duhem 1903). Synthetic methods, guided by **R**, build up the mechanism from the sizes, shapes, and masses of its elementary bodies and fundamental forces acting on them, construct the law of motion in differential equation form, and compare with experiment the results obtained when initial conditions are set. Only synthetic methods were used for much of modern physics, Duhem says, and he cites some celebrated results: the Cartesian explanation of weight by vortex motion, Lesage's explanation of gravity by impulses of particles on bodies, kinetic theories of gases, Kelvin's gyroscopic ether, Maxwell's mechanical models of electromagnetism, and contemporary mechanical models of light, electricity, and new radiations proposed by Lorentz, Larmor, J.J. Thomson, Langevin, and Perrin. Most contemporary physicists, Duhem points out, have concluded that synthetic methods cannot deliver mechanical explanations of natural phenomena that are complete, unified, general, coherent, or empirically adequate. Instead, Duhem claims, like Poincaré, most contemporary physicists have turned to analytic treatments.

⁸ On Duhem's telling of the history each approach has its heroes: Descartes, Huygens, Boscovich, Laplace, Poisson, Kelvin, and Maxwell favor the causal explanatory approach, while Newton, Fresnel, Ampère, Fourier, Rankine, Helmholtz, Gibbs, and Duhem himself favor the abstract representative view.

⁹ He mentions Laplace's celestial mechanics, Briot's hypothesis that ethereal atoms attract each other with forces that are proportional to the inverse 6th power of the distance, and Maxwell's hypothesis that gas molecules repel each other by inverse 5th forces.

Synthetic Treatments: Problems

Synthetic treatments are guided by **R**, by a picture of the universe in which all empirical regularities are the effects of fundamental processes involving fundamental entities. They attempt to derive from the bottom up the equations of motion of an isolated mechanical system *S* from the laws governing *S*'s elementary parts: the state of *S* is determined by the positions and motions of its component bodies (understood ultimately as fundamental particles), and the motions of *S* are determined by the motions of its parts and the forces to which they are subject, generally assumed to be inverse functions of their relative distances from each other. It is a physics of differential equations whose natural class of applications is initial value problems. While this worked very successfully to describe the motions of heavenly bodies, Duhem argues, there were many difficulties, especially when it came to dealing with terrestrial phenomena. Some of the difficulties concerned empirical adequacy. For example, Poisson's physical mechanics, a synthetic theory, predicts various bulk and elasticity ratios that disagreed with experiment and, in some cases, were absurd. Some of the difficulties were conceptual. According to mechanism, energy added to a system, like that produced by heat or friction, is converted into energy of the system's elementary bodies. But now the same problem arises at the lower scale: how is energy distributed to an elementary body *b*? Changes in *b*'s kinetic energy *T* must be compensated by corresponding changes somewhere. Moreover, both energy distribution at the microscale and spectroscopic data implied that molecules, if they exist, must have elastic properties like modes of vibration. It was therefore important to have a better account of elasticity. But accounts of elasticity attempted by physical mechanists like Poisson were empirical failures. Some of the difficulties were mathematical. Any physical system will have a huge number of degrees of freedom, and the system of differential equations needed to solve the problem of its motion will become unsolvable. To get around these tractability problems, various techniques appealing to macroscopic constraints and boundary conditions have to be deployed to narrow down the number of degrees of freedom to a manageable set. But these techniques are not validated by the synthetic basis (they are added from above, not derived from below); sometimes they cannot be understood within the conceptual framework of the synthetic approach and their consistency with the basic picture is questionable. Finally, some of the problems concerned generality/extendibility. Conservation of energy can be derived from Newton's laws only for conservative systems, where no net work is performed by forces acting on the system. Few natural systems are conservative; indeed, many concrete systems (steam engines, e.g.) lose energy through heat or friction. Extending synthetic treatments to such systems thus became pressing.

Physical mechanists had responses to these problems, but they seemed stretched, ad hoc, and objectionably complicated to Duhem. Poisson, for example, adds elements to his models that are inconsistent with his background assumptions and is forced to replace summation by integration, a replacement that requires crude approximating conditions. Duhem's criticisms of them are trenchant: they employ "ruses and chicanery", retain the theory only by "subtleties and subterfuges" (Duhem 1903, 45), and lack mathematical rigor. His criticisms of Kelvin, Lodge, Maxwell, and the Victorian penchant for synthetically constructed models are caustic. He comments with Gallic flair on Lodge's *Modern Views of Electricity*: "We thought we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory" (Duhem 1991 [1906], 71). He points out that there are as many kinds of material molecules as there are kinds of physical phenomena or experimental laws (Duhem 1991 [1906], 82-83). Similar critiques are directed at Maxwell's and Lodge's different mechanical analogies of electromagnetic phenomena and at Kelvin's vortex atom model. Faced with failure of a model, they switch to new models that are inconsistent with other models they use. Such theories, Duhem concludes, cover only "a miniscule fragment of Physics" and the "fragmented representations may not be welded together to form a coherent and logical explication of the inanimate Universe" (Duhem 1903, 100).

In a nutshell, then, physics developed according to the synthetic approach had failed, according to its critics. "Visualizable" material points or atoms subject to position-dependent central forces, so successful for representing celestial phenomena, were ill-suited to represent electromagnetic phenomena, "dissipative" phenomena in heat engines and chemical reactions, fluid phenomena, etc. Deformable bodies and viscous fluids are conceptually difficult to construct from atom-like material points; shearing forces are incompatible with central force assumptions; frictional and electrical forces are velocity-dependent. Physics had become a disorganized patchwork of poorly understood theories, each dealing with special cases in its own domain

and inconsistent with others, without adequate unified foundations or empirically determined values of microscopic parameters. Lacking coherence, unity, and empirical determinacy, these theories could not claim to be explanatory or realistic.

Analytic Treatments: Promises

This is where the new physics of principles (Poincaré) or physics developed on the analytic approach (Duhem) comes to the rescue. Physicists responded to this crisis, Poincaré explains, not by giving up the dream that the universe is a machine, but by, in a sense, side-stepping the problems. Suppose we have a machine whose initial and final wheels are open to view but whose intermediate machinery for the transmission of energy between the two is hidden. We can determine by experiment that the final wheel turns 10 times less quickly than the initial wheel and, using the principle of conservation of energy (PCE), determine that a couple applied to the one will be balanced by a couple 10 times greater applied to the other. In order to know that equilibrium is maintained by this compensation, we do not need to know how the forces inside the black box compensate each other. Similarly, using the principles of dynamics, we can draw conclusions about macroscopic motions based on observations of them without knowing anything about the microscopic machinery, conclusions that will hold true whatever the microscopic details may be. In addition to PCE Poincaré lists several other principles whose application to physical phenomena suffices “for our learning of them all that we could reasonably hope to know” (Poincaré 1913, 300).¹⁰

Similarly, on Duhem’s view, analytic treatments develop from general principles like PCE and Carnot’s Principle (Duhem 1911, Vol I, 2). Historically they began with Lagrange, who condensed statics into a general principle, the Principle of Virtual Velocities (PVV). PVV tells us that a mechanical system X is in static equilibrium just in case in all infinitesimal virtual displacements of X the forces applied to X perform zero work. In a perfectly balanced see-saw, for example, the sum of the work done by both the external forces (like gravity) acting on it and the internal forces holding it together is zero. Lagrange’s analytical mechanics has several attractive features that were later heavily exploited in analytical treatments. First, it was extendible. Using d’Alembert’s principle, Lagrange showed how to extend PVV from statics to dynamics: we simply add fictitious “inertial” forces to balance the external forces that are really acting on X to produce its acceleration, so that X is in equilibrium at each instant. Then the work done by the external, internal, and inertial forces will sum to zero. Second, it provides a powerful algebraic method (Lagrange multipliers) that uses constraints in a principled manner to reduce the number of degrees of freedom and thereby to overcome the tractability problems mentioned earlier. Third, Lagrange heavily relies on conjugate pairs of generalized coordinates/quantities and actions that are mathematically related to each other as are position and force: the quantities are “position-like” in the algebraic sense that their empirical behavior is related to the empirical behavior of their 1st and 2nd derivatives as position is related to velocity and acceleration. The actions can be interpreted as force, moment of a couple, surface tension, or pressure for corresponding generalized coordinates understood as distance, angle, surface, or volume, respectively; the products of the actions and the corresponding coordinate shifts yield generalized work (as [force times distance moved] is work) as well as generalized versions of “kinetic energy-like” quantities (functions of the squares of the 1st derivatives of the coordinates), “potential energy-like” quantities (functions of the coordinates but independent of their 1st derivatives), etc. This history is sketched in (Duhem 1903, Part I).

Duhem was struck by the analogy between these methods of Lagrangian mechanics and the pioneering methods of thermodynamics developed by Clausius, Helmholtz, and Gibbs, which also relied on equilibrium as a central concept, on work-energy relations, and on highly abstract mathematical processes mimicking real processes as the Carnot engine mimics heat engines. Throughout his life as a theoretical physicist, he formulated, defended, and actively pursued a program of energetics (generalized thermomechanics), extending analytical principles to a wide range of mechanical, thermodynamic, chemical, and electromagnetic systems. In the style and method of Lagrange’s analytical mechanics, Duhem further generalizes: the conjugate coordinates α may be any collection of variables, functions of which determine the physical state of a system (including its mechanical, thermodynamic, chemical, electrical, and magnetic

¹⁰ He lists degradation of energy, equality of action and reaction, Galilean relativity, conservation of mass, and least action.

state); the corresponding actions A are just abstract analogs of mechanical force that are empirically determinable; virtual displacements become virtual modifications of any variable determining the state; locomotion becomes any change of physical state; and equilibrium is a similarly generalized notion covering mechanical, thermodynamic, chemical, and magnetic equilibria.¹¹ On Duhem's generalization, the α -coordinates may include standard position and velocity as well as entropy, volume, number of component substances, electric and magnetic charge, etc. Similarly, the A -actions may include standard distance-dependent forces as well as velocity-dependent forces (like friction), temperature, pressure, chemical potential, and various actions associated with electric and magnetic fields.

In this way he hoped for a truly general unified theory of rational mechanics that would have better empirical support than rivals and be mathematically and conceptually coherent. This theory expresses a tree at the root of which is the generalized principle of virtual modification covering all systems in mechanical and chemical statics. These systems, like our earlier perfectly balanced see-saw, are in equilibrium if, and only if, for every virtual modification achieved without temperature change, the work performed by actions "which are to symbols for various quantities what forces are to coordinates of mechanical systems" (Duhem 2002 [1901], 293) is balanced by the internal thermodynamic potential of the system. The tree grows by addition of branches for various kinds of systems, in effect by supplementing the root equation with new terms to balance work for that type of system, analogously to Lagrange's earlier extension from statics to dynamics by addition of balancing inertial forces. Mechanical and chemical statics are extended to mechanical and chemical dynamics, to viscous fluids, and to a host of systems classified by their phenomenological properties mathematically expressed: systems with friction, static systems exhibiting hysteresis (like annealed steel, permanently dilated glass, colloidal absorption of water vapor); dynamical systems involving hysteresis; thermal systems without friction or hysteresis (reversible heat cycles); thermal systems with friction and/or hysteresis (irreversible heat cycles); etc. The various principles thus supplemented by their own appropriate terms sitting on the various branches of this tree yield equations of "motion" for the type of system characterized by the principle. Duhem sketches these extensions in (Duhem 1903, Part II) and (Duhem 2002 [1901]) and lays out the details in his major (1000+ pages) text in physics, (Duhem 1911).

The perceived superiority of these analytic over synthetic treatments rested on the following reasons. First, and most importantly, they allowed physics to avoid hidden mechanisms, and this was considered by many to be a positive given the lack of empirically determined specific information about particles. Just as we can explore the energy connections between Poincaré's two wheels without knowing anything about their physical connections, we can use our balance equations to calculate, for example, the value of the internal thermodynamic potential or the entropy change of a system from empirically determinable quantities like temperature, pressure, and change of volume, without knowing what constitutes any of these quantities. Second, they promised more empirical success than synthetic methods. Writing in 1892, the elastician A.E.H. Love claimed that the best modern experiments supported the multi-constant results of the analytical theories over the rari-constant results of Poisson's synthetic theory (Love 1892, 14). Third, by avoiding hidden mechanisms and developing coherent continuum theories of elasticity they allowed physicists to side-step problematic conceptual questions about how energy is distributed among particles and the whole problem of replication of macro-problems at the micro-level. Fourth, by enabling the free choice of suitable generalized coordinates that fit constraints, and by inventing algebraic procedures (like Lagrange's method of multipliers) for reducing calculational complexity in a principled way, the analytic mechanists avoided the mathematical problems mentioned earlier. Finally, the theories promised to be extendible to dissipative systems in principled ways.

In a nutshell, then, physics developed according to the analytical approach promised to succeed where the old physics had failed. However, what most inspired the proponents of the new physics of principles was the generality they promised. Lagrange's analytic mechanics unified under a single principle, the principle of virtual velocities, long-known laws like the ancient law of the lever and Pascal's law of hydrostatic pressure, was extendible to cover a wide class of dynamical problems, and provided a systematic way of solving them. Gibbs and Helmholtz further extended that system to thermodynamics and physical chemistry. Duhem hoped to extend it to wider and wider classes of dissipative phenomena.

¹¹ The products on the right side of the thermodynamic identity, $dU = TdS - PdV$ (where U , T , S , P , and V are internal energy, temperature, entropy, pressure, and volume, respectively) are conjugate action-coordinate pairs.

Reactions to the Crisis

As a result of the crisis, physicists became increasingly pre-occupied with foundational efforts to put their house in order. There was widespread belief that the most promising physics required general analytical principles that could not be derived from Newtonian laws and the abstract concepts (action, energy, internal potential, entropy, absolute temperature) needed to construct and apply these principles could not be built from the ordinary intuitive concepts (position, mass, force) of classical mechanics. The more reflective physicists, however, reacted in different ways to this state of affairs. Here we look at four of them in ascending order in their opposition to realism.

Kelvin seems to have held on to the physics of mechanisms to the end, claiming that synthetic models were necessary and sufficient for understanding – “I am never content until I have constructed a mechanical model of the subject I am studying. If I succeed in making one, I understand; otherwise I do not” (Kelvin 1904, Lecture 20) – and that “there must be something in this molecular hypothesis and that as a mechanical symbol, it is certainly not a mere hypothesis, but a reality” (Kelvin 1904, Lecture 1). It should be noted, though, that despite his hard-headed realism Kelvin, the youth, was a principal early developer of energy physics; so, though wedded to the mechanical viewpoint, he was flexible enough to experiment with other styles of theorizing.

Although Maxwell had achieved great success in the *Treatise* using the analytical (or as he called it, the “dynamical”) approach, he nevertheless felt that its methods were too algebraic and did not provide a proper understanding of the phenomena unless they were underwritten by physical ideas involving forces and mechanisms. In this he followed the tradition of the “northern wizards”, Thomson (Kelvin) and Tait, who developed analytical approaches which were designed to facilitate ignorance of coordinates (avoidance of hidden mechanisms) but were based on work-energy theorems themselves based on Newtonian impetus. He holds that the electromagnetic field must be a medium for energy transport but admits that we lack any clear representation of the details of its action, and he proposes the mathematical relations between the phenomena that he develops in the *Treatise* as a first step toward clarity. Maxwell’s approach seems both provisional and commendably tentative.¹² He employs ignorance of coordinates tentatively and methodologically: given our present ignorance of the hidden mechanisms and the absence of empirically determined values for many of their parameters, we should press on with theorizing that will allow us to avoid them and hopefully learn more about at least the form of their parameters from those theories. Maxwell, we might say, espoused a local variety of antirealism about action-at-a-distance forces. Though he employed analytical techniques, he was no antirealist of the in-principle sort. (He believed in atoms but acknowledged that not much was known about them, for example.)

But others, like Poincaré and Duhem, reacted to the crisis by espousing more global forms of antirealism. Poincaré seems to have taken a less militant approach to the crisis. Though he saw there were problems with the causal explanatory approach, he also saw problems for the principles approach looming on the horizon. He refers to these problems as a possible 2nd crisis in physics: the experimentally acknowledged random motion of atoms, the null results of the Michelson-Morley experiments, and newly discovered radioactivity, respectively, were challenging the universal validity of the principles of degradation of energy, Galilean relativity, and conservation of mass and energy.¹³ In 1905 none of these experimental results was well understood; so it was not clear how theory would respond to the challenges. But Poincaré is hopeful that they will be resolved and the theories that meet them will retain the best current principles as approximations. Poincaré adopts a structuralist position: current entities may be discarded as past ones were, but the structural features of the world or of the phenomena that are expressed by the mathematical equations of current theories will be retained in future theories.

¹² His mixed approach was, of course, roundly criticized on all sides. Kelvin could not understand the *Treatise* as providing a physical theory because he felt Maxwell had too much focused on the mathematics and had lost sight of the mechanisms altogether. Poincaré and Duhem complained that Maxwell had too much and unnecessarily focused on mechanisms and had lost sight of a rigorous, coherent development of the abstract concepts and equations.

¹³ As Maiocchi and others have pointed out, Duhem completely ignored this 2nd crisis to which relativity and quantum mechanics responded, believing instead that a physics of principles, as realized in his generalized thermomechanics, was displaying (and would continue to display) steady progress toward a natural classification (Maiocchi 1990).

Duhem, as we have seen, rejected all physical theorizing guided by **R** that appealed to underlying causal mechanisms and instead placed all his confidence in analytic physics guided by its associated **NC**-aim. It is hard to emphasize how different the analytic and synthetic styles of theorizing are. The synthetic style deals with relatively concrete “visualizable” bodies, subject to actual displacements that result from experimental manipulation and the application of physical forces, and moving in paths in spacetime. The generalized analytic style deals with highly abstract properties of abstract systems classified under an abstract principle, **PVV**, subject to virtual displacements that result from conceptual manipulation, and “moving” in paths that are a continuous sequence of static states (each of which is allowed enough “time” to relax to equilibrium from a virtual manipulation). Natural classifications exploit formal, mathematical rather than sensible, intuitive analogies and end up classifying phenomena in ways that are unexpected from the classificatory perspective of everyday common sense. Duhem thinks of the tree structure that results from these analytic techniques as a natural classification of the phenomena into types of systems. Such a classification would be unified (since organized under one general principle, the root equation) and completely general (since new branches could be added in a well-motivated manner as new systems were discovered).

Aftermath

With hindsight we can see that each side – the proponents of mechanisms and the proponents of principles – turned out to be partly right and partly wrong about the physics. On the one hand, a physics of principles was partly vindicated by later developments. Einstein held that proper understanding requires synthetic theories but progress is often hindered by premature synthetic theories (Howard 2004). In such cases principles theories can come to the rescue by providing extra constraints that make more determinate the synthetic options, and Einstein’s own theories of special and general relativity do exactly that (as Maxwell’s had earlier). During the 2nd half of the 20th century Clifford Truesdell and his students, using new mathematical techniques, provided a rigorous grounding for much of the macroscopic physics of continua that was conceptually and mathematically problematic in the 19th century and that pushed physicists to develop analytic techniques. These contemporary treatments accept a background of atoms and fundamental forces, but they do not try to explain continuum phenomena in terms of atoms and fundamental forces. Instead, they work entirely at the macroscopic scale and, following Duhem’s lead, try to impose conceptually and mathematically coherent order on the domains they study.

On the other hand, a partial vindication of atomism was just around the corner (due to Einstein’s theoretical and Perrin’s experimental studies of Brownian motion), and nearly everyone, with the exception of Duhem, was converted. No doubt, as Poincaré’s references to indifferent hypotheses and Duhem’s references to “the absolute indeterminacy of the masses and hidden motions” (Duhem 1903, 97) make evident, the inability of scientists before Perrin’s experiments on Brownian motion to empirically determine with any accuracy the properties of atoms and molecules (like their absolute sizes, gram-molecular weight, and number per mole) played an important role in supporting the skepticism/agnosticism of the anti-atomists. But they greatly underestimated the ingenuity of theorists and experimenters and their ability to devise hypotheses that would tie empirically measurable parameters to parameters of elementary bodies sufficiently tightly to determine the latter. The story of how this work led to Perrin’s multiple determinations of the values of various parameters including Avogadro’s number and their interlocking, mutually supporting consilience is told in various places, e.g., (van Fraassen 2009). But fast-forwarding another twenty-five years or so, quantum mechanics had become generally accepted as the most empirically adequate account of atomic behavior, and though everyone believed in atoms, hardly anyone believed their behavior could be modeled in terms of physically familiar parameters and operations, and the abstract conception had returned in full force. Scientific progress was made, but it appears to have assimilated strands from both the synthetic and analytic traditions.

Some Philosophical and Historiographical Lessons

I conclude by drawing some lessons, largely negative, about the current practice of appealing to the history of science to draw philosophical conclusions about science.



First, we should be wary of using history to set limiting scientific images like **R**, **AR**, and **NC**, characterized in terms of aims and goals. Such restrictive images sound suspiciously *a priori*. In setting a causal explanatory agenda for science **R** presupposes that in principle everything is explainable bottom-up from the workings of fundamental entities or stuff. But, other than by an act of faith, we do not have good reasons to believe that. We do not have bottom-up explanations of entropy and many of the processes Duhem and his Truesdellian successors study. Nevertheless, entropy is an important physical quantity that provides important information about physical systems. If proper science required us to fathom the deep explanatory structure of the world that causally explains the unfolding of all else, then continuum mechanics and macroscopic thermodynamics wouldn't count as science. But surely they are science; it's just that they are not illuminatingly organized under **R**. But, by the same token, contrary to Duhem, neither do we have good reason to believe that the path to progress lies only in the pursuit of **NC**. If proper science required that, then much of 20th and 21st century physics wouldn't count as science.

The 19th century example and its aftermath should make us question notions like the proper aim and form of physical theory, since such notions may be responding, as they were in the 19th century writings, only to contingent features of our current and past theories. Not only was Duhem partly wrong about the path physics would take, he was completely wrong about the proper aim of physics. It is one thing to propose freeing physics from unsuccessful mechanical conceptions and advocate the pursuit of relatively promising analytical theories. It is quite another thing to restrict the aim and scope of physics to the discovery of real relations between hidden entities underlying the phenomena (Poincaré) or to the non-literal abstract representation of the phenomena that leads to a "natural classification" (Duhem). Why didn't the antirealists follow Maxwell's example and say, "Well, right now, we don't know enough about the minute workings of nature, and we should use analytical techniques or indeed any other techniques we can come up with to see whether we can impose more order on the phenomena which in turn might provide us with more empirical information that might be used to better home in on the minutiae"? We should be, like Maxwell, as humble about our philosophy of science as about our science itself, because nature can surprisingly force us to change our most entrenched historical course.

Duhem provides a good example of the need for caution. At his best, he proceeds urging such caution about inferences from past and present states of physical theory to its future states and about inferences from present states of physical theory to conclusions about the world underlying the phenomena (Duhem 1991 [1905]). He also acknowledges that newly discovered radiations "have revealed (...) some effects so strange, so difficult to subject to the laws of our Thermodynamics, that no one would be surprised to see a new branch of Mechanics swell up from [their] study" (Duhem 1903, 185). And he is modest about the fate of his general thermodynamics: "It would be quite presumptuous to imagine that [this] system (...) will escape the fate common to the systems that have preceded it (...); but (...) [the theoretician] has the right to believe his efforts will not be sterile; through the centuries the ideas that he has sown and germinated will continue to increase and to bear their fruit" (Duhem 1903, 188-189).

But though Duhem attempted to be impartial, he did not succeed. The historical evidence seems not to support as clear a distinction as he draws between the good guys and bad guys, with a given physicist being singularly committed to one goal rather than another. On the one hand, Duhem's Newton made the first great contribution to natural classification (in *Principia*), yet the unofficial Newton toyed with causal explanations of gravity, and Newton's emissionist theory was not only causal but arguably sufficiently influential to retard the mathematical development of optics. On the other hand, despite his atomism, for which Duhem criticizes him, Huygens' optics was purely mathematical and provided the basis for Fresnel's subsequent mathematical development of the wave theory. Similarly, Duhem's examination of extant theories of mechanics downplays the fact that many of the mechanists whom he vehemently criticized contributed significantly to the abstract approach Duhem favored. Kelvin virtually invented the analytic approach to heat and energy in the 1850s and strongly influenced Rankine's energetics program, which Duhem acknowledges as his inspiration. And Maxwell's *Treatise on Electricity and Magnetism* was the 19th century zenith of the principles program applied to the novel phenomena of the day. Duhem was nothing if not a sensitive historian – his massive work on medieval physics and the clear superiority of his histories of mechanics and heat compared with those of Mach amply demonstrate this. He was surely aware of these subtleties in the historical record. Unfortunately, he largely ignored them. It would be all too easy to convict Duhem of partiality, to claim that his views were motivated primarily by the desire to combat certain kinds of Godless cosmologies associated with metaphysical atomism and physical mechanism and to defend a

cosmology and physics more closely aligned with the teachings of Mother Church. Perhaps he was consciously or unconsciously influenced by such desiderata. But, when assessing his motivations we must also attend to what he literally committed to print, and here he is thoroughly frank about his metaphysical and religious predilections and emphatic that his conclusions about the connections between physics and metaphysics are based entirely on his examination of physics and its history. I take him at his word and doubt that this omission had dishonest motives. Nevertheless, one can only assume he thought these historical connections to be unimportant because they didn't fit the pattern of progress and cumulativeness he saw and the image he endorsed.

The general problem with historical extrapolations of the kind Duhem and our own contemporaries want to make is that it is all too often too easy to find a suitable pattern to project. His attempts to extrapolate structures and predict the future of physics should make us wary of all such arguments. Historically sensitive and cautious though he was, Duhem blundered. Why should we think we can do better? The history of physics is a Delphic oracle and its future, shaped as it will be by our contingent and accidental approach to the world, is unlikely to be predictable with any confidence.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 85-92

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Duhem in Pre-War Italian Philosophy: The Reasons of an Absence

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Abstract:

The article illustrates the presence of Duhem's thought in Italian philosophical culture until the Great War. This presence was very scarce, so we must speak of an absence. One can identify the causes of this absence with the fact that all the great Italian philosophical currents, in different ways, have had little interest or been manifestly opposed to the tensions that came from abroad after the great discussion about the crisis of mechanicism in which Duhem was an important protagonist.

Keywords:

Pierre Duhem; Ernst Mach; Italian Philosophy; History of Science

Received: 13 April 2017. Accepted: 16 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.08>

85

In the years when Duhem was reflecting on the philosophical value of science, the philosophy of science was not attracting much attention in Italy. At the turn of the twentieth century, the idealist school of Benedetto Croce and Giovanni Gentile was taking centre stage on the Italian philosophical scene. Both Croce and Gentile were very critical of the importance of philosophy in science, albeit for different reasons.

In his "Logica come scienza del concetto puro" [Logic as the science of pure concept] published in 1909, Benedetto Croce maintains that scientific knowledge only has an instrumental, practical value, and that it is composed of pseudo-concepts. This critique became famous, but it was not anything new in Croce's intellectual path. What he did in 1909 was simply reiterating and developing a belief he had already acquired years – about a decade – earlier, while he was reflecting on historical and literary themes, unaware of the contemporaneous debate on exact sciences. He learned about the thought of Mach, Avenarius and Poincaré (but not Duhem) only when his critique of science had turned into a deeply rooted conviction. "About the time" he began to study Hegel, in 1905, Croce also read "the new gnosiologists of science and the blundering pragmatists, obtaining a proof of his critiques of aesthetic doctrines" (Croce 1945, 401). It is irrelevant whether Croce read Mach a few months before or after writing his Logic, because the discourse on sciences presented in this text does not owe anything to Mach or other epistemologists. It belongs solely to Croce in structure, style and arguments. Wherever traces of other thinkers may be found, they are always such generic and widespread theses that it is impossible to establish whether Croce was referring to Mach or Papini.

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Nevertheless, some of those generic and widespread theses are the point of arrival of Croce's deprecating discourse on the cognitive power of science. It is true that Croce draws those conclusions by following his personal path – a path that is theoretically flawed, as it overlooks the arguments of the more advanced epistemology and neglects the practice of science – but his conclusions are not personal at all.

Croce's so often quoted opinions on science mirrored analogous judgements that were widespread among the numerous European and American varieties of Bergsonism, Conventionalism, more or less mystical Pragmatism, Fictionism, Empirio-criticism, but also – by then – of Italian Positivism.

Around 1905 also Italian Positivism – despite the roughness and narrow-mindedness of many of its representatives – had become aware of the ongoing discussion about the crisis of the mechanical philosophy in Europe, the “failure of science”, the rebirth of Idealism, Contingentism and so on. For instance, in “Della conoscenza del fatto naturale e umano” [Of the knowledge of the natural and human fact], already in 1896, the positivist Giuseppe Tarozzi presented an idea of science that was perfectly in line with the movement of the “destruction of reason” that was permeating European culture; a concept that was particularly akin to Bergson's ideas. Another example is Giovanni Marchesini's “La crisi del Positivismo e il problema filosofico” (1899) [The crisis of Positivism and the philosophical problem], a book whose main goal is to counter the thesis that attributes a symbolic value to scientific concepts, although admitting that it was Positivism that contributed to the emergence of Scepticism. This statement clearly contrasts with the ideas of Fouillé, one of the main French representatives of the idealistic revolt against scientific Naturalism. Another text worth mentioning is “Sopra la teoria della scienza” [On the theory of science] (1903), an early work by Annibale Pastore, another opponent of Neidealism. Pastore presented a similar analogical-fictionist conception of Modelism, perceived as the main scientific method, which mirrored, widened and specified the ideas of one of Pastore's masters, physicist Antonio Garbasso, a feisty adversary of Croce.

As a matter of fact, Croce's discourse on science relegated it to the realm of “usefulness” and simply reiterated an opinion that was widely shared or at least known by many Italian scholars. Quite opportunistically, Croce came to the same conclusions on the troubles of nineteenth-century science as many other European scholars, but the details of such conclusions were foreign to his personality. Particularly, the absence of Duhem's name in Croce's writings is a sign of his superficial relationship with epistemological works.

Gentile was even less interested in epistemology than Croce. In his famous controversy with mathematician Federigo Enriques, Gentile refrained from an epistemological discussion, shifting his focus from the “critique of science” to Enriques' “scientific philosophy”, identified as the “Naturalism” that is typical of all science-based philosophies. Even in his systematic works Gentile traced the critique of sciences back to the critique of a vaguely outlined single type of philosophy, which, in his view, encompassed all the science-based philosophical varieties.

Unlike Croce, Gentile could not criticize sciences and separate them from philosophy by attributing them a mere practical value, as in his philosophy a clear distinction between theory and praxis is impossible: like philosophy, science has a cognitive value, too. The difference between science and philosophy lies in the lack of universality of the object in science. In fact, every science is particular and refers to a limited object. Hence its need to presume the object of its investigation and to see reality as nature, made of elements that can be studied separately. So, Dogmatism and Naturalism are the two distinctive characters of every science, and Gentile finds them after a very generic analysis that has no relation whatsoever with epistemological critique. On the contrary, the immediate conclusion that Gentile draws from his investigation appears to be in contrast with what was emerging in epistemology at the time. As epistemological critique was often labelled as an allied of “idealistic reaction” at the beginning of the century, Gentile established a strong and steady relation between science and Materialism. He speaks of a “logically necessary tendency of science in all times towards the mechanical philosophy and Materialism” (Gentile 1924, 198) and maintains that “science as philosophy has always stood up against philosophy, which, overcoming the mechanical philosophy, Empiricism and Dogmatism, has tried to turn into a universal idea of the world in its metaphysical reality”. He also theorises the incompatibility between being a scientist and being an idealist: “Scientists, because of the very spirit of science – which is not and does not want to be philosophy –, have always supported one philosophy: the most naïve and weak of its forms” (Gentile 1924, 199).

As a matter of fact, Gentile's thesis about the theoretical separation between science and Idealism only made sense within his system and, as such, it was built around a logical pattern that was completely extraneous to scientific dynamicity, even though it mirrored exactly the image of science as outlined by

positivist Scientism. During the nineteenth century, the bond between science and Materialism had been very strong (but not exclusive), but Gentile's vision turns a historically framed idea of science – that of the age of Positivism – into an absolute and unhistorical idea.

By theorising a clear-cut contrast between science and Idealism, Gentile contributed much more than Croce to the separation of Italian philosophy from science at the beginning of the twentieth century. In Croce's view, science differed from philosophy, but for Gentile it was an enemy to be defeated for the triumph of Idealism – hence the crusade-like spirit of his works.

Gentile was certainly aware of the existence in Europe of interpretative tendencies – very often within science itself –, which prefigured the divorce between science and Materialism already in the nineteenth century. However, these tendencies could be perceived as philosophical critiques coming from the outside and not created by internal developments of the scientific thought, as these developments were invariably materialistic. Epistemologist could be easily ignored: in the many pages of Gentile's works there is no mention of Mach, Poincaré and Duhem.

Croce's and Gentile's critiques only developed within the framework of superficial and instrumental relationships with the major epistemologists of the time, or even without any familiarity with their works, and one gets the same impression by reading the works of the main targets of the critique of Idealism – the positivists.

Italian Positivism had dealt with psychology, anthropology, sociology, law, but mathematized natural sciences had never been among its interests. In the books by Tarozzi, Marchesini and Troilo one can find long discussions about the nature of science, but they only deal with the concept of science or experience, hypothesis, law, symbol, etc., in very generic terms, without any reference to physics, and the authors seem not to have a real knowledge of the epistemologists who were turning physics into one of the most important motifs of philosophical analysis. Therefore these works appear backward and narrow-minded.

In order to find references to the questions of physics or to authors such as Mach or Duhem, we must turn our attention to less elaborated works, such as articles or reviews. Of course, this proves the weak impact of these themes on the overall development of Positivism. Mach is the name that is most often quoted, although very limitedly, on positivist journals. But it is a reduced, simplified and criticised version of Mach.

In 1900 Enrico Morselli, in his review of Karl Pearson's "The grammar of science" on *Rivista di Filosofia e Pedagogia* paints both Pearson and Mach as mediocre scientific popularisers: "Pearson's work is essentially an educational text, [which is echoed in Europe by] the books of Prof. Mach, who excels in his most difficult and useful work of popularising knowledge" (Morselli 1900, 83).

In 1903 Giovanni Cesca published in the same journal one of the few Italian articles dedicated to Mach (and to his "follower" Ostwald), in which Mach is presented not as a scholar who has drawn certain philosophical conclusions through a precise reflection on his work as a scientist, but, on the contrary, as a scientist who – like all scientists – is invariably confined in the narrow realm of Empiricism and Materialism and, when he comes to suffer these limitations and starts philosophising, falls naively and hastily for "those doctrines opposing Positivism and the mechanical philosophy" (Cesca 1903, 248). Cesca's critique to Mach proceeds along lines that are completely unrelated to Mach's arguments, ignoring the reasons of epistemology and articulating a totally non-analytical discourse: Cesca only counters Mach's idea by postulating a series of philosophical necessities that make philosophy independent and superior to physics. In Cesca's view, Mach has developed an "extreme Idealism", a "doctrine of absolute Phenomenalism or Idealism" (Cesca 1903, 249). Mach's mistake is not having recognised that science does not only have a hypothetical and economic part, but also a positive part, which is based on the data coming from the experience of all mankind. One must admit an object in juxtaposition with a subject, something outside us that serves as the basis for our sensations. Like science, philosophy does not settle for a "quantitative conception of physical phenomena". It aspires to reaching the qualitative causal explanation (Cesca 1903, 249). With this goal it relies on "empirical metaphysics explaining the causes, the laws of the becoming of facts, which reduce them to their ultimate constitutional elements and show the essential and peculiar qualities of each one of them" (Cesca 1903, 265).

It is clear that such an approach, interested as it is in the "causes" and "essential qualities" of phenomena, could only have a few points of contact with an anti-metaphysical author such as Mach. The differences from Duhem were just as radical, if only Cesca had read his works.

Cesca's concern with rejecting the possible sceptical and subjectivist results of the new critique to physical sciences in the name of a supposed positive philosophy – a philosophy that transcends physics by

providing steady fundamentals whenever physics leans dangerously towards Pyrrhonism or Idealism – can also be found in other positivists who – more or less incidentally – deal with some of the great physicists of the mechanical philosophy. That is the case of Adolfo Levi, the author of a 1909 essay on Mach's Phenomenalism, and Adolfo Faggi who, polemicizing with a book by Igino Petrone (1900) that criticises the mechanical philosophy on the basis of Stallo's and Boutroux's books, states that science must not fall prey to subjectivism: it may maintain the schemes of mechanistic determinism, so long as it recognises the superiority of philosophy over quantitative science. Philosophy can answer questions that would not find any answer in science, and Faggi (1900, 386) reiterates this idea with accents that recall Gentile's famous thesis of contrast between science (the world of dead things) and philosophy (the theory of live reality).

Italian Positivism was therefore much busier defending itself from the new epistemological critique than understanding and using it. The comparison between these two problems was sporadic and basically unimportant for this current of thought. Italian Positivism was already sailing on troubled water and by failing to recognise the latest and most stimulating trends in the field of the philosophy of science it definitely separated its rhetorical and undetermined idea of science from real science.

Pragmatism – first a travel companion and then an enemy of Idealism – was the loudest and most quarrelsome philosophy of the first decade of the twentieth century. Its fiercest upholders – Prezzolini and Papini – were essentially political and literary philosophers, and no interest for science can be detected in pragmatist journals such as “La Voce” and “Leonardo”. There is indeed some hint at “positive” science in the discussion about psychiatry and Lombroso's school in particular. Apart from that, science is discussed only as philosophy through the mediation of Nietzsche and Bergson (and their followers), whereas more serious critics of the nineteenth-century mechanical philosophy, such as Boutroux and Milhaud, are not mentioned at all.

The only one in the pragmatist group to have some interest for modern epistemology was Vailati. However, Vailati's reflection on physics, too, seems marginal and strongly influenced by his personal interest. Vailati devoted painstaking and accurate analyses to problems concerning mathematics, logic, psychology, linguistics, history of ancient science, but he wrote almost nothing about modern physics. Rare hints to themes touched by anti-mechanist critics can be found in Vailati's works, as he comments on authors such as Mach and Duhem, but his analysis of their thought is strongly limited by his personal interests, which make him lose sight of the epistemological value of such fundamental works. Thus, his reviews of Mach's books focus mainly on the psychological side of his work and Mach is considered, quite simplistically, as the author of a “psychology of scientific methods” (Vailati 1911a, 43). In his comment on Duhem's *La théorie physique*, which Vailati is the first to bring to the attention of the Italian public already in 1905, at a time when it had only appeared as separate articles, he only quotes those “conclusions” that are “strictly related” to the “philosophical direction represented by Leonardo in Italy”, namely Pragmatism (Vailati 1911b, p. 593). Although he smartly understands that Duhem's fundamental thesis is the holistic one, Vailati only touches that subject in relation to the influence it could have on the pragmatic concept of meaning, and the remaining part of Duhem's work, relevant and complex as it is, is completely ignored.

In the depressing Italian philosophical landscape a pugnacious group of opponents of Idealism and Pragmatism emerged. Since 1907 this group identified with the journal “Cultura filosofica”, edited in Florence and directed by Francesco De Sarlo. The journal's fundamental idea was the firm conviction of the inseparability of scientific and philosophical knowledge.

It can be said that, throughout the first years of its existence, the journal remained consistent, exploring the numerous links between philosophy and a vast array of scientific disciplines, including mathematics, biology, psychology, law and finally also physics.

It was on the pages of “Cultura filosofica” that the names of Mach, Duhem, Poincaré, Milhaud, etc. started to be mentioned more often, although it must be said that there are no traces of autonomous reflections by Italian scholars on determined scientific problems, and physics appears only through the mediation of these foreign authors.

The journal's first article is dedicated to Mach: “La conoscenza scientifica secondo E. Mach” [Scientific knowledge according to E. Mach], by director De Sarlo. The fundamental coordinates of the journal's interpretation of coeval epistemological critique are already outlined in the article: on the one hand, a psychological interpretation of the main works of Mach and his followers; on the other hand, a strong critique of these authors' conventionalistic, nominalistic and pragmatistic statements. It should not be forgotten that De Sarlo was then involved in an intense debate with Prezzolini's and Papini's pragmatistic

group, which had assimilated the most subjectivistic results of German Empirio-criticism and French *nouveau* Positivism. In De Sarlo's view, to criticise Mach's economic vision of science meant to fight the ideas of Mach his Italian rivals had re-elaborated.

Faithful to the idea that psychology was meant to have fundamentally important philosophical functions, and having presented his critique of Mach's science as founded on two cornerstones (history of science and "the psychology of the scientist") (De Sarlo 1907, 2), De Sarlo turns his attention to what he thinks is the main question posed by Mach, namely the objective value of scientific laws.

Although admitting that Mach's texts about history describe progress as inseparable from a realistic vision of science, De Sarlo attacks the theoretical formulations of Mach's epistemology – as they are invalidated by an unacceptable subjectivism – by counterposing a rationalistic objectivism: "It is impossible to understand how irregular successions of phenomena can lead someone to look for causes in those changes, unless we admit that the need for reason is inherent to the human mind" (De Sarlo 1907, 4).

The journal later published other articles that intended to criticise Mach in order to reaffirm a scientific Objectivism that, far from going back to the typical ideas of positivistic Empiricism, was rooted in an atmosphere of idealistic rationalism, which strived more and more towards an agreement between science and religion. Along with Mach, other critics of the mechanical philosophy were brought to the public attention – although limitedly – and attacked, e.g. Milhaud, Ostwald, Boutroux and Duhem. The latter was the one who was given the least relevance. However, they were told apart from the critics of science belonging to Bergson's school, who were considered just as literates, incapable of any actual analysis of the theories on the history of science.

Antonio Aliotta, De Sarlo's student, stands out in particular. Starting from 1908, he conducted a critical overview of the philosophy of his time, writing several essays which were later collected in the highly significant "La reazione idealistica contro la scienza" (The idealistic reaction against science), published in 1912. This text is certainly one of the most authoritative works written in Italy in those years about the crisis of the mechanical philosophy and its philosophical consequences.

Aliotta has the merit of divulging in Italy philosophers of science who were previously almost unknown, but his work gives an image of the positivistic concept of science that is historically shaped in favour of the arguments it sets forth.

That is not only clear in the very questionable measure of relevance given to the various authors – for example, Aliotta dedicates only half a page to Nietzsche and a whole chapter to Annibale Pastore –, but mostly in the historical path outlined by the book, according to which the crisis has been a predominantly philosophical event, whereas scientific developments have only played a minor role. There is almost no mention of the mechanical philosophy on a scientific level, or only a vague hint as Aliotta quotes passages of history of science by an author who had some interest in history. Therefore the chapter about Duhem, who wrote profusely about history, is quite rich with historical observations, whereas other chapters do not even touch upon the subject of nineteenth-century science. While reading Aliotta's work, one gets the impression that the author saw the "anti-intellectual reaction" as an essentially philosophical phenomenon, a process that developed on an autonomous philosophical level with only occasional points of contact with science. It does not seem Aliotta understood that in those years there had been important scientific novelties, which would eventually lead to new science-oriented philosophical developments. Aliotta was convinced that the mechanical philosophy – which philosophers had shown to be no longer conceivable in dogmatic and realistic forms – still remained a valid scientific scheme, as only it could satisfy mankind's need for intelligibility.

According to Aliotta, the mechanical philosophy must be taken as an ideal explanatory scheme that is rooted in the needs of reason and finds its motivation in it. The mechanical philosophy is irreplaceable, because we cannot think of the world but through mechanistic concepts. Thus, the concrete developments of physics in those years could not be of any interest for this vision, which saw philosophy as unrelated and superior to science and could not imagine the downfall of the mechanistic scheme. This explains the absence – or at least the sporadic mentioning – of science in Aliotta's works.

While writing his essay, Aliotta moved the fundamentals of his rationalism away from the needs of reason and towards religion. He originally criticised Conventionalism, Pragmatism, Economicism by appealing to a principle of rational order as an indispensable postulate for knowledge. Then, after 1910, in line with a similar trend in the journal "Cultura filosofica", his critique found an increasingly steady anchor in the idea of God as the guarantor of the world order: "Those who do not believe cannot and must not believe

in the objective and universal value of science” (Aliotta 1912, 219). Duhem’s complex solution of the relationship between science and religion was therefore replaced by an anathema.

The growing interest of Aliotta and the whole of De Sarlo’s group in spiritual problems in the years before the war was paired by the decreasing attention for the philosophy of science.

The same can be said about another philosopher who stood quite close to De Sarlo in his battle against Croce: Bernardino Varisco.

Varisco had studied mathematics and had written about questions related to physics in a few chapters of his book “Scienza e opinioni” [Science and opinions]. Arguing with Mach (but without knowing the coeval epistemological debate beside Mach) he upheld his characteristic thesis that the notion of force is not conventional, that it has an objective equivalent that manifests itself in the bodies’ action by contact. According to Varisco, the fact that bodies interact by contact is undeniable. Therefore also the validity of the principle of causality is to be considered a fact that no argument can deny. That blocked the way for any conventionalistic and subjectivistic vision. This appeal to common sense to put an end to “byzantine” philosophical discussions remains a distinctive aspect of Varisco’s philosophy also when he devotes himself to the study of the major epistemologists, around 1906, and he uses it to give substance to his philosophical conception of science, which appears quite abstract and generic in his 1901 book.

By studying Naville, Renouvier and Duhem, Varisco intended to continue his battle against Mach, who in the meantime had become a weapon in the hands of the idealists and the pragmatists.

This mainly polemic objective compromises Varisco’s epistemological works. He is so intent in trying to find confirmations for his personal theses that he deforms the interpretation of the authors’ work. Duhem is a good example of that. The holistic thesis, which Duhem had conceived as a logical-epistemological thesis on the procedures of empiric control on the theories of mature science, is assimilated by Varisco to his own notion of “general pressure of experience” (Varisco 1906, 48), which is a psychological notion, valid for the entire human experience, including the one of the cave man. Similar systematic distortions make Duhem’s text compatible with the existence of an objectivism that is actually closer to the everyday man (whose common sense often inspires Varisco) than to the refined French epistemologist.

In his 1909 essay “I massimi problem” [The major problems], Varisco abandoned this kind of studies and turned with increasing determination to metaphysical questions, setting off on a path that would lead him to reconcile with one of his main adversaries, Giovanni Gentile, after the war.

Whereas the “lay” supporters of religion, such as De Sarlo and Aliotta, intervened on scientific problems, discussing and criticising the wave of sceptical philosophical theories following the crisis of the mechanical philosophy in physics, the Catholic cultural world chose to ignore this question. Of course, the Italian representatives of Modernism used Pragmatism and Bergsonism to build their own theological theses, but they completely overlooked all the existing ties between those philosophical positions and science. The traditional Catholic culture, instead, turned to scientific problems, but in such forms as to exclude – particularly in the case of physics – philosophical discussions.

Starting from 1900, the Italian Catholic Society of Scientific Studies began publishing its own journal, *Rivista di fisica, matematica e scienze naturali* [Review of physics, mathematics and natural sciences] under the direction of the bishop of Pavia, Pietro Maffi.

Scientists of high renown, such as Angelo Battelli, Lavoro Amaduzzi, Giuseppe Gianfranceschi, Rinaldo Ferrini and several clergymen wrote about physics in the magazine. These articles look very much like the ones in the pedantic and insignificant annals of the most peripheral academies at a time of positivist domination. Marginal arguments discussed along the guidelines of the most rigid experimentalism, no methodological, theoretical or philosophical discussion whatsoever.

Whilst, on that same journal, Agostino Gemelli was starting to outline his vibrant critique against the mechanical philosophy in biology, physics was only seen within the framework of a “severe apologetic method” (Minutes of the meeting of the Society 1903, 315), opposed to that of “polemic apologetics”, a method based on the idea that no contraposition between faith and scientific truth was possible. It defined itself as a “positive” search for scientific truths, which would eventually – without any mediation through a philosophical and theological analysis – defeat those who wanted to turn science into an instrument against religion. The “arrogant wielders” of science were making the mistake of paying too much attention to shaky hypotheses that were destined to collapse when exposed to the test of facts. Materialists and positivists based their ideas on “castles in the sky” (Tuccimei 1903) that a really positive search would have caused to crumble. The Catholics were called to the search for the truth revealed by experience.

Convinced that “facts are divine and theories are human and therefore subject to mutations and also extinction” (Alasia 1904, 511) (in support to this thesis, Duhem’s early historical works were inaptly quoted), the editors of the journal avoided all discussion about theories and therefore about the philosophical conclusions that could be drawn from them. The “positive” realm of facts was only abandoned for obituaries of Catholic scientists or the publication of some clergymen’s contributions to science and technology.

This lack of interest for the great theoretical and methodological questions translated into very casual descriptions of key events in the history of the relationship between science and faith, such as the problems of the Copernican revolution or Galileo. In the name of the supreme value of facts, the journal offered studies that would never induce an unknowing reader to suspect that the Church ever stood against science.

There were only rare articles proposing religious beliefs, rather than facts, as the judge for scientific theories. The position of the journal’s director, Monsignor Maffi, seems to have had little influence: he postulated the faith in the wise doing of a perfect Maker of the universe as the grounds to reject theories, basically in the name of an unclear idea of simplicity. Maffi expressed himself vehemently against Positivism, which, by separating science from faith and giving it autonomy, had turned it into an inert and lifeless scheme, unable to relate to men and mean something to them.

Such reprimands are unlikely to have seemed attractive to the majority of the journal’s collaborators. The positive *apologia* promoted by the journal was based on the idea that sooner or later a vision of nature that was fully coherent with the religious dogmas would emerge from the pursuit of factual truth. And the journal’s pages were full of that “cold”, “silent” and “lifeless” science Monsignor Maffi was so strongly against.

Needless to say, Duhem’s refined epistemology-based apologetics found no space whatsoever either among the adorers of the “fact” (a notion Duhem had destroyed), or in the ideas of Maffi, who chose religion as the judge of scientific theories, whereas Duhem had separated the two areas and established they were to be connected only with the help of history.

This overview, short as it may be, should have demonstrated how the Italian philosophical circles were inherently unfit for studying and appreciating Duhem’s thought. For different reasons, Idealism, Positivism, Pragmatism, the De Sarlo group and the Catholics were all travelling on rails that could never cross paths with Duhem’s complex philosophy. However, not only philosophers were to blame. Italian scientists must be held responsible as well, as they did nothing to highlight the debates that were taking place among their French colleagues. After all, why should philosophers have been aware of Poincaré, Duhem, etc., while scientists were not? The Italian scientific circles neglected almost completely the scientific theories that served as the backdrop for the epistemological debate, namely Maxwell’s electromagnetic theory – which introduced the question of mechanical models – and thermodynamics – which exposed the unsustainability of the mechanical philosophy. Without this scientific background, the thoughts of Poincaré or Duhem lose all meaning and the philosophers’ lack of interest for them becomes understandable, if not excusable. However, the story of the Italian scientific community in those years is another story.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 93-107

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Was Pierre Duhem an *Esprit de finesse*?

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Abstract:

Although Pierre Duhem is well known for his conventionalist outlook and, in particular, for his critique of crucial experiments outlined in his thesis on the empirical indeterminacy of theory, he also contributed to the scholarship on the psychological profiles of scientists by revising Pascal's famous distinction between the subtle mind and the geometric mind (*esprits fins* and *esprits géométriques*). For Duhem, the ideal scientist is the one who combines the defining qualities of both types of intellect. As a physicist, Duhem made important theoretical contributions to the field of thermodynamics as well as to the then-nascent physical chemistry. Due to his rejection of atomism and his unrelenting critique of Maxwell's electrodynamics, however, in his later years, Duhem's work was surpassed and abandoned by the dominant tendencies of physics of the time. In this essay, I will discuss whether Duhem himself can be understood through the lens of his own account of the scientist's psychological profile. More specifically, I examine whether the subtle mind – to which he seems to assign greater cognitive value – in fact plays a key role in Duhem's critique of the English School (*école anglaise*), or if his preference for the axiomatic structure of theoretical physics shows a greater affinity with the geometric mind.

93

Keywords:

Pierre Duhem; subtle and geometric minds; abstract and axiomatic theories; physical theory

Received: 30 March 2017. Reviewed: 15 May 2017. Accepted: 30 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.09>

Introduction

Perhaps the most overlooked, among Pierre Duhem's diverse contributions to the understanding of the sciences, is his study of the psychological dimensions of scientific practice based on his approach to the Pascalian distinction between the subtle mind and the geometric mind (*esprit de finesse* and *esprit de géométrie*). There are several reasons for this oversight, but the most evident comes from the point of view of classical philosophy of science, since the emphasis made on the distinction between the context of discovery and the context of justification left aside historical, sociological, and psychological features of scientific practice to focus solely on the logic of justification. Another, more precise, reason is the belief that Duhem makes use of Pascal's dichotomy to settle scientific matters by means of nationalistic prejudices. By the other side, Duhem's appropriation of the Pascalian distinction seems difficult to hold because one finds

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problems to determine whether we are deal with a sharp and fundamental distinction, or whether Duhem makes informal use of it in order to support his view about the value of theoretical physics as abstract theory. Taking the latter interpretation allows us to deal with the inconsistencies that stand out when we closely examine the way in which Duhem reworks that distinction to discuss the scientific contributions of what he calls *l'école anglaise* (English School) in contrast to his argument about the German and French way to build the physical theory in his late “war writings.” In what follows, I will compare and contrast Duhem’s use of the Pascalian distinction in his treatment of the theoretical practice of the English School in *La théorie physique* as well as in his early writings on the subject; namely, in his review of the French translation of William Thomson’s papers. I argue that, although he claims that both modes of thought coexist at the heart of the scientific community, and that the improvement of theoretical physics renders impersonal its findings (cf. Duhem 1987a [1893a], 144; 1915, 103), Duhem’s philosophical and methodological papers exhibits an unquestionable preference for the subtle mind. Then, I will examine whether Duhem’s theoretical practice coincides with his own account of the subtle mind, or if, on the contrary, it ultimately corresponds with the geometric mind. Finally, I hope that this essay sheds light on other aspects of Duhem’s thought that may be worth revising.

Modes of Thought

There are several approaches to understand how human creativity works, but all of them share a certain parallelism that makes it possible to reduce them to a kind of cognitive dualism. The most popular among these approaches is by Isaiah Berlin (1953, 1), found in a fragment by the ancient poet Archilochus: “The fox knows many things, but the hedgehog knows one big thing.” This analogy is quite productive, allowing us to categorize the intellectual world into those who are guided by a single regulatory principle – at most a handful – and those who make use of all kinds of assertions without a concern for internal consistency as long as they achieve their intended goal.

Those with a hedgehog’s mentality need order and a system; whereas, on the other hand, those who possess a fox’s mentality can navigate – without difficulty – in a sea of information without details, for that matter, disregarding their intended goal. This distinction is not exclusive. We need not assume we are dealing with a sharp distinction, since Berlin makes use of it only as a guiding principle to locate the salient features of specific thinkers by classifying them as either foxes or hedgehogs. Thus, he characterizes Aristotle, Montaigne, and Erasmus as foxes, and Plato, Lucretius, Pascal, and Hegel as hedgehogs. There are, indeed, those who dream of being of the opposite mindset despite their nature. Thus, for example, in his early years, Wittgenstein – a fox by nature – thought of himself as a hedgehog. The reverse, however, seems implausible, if not impossible. It is for this reason that, for James (1981 [1907]), far from being a purely intellectual matter, a distinction of this kind is a matter of temperament; this despite the fact that the majority of us are incapable of possessing a well-defined intellectual temperament (in this case, we are only ordinary people). With regard to philosophical inclinations, for James, one is either an empiricist or a rationalist according to his temperament, not by choice. In other words, we do not choose to be foxes or hedgehogs, we simply are one or the other.

If we put Berlin’s distinction in Jamesian terms, a fox would be, by definition, an empiricist, a lover of crude facts and would, therefore, be of a rough mindset. Whereas the hedgehog would always be a rationalist, a lover of abstract principles and therefore, his or her mind would be subtle. Here, nonetheless, James performs a sleight of hand insofar as he makes use of the known Pascalian distinction between the subtle mind and the geometric mind. Moreover, since James was familiar with Pierre Duhem’s oeuvre, he takes from it what better suits his pragmatist character.²

Having said that, although there are commonalities between the overall methods of each of these mindsets, for Duhem, the Pascalian distinction serves, first and foremost, the explicit purpose of

² In the fifth Lowell lecture, entitled “Pragmatism and common sense,” James (1981 [1907], 86) writes: “Just now, if I understand rightly, we are witnessing a curious reversion to the common sense way of looking at physical nature, in the philosophy of science favored by such men as Mach, Ostwald and Duhem. According to these teachers no hypothesis is truer than any other in the sense of being a more literal copy of reality. They are all but ways of talking on our part, to be compared solely from the point of view of their *use*. The only literally true thing is *reality*; and the only reality we know is, for these logicians, sensible reality, the flux of our sensations and emotions as they pass”.

differentiating the way English physicists conceive of physical theory from the French and German view. However, this theoretical articulation does not appear in Duhem's work until the later period of his intellectual development, as we do not find aspects of this in his early writing in what will serve as the basis for the chapter that makes up the first part of *La théorie physique* – a chapter he devotes to abstract theory and mechanical models.

Before delving into an examination of such transformation in Duhem's thought, it is appropriate to take a moment to consider the historical context that gives rise to and explains some of its most prominent features. To a certain extent, it is here that we can locate a turn from Duhem the physicist to Duhem the methodologist or philosopher of physics – or, to put it in contemporary terms, to a physicist's explicit formulation of his *scientific philosophy*.³

We can wonder, however, whether we are dealing with changes in the particular intellectual orientation of a specific physicist or with a mode of thought common to a transitional period in the establishment of a new field of knowledge. In order to answer this question, let us turn to the distinction Holton appropriates from Nietzsche to reestablish the debate about public image of science during the second half of the twentieth century.

The New Apollonians and Dionysians

Following the dominant standpoint of our current scientific framework – reaffirmed by Kuhn's contributions – today, most members of the scientific community ignore the epistemological questions that emerge at the heart of scientific practice; and when they do address them, it is only as a pastime not unlike stamp collecting or heraldry. In the new division of intellectual labor, which brought about a reconfiguration of knowledge in the twentieth century, the ones in charge of understanding and defending scientific practice are professional philosophers of science like the Logical Positivists, Karl Popper and his predecessors, and the current naturalized philosophers. Holton calls these *New Apollonians*.⁴ On the other hand, we find the critics of science, who question its reductionism and its complex relations with power and the industry. Holton calls these *New Dionysians* or *Neodionysians*. Both Neodionysians and New Apollonians enjoy a degree of recognition among broad sectors of society. They also exert some pressure on the scientific community, although the latter pays little attention to their claims and demands. Holton's essay itself is a rare exception, and perhaps he owes his reputation as an outsider with an understanding of science to the fact that, since the beginning of his academic career, he was associated with one of those New Apollonians who rose from the ranks of the Vienna Circle, namely Philipp Frank. At the same time, it appears that this very proximity made Holton lose sight of the fact that the first generation Apollonians were either scientists in their own right or thinkers trained in some branch of science. It is not difficult to see, then, that Frank belongs to that lineage of philosopher-scientists who contributed to the stability of theoretical physics toward the end of the nineteenth and beginning of the twentieth centuries.⁵

During this transitional period, the quest for a disciplinary identity engenders a debate among physicists themselves: they argue explicitly on the scope and value of their conceptual elaborations; they establish the boundaries of experimental physics in light of the limits of theoretical physics; and they resort to ingenious metaphors to explain the relationship between the two subdisciplines. Thus, for example, Poincaré compares physics with a library that is constantly growing, where experimental physics is in charge of acquiring new books (i.e., facts), while mathematical physics is in charge of composing the catalogue

³ Although we may think it was Abel Rey who, in 1904, coined the term *philosophie scientifique* to refer to Duhem's conception of science, the phrase was already in use several years before and can be found, for example, in Paul Tannery's reviews in *Revue philosophique de la France et de l'étranger*.

⁴ Holton (1978, 102) writes: "The philosophers who have taken it on themselves to protect rationality in the narrowest sense of the word are also members of a long tradition. Some of their genes can be traced back to the logical positivists of the pre-World War II period, who are themselves descended from a long line of warriors against the blatant obscurantism and metaphysical fantasies that haunted and thwarted science in the nineteenth and early twentieth centuries."

⁵ Cf. Laszlo Tisza's report on Frank's undertakings as a physicist, whom he regarded more as a philosopher of science, or, in the best-case scenario, as a philosopher of physics (cf. Blackmore, Itagaki and Takana 2001, 68-69).

(and therefore, is the one responsible for grouping and categorizing facts);⁶ meanwhile, Duhem (1987a [1894]) makes sure to point out that there are no experimental observations devoid of theory nor crucial experiments.⁷

Nonetheless, we can easily lose sight of the relevance of metatheoretical questions once the disciplinary domain has been fully delineated. As Bordoni (2012) and others have argued, we cannot have a suitably clear idea of Duhem's contributions to theoretical physics if we do not take into account the role metatheoretical considerations play in the process of institutionalization of the discipline. I argue, however, that the process of institutionalization in question has different characteristics from those elucidated in previous scholarship on Duhem. According to Roberto Maiocchi (1990, 386), for example, "It is not the crisis of science, but its successes which impose upon Duhem the necessity of epistemological reflection."⁸ Broadly speaking, by the *success* and *crisis* of physics (and chemistry), Maiocchi refers to what is usually catalogued under the so-called internal history of science, whereas the process I have in mind corresponds, more or less, to its external history. That being said, however, I do not find the distinction between internal and external histories adequate to describe the complex interrelations that took place among physics, philosophy, and the public image of science of the time, and, in particular, in the organization of science during the *Troisième République* (Third Republic), as well as in the preceding, chaotic decades. For a number of reasons, it is a mistake to speak of the success of the discipline in the last decade of the nineteenth century, except for in hindsight, since its physiognomy was actually determined at the time if we consider, for example, that rational mechanics was regarded as a branch of mathematics while, previously (before Maxwell's theory), other branches were regarded as unrelated – as was the case with electric and optical phenomena. This lack of disciplinary cohesion manifests itself in different ways in the processes of institutionalization. Suffice it to say that in the Netherlands there were only two university chairs in theoretical physics until well into the twentieth century. As far as France is concerned, we may recall the decades of theoretical scarcity that separate Fresnel, Ampère, Cauchy, and Fourier from Poincaré and Duhem (cf. Buchwald and Hong 2003). On the other hand, the turn of the century witnessed an increased interest in science among the general public, which did not go unnoticed for scientists and philosophers (the Apollonians and Neodionysians, to use Holton's terminology), insofar as they described it as the "bankruptcy of science" (*faillite de la science*).

This lack of perspective is also evident, for example, in one of the first English commentaries on Duhem's oeuvre. In the last chapter of *The methodology of Pierre Duhem*, under the section entitled "Critical remarks and conclusions," Armand Lowinger (1967, 163) states:

The fundamental idea guiding our criticism is the modest role which we conceive methodology to play vis-à-vis science. Methodology takes science for granted and is essentially a description of the scientific process. With regard to every question, therefore, which arises concerning the scope and meaning of science, it always has to keep a weather eye on the actual scientific process as it is carried on in the laboratory and in the study of the scientific theoretician and to give as faithful an account of it as possible. It must explain the scientific process, not explain it away by some sort of verbalistic or conceptual legerdemain; it must follow after science, not attempt to dictate or domineer science.

Indeed, Lowinger's remarks make sense once the disciplinary field achieves a considerable degree of institutionalization and normalization. Furthermore, as noted above, Holton and Lowinger – as is, and can only be, the case for most scientists – view methodological questions as *a posteriori* to scientific practice itself, and, therefore, tend to display a strong bias against normative approaches in the philosophy of science.

⁶ He (1905, 144) concludes: "If the catalogue is well done the library is none the richer for it; but the reader will be enabled to utilize its riches". "Si ce catalogue est bien fait, la bibliothèque n'en sera pas plus riche. Mais il pourra aider le lecteur à se servir de ces richesses" (Poincaré 1905, 160).

⁷ According to Maiocchi (1990, 392), the main concern of Duhem's reflections on the nature of physical theory was to critique the empiricist basis of the positivist conception of science, but it is difficult to ascribe such conception even to Comte, as Elias (1978, ch. 1) and Laudan (1981, ch. 9) have rightly pointed out.

⁸ For a critical, albeit sympathetic, analysis of Maiocchi's preceding study on which this essay is based, refer to Stoffel (2002, 87-94).

This is not, however, a critique that affects Duhem since his reflections on theoretical physics rest on a careful analysis of scientific practice based on several schools of thought, and since they do not aim to regulate the practice of physics, at least not explicitly. On the other hand, Lowinger is right in subsuming methodological questions under the category of metaphysics and in pointing out that Duhem's thesis on the autonomy of physics excludes any attempt at legislating methodological matters.⁹ As I will explain below, Duhem *feels* that methodological controversies will fade away with time once common sense becomes the *bon sens* of the scientific community.

The English Scientific Practice of the Nineteenth Century

As noted above, in his early analysis of English science, Duhem does not employ the famous Pascalian distinction between the subtle and geometric minds; however, he does hold that its defining qualities – those that set the English apart from the French and German scientists – help us identify the fundamental character of the English mentality (*esprit*). For example, English scientists stand out in their striking ability to imagine complex sets of countless, concrete objects, without losing sight of the place each of these occupies and the relations they have with each other. Thus, rarely do English scientists engage in more abstract research, and when they do, the results tend to be unsatisfactory. This approach is found equally among writers, philosophers, and scientists. When we focus on the activity of the English theoretical physicists, the first thing that stands out, Duhem argues, is the use of what they call a “model.” Unlike the abstract theory of German and French physicists, models allow us to establish a mental image of the phenomena in question. Nonetheless, the English scientists' insistence on the construction of models leads them to equate theory with the models themselves, which is evident in W. Thomson's (Lord Kelvin) assertion that it is only by means of the creation of models that we can understand physical phenomena. However, the English School's notion of model should not be equated with the abstract notion of a mathematical model employed in contemporary science; after all, Duhem was primarily concerned with the use of mechanical models; that is, with representations that imitate or simulate the phenomenon in question in a mechanical fashion, such that “understanding the nature of material things will be the same thing as imagining a mechanism that will represent or simulate the properties of bodies by its action.” (Duhem 1996 [1893], 55)¹⁰ As Duhem also notes, it is not the insistence on the mechanical representation of phenomena that sets the English School apart, but rather the particular manner in which it brings about this aim by means of models. In the young Duhem's budding, positivist interpretation of the history of physics, mechanistic explanations epitomize the triumph of the imagination over reason, or, as he claims later, of the subtle mind over the geometric mind, of modern science over the rationalist metaphysics of Scholasticism:

If Descartes and the philosophers who followed him refused to admit the existence of any property of matter not reducible to geometry or kinematics, it is because any such quality would occult, and, being conceivable only by reason, it would remain inaccessible to the imagination. The reduction of matter to extension by the great thinkers of the seventeenth century showed clearly that during that period, the metaphysical sense, exhausted by the excesses of scholasticism during its decadence, entered into the decrepit state in which it still languishes today. (Duhem 1996 [1893], 55-56)¹¹

It may be worth recalling that this brief, historical observation squares with Whitehead's later interpretation in *Science and the modern world*, wherein he argues that it would be wrong to regard Galileo's

⁹ In “La valeur de la théorie physique”, Duhem (1991 [1954], 334) sharply remarks: “The study of the method of physics is powerless to disclose to the physicist the reason leading him to construct a physical theory.”

¹⁰ “[C]omprendre la nature des choses matérielles, ce sera imaginer un mécanisme dont le jeu représentera, simulera, les propriétés des corps.” (Duhem 1987a [1893a], 119)

¹¹ “Si Descartes et les philosophes qui l'ont suivi ont refusé d'admettre l'existence de toute qualité de la matière qui ne se réduisait pas à la géométrie ou à la cinématique, c'est parce qu'une telle qualité était *occulte*; parce que, concevable seulement par la raison, elle demeurait inaccessible à l'imagination; la réduction de la matière à l'étendue par les grands penseurs du XVII^e siècle montre clairement qu'à cette époque le sens Métaphysique, épuisé par les excès de la Scolastique en décadence, entrainé en cet état de décrépitude où il languit encore aujourd'hui.” (Duhem 1987a [1893a], 119-120) Cf. Duhem 1906, 115.

natural philosophy as a revolt of reason against the dark forces of tradition, since, on the contrary, and as Galileo's friend Paolo Sarpi's account of the Council of Trent demonstrates, his was an anti-intellectualist movement in line with the anti-metaphysical attitude of the Counsel.

Duhem will further elaborate his account of the Cartesian conception of physics in the first chapters of *La théorie physique*, underscoring the explanatory (metaphysical) aspects underlying the system (especially in the *Optics*) – even when, for Duhem, these aspects are dispensable from the representational or logical point of view of theoretical physics. Likewise, in an essay published between the aforementioned texts entitled “L'évolution des théories physiques, du XVII^e siècle jusqu'à nos jours” (1896), Duhem situates Descartes' theory as an important development in physics insofar as it overcomes the hidden entities of the physics of the preceding era and incorporates the theories of the English School as a form of Neocartesianism, or as a partial return to it. But these remarks do not prevent Duhem from noting that Cartesian mechanics is ultimately false; nor will they prevent him from foreseeing, without much success, that this *cartésianisme nouveau*, like its predecessor, will render “the mind [...] discouraged by the complexity, the bizarreness, the arbitrary and far from natural ways, by the improbable combinations which it employs in ‘constructing the world machine.’” (Duhem 2002, 209)¹² This position, founded on sentiments and not on logic, represents an improvement from the ambivalent assessment evident in his critical review of W. Thomson's physics, and of the English School more generally.

For now, I hasten to note that the distinction between the subtle mind and the geometric mind that Duhem will employ in the well know chapter of *La théorie physique* does not appear in this essay. In *La théorie physique*, Duhem revises his initial views with the intention of offering a systematic exposition of the aim of theoretical physics. Nonetheless, it is important keep in mind the position that Duhem outlines in the early texts with regard to the transitory character of English physics, which he attributes to its *arbitrary* character and lack of natural ways – features that differ from the characteristics proper to the subtle mind, but which also constitute aspects of the physical theory conceived as abstract representation. Is there, then, an evident contradiction in Duhem's outline here? Scholars like Martin (1991, 107-108) argue that Duhem fell prey to imprecisions and shortcomings due to his approach to writing and revising his early texts. For this reason, it would be important to determine whether Duhem himself noticed these flaws, given that clarifying this issue would be crucial to determine whether Duhem was a subtle or geometric mind.

Hermeneutical Perplexities in Duhem's Realism and Conventionalism

Aside from whether we are dealing with a mistake in exposition or with a more profound inconsistency, it is unquestionable that the verification of that fact leads to the establishment of hermeneutical warnings and precautions regarding the scope of what I am outlining in this essay; but neither should we lose sight of the particular context of Duhem's claims if we wish to eliminate readings that contribute to an increase in perplexity. In order to try to understand Duhem himself, it may be necessary to appeal to the hermeneutical criteria Pascal (1910, 684) sketches in one of his well-known reflections: “We can only describe a good character by reconciling all contrary qualities, and it is not enough to keep up a series of harmonious qualities without reconciling contradictory ones. To understand the meaning of an author, we must make all the contrary passages agree.”¹³

As I have noted before with regard to the contemporary scientific understanding of Duhem's methodological inquiries, it is equally important to know what it is exactly that Duhem opposes when he affirms that the end or purpose of the physical theory is to represent experimental laws, and not to explain them. Besides, when we undertake a contemporary reading of this assertion (that is, presupposing a current meaning of *explanation*), we arrive at the conventionalist conception usually attributed to Duhem. Nonetheless, when someone does that, she or he overlooks the fact that the representational and explicative

¹² “À rebuter l'esprit par la complication, par la bizarrerie, par l'allure arbitraire et peu naturelle, par l'in vraisemblance des combinaisons qui lui servent à ‘construire la machine du monde.’” (Duhem 1987a [1896], 228) Cf. Duhem 1987a [1893b], 82.

¹³ In the original: “On ne peut faire une bonne physionomie qu'en accordant toutes nos contrariétés et il ne suffit pas de suivre une suite de qualités accordantes sans accorder les contraires. Pour entendre le sens d'un auteur il faut accorder tous les passages contraires.” (Pascal 1963, 257)

character of the physical theory runs parallel to the distinction between physics and metaphysics underpinning the famous thesis on the independence of theoretical physics from metaphysics. In addition, according to Duhem, she or he also forgets that the elimination of the explanatory element is not a matter of methodological normativity, but rather a historical stage in the development of theoretical physics. In my opinion, this is evident when in the early paper on the English School, Duhem describes and confronts the manner in which W. Thomson appeals to the imagination and not to reason when representing the properties of the elements involved in the phenomena in question. These elements are named after objects present in everyday life, and their properties (e.g. fluidity and condensation) behave in the same manner that do normal liquids and air. Generally speaking, “their nature does not need to be defined philosophically. It suffices that their properties fall under senses. The mechanisms they serve to make up are not destined to be grasped by reason; they are destined to be seen by the imagination.” (Duhem 1996 [1893], 57)¹⁴ For this reason, Duhem points out that the physics of English scientists is the physics of engineers; whereas, on the other hand, the physics of Continental scientists is usually philosophical. To use the productive metaphor of *La théorie physique*, when we delve into English physics, “we [think] we [are] entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory.” (Duhem 1991 [1954], 71)¹⁵

It would be a mistake to claim that Duhem is contradicting himself when he argues that “the English School has thus acceded entirely to purely mechanical explanations of physical phenomena”, (Duhem 1996 [1893], 55)¹⁶ or when he states, “this predilection for explanatory and mechanical theories is, of course, not a sufficient basis for distinguishing English doctrines from the scientific traditions thriving in other countries.” (Duhem 1991 [1954], 72)¹⁷ To frame the issue as a question: can we legitimately read the term *explanation*, presupposing the meaning that Duhem gives to metaphysical explanation, which he had previously rejected? In my opinion, we cannot do so, just as we cannot equate abstract reason with the metaphysical reason of Scholasticism, or geometric reason with the pure reason that conceives hidden causes. When we assign a rigid and exclusive meaning to the notions of “conventionalism” and “realism” these apparent contradictions inevitably leave us with a reading of Duhem marked by false dilemmas. Hence, we cannot reconcile all the paradoxes that emerge when the two notions are used as opposites. The same can be said about the subtle mind and the geometric mind, since, while we can have a sense of what Duhem means when, in *La théorie physique*, he claims that the way of conceiving the English theory of physics corresponds to the broad mentality, or subtle mind, this does not mean that there are no geometric minds who foster the creation of abstract theories among the English scientists, as is in fact the case with Rankine.

It may be the case that the clarification of these concepts would suffice to answer the question posed as title of this paper in the affirmative; nonetheless, it is evident that the relationship between the English School and the subtle mind has, in *La théorie physique*, a negative connotation – one of rejection – and which differs from the positive connotation he give it in *La science allemande*, wherein he links it to French science.

The Philosophical Dimension of Abstract Theory

As stated before, we do not find the references to Pascal of the late writings in Duhem’s early methodological writings, nor is there an appeal to the distinction between the subtle mind and the geometric mind, which appears for the first time – albeit in a rather implicit manner – in 1902, with the publication of *Le mixte et la combinaison chimique*. The only foregoing, explicit “philosophical” reference to the distinction can be found in his essay on the development of the theory of physics, and in that case, only to further support Duhem’s

¹⁴ “Leur nature n’a pas besoin d’être philosophiquement définie; il suffit que leurs propriétés tombent sous les sens; les mécanismes qu’ils servent à composer ne sont pas destinés à être saisis par la raison, ils sont destinés à être vus par l’imagination.” (Duhem 1987a [1893a], 122) Cf. Duhem 1906, 118.

¹⁵ “[...] nous pensions entrer dans la demeure paisible et soigneusement ordonnée de la raison déductive; nous nous trouvons dans une usine”. (Duhem 1906, 111)

¹⁶ “[...] l’École anglaise est donc acquise entièrement aux explications purement mécaniques des phénomènes physiques.” (Duhem 1987a [1893a], 119)

¹⁷ “Cette prédilection pour les théories explicatives et mécaniques n’est pas, assurément, un caractère qui suffise à distinguer les doctrines anglaises des traditions scientifiques qui fleurissent en d’autres pays.” (Duhem 1906, 114)

negative valuation of English physics discussed above.¹⁸ When he argues, however, that a unyielding sentiment leads him to think that Thomson's and Maxwell's theories cannot be adequate, he is implicitly appealing to a distinctive quality that he will associate with the *esprit de finesse*; this time, not only in *La théorie physique*, but more importantly, in the texts that make up *La science allemande*. However, in the former context, this intuition turns out to be somewhat paradoxical since this sentiment lends support to the conception of the theory of physics as abstract representation of experimental laws. As Duhem argues in this essay, however, this approach is first and foremost logical, philosophical, and metaphysical, while the English School's conception is imaginative, anti-metaphysical, and thus, practical. In order to show this, he notes that Thomson does not pose any philosophical problem (e.g. whether the resulting elements of matter can occupy varying volumes, that is, if they can be condensed) since their approach to build mechanical models is not structured to be grasped by reason, but rather by the imagination (cf. Duhem 1987a [1893a], 122). This is the anti-metaphysical feature of English physics. In short, English physics lacks a *cosmology*.¹⁹

In the case of mathematical tools, the logico-philosophical nexus is linked to the process of abstraction employed to determine concepts in physical theory; however, in the case of the English School's mechanical models, algebraic analysis is readily available to represent relations established in the model without a concern for the existence of an analogy with the actual properties of bodies; that is to say, whether or not the algebraic magnitudes correspond to real elements. By the same token, there is no concern for the logical origins of equations. In fact, the fundamental differences between both conceptions about the physical theory can be figure out whether the theory exhibits or not an axiomatic structure. For example, Duhem reproaches the chaos Maxwell introduces into electrodynamics when he determines the behavior of dielectric bodies by means a new element – namely, the displacement current –, which Duhem views as strange and lacking in adequate characterization. In sum, the lack of definitions of the new electromagnetic elements, by means of axioms and postulates, makes us think that English theory “c'est le système des équations de Maxwell.” (Duhem 1987a [1893a], 126)²⁰

On the contrary, axiomatic abstract theory as conceived by the German and French scientists of the time satisfies – through the rigorous, logical sequencing of all its elements – the criteria of unity, order, and

¹⁸ Cf. note 11. “An invincible sentiment warns us that matter cannot be constituted as W. Thomson and Maxwell imagine, and we are tempted to agree with Pascal: “This is all ridiculous; for it is all useless, uncertain and laborious.” (Duhem 2002, 209) In the original: “Un sentiment invincible nous avertit que la matière ne saurait être faite comme l'imagine W. Thomson ou Maxwell, et nous sommes tentés de nous écrier avec Pascal: ‘Tout cela est ridicule; car tout cela est inutile, et incertain, et pénible’” (Duhem 1987a [1896], 228). In his review of Leray's *Essai sur la synthèse des forces physiques*, Duhem (2006, 19) had already cited said aphorism, but he immediately points out that “[Pascal] carefully retains this useful and practical consequence of Descartes's system, the refusal to explain every natural effect by inventing a new propriety, a special virtue.” In the original: “Retient soigneusement cette conséquence utile et pratique du système de Descartes qui se refuse à expliquer chaque effet naturel par l'invention d'une propriété, d'une vertu special.” (Duhem 1987a [1893b], 66)

¹⁹ In his reply to the critique launched by the Thomist Eugène Vicaire to his essay on the subject of physical theory, Duhem (1996 [1893], 30) clarifies the modern meaning of the distinction between physics and cosmology as follows: “To conform to contemporary usage, we give the name *physics* to the experimental study of inanimate things, considered in three phases: the observation of facts, the discovery of laws, and the construction of theories. We regard the investigation of the essence of material things, insofar as they are causes of physical phenomena, as a subdivision of *metaphysics*. This subdivision, together with the study of living matter, forms *cosmology*.” In the original: “Nous nommons *physique* l'étude expérimentale des choses inanimées envisagée dans ses trois phases: la constatation des faits, la découverte des lois, la construction des théories; nous regardons la recherche de l'essence des choses matérielles en tant que causes des phénomènes physiques comme une subdivision de la *métaphysique*, subdivision qui forme, avec l'étude de la matière vivante, la *cosmologie*.” (Duhem 1987a [1893c], 85). On the relationship between this essay and his review of the English school, see Leite (2006, section 2.2), and, more broadly, Leite (2016).

²⁰ Few lines before, he writes: “Maxwell studies the transformation of the equations of electrodynamics in their own terms, most often without seeking to see behind his transformations the coordination of physical laws. He studies them as one examines the movements of a mechanism. This is why is a futile effort to seek behind these equations a *philosophical idea* which is not there.” (Duhem 1996 [1893], 60) The original reads: “Maxwell étudie en elles-mêmes les transformations des équations de l'électrodynamique, sans chercher le plus souvent à voir sous ces transformations la coordination des lois physiques; il les étudie comme on regarde les mouvements d'un mécanisme; voilà pourquoi c'est un labeur illusoire de rechercher, sous ces équations, *une idée philosophique* qui n'y est pas.” (Duhem 1987a [1893a], 126; my italics)

simplicity proper to deductive reasoning. These epistemological criteria define the philosophical dimension of the physical theory conceived as an abstract theory;²¹ however, once Duhem reworks this essay and includes it in *La théorie physique*, this dimension eventually disappears and is substituted by the association of abstract theory with the geometric mind (which before only appears as '*les facultés logiques de l'esprit*'), and by the economic conception of scientific thought.

There are two reasons that may have motivated these modifications. The first is the definitive disassociation of the axiomatic structure of the physical theory from cosmological presumptions, which, within the mechanistic tradition, were linked to the deductive capacity of abstract theory. This separation constitutes an acknowledgment of the limits of his science on the part of the physicist, an acknowledgment that emerges from the ephemeral character of the cosmological element within the development of physics, since, as Duhem argues in *La théorie physique*, everything that is good within a physical theory can be found in its representational components, while its unstable and sterile elements are found in its explanatory components. Or as Duhem asserts more emphatically:

What is lasting and fruitful in these is the logical work through which they have succeed in classifying naturally a great number of laws by deducing them from a few principles; hat is perishable and sterile is the labor undertaken to explain these principles in order to attach them to assumptions concerning the realities hiding underneath sensible appearances. (Duhem 1991 [1954], 38)²²

We should point out, albeit briefly, that this fundamental feature of the growth and development of physical theory suffices to overthrow any simplistic and untenable idea about the accumulative character of physics in Pierre Duhem's thought since what is currently referred to as a scientific revolution would be nothing other than the substitution of cosmological components – which given their own explicative nature are, for their protagonists, as dramatic as they are incommensurable. Nevertheless, cosmological components are not the only factor under consideration, since other elements related to the representation of phenomena come into question; for example, the emergence of new discoveries or the difficulty in assigning magnitudes to physical properties – aspects that fall outside the field of competence of logical analysis, and that, therefore, refer back to the imagination or to intuition. Moreover, as Crowe (1990) notes, in *La théorie physique*, Duhem opposes the development of the physical theory to the properly accumulative development of mathematical theories.

As I mentioned above, the notion of abstract theory that Duhem has in mind refer to the axiomatic structure of the physical theory, and the ideal of such structure is still the system explained in Euclid's *Elements*. This is sufficiently evident when he claims that nothing keeps us from providing Maxwell's equations with an appropriate axiomatic formulation in the future:

No doubt what is exact and truly fertile in the work of Maxwell will one day take its place in a coherent and logically constructed system, in one of those systems in which thoughts are conducted in order, in the image of Euclid's *Elements*, or of those majestic theories unfolded by the creators of mathematical physics. (Duhem 1996 [1893], 64)²³

²¹ "Without doubt, all branches of pure and applied mathematics treat concepts that are abstract. It is abstraction that furnishes the notions of number, line, surface, angle, mass, force, temperature, and quantity of heat or electricity. It is abstraction, or *philosophical analysis*, that separates and makes precise the fundamental properties of these various notions and enunciates axioms and postulates." (Duhem 1996 [1893], 58) In the original: "Sans doute, toute branche des mathématiques pures ou appliquées traite de concepts qui sont des concepts abstraits; c'est l'abstraction qui fournit les notions de nombre, de ligne, de surface, d'angle, de masse, de force, de température, de quantité de chaleur ou d'électricité; c'est l'abstraction, c'est l'*analyse philosophique* qui démêlent et précisent les propriétés fondamentales de ces diverses notions, qui énoncent les axiomes et les postulats." (Duhem 1987a [1893a], 123; my italics).

²² In other words: "Ce qui, en elle, est durable et fécond, c'est l'oeuvre logique par laquelle elles sont parvenues à classer naturellement un grand nombre de lois en les déduisant toutes de quelques principes; ce qui est stérile et périssable, c'est le labeur entrepris pour expliquer ces principes, pour les rattacher à des suppositions touchant les réalités qui se cachent sous les apparences sensibles." (Duhem 1906, 57-58)

²³ In the original: "Sans doute, ce qu'il y a d'exact et de vraiment fécond dans l'oeuvre de Maxwell prendra place, un jour, dans un système cohérent et logiquement construit, dans un de ces systèmes où les pensées sont conduites par ordre, à l'image des *Eléments* d'Euclide ou de ces majestueuses théories que déroulaient les créateurs de la physique

Duhem's own scientific endeavors usually exhibit an axiomatic structure and grant high esteem to those who, like Gibbs and Helmholtz, proceed in similar fashion. For example, in his "Commentaire aux principes de la thermodynamique" (1892-1894), Duhem justifies his axiomatic treatment of theory by arguing that a return to the foundations allows us to evaluate the degree of development a theory has achieved in an extended period of time, and to predict new consequences, but also to overcome the obstacles that have accumulated during a given period.²⁴ Donald Miller (1970, 229) has claimed that the axiomatic outlook that Duhem employs with regard to the first law of thermodynamics was novel in physics while it simultaneously anticipated, to some extent, the inquiry into the foundations of mathematics that took place at the turn of the century. Yet this claim is an exaggeration with regard to the work on the foundations of mathematics since Duhem's axiomatic outlook is, in fact, informal (or intuitive) with respect to the initial definitions because they do not present themselves in symbols, and there is no trace of the distinction – even in a primitive form – between the language-object and the metalanguage by means of which the axiomatization takes place.²⁵ In short, he does not sketch a method to address the mathematical problems proper to axiomatization, such as the nature of rudimentary terms, the independence and self-sufficiency of a given cluster of axioms, or the consistency and comprehensive nature of the system.²⁶ But neither would he have motivations to do so, since, although he was awarded the degree of Doctor in Mathematics, he did so with a dissertation on the theory of physics – focusing on "magnetism by influence" (*aimantation par influence*)–,²⁷ employing an axiomatic framework proper of the geometric mind.

On the other hand, at the outset, he specifies which theories are presupposed (geometry and kinematics) in the process of establishing of a system, but he also discusses, at length, the philosophical considerations that seem to not belong to an axiomatization, which takes place when he holds that it is impossible, and useless, to know the real constitution of matter, or when he speaks of physicists who deny the possibility of bodies that are the result of mixtures or combinations of two bodies A and B.²⁸

mathématique." (Duhem (1987a [1893a], 131) In *La science allemande*, he notes Helmholtz's and Hertz's respective treatments of the subject – although he assigns a greater success to the latter (cf. Duhem 1915, 128-129). On the other hand, his reference to the Euclidian framework should not lead us to think that Duhem overlooks the logical shortcomings of that axiomatization, i.e., the independence of its axioms (cf. Duhem 1915, 113-114).

²⁴ "It becomes necessary to return to the foundations on which the science is based, to examine anew their degree of soundness, to assess exactly what they can support without giving way. Once this work is done, it will be possible to build up the new consequences of the theory." (Duhem 2011 [1892-1894], 35)

²⁵ Cf. Miller (1970, 229). It appears Duhem was not familiar with the axiomatic systems developed by Frege and Hilbert – now known as Hilbert systems – nor with the new mathematical logic Couturat sought to introduce, without much success, in the French intellectual milieu based on the works of Peano, Schröder, and Russell. For a brief approximation to Duhem and Couturat, see Hernández (2016).

²⁶ This does not mean that he refrains from framing the question in a traditional way and with regard to the roles the subtle and geometric minds play within them: "[...] the axioms that a science of reasoning demands that we grant to it ought no merely to agree among themselves without any shade of contradiction. They ought, further, to be as few in number as possible. Consequently, they ought to be independent one from another. If one among them, in fact, could be demonstrated by means of the others, it would be deleted from the number of the axioms and relegated to the class of theorems [...]. To find out whether all the axioms of Euclid are truly independent of each other is a question under the jurisdiction of the mathematical mind [...]. But to decide whether the postulate of Euclide is true is a question that the mathematical mind, left to itself, could no answer. It must, in this case, have recourse to the aid of the intuitive mind." (Duhem 1991 [1915], 87-88) In the original: "[...] les axiomes qu'une science de raisonnement demande qu'on lui concède ne doivent pas seulement s'accorder entre eux sans l'ombre d'une contradiction; ils doivent encore être aussi peu nombreux que possible; partant, ils doivent être indépendants les uns des autres; si l'un d'entre eux, en effet, se pouvait démontrer à l'aide des autres, il devrait être rayé du nombre des axiomes et relégué parmi les théorèmes [...]. Reconnaître si tous les axiomes d'Euclide sont vraiment indépendants les uns des autres, c'est une question qui ressortissait à l'esprit géométrique [...]. Mais décider si le postulatum d'Euclide est véritable, c'est une question à laquelle l'esprit géométrique, abandonné à lui-même, ne saurait donner de réponse; il lui faut, ici, le secours de l'esprit de finesse". (Duhem 1915, 113-114)

²⁷ Duhem's theory only deals with the magnetism of solid bodies, such as crystals, with quite modest theoretical intentions: "nous espérons que le présent travail, quelque restreint qu'il soit, aura contribué à élucider quelques points obscurs ou douteux dans la théorie de l'aimantation par influence." (Duhem 1888, 136)

²⁸ Cf. Duhem 2011 [1892-1894], 38. It may be worth recalling that, in the introduction, Duhem acknowledges that his treatment may be viewed as more philosophical than mathematical as to be included in the *Journal de mathématiques pures et appliquées*.

That being said, the second reason why Duhem was able to suppress the characterization of abstract theory as a philosophical view of the physical theory is also linked to another negative aspect of the axiomatic structure since as the deductive capacity of the theory promotes the desire to overcome the representational domain in search of a cosmological explanation of laws. Similarly, the exclusive attachment to consequences derived from the theory makes philosophers hostile toward any discovery not previously accounted for by the theory. In contrast to the English School, whose model favors technological invention and application, abstract theory, for a young Duhem, has the shortcoming of fostering “an unimaginative mindset, hostile to novelty, and for which Continental scientists, and their academies tend to be reproached.” (Duhem 1996 [1893], 70)²⁹

In *La théorie physique*, Duhem removes both shortcomings of abstract theory as an axiomatic system. In addition, he undermines the positive aspects of the mechanical models while simultaneously, complaining about its lack of logical rigor and its de-structured quality – a critique already at work in some of his scientific works.

What is the Extent of Pascal’s Influence?

If we compare Duhem’s critical review of the English School with Chapter IV of the first part of *La théorie physique*, what is most evident is that the association of the subtle mind with the English mentality does not add substance to what Duhem argues in the preceding essay since the changes described above are also not associated with the English mentality, but instead, to the Continental one, which favors an axiomatic outlook. Is Pascal’s influence on Duhem, then, more apparent than real? Given that Martin (1991), Stoffel (2007), and Cortese (2016) hold – with their respective differences –³⁰ that there is a marked Pascalian influence on Duhem’s thought, it seems convenient to outline some of the arguments that lead me to believe that this influence is, at least, not as significant as the three authors argue.

The first, and most apparent, evidence for the rhetorical, rather than actual, use of the distinction between the subtle mind and the geometric mind lies in the fact that in *La théorie physique*, the subtle mind is associated with the English way of doing physics, while in the writings collected in *La science allemande*, the subtle mind is primarily associated with the French mentality, while the geometric mind is associated with the German mindset. In other words, when compared to the English, the French display a deductive mindset; whereas, compared to the Germans, the French display a broad, but weak mode of thought. How is this possible? If we immediately rule out the hasty reading that ties nationality, strictly speaking, with one of the two mentalities, it becomes clear that the modes of thought are defined not in function of the specific nationality, but rather by the physicists’ approach to the physical theory, since, if Duhem has gone to great lengths to criticizing the English approach it is precisely because the success of mechanical models has led to their use beyond the English channel, and to their triumph in the kingdom of abstract theory, namely, France and Germany.

On the other hand, we can delimit the scope of the subtle mind in the French context if we grant that both French and German scientists are advocates of abstract theory, but that they differ in the way they view a system’s axioms and postulates, so that the geometric mind depends on because of the consequences they may derive, while the subtle mind is capable of *feeling* or *intuiting* their truth. However, although this interpretation is plausible in theory, it has the burden of being appropriate when applied to mathematical theories, but indefensible when applied to the physical theory. This is the case because the subtle mind is the one in charge of filling in the gaps the geometric mind is unable to reach, which exceed the domain of principles and of the physical theory itself, as is the case with the relationship between theoretical and

²⁹ “Our need to admit nothing except what can be clearly deduced from accepted principles makes us mistrustful of any unexpected discovery. This need leads to the bureaucratic mind, hostile to novelties, for which continental scientist and their academies are so often reproached.” (Duhem 1996 [1893], 70). In the original: “Notre besoin de ne rien admettre qui ne se déduise clairement des principes reçus nous rend méfiants à l’égard de toute découverte inattendue; de ce besoin découle l’esprit routinier, hostile aux nouveautés, si souvent reproché aux savants du continent et aux académies qu’ils composent.” (Duhem 1987a [1893a], 140)

³⁰ Stoffel (2002) makes a strong critique of some of Martin’s main theses without denying for that matter the influence of Pascalian thought, while Cortese (2016) follows Stoffel’s (2007) reading closely.

experimental physics; as Duhem says, it is not something to be deduced, but rather *intuited* (Duhem 1915, 131).

Besides, as he states in *La théorie physique* – but also in his application for admission into the Academy of the Sciences, and elsewhere – theoretical laws are free creations of the intellect and their permanence is determined by their ability to synthesize experimental laws and by the productivity of their consequences.³¹ What truth, then, can be felt about a law, like the law of conservation of energy, which is taken to be a hypothesis that must be verified by means of its most immediate and distant consequences?³² We can respond to this question by arguing that it is up to the subtle mind to contrast and verify the theory; but this response suffers from the great inconvenience of presupposing that the problem at hand pertains to experimental, and not theoretical physics (except, perhaps, if the point is to free theory from hasty refutations). Moreover, for Duhem, contrary to axioms in mathematics, in physics, common sense does not suffice to *feel* or *intuit* the truth of principles; instead, scientific experience – which does stop with the perfection scientific instruments and the emergence of new discoveries – is necessary:

More complex yet is the choice of hypotheses upon which will rest the entire edifice of a doctrine pertaining to experimental science, of a theory of mechanics or physics. Here the matter which ought to furnish the principles is no longer common experience, spontaneously available to every man from the time he leaves infancy. It is scientific experiment [*expérience*]. To the mathematical sciences common experience furnishes autonomous, rigorous, definitive data. The data of scientific experiment are only approximate. The continual improvement [*perfectionnement*] of instruments increasingly modifies them, while the fortunate chance of discovery each day comes to enlarge the treasury with some new fact. (Duhem 1991 [1915], 81-82)³³

I think it unnecessary to expand on how problematic it is to give full significance and coherence to an intuition that requires a scientific experience, which renews itself endlessly in light of multiple factors, but that, at the same time, pretends to attain – in advance – the truth itself about hypotheses that are accepted as highly arbitrary and subject to revision according to pragmatic criteria linked to the productivity of their consequences. On the other hand, it seems appropriate to suggest that this appeal to scientific experience foreshadows a key concept in physics' recent historiography, which Buchwald and Hong (2003, 180ff) have called unarticulated knowledge, referring to the implicit knowledge that makes possible the configuration of a theory but which also guides experimental practice in the laboratory.³⁴

³¹ *Notice sur les titres et travaux scientifiques de Pierre Duhem*, written in May of 1913, but published posthumously, which deals with the supposed Newtonian method to arrive at principles by means of inductive reasoning, notes that according to Energetism: "The principles are laid down as pure postulates, arbitrary decrees of human reason; they are considered to have successfully fulfilled their role when they yield numerous consequences that conform to experimental laws." (Duhem 1987b, 334) Unfortunately, the English translation lacks the section devoted to his work as a physicist.

³² With regard to the first law of thermodynamics, he notes: "[...] it is a *physical hypothesis* [...]. It is for experience to verify its immediate and more distant consequences." (Duhem 2011 [1892-1894], 63)

³³ "Plus complexe encore est le choix des hypothèses sur lesquelles reposera tout l'édifice d'une doctrine appartenant à la science expérimentale, d'une théorie de Mécanique ou de Physique. Ici, la matière qui doit fournir les principes, ce n'est plus l'expérience commune, celle que tout homme pratique spontanément dès qu'il est sorti de l'enfance; c'est l'expérience scientifique. Aux sciences mathématiques, l'expérience commune fournit des données autonomes, rigoureuses, définitives. Les données de l'expérience scientifique ne sont qu'approchées; le perfectionnement continu des instruments les retouche et les modifie sans cesse, tandis que le hasard heureux des découvertes, chaque jour, de quelque fait nouveau en vient grossir le trésor." (Duhem 1915, 106)

³⁴ As Buchwald and Hong (2003, 181) elucidate, this implicit knowledge can become explicit at a given moment: "Specifically, by 'unarticulated knowledge' we intend knowledge that is generally unexpressed but that guides research. This not at all the same thing as *unexpressible knowledge*, such as the kind of skill that is needed to form a beautiful piano leg on a lathe. Not at all – it is knowledge that is *unexpressed*, that exists below the surface of explicit discourse. Such knowledge is accordingly tacit, in the sense of unspoken, but it can be – and often eventually is – heard, particularly when a science settles into a reasonably stable form."

Closing Remarks

If what I have argued thus far seems plausible, then we can ask whether Duhem's use of the distinction between the two mentalities has the significance scholars like Martin, Stoffel, and Cortese ascribe to it; or whether, on the other hand, Duhem resorts to the dichotomy because of its popularity among the French audiences at the turn of the century without much of a concern for a consistent and systematic treatment. The latter may be due to the fact that Duhem's oeuvre addressed three different audiences: those who, following Holton, I have called New Apollonians and Dionysians, and, of course, their theoretical and experimental counterparts. Additionally, in some cases, a number of these writings were revised and published – in part or in whole – for a different type of audience, as is the case, for example, with *La théorie physique*, but also with *Le mixte et la combinaison chimique* (1902), which takes up previously published essays with a philosophical audience in mind, and is, therefore, published in *La revue de philosophie* – a journal with Catholic inclinations in which Duhem participated in from its inception.

As I noted toward the beginning of this essay, the philosophical inquiry into the end and value of theoretical physics is related to the process of institutionalization and recognition of the discipline; or, as Bordoni (2012, 128) states, “the emergence of theoretical physics corresponds to a new sensitivity to meta-theoretical issues: we find explicit designs of unification, and explicit methodological remarks, as well as explicit questioning of the foundations of physics.” In my opinion, however, Bordoni is not appropriately consistent when, immediately following the above quote, he argues that “[s]cientists did not entrust philosophers with reflections on aims and methods of science: metatheoretical remarks began to emerge from inside science, rather than being addressed to science from the outside.” There is a simple explanation for this. When a discipline is in the process of its stabilization and professionalization, it is not easy to determine who is inside and who is outside. In the case of physics, as Bordoni himself acknowledges, “Maxwell, Boltzmann, Rankine, Gibbs, Helmholtz [...] may all be described as natural philosophers and physicists,” but there are also protagonists whose professional profiles put them on the side of engineers, mathematicians, *self-made men* (as in the case of Faraday), amateurs, and philosophers.

On the other hand, in many cases, methodological discussions are aimed at literate audiences, at young students (as was the case with most of the essays collected in *La science allemande*), but also at New Apollonians (like Abel Rey) and New Dionysians (like Bergson and Le Roy). In my opinion, Duhem resorts to Pascal's distinction, because – besides his undeniable admiration – it is present in the collective imaginary of the French people when it was not unusual to resort to it as a rhetorical and stylistic device, as can be seen in the profiles of the characters developed by Saint-Simon (cf. van Elden 1975). Therefore, my reading does not assume a skepticism toward his sympathy for Pascal, neither would I call into question the claim that Duhem saw himself as an *esprit de finesse*. It does not follow from this, however, that he can be regarded as a disciple of Pascal's, as his daughter Hélène claimed (1936, 229), or that there is a strong Pascalian influence on Duhem's main theses on theoretical physics.

For example, Stoffel (2007, 287) lists three themes “ponctuelles et textuellement attestées” that, regardless of how much we stretch them, do not justify talk of a decisive influence, since the very fact of referring to related *themes*, and not to ideas and theories, suggests, in principle, a weak connection. Moreover, in each case, we can have serious reservations about the possibility of attributing a Pascalian influence. The first thematic affinity Stoffel points out is the critique of mechanism; the second one refers to the different orders of knowledge; and the third refers to the distinction among the different kinds of mindsets or intellects. However, it should be evident that to take Pascal as a critic of mechanism because of his critique of Descartes, is, on the one hand, to mistake a part for the whole; on the other hand, it is to overlook the existing overlaps between both seventeenth-century thinkers, which, moreover, Duhem himself points out.³⁵ Regardless, if we can claim a significant connection between Pascal and Duhem, it is the one Duhem (1905) makes with regard to the evaluation of Pascal's scientific work, where he argues that while we cannot

³⁵ Alluding to Pascal's well known Aphorism 60, Duhem goes so far as to equate the universality of logic to moral law: “It is beyond argument that logic is unitary. Its principles impose themselves, with the same ineluctable rigor, on the French, the English, and the Germans [...]. In the same way, the moral law is the same on either side of the Pyrénées.” (Duhem 1996 [1893], 73) In the original: “Il est hors de contestation que la logique est une; que ses principes s'imposent, avec la même inéluctable rigueur, à un Français, à un Anglais et à un Allemand [...]. De même la loi morale est identique en deçà et au delà des Pyrénées.” (Duhem 1987a [1893a] 144-145)

attribute the discovery of great truths to him, his merit lies in his reconfiguration of preceding knowledge. In Duhem's case, this work of conceptual elucidation takes place through the axiomatization of a unified abstract theory underlying his project for a general, or energetic thermodynamics. These organizational abilities, however, cannot be ascribed to the activities of a scientist who possesses a subtle mind, but rather to one with a geometric mind.

Acknowledgement

I would like to thank Alberto Bejarano Romo for translating this article.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 108-111

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Was Duhem Justified in not Distinguishing Between Physical and Chemical Atomism?

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Abstract:

Chemists in the late nineteenth century were apt to distinguish the theory of chemical structure they advocated as chemical, as opposed to physical, atomism. The failure on Duhem's part to consider any such distinction in his critique of atomism might be taken to be a lacuna in his argument. Far from being a weakness in his stance, however, I argue that he had good systematic reasons for not taking such a distinction seriously.

Keywords:

Pierre Duhem; atomism; caloric; chemistry; thermodynamics

Received: 13 December 2016. Accepted: 03 March 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.10>

108

In this short note I want to take up an aspect of Duhem's critique of atomism relating to a nineteenth-century distinction between physical and chemical atomism. Chemists such as Williamson and Kekulé who developed molecular theories of the underlying nature of chemical substances in the wake of Dalton's atomic explanation of the laws of constant and multiple proportions thought of their theories of matter as concerned with chemical atomism. This they distinguished from what they called physical atomism, one of the major applications of which was the kinetic theory of gases developed in the same period, possibly because they could see no systematic connection between the two. At all events, chemical atomism was specifically concerned with the problems chemists were interested in—the variety of chemical substances and their interactions. Physicists concerned with analysing matter were not interested in distinctions of substance. Duhem took no account of this distinction in his critique. Is this an omission that weakens his argument? I think Duhem had good systematic reasons for not acknowledging a nineteenth-century distinction between physical and chemical atomism that are part and parcel of his overall argument.

Duhem developed his detailed critique of atomism in chemistry at the turn of the twentieth century. In retrospect this seems to have been a misdirected effort. Whether it was undermined by the current state of chemistry at the time is, I think, doubtful. But my interest in the matter is to understand what his arguments were and whether it was reasonable for a man in Duhem's position to propound them. Having written extensively on this subject earlier (Needham 1996, 2004a, 2004b, 2008), I don't intend to rehearse all the

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details here but simply to emphasise a point of fact and pursue one aspect of his view. The point he recognised is that the laws of chemical combination (constant, multiple and reciprocal proportions) are just that—concerned with proportions. As such they don't entail that matter is discrete; though consistent with atomism (or discrete matter at one or more levels), they are also consistent with a continuous view of matter. Duhem's response was that the reasonable position to adopt was one of neutrality between these two interpretations until decisive reasons favoured the one or eliminated the other. Most of the effort in Duhem (1892) and (1902) went into elaborating a neutral interpretation of the use of chemical formulas embodying the basic laws, which he established as perfectly possible even though some might think it easier to adopt an atomic interpretation.

Even if decisive reasons on which to base a choice were not available, it was quite legitimate to examine what was currently on offer and give voice to problems arising. Here it is apposite to raise questions about the very coherence of the notion of an atom. Doubts on this score have been a feature of the atomic debate since ancient times. They were a live issue in the latter part of the nineteenth century and constituted one line of thought in the general scepticism regarding atomism that was rife in the nineteenth century. A well-known example is the paradox of atomic collision. Either direction is changed instantaneously, requiring what is impossible, namely an infinitely large force, or the "atoms" are elastic in virtue of a structure of subatomic parts and hence not atoms after all. Another example is the discrepancy between the specific heat ratios of diatomic gases as observed on the basis of thermodynamic reasoning and as calculated on the basis of the kinetic theory.

But aren't these worries about the physical nature of atoms, which chemists could circumvent by focusing on chemical atoms? This strategy might be interpreted to the effect that chemists were thereby avoiding any claim to adopt an atomic theory of chemical substances. The term "chemical atomism" should in that case be understood as being used in what analytic philosophers like Nelson Goodman called a syncategorematic sense. It is not a certain kind of atomic theory, just as a broken glass is not a certain kind of glass—not something which is both a glass and broken. Although the expression is built from two distinct words, they don't each retain their separate senses in the combined expression. If this is so, and what is meant is simply a theory founded on the laws of proportion, then it is not substantially different from Duhem's account, which is not an atomic theory. I find it very difficult to see how a view or theory can be regarded as an atomic view or theory unless it says something about atoms—ascribes to them properties from which the macroscopic properties of chemical substances can be derived.

This brings us to the related line of questioning concerning how the atomic hypothesis could provide any explanation of chemical combination or whatever it is they are postulated to explain. Dalton was clear about this. He ventured to explain his law of partial pressures by endowing his atoms with a coating of caloric, so distributed about the atoms of a particular kind that they repulsed other atoms of the same kind but not atoms of different kinds. This raises a number of questions, of course: What distributions of caloric could function in this way? Would the repulsive power of caloric allow atoms of the same and different kinds to combine chemically to form polyatomic molecules of elements and compounds, as distinct from a mechanical mixture? What is it that explains the combining power if the caloric explains the repelling power? Above all, there is the question of whether trying to explain repulsion or combination by postulating a substance endowed with just these proclivities isn't directly circular or leads to an infinite regress, as Lavoisier realised when speaking of the tendency of air to expand and increase in pressure with temperature by virtue of the elastic property of caloric:

It is by no means difficult to perceive that this elasticity depends upon that of caloric, which seems to be the most eminently elastic body in nature. Nothing is more readily conceived, than that one body should become elastic by entering into combination with another body possessed of that quality. We must allow that this is only an explanation of elasticity, by an assumption of elasticity, and that we thus only remove the difficulty one step farther, and that the nature of elasticity, and the reason for caloric being elastic, remains still unexplained. (Lavoisier 1789, 22)

Duhem thought that ascribing atoms combining powers in the form of atomicities (valencies) amounted to simply reading properties of elements in compounds apparent at the macroscopic level into atoms, providing, to paraphrase Lavoisier, an explanation of the combining power of elements by an assumption of

the combining power of elements. The atomic theory of the hydrogen molecule proposed by Heitler and London is not at all like this, but provides a substantial theory of the quantum nature of hydrogen atoms from which the stability of the hydrogen molecule is derived. What was on offer at the time Duhem was writing which went beyond empty ascriptions to atoms of the properties that were to be explained seems to have been restricted to ideas about the shape of microentities or the vortex theories. These may not have incited a great deal of interest on the part of chemists (although Jones (1902, 38-9) is an example of a chemist who found some interest in vortex theories) and Duhem's dismissing them without difficulty may have been equally uninteresting to chemists. But it served to make the point that there was nothing on offer beyond tautology that looked like a promising start to an atomic theory of chemical combination.

Whatever the explanatory merit of a substantial atomic theory, how could it be anything but a physical theory of atoms? To say otherwise is surely to court the occult, bringing to mind the pre-enlightenment division of the universe into sub- and superlunary regions with modes of explanation peculiar to each. Certainly Duhem thought it was anathema to modern science to bifurcate phenomena into separate realms with laws appropriate to each. This was the philosophical basis of his opposition to Berthelot's defence of the principle of maximum work, which presupposed that a distinction could be made between physical and chemical processes and the principle restricted to the latter. Let us recall Duhem's case. According to Thomsen's law of maximum work, chemical reactions proceed spontaneously only if they are exothermic. Although the majority of reactions conform to this principle, evidence of exceptions bringing into question the universal validity of the law was mounting. Berthelot proposed to interpret the apparent counter instances as constituted of two processes, a chemical change alone subject to the law of maximum work, and a physical change not restricted by the law. Duhem (1886, ii-iii) maintained that the demarcation between chemical and physical phenomena was illegitimate, criticising the distinction on which Berthelot's defence of the law was based as ad hoc.

Sulphuric acid, for example, combines with ice and this combination produces cold. In order to bring this exception within the rule, the reaction must be divided into two phases: one part being the fusion of ice, a *physical* phenomenon which absorbs heat, and the other part, the combination of liquid water with sulphuric acid, a *chemical* phenomenon which releases heat. But it is by a purely mental conception, and not as a representation of reality, that it is possible to thus decompose a phenomenon into several others. Moreover, accepting that chemical phenomena obey the law of maximum work while physical changes of state would be free is to suppose that there is between the mechanism of these two orders of phenomena a line of demarcation which the work of Henri Sainte-Claire-Deville has removed. (Duhem 1886, ii-iii)

Berthelot's interpretation supposes that a chemical reaction produces a reduction in internal energy of the reacting material, and thus that a stable state of chemical equilibrium corresponds to the lowest possible value of energy of the system, just as does the stable state of a mechanical system. The failure of Berthelot's rule shows that energy alone cannot serve as the basis of a general criterion of chemical equilibrium. If the analogy with mechanical systems is to be upheld, a generalisation of mechanics is required and something other than energy must be found to play the role analogous to that which the potential plays in mechanics. Duhem goes on to show how work in thermodynamics by Massieu, Horstmann, Helmholtz and Gibbs had led to a better appreciation of the conditions governing chemical equilibrium. All cases could be accommodated in terms of the general notions of thermodynamic potentials, which take account not only of the energy change, as Berthelot in effect did, but also of the entropy change.

Duhem continued to argue in this spirit for a unified view of science according to which all phenomena are subject to the same general principles rather than constituting different worlds, notably when rejecting the reduction of thermodynamics to mechanics in favour of a vision in which the old mechanics is incorporated into a broader theory (Duhem 1892; 1892; 1894). Unification, not by reduction to preconceived ideas but by expansion and integration into a general theory without internal contradictions is the way to achieve the goal of what he called a natural classification.

To summarise, then, advocacy of a specifically chemical atomism might be seen as a device for avoiding commitment to a discrete view of matter at the microlevel. But the rejection of any substantive distinction between chemical and physical realms was a matter of principle for Duhem. The absence of any

recognition of a distinction between chemical and physical atomism that chemists of the time might have entertained is one of the strengths of his general argument.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 112-126

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Bon sens and noûs

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Abstract:

This paper is intended to link the notion of *bon sens* with the Greek notion of *noûs*, that exposes the role played by the first notion in the thought of Pierre Duhem and explains the concept of *noûs* in the thought of Aristotle. Later, it attempts to carry out the explanation of the link that can have both notions.

Keywords:

Pierre Duhem; *bon sens*; *noûs*; science; philosophy; history of science

Received: 30 March 2017. Reviewed: 15 May 2017. Accepted: 30 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.11>

Introduction

The significance of a thinker can be measured by the number of defenders and critics of his thought. If we use this measuring scale of transcendence, we can see that Pierre Duhem is located at the highest levels of transcendence. The meaning of the word transcendence is “to overpass”, the transcendental and the transcendent is what is beyond, which go beyond; the thought of a human being can be transcendent and transcendental because it exceeds and goes beyond the time in which it is produced; to exceed and go beyond the moment in which something has arisen can be done to the back or to forwards, this is meant in the case of the time you can go beyond the present to the past or to the future. The thought of Pierre Duhem is transcendental and transcendent in that sense because it goes towards the past and the future.

The thought of Duhem, like the thought of all great thinkers, goes to the past because it comes from a tradition and moves towards the future so it becomes a classic. We can go back and forward through time because we think about the issues that belong to both the past and the present as well as the future. The problems that Duhem deals with, are those that belong to the three times. In this paper, I try to address one of these problems: the question of the *bon sens*. This matter has been studied by many specialists who have devoted much time to the study of the thought of the French thinker, which will be mentioned throughout this work. Here I intend to address the issue from a unique perspective. In fact, I will try to study the *bon sens* going toward the past and the future.

To carry out this, I tried to follow retrospectively the footprints of the tradition of which it may come from the notion of *bon sens* and I came up with the Greek notion of *noûs* and explained how they can be

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linked these two notions. If this attempt is successful, then we can show that the question of the *bon sens* is one of those that belongs to the three moments in time. To state as clearly as we can, according to the scholars of this kind of thought pointed out the importance of the issue that we are dealing with in the system of thought of Duhem; subsequently, I expose the link that can be established between the thought of Pierre Duhem and the tradition, in particular with the Greek thought and, more specifically, with the notion of *noûs*; finally indicated how we can show the transformation of the Greek notion of *noûs* to become the notion of *bon sens*, through the changes experienced by the western tradition.

The *Bon Sens* in the Thought of Pierre Duhem

Russell Niall Dickson Martin (1991, 6-13) has pointed out that to understand both the reception of the works and to understand the thinking of the historical figure Pierre Duhem, it is necessary to consider both the internal and external factors. Among the external factors mentioned the religious, the political and the philosophical context. Among the internal factors mentioned "the method of working" and "the habits of publication of Duhem" (Martin 1991, 193-199). Martin explains the work method of Duhem and asserts that he had to pay a price for the ease of writing, resulting in a single draft without notes and had to pay an even higher price by having to produce a complete form with their results in the atrocious conditions of isolation in Bordeaux and with a single opportunity to consult all the manuscripts that he wanted. "It was all done on the run with no chance to reflect", and without the opportunity to make changes that affected the integrity of his work, "changes of view sometimes both exaggerated and partially concealed by Duhem's publication habits".

On the other hand, according to Martin, the publishing style typical of Duhem consists of literal reprints of a series of articles that appeared in a journal, adding prefaces. For example, as was the case of the work entitled *Les origines de la statique* that were first published in the *Revue des questions scientifiques* from autumn 1903 to at the end of the year 1906. According Martin, there is evidence that the same composition methods are followed in other works of Duhem, particularly, in *La théorie physique*; "There can be little argument that this was how the *Études* was written, and a detailed analysis of the *Système* would certainly reveal evidence of similar methods of composition, though here the scale of the work probably made it inevitable." This habit of publication is played by Martin as a publication of the work before being completed. The consequences of these habits are summarized by this critical as follows: "at worst unperceived changes of view during composition, works started before I knew where his argument was going to lead. Changes in overall attitudes during his career, writing liable to mislead the inattentive reader into serious misreadings" (Martin 1991, 194-195).

The shifts in perspective not recognized are as suggested in the above quote, the most important consequences of "the habits of publication" and "the method of working" of Duhem. In fact, Martin believes that these changes in point of view in the works of the French physicist have consequences, one is the way his works were received and, the other is if we are trying to understand his thinking, we must understand the changes. Some of these changes have been identified in the book of Martin as cited above, one of them mentioning:

During this essay, I have pointed out many examples of such shifts. Lemonnier's example of Albert of Saxony is one case: after initially seeing him as an original contributor to mediaeval mechanics Duhem later saw him as a repeater of the ideas of others. Another is the change of focus in *To save the phenomena* from methodology to cosmology. Yet another is the shift in the *Système du monde* from cosmology to the overall relations of physics with philosophy and theology. These shifts affect individual works, but there is one that may have caused more trouble than any other: the increasing emphasis on the Pascalian methodology of *bon sens* as Duhem's career progressed. (Martin 1991, 196)

According to the above quote, the Pascalian methodology of the *bon sens* was taking greater importance to the extent that the thought of Duhem progressed and this change of importance of the *bon sens* is the change of perspective that has most concerned and disturbed the understanding of the thought of the French Catholic thinker. In this way, we have come to the central point, which is the interest of this

small written: the origins of the concept of *bon sens* in the thought of Pierre Duhem. However, we must point out that we do not intend to confine ourselves to the thought of Pascal as the source of the *bon sens* in the thinking of Duhem. We believe that the influence of Pascal on the thought of Duhem has been sufficiently demonstrated by the research of various authors interested in the thought of the latter². Instead I try "to speculate" about the remote source and implicit of the *bon sens*, which is also the source of thought of Pascal: the western thought, resulting from the merger of Christianity and the pagan Greek thought. I think that despite the prominent role of the apologetic aspect of the thought of Duhem, the Christianity and particularly, the Catholicism has not received sufficient emphasis on its relationship with the notion of *bon sens*, to which this essay aims to contribute.

Of course, we say that we intend "to speculate" to the extent that in our exposure there are a high degree of conjecture, and in any way, I intended to present this exposure with demonstrative character. In addition, I should point out that I do not intend to draw the complete history of the transformation of the concept of *noûs* going through each of the links until become the *bon sens* which requires a much broader dimension than this little essay. In revenge, I intend to provide some evidence suggesting that the *bon sens* is in part the result of the transformation of the Greek concept *noûs*.

Bon Sens, Science, Philosophy and History in Duhem

Before anything else, it is necessary to have a brief overview of the influence not only of the thought of Pascal on the general thought of Duhem, but also an overview of the reasoning of the physicist of the 19th century. This overview will provide us the opportunity to locate the place of the *bon sens* within the general thought of Duhem. Jean-François Stoffel, who has been devoted to the study of the relationship between the thoughts of both these thinkers, besides he emphasizes the various topics in which we can find evidence of such relationship³. He has pointed out that it is not enough with the mention of scattered topics, besides we required, in one area, observe the influences of which there is no textual evidence and, on the other hand, we need trying to gather the scattered influences in the center of a global system of thought, because "it seems to us that an influence so proven and so extensive can't in an author like Duhem be limited to a succession of particular subjects, but must indicate a much more fundamental existence of a vision of the common world which is both scientific, philosophical and religious" (Stoffel 2007, 293)⁴. For our part, we can add that, this "vision du monde" also extends to the historical.

In addition, in a previous piece of work Stoffel has pointed out the problem of the historical paradox that Duhem always want to be known and recognized as a physicist and, however, history has played a joke, because it has been better recognized in the intellectual context as a philosopher of physics and as a historian of science, to the side of Paul Tannery or Alexandre Koyré, but not as a physicist, to the side of Max Planck or Einstein (Stoffel 1995, 49-50). In the light of the studies that have been developed on the thought of Duhem from Abel Rey and up until the present, and despite the efforts of the own Duhem to be recognized as a theoretical physicist, we can point out that the history not only has not recognized Duhem as theoretical physicist, but also that when it is recognized as a philosopher of physics and as a historian of science it is almost always associated with claims apologetic.

² The influence of thought of Pascal on the thought of Duhem has been highlighted by the latter's own contemporaries such as his own daughter Hélène as well as by friends and acquaintances. More recently, since 1991 Martin pointed out that influence and from 1993 Stoffel has been working in that direction. Cf. Hélène Pierre-Duhem 1936; Martin 1991; Stoffel 1993; 2002; 2007.

³ Stoffel mentions the following topics in which there is textual evidence of the influence of the thought of the philosopher of the 17th century on the thought of the scientist of the 19th century: the critique of mechanism, the capabilities of human intelligence, the different types of spirits, the research of a via media between realism and a dogmatic skeptical phenomenalism, the management of knowledge, the truth of the first principles as well as the inability to define everything and, finally, the philosophy of history optimistic and providential (Stoffel 2007, 287-293).

⁴ "Il nous semble qu'une influence aussi avérée et aussi étendue ne puisse, chez un auteur comme Duhem, se limiter à une succession de thèmes particuliers, mais qu'elle doit signaler, entre les deux penseurs, l'existence, beaucoup plus fondamentale, d'une vision du monde commune qui est à la fois scientifique, philosophique et religieuse" (Stoffel 2007, 293).

Let us add that, in relation to the global system of thought, it has been always highlighted the extra-scientific influences about the thought of Duhem; in this sense, looking for Ariadne's thread that guide the understanding of the global thought of the French physicist, R. N. D. Martin has pointed out a "hidden agenda" in which items are found religious, cultural and political factors that have influenced the thought of Duhem⁵. The global system of thought of Pierre Duhem, like that of any thinker, we can approach it from the following general themes which, furthermore, are also the subjects in which it has been pointed out the influence of thought of Pascal: the scientific, philosophical, historical and religious.

The Scientific Aspect

In the discussion on the existence or not of an antagonism between science and religion, in the words of the own Duhem, between *l'esprit scientifique* and *l'esprit religieux*, in his famous letter to Bulliot of 21 May 1911, Duhem notes that the defenders of the existence of such antagonism argue that the logical analysis reveals the radically different methods by which science (rigorous that part of axioms and checks by the experience) and religion (aspirations and intuitions, vague) are produced; however, Duhem thinks that the antagonism between the methods of one and another is apparent. In accordance with Duhem, this opposition between the methods of both human activities is due to a superficial and false analysis of such methods. But to him who has penetrated to the heart and has captured the vital principle of the methods, captures what provides its diversity and what keeps together these united procedures.

It sees a same human reason use the same means essential to arrive at the truth; but in each domain, this reason is adapted to the use it makes of these means to the special object from which it wants to acquire the knowledge. [...] It is recognized then, that to get to the truths of religion, human reason does not employ other means than those that has served to achieve the other truths; but she uses it in a different way, because the principles from which part and the conclusions to which it tends are different (Hélène Pierre-Duhem 1936, 164-165).⁶

115

In addition, in a letter to Joseph Récamier which is quoted partially by Picard, Jordan, Jaki and Martin without identifying the receiver, but that Stoffel has identified by Hélène, the daughter of Duhem⁷, shows that the latter not only affirms the use of the same human reason in various orders of knowledge, but also takes the same point of departure; in such a way that the so-called radical difference of the procedures is only apparent:

To force to reflect on these difficulties, I have come to realize that it can say the same of all sciences, from those that are taken as the most rigorous, physics, mechanics, even the geometry. The fundamentals of each of these buildings are made of notions which it claims to understand, despite they will not be defined; of principles that are insured, despite it not having any demonstration. These

⁵ In the same sense of "hidden agenda", Michel Puech (1996) points out that the history of the sciences of Duhem is a "crypto-theology of Providence".

⁶ "Il voit une même raison humaine", says Duhem, "user des mêmes moyens essentiels pour parvenir à la vérité; mais en chaque domaine, il voit cette raison adapter l'usage qu'elle fait de ces moyens à l'objet spécial dont elle veut acquérir la connaissance [...]. Il reconnaît alors que pour aller aux vérités religieuses, la raison humaine n'emploie pas d'autres moyens que ceux dont elle se sert pour atteindre les autres vérités; mais elle les emploie d'une manière différente parce que les principes dont elle part et les conclusions auxquelles elle tend sont différents" (Hélène Pierre-Duhem 1936, 164-165).

⁷ Cf. Stoffel (2001, 79, footnote). Hélène Pierre-Duhem (1936,156), for its part, speaking of his father the Christian points out, in the same sense, that what it is objected to the faith you can also object to the science considered to be the most rigorous and, recalling the charter loss directed by his father to his friend Récamier, quotes the words of the letter to note the impossibility of defining concepts as clear as *corps, âme, Dieu, mort, vie, bien, mal, liberté, devoir*; and, thus, the impossibility of proving propositions so certain as: "Le monde n'a pas en lui-même une raison d'être de son existence. Je dois faire le bien et éviter le mal". The passage quoted from this letter concludes by pointing out that our sciences more certain resting on foundations of the same nature.

notions, these principles, are formed by *le bon sens*. Without this foundation of the *bon sens*, nothing scientific could have some science. (Picard 1921, 41)⁸

Stoffel cites an article of Édouard Jordan, which this issue has been developed, almost in the same terms, and mentions that there is no doubt that Jordan may object to Duhem that the principles of spiritualism or of faith are not justified:

But answered that, in spite the illusions to the contrary, it is the same for all the sciences including those that are taken as the most rigorous, even the geometry. They are based on the notions that we claim to understand, despite not being able to define and that are provided by the *bon sens*. (*apud* Stoffel 1995, 64)

Thus, in its position with respect to the existence or not of an antagonism between science and religion, it is the *bon sens* as a fundamental part of the understanding of Duhem on the procedures of science.

In addition, on the other hand, Martin Hilbert (2000, 6) in his thesis for the degree of Doctor of Philosophy, has pointed out the relationship between the Energetics and the notion of natural classification in the thinking of Duhem. The notion of natural classification was presented by Duhem in his article entitled "L'école anglaise et les théories physiques, à propos d'un livre récent de W. Thomson". This paper attempts to respond the question on why it should prefer a coherent theory rather than a set of inconsistent theories? The answer is that the perfection of a theory depends on a certain degree of approximation to the ideal theory, which is "the total metaphysical explanation and adequate to the nature of material things: this theory, in fact, classifies the physical laws in an order that will be the very expression of metaphysical relations between the essences, which emanate such laws; they give us the natural classification of the laws in the true sense of the word" (Duhem 1987, 136).

On the other hand, in *La théorie physique* Duhem, establishes a close link between the *bon sens* and the notion of natural classification. In chapter II of the first part, he explains that the notion of the physical theory as a symbolic representation of the experimental laws, implies that the logical order and the "artificial ordering" of these laws are manufactured by the theoretical physicist, however he has the "presentiment", to the degree to become a firm conviction, that the order is a reflex of an ontological order, which is a reflex of a "natural classification"⁹. To justify this conviction, the theoretical physicist needs to transcend the methods and procedures of the theoretical physics, he needs to refer to: "an intuition in which Pascal has recognized one of those reasons of the heart 'which reason does not know', he affirms his faith in a real order, which theories are an image clearer and more faithful every" (Duhem 1906, 38-39).¹⁰

Finally, in various places, and particularly in *La science allemande*, Duhem sets out the tasks that the *bon sens* perform within the activities of the science: the common knowledge, common sense, good sense and the spirit of fineness provide the axioms of mathematics and the hypotheses of the physical theory. The first lesson of *La science allemande* entitled "Les sciences de raisonnement" ask about the question of what is the source of the axioms that are the foundation of these sciences? Duhem responds quoting Pascal: "We know the truth, not only by reason, but also by the heart", said Pascal; of the latter is how we know the first principles. The second lesson of this work is entitled "Les sciences expérimentales" and ask about the question how do the experience provide a proper hypothesis to serve as a principle of the experimental science? Duhem responds with an example of the procedure used by Pasteur: who tested a preconceived idea and through "trial and error" modifications are made in accordance with the facts are

⁸ "À force de réfléchir à ces difficultés, je me suis aperçu qu'on en pouvait dire autant de toutes les sciences, de celles qu'on regarde comme les plus rigoureuses, la Physique, la Mécanique, voire la Géométrie. Les fondations de chacun de ces édifices sont formées de notions que l'on a la prétention de comprendre, bien qu'on ne puisse les définir, de principes dont on se tient pour assuré, bien qu'on n'en ait aucune démonstration. Ces notions, ces principes, sont formés par le bon sens. Sans cette base du bon sens, nullement scientifique, aucune science ne pourrait tenir; toute sa solidité vient de là" (Picard, 1921, 41).

⁹ In paragraph X of chapter IV, which is the last of the first part, Duhem points out another trend which is the inseparable companion of the trend toward the natural classification that trend toward the unit.

¹⁰ In the last three sections of "The physics of a believer" Duhem emphasizes the need to transcend the method of theoretical physics, that is, to resort to the method of metaphysics, to justify both the tendency to unity as the tendency to the natural classification by the analogy between the physical theory and cosmology.

directing: "In this work of successive retouch-ups which a first idea, necessarily risky and often false, ends up developing a fruitful hypothesis, the deductive method and intuition play one another their role; but here it is much more complex and more difficult define this role that in a science of reasoning" (Duhem 1915, 27). In this way, notions richer in content, but less accurate and less analyzed that the notions of the science of reasoning are produced for the joined role of deductive and intuition methods: the rules of logic are not effective enough to be able to reason about these notions, they must be supported by "a kind of sense of fairness which is one of the forms of good sense" (Duhem 1915, 28).¹¹

The Philosophical Aspect

First, in the criticism by Abel Rey, not of the physical but of the scientific philosophy of Duhem, he accuses him of doing metaphysics by taking as a starting point a determined notion of human knowledge. In fact, Rey said that Duhem has succumbed to the temptation of metaphysics, having on his head a preconceived idea of the value, of the limits and of the nature of science. Duhem responds, in "The physics of a believer", that his "physical system" by its origins and its consequences is positive. Of this to succumb to the common temptation, Rey may conclude that:¹²

Unless we limit science to nothing but the collection of empirical recipes – and even this pretension, isn't the result conscious of a metaphysical tendency? – at times the metaphysical problems are so close to the great scientific questions; the human spirit is so eager to prolong its curiosity by imagining new whys it is chimerical and abstract to want to put an insurmountable, and above all unsurpassed, gap between science and metaphysics. (Rey 1904, 733)¹³

Does Duhem put an insurmountable gap between science and metaphysics? It does not appear, but it *distinguishes* the science of metaphysics because it discusses their mutual independence and autonomy. But before arguing in favor of the negative answer to this question, let's ask: what is the basis on which the autonomy between science and metaphysics rests? The answer to this question is found in article "Physique et métaphysique" of 1893, which sets forth the following which supports the view of Abel Rey before mentioned: human beings do not have direct knowledge of the essences of the external things, but of the phenomena and of the succession of these phenomena (Duhem 1987, 86). In these lines, a distinction is made between *essence* and *phenomena*.¹⁴ This distinction is the basis for distinguishing between science and metaphysics. The essences are conceived as causes of the phenomena.

The intelligence of man, on the one hand, know directly the phenomena and the laws according which these phenomena are related and, on the other hand, known indirectly something of the essences because the knowledge of the effects allows us to know something about the substances that produce these effects; however, this knowledge is neither complete nor adequate for these substances (Duhem 1987, 86). The distinction between essences and phenomena has led us to the distinction of two types of knowledge: the knowledge that human intelligence has of the phenomena and their laws, on the one hand, and, on the

¹¹ In section X of chapter VI of the second part, section entitled "Le bon sens est juge des hypothèses qui doivent être abandonnées", also shows the reasons why the *bon sens* is the judge on the choice of the hypothesis.

¹² Duhem does not seem to respond to the accusation of Abel Rey on "Mais nous n'avons eu ici que l'intention d'examiner *la philosophie scientifique* de M. Duhem, et *non l'oeuvre scientifique elle-même*" (Rey 1904, 743-744, emphasis added), as its defense goes to show that your "physical system" is positive and does not depend on the metaphysics or religion. However, the assertion of Rey that Duhem makes metaphysics can justify the response of the latter.

¹³ "A moins de borner la science à n'être qu'un recueil de recettes empiriques – et encore cette prétention n'est-elle pas le résultat plus ou moins conscient d'une tendance métaphysique? – les problèmes métaphysiques sont, à certains moments, si près des grandes questions scientifiques, l'esprit humain est si désireux de prolonger sa curiosité, en imaginant toujours de nouveaux pourquoi, qu'il est chimérique et abstrait de vouloir mettre un fossé infranchissable, et surtout infranchi entre la science et la métaphysique" (Rey 1904, 733).

¹⁴ This distinction can lead to interpret the thought of Duhem as a kind of kantism, however, for Kant *things in itself*, the essences are unknowable, while, for Duhem, the essences can at least in part be known by the intelligence of man; at this point he follows the principle of deducting the causes through the phenomena.

other hand, partial knowledge of the essences that are the root causes of the phenomena. The second of these sciences is one that receives the name of metaphysics. The first is the positive science which is divided into various branches depending on the nature of the phenomena being studied.

The distinction between physics and metaphysics carried out by Duhem was severely criticized because, to exclude the causal explanation of the phenomena from the task of the physical theory and reserve this task the field of metaphysics. However, Duhem has emphasized the need to transcend the method of theoretical physics because basically there are two trends: 1) the need to achieve a coherent and unique theory and 2) the need to achieve a natural classification of the laws of physical theory. In other words, the affirmation that a coherent and unique theory is possible and the claim that the classification of the laws of physics theory tends to reflect the ontological order of things, both statements are metaphysical claims that cannot be founded by the procedures of the positive sciences, but that are supported by the good sense. Duhem is aware that these statements are of a metaphysical character as shown in "The physics of a believer":

Let us indicate what is the present form of physical theory which seems to us to tend towards the ideal form and which is the cosmological doctrine which seems to have a stronger analogy with this theory. We do not intend to give this indication in the name of the positive method proper to the physical sciences. From what we have said, it is clear to the evidence that it exceeds the limits of this method, that this method can neither confirm nor contradict it. In giving the [indication] we know that we have abandoned the domain of physics, thereby penetrating the proper domain of metaphysics, we know that after having toured the latter domain in our company, a physicist may very good refuse to follow us on the proper domain of metaphysics, without violating the rules that logic imposes (Duhem 1914, 463-464).¹⁵

In this way, the Duhemian distinction between physics and metaphysics is not a radical separation as can be understood by Abel Rey and by the interpretations of the thought of Duhem that put emphasis on the apologetic role of this thought. Then, it seems me clearly that, although Duhem carries out a distinction between physics and metaphysics, this does not mean that he can't formulate metaphysical statements or for him the metaphysics is a kind of knowledge¹⁶; and, on the other hand, that there is the possibility of an articulation between the two areas of knowledge.¹⁷

The Historical Aspect

The distinction between physical theory and metaphysical – including religion in this last –, more specifically, the argument about your mutual independence, where the physical theory may not be useful to defend the religion but neither can serve to attack it, seems to correspond with the idea expressed by Ariew and Barker, who affirm: "For most of the nineteenth century, scholars treated 'medieval science' as an oxymoron. Since nothing from the middle ages was worthy of the name 'science', no history of medieval science could be written" (Ariew and Barker 1992, 324). This does not seem to agree with the idea of a historical continuity of science, as it can be thought that, on one hand, are declared as independent of each other, but on the other hand it is accepted that the initiated mechanical science for Galileo inherited of the Middle Ages its

¹⁵ "[...] il nous sera permis d'indiquer quelle est la forme actuelle de théorie physique qui nous paraît tendre vers la forme idéale, et quelle est la doctrine cosmologique qui nous semble avoir, avec cette théorie, la plus forte analogie. Cette indication, ce n'est pas au nom de la méthode positive propre aux sciences physiques que nous prétendons la donner; après ce que nous avons dit, il est clair jusqu'à l'évidence qu'elle excède la portée de cette méthode, que cette méthode ne peut ni la confirmer, ni la contredire; en la donnant, en pénétrant par là sur le domaine propre de la Métaphysique, nous savons que nous avons délaissé le domaine de la Physique; nous savons qu'un physicien, après avoir, en notre compagnie, parcouru ce dernier domaine, peut fort bien, sans violer les règles que la logique impose, refuser de nous suivre sur le terrain de la Métaphysique" (Duhem 1914, 463-464).

¹⁶ From 1893, in his article "Physique et métaphysique", Duhem talks about the metaphysics assuming it is a type of knowledge.

¹⁷ Lucas Roumengous (2016) has developed this articulation in his work entitled precisely: "L'articulation entre physique et métaphysique chez Pierre Duhem".

principles and the formulation of their essential propositions (Duhem 1984, preface, V). This appearance of contradiction can reinforce the idea that in the thinking of Duhem, there is a "crypto-theology of Providence".

In previous pages, it was stated that the distinction between physics and metaphysics is supported on the distinction between phenomena and essences. This last distinction also is the basis of the notion of theoretical physics as symbolic representation of the experimental laws and not as an *explanation* of the causes essential hidden under the phenomena. In addition, the distinction between phenomena and essences is the foundation upon which rests the idea of "save the phenomena", such as the word *phenomena* of this sentence can indicate it; in effect, the value granted by the theorists to the scientific hypotheses have two alternatives: realism or instrumentalism¹⁸ which is developed historically in *Sauver les phénomènes: Essai sur la notion de théorie physique de Platon à Galilée*:

To save the phenomena is perhaps the most controversial of all Duhem's works, and the easiest to misinterpret if not read with sufficient care. In it all the various criticisms of his work seem to come together: excessive positivism; Neo-Scholasticism; apologetic for the Roman authorities. But it cannot without qualification be labelled both neo-scholastic and positivist, or both positivist and Catholic apologetic. (Martin 1991, 163)

If all criticism of the work of Duhem are presented together in *To save the phenomena*, it is because all aspects of this work are displayed together. As has already been pointed out, the notion of science as a symbolic representation (its scientific aspect) and the distinction between physical and metaphysical theory (its metaphysical aspect) and the struggle between realism and instrumentalism throughout history, are based on the distinction between essence and phenomena. All these aspects, as has been shown, seem to lead the Ariadne's thread that guides the work scientific and philosophical and historical of Pierre Duhem: the apology of the ecclesiastical authority.

However, Duhem assigned another task to the history of science. As is well known by all the scholars of the thought of the French physicist, Duhem has exposed in various places such a task to be played by the history of physics in the conception of the physical theory. In 1892, the article "Quelques réflexions au sujet des théories physiques", it is exposed, in paragraph 7, "Le rôle des théories mécaniques dans l'histoire de la science", and it answers the question: "If these theories are based on an idea of the role of physics that is so completely erroneous, how does it come about that they have been able to make such great progress in physics?" The answer to this question is that in the "evolution" of all science mechanical theories correspond to the first stage of the development of the physical theory. We noticed that here it is the role of the mechanical theories in the history of science and not – as in other parts of the work of Duhem – of the importance of the method of the history in physics. The interest of the exposed in this article of Duhem is that already on this date is the idea of "evolution" and the development of the theories of physics and, therefore, of a conception of the history of physics, idea reinforced with the analogy of the development of human intelligence.

Of the above, arises the problem of what is the purpose of the evolution of the theories of physics, whose stage of childhood are the mechanical theories? The answer to this question can be found in *La théorie physique*, chapter III of part one – "Les théories représentatives et l'histoire de la physique" – whose first section is dedicated to explaining the role of the *natural classifications* in the evolution of the physical theories: The purpose of the physical theory is to become a natural classification. To explain the role of these *natural classifications* it asks: If the theory must become a natural classification, whether it should seek a group the phenomena as are grouped the realities, then the most secure method of achieving this goal is not that of search before all what are these realities? (Duhem, 1906, 45-84).

The solution of this questioning is carried out by means of the distinction of two constituent parts of any theory that seeks to explain the phenomena: first a representative part and second an explanatory part. The development of each of these parts is performed independently from one another, your link is "very weak and superficial" and the explanatory part is juxtaposed to the representative part like a parasite. In

¹⁸ From the work of Paul Needham (1998), Brenner (1990), and Stoffel (2002) has deployed the debate on the realism and/or instrumentalism in the thinking of Duhem. More recently, an amount of different types of Duhemian realism has been deployed: the structural realism of Elie Zahar (2000), the motivational realism of Merikangas Karen Darling (2003). Consult the work of Fábio Leite (2017) for this theme.

addition, this independent development of each one of the two parts of the physical theory has a consequence related to the history of physics:

While the progress of experimental physics put the theory in difficulties, while forced to modify it, to transform it, the purely representative enters almost whole in the new theory, gives you the inheritance of everything more valuable than had the old theory; while the explanatory part succumbs to leave their place to another explanation. Thus, for a continuing tradition each physical theory inherits the part of the natural classification that has been able build to that which follows. (Duhem 1906, 48)¹⁹

Thus, according Duhem the role played by the history of science generally and the history of physics particularly is reach the goal of becoming the natural classification, that is, a classification that reflects the ontological order. However, since the belief or conviction in a tendency to the natural classification, has as its foundation on "an intuition in which Pascal has recognized one of those reasons of the heart 'which reason does not know', he affirms his faith in a real order, which theories are an image clearer and more faithful every" (Duhem 1906, 38-39), then the apologetic role of the history of physics is maintained.

In sum, in the three general and principal parts of thought of Pierre Duhem, it keeps the apologetic aspect of such thought. In fact, in all these parts of the thinking of the French philosopher the notion of *bon sens* has a fundamental role and the *bon sens*, in turn, is based on the doctrine of the heart of Pascal, as we hope to show below.

Duhem and Aristotle

Some of his commentators and selfsame Duhem have pointed the Aristotelian background of his thought. Martin has rightly pointed out that in the conclusion of *Le mixte et la combinaison chimique: Essai sur l'évolution d'une idée*, Duhem explained an analogy between your physics and that of Aristotle, and in the "Physics of a believer" is included the section IX entitled: "De l'analogie entre la théorie physique et la cosmologie péripatéticienne". Stoffel in his turn has said that the Duhemian phenomenalism is formulated not only to respond the criticisms and dangers, but that from very early has its roots in an Aristotelian perspective, recognized by Blondel from 1893 by calling to your correspondent: my dear peripatetic (Stoffel 2007, 339).²⁰ Martin has related directly and explicitly the "Aristotelian flavor" of the thought of Duhem with its methodology of the *bons sens*. However, Martin appears to reject the Aristotelian influence on the thinking of the French thinker because it is based on superficial analogies. Probably Martin is right because this influence is not always carried out directly, but through multiple transformations occurred over a long period in the time, as we hope to show with regard the relationship between the notion of *noûs* and *bon sens*.

In chapter IV of its magnificent book about Duhem, Martin relates the Aristotelian *epagôgè* with the problem of the infinite regress,²¹ a problem that arises from the claim that we should not accept any proposition that has not been proven and we should not accept any term that has not been previously defined. The rigorous application of this idea leads, of course, the infinite regress in the demonstrations and in the definitions. According to Martin, Aristotle proposes as a solution the *epagôgè*, which in the Stagirite is

¹⁹ "Lorsque les progrès de la Physique expérimentale mettent la théorie en défaut, lorsqu'ils l'obligent à se modifier, à se transformer, la partie purement représentative entre presque entière dans la théorie nouvelle, lui apportant l'héritage de tout ce que l'ancienne théorie possédait de plus précieux, tandis que la partie explicative tombe pour faire place à une autre explication. Ainsi, par une tradition continue, chaque théorie physique passe à celle qui la suit la part de classification naturelle qu'elle a pu construire..." (Duhem 1906, 48).

²⁰ Stoffel añade: "Duhem poursuivra d'ailleurs dans cette voie aristotélicienne en restaurant les qualités au lieu de s'en tenir aux seules quantités et, une dizaine d'années plus tard, en établissant, d'une manière qui paraîtra peu convaincante, une certaine analogie entre la thermodynamique et la cosmologie du Stagirite" (Stoffel 2007, 340).

²¹ Martin points out that: "Aristotle, almost certainly the originator of both the infinite regress argument and of the formal logic, without which it could hardly have been formulated. [...] Aristotle's answer, *epagôgè*, often translated 'induction', seems to be a kind of intuitive process in which in course of continuous immersion in experience, the principles of science emerges from the contemplation of many instances of its objects" (Martin 1991, 72).

closely linked with the *aisthesis* or sensation. Aristotle's the manner to solve the problem, says Martin, is to be compared with the treatment that gives Leibniz in his letter of April 1686, addressed to his friend skeptical Simon Foucher, where it is concluded that: "Proofs could not go to infinity [...]; but it remains the case that there is no way of proving these principles" (Martin 1991, 73). To finish the chapter IV of his book, Martin points out that Pascal has his special way of dealing with the problem of infinite regress: "It is in the *Pensées* that the full implications of his position emerge, with his doctrine of the heart that has its reasons that reason does not know". This specialist in Duhem's work appoints one of the most well-known fragments of the *Pensées*, and concludes:

However, here, Pascal has other ends in view, an *Apology for the Christian religion* [...]. Reason and sense thus play different *rôles* in different subject-areas, which, [...], are separated from one another, separated by method as they are separated by the subject-matter. Equally, [...], Pascal has separated off faith from physics in just the manner we have seen in Duhem, ruling out equally the use of natural reasoning to defend Christianity and to attack it. (Martin 1991, 75)

Martin has enabled us to establish a close relationship between the methodology of the *bon sens* and the Aristotelian *epagôgê* through the problem of the infinite regress; and between the problem of *infinite regress* and the solution proposed by Pascal to this problem with "his doctrine of the heart". According to Martin, his doctrine leads precisely to the separation, at the same time methodological and thematic, Duhemian and Pascalian, of two areas that do not touch each other: the reason and the senses are two faculties that serve entirely different areas, in such a way that the natural reason may not be useful neither to defend nor to attack the faith. However, let's not forget that to Aristotle, the virtue which allows us to grasp the principles is the *noûs* through the procedure of the *epagôgê* which appears to be the solution proposed by the Greek philosopher the problem of the infinite regress. In sum, it is possible to establish a relationship between the *bon sens* and the *noûs* and "doctrine of the heart". We need to return to the thought of Aristotle to explain this possible relationship.

Aisthesis and *Noûs* in Aristotle

In the sixth book of the *Nicomachean ethics* we find the famous Aristotelian classification of the different kinds of virtues by which the knowledge is acquired: *téchne*, *epistéme*, *phrónesis*, *sophía* and *noûs* (Aristotle 1926, 1139: 15-20). Later, in this work, it is argued that only the *noûs* can capture the principles. Aristotle explains what should be understood by each of these capacities and concludes that the principles can only be captured by the *noûs*, although it is not very clear what the procedure by which the *noûs* captures the principles is. In the *Posterior analytics* Aristotle expresses:

Since we learn either by induction [*epagôgê*] or by demonstration [*apodeixis*]. Now demonstration proceeds from universals and induction from particulars; but it is impossible to gain a view of universals except through induction [*epagôgê*] (since even what we call abstractions can only be grasped by induction [*epagôgê*], because, although they cannot exist in separation, some of them inhere in each class of objects, in so far as each class has a determined nature); and we cannot employ induction if we lack sense-perception, because it is sense-perception that apprehends particulars. It is impossible to gain scientific knowledge of them, since they can neither be apprehended from universals without induction, nor through induction apart from sense-perception [*aistheseos*] (Aristotle 1960, 81 a35-b5).

The widespread and common interpretation of this paragraph is that the described procedures are two: 1) one, take as starting point the universal and through a deductive procedure is inferred a conclusion or conclusions²²; and, 2) another, ask for the origin of the knowledge of universals (*Posterior analytics*, 99b

²² In the *Nicomachean ethics* Aristotle (1926, 1139b 20-35) uses a different terminology. It says here that the whole *episteme* has the ability be *didaktè* (taught) and *matheton* (learned); the teaching can be by *epagôgê* or *sillogismòs*.

15 ss), Aristotle concludes that we must have a *dýnamis*, a faculty that captures the universal (99b 30-35). In the first lesson of *La science allemande*, which is about the "sciences of raisonnement", Duhem (1915, 4-22.) explains the behavior of these sciences, corresponding to the previous Aristotelian explanation and concludes questioning: "Des axiomes, quelle est la source?"

In the final part of the *Posterior analytics* (99b-100a.) seems to be exposed, in some detail what was stated at the beginning of the *Metaphysics* (980a-981b). The first work Aristotle deals with how to acquire the principles, how they become known, and questioned what is your *hexis*. He proposes two alternatives: 1) we possess them (innately) and 2) we acquire them through a procedure. The first alternative is discarded as absurd, since we would have something of which we were unaware and the second alternative is in contradiction with the idea that all knowledge comes from previous knowledge. Without clarifying this contradiction, it is concluded that there must be a *dýnamis* or power which we acquire the principles.

Immediately after, it is stated that there is an innate *dýnamis*, a faculty to distinguish, called *aesthesis*, sensation. There are two ways to put into action this power: 1) with persistence or perseverance and 2) with neither perseverance nor persistence. When this faculty is done without persistence, there is no more knowledge than the sensation selfsame, and when she performed with persistence there is a knowledge besides of the sensation: of the sensation arises the memory and of the repeated many times memory of the same thing arises the experience and of this experience arises the principle of art and science. Aristotle explains that when in the soul some of the entities persists or is still present, then for the first time it is presented in soul the universal; and in the next place, produces a new detention or persistence of the initially achieved universal and so on until the first universals. Aristotle concludes by saying that: evidently we know necessarily by *epagôgè*, since this is how the *aesthesis* produces the universal. Finally, it explains that of all the *hexis*, the habits exposed in the *Nicomachean ethics*, the *episteme* and the *noûs* have a relationship and which the latter is the most accurate of all, since the *archai* (principles) are better known than the knowledge provided by the *apodeixis* (demonstrative procedure) and, therefore, the principles are captured by the *noûs* and that there is nothing more true than this one (*Posterior analytics* 100b 1-15).

In the paragraph immediately above, we shows a close relationship between the *aesthesis* (the sensation) and the *noûs* (faculty that captures the principles): the first is the starting point of the *epagôgè* which is the procedure through occurs the universal in the soul, the universal is another name for the principles; while the second – *the noûs* – is both the *hexis* and *areté* that captures the principles and is the starting point of the demonstrative procedures (*apodeixis*), therefore, too it is the starting point of the *episteme* (science). While the ability to sense – *aisthesis* – is the point of departure of the procedure; the *noûs*, ability of grasp the principles, the noetic catchment, is the point of arrival of procedure by which are provided us the principles of knowledge.

From the *Noûs* to the *Bon Sens*

A manifestation of the syncretism product from the confrontation of the Greek culture with the Alexandrian-Jewish culture was carried out by the task of translation of the sacred texts of Judaism. This translation was commissioned by Eleazar, the Jewish high priest, to 72 Jewish priests, and requested by Ptolemy II Philadelphus. So, this translation is known as the bible of the 70. According to the *Dictionary of theology* of Lothar Coenen and Erich Beyreuther and Hans Bietenhard, the Greek word *noûs* is related to a group of words: *noéo*, *diánoia*, *énnoia*, *nóema*, *anoia*. "If it is compared the central rôle played by the *noûs* in Greek thought, one is surprised by the low use that make the LXX of this group of words. The word appears with a greater relative frequency is *diánoia*, with 75 testimonies; on the contrary, *noûs* and *noéo* have only 35 testimonials each; the other derivatives are scarce even more" (1994 [1971], 10).

According to the authors of this dictionary, "the limited presence of this group of words in the Septuagint is because the Hebrew does not have an equivalent of the Greek [word] *noûs*", which is often translated by the words *Léb* or *lébáb*, which in the version of the Septuagint are used 6 times to translate the Greek word *noûs* and 38 times to translate the Greek word *diánoia* which 'almost always is replaced by *kardia*", "heart". Likewise, in the version of the Septuagint, also it is used [the Greek word] *diánoia* (about

The syllogism part of principles that cannot be tested by syllogism, but by the *epagôgè*. However, it is not very clear procedure in which consists the *epagôgè*.

75 testimonials) to translate the Hebrew word *Léb* or *lébáb*, "heart"; in addition, are also used some twists as: "an honest sense" (4 Mac 1, 2, 16; 35); "a pure sense" (8, 3 TestBen); "good sense" (4, 12). Also in Josephus: a *healthy sense* (Ant. 8, 23); in a very typical manner of Old Testament, the heart is called organ of knowledge (Prov 16, 23; Is 6.10).

According to the *Dictionary of theology* cited earlier, Philo of Alexandria used the Greek words *noûs* and *diánoia*, referring to them as "the divine in the human" (Det. Pot. Ins. 29), "an organ of divine knowledge" (Virt. 57), that which makes man immortal (Op. Mund. 135). In addition, according to Marta Alesso, "Philo says that the soul is composed of the following parts: the 'guideline' (*hegemonikon*) or 'rational' (*logikon*) part which is the same as the intellect (*noûs*) and the part irrational (*alogos*)..." (Alesso 2011, 22). We emphasize that are identified *logos* and *noûs*. The encounter of Greek philosophy and the Jewish religion transformed the *noûs*, both identifying it with the *logos* and transforming it into the *kardia*, the heart. Something similar happens in Christianity, to display it extensively quoted from the *Dictionary of theology*:

If we throw a first glance superficial on the frequency with which the group of words related to *noûs* in the New Testament, confirms the finding that we did in the LXX: neither in the N[ew] T[estament] plays a central rôle. *Noûs* is attested only 24 times; *katanoéo* and *noéo* are 14 times each; the other forms derived are still more rarely. However, this statistical glance provides us a false impression. Certainly, the theme of the *noûs* plays a secondary rôle in the Gospels (with the exception Lc [Lucas], which uses 8 times *katanoéo*) and in the Post-Paulines letters; instead, the type of Hellenistic Greek mentality is found most frequently in Paul (21 of the 24 testimonies of *noûs* belong to the so-called *corpus paulinum*). The noun *noûs*, which appears in Paul and the writings of the Post-Paulines (Eph, Col. and pastorals) means *mind* in terms of *discernment*, *the ability to judge*, *ability to discern* (e.g. 2 Thess 2, 2). But this discernment is the religious insight, the ability to judge religious, which is situated next to the consciousness (Tit 1.15). Thus, *noûs* occupies a place parallel to the faith, which in the Pastoral Letters comes to mean "religion"; in Rom 7, 23 Paul writes: "In my body I perceive different criteria that is waring against the criteria of my *noûs*". Later, in verse 25 it is said: "On the one hand, with my *noûs* (is to say: I as *noûs*) I am subject to the law of God; on the other, with my instincts (that is to say: I as meat) I am the law of sin". This *noûs* is the same as the *éssó ánthrópos*, the inner man (that is: inner man, in the most intimate, in his own interiority; cf. Rom 7.22; Eph 3.16) or the *egó* (Rom 7, 9.10.14.17.20.24.25), the authentic self, that can discern between good and evil. The ego recognizes that the law is good; the ego wants to comply with the law, but the law recognized by the *noûs*, law of religious insight, contradicts the other law of sin. Therefore, here *noûs* is the knowledge and understanding religious, who recognize and honor the law of God. Those appointments where *diánoia* is in *parallelismus membrorum* with *kardia*, *heart* show it like this. (Heb 8.10; 10.16; of the LXX Jer 38, 33) (Coenen, Beyreuther and Bietenhard 1994 [1971], 12-13)

The meeting of Greek thought with Judeo-Christian thought has transmitted to us the Greek word *noûs* through the notion of *kardia*, *heart*, both words with the meaning of "the capacity of capture first principles". The predominance of the Jewish-Christian thought on the pagan Greek thought, derived from his meeting, obliterated the Greek concept of the word *noûs* and privilege the Jewish-Christian notion of the word *heart* or from the Greek word *kardia* thought-out of Jewish-Christian way. This predominance of the Jewish-Christian thought can be displayed even we review one of the foundations of the Reform movement. According to Richard Popkin (1989), in fact, one of the fundamental principles of the reform of Luther is his questioning of the authority of the criteria of Pope and councils for accepting the truth of the scriptures. If Luther rejects the authority of the Pope and councils as a criterion of truth of the Bible, then what is the criterion that the reformer accepts to determine the truth of the Scriptures? The answer is to be found in various places in the work of Luther, let us mention two, the first of the *Discourse at the diet of worms* in 1521, and the second, at the *De servo arbitrio* of 1525:

Since your distinguished majesty and your lordship demand me a response, I will give it unabashedly: Unless I am convinced by the testimony of Scripture or for obvious reasons – because I do not believe in the Pope nor in the Councils alone, since clearly they were wrong and have often contradicted themselves – I am chained by the scriptural texts that I have quoted and my *conscience* is captive to

the Word of God. I cannot and do not want to recant in nothing, because it is neither safe nor honest to act against self-awareness. That God help me. Amen (Luther, 2006, 175, emphasis mine).

In *De servo arbitrio*:

But, as I said before, I will not openly express myself. In the meantime, I excuse your *very good intention of heart*; but do you go no further; fear of the Spirit of God, who searches the reins and the heart, and who is not deceived by artfully contrived expressions. I have, upon this occasion, expressed myself thus, that henceforth you may cease to accuse our cause of pertinacity or obstinacy. For, by so doing, you only evince that you hug in your heart to Lucian, or some other of the swinish tribe of the Epicureans; who, because he [Epicuro] does not believe there is a God himself, secretly laughs at all those who do believe and confess it. Allow us to be assertors, and to study and delight in assertions: and do you favor your Sceptics and Academics until Christ shall have called you also. The Holy Spirit is not a Skeptic, nor are what he has written on our *hearts* doubts or opinions, but assertions more certain, and more firm, than life itself and all human experience (Luther 1931, 9, emphasis mine).

It seems, then, that the “doctrine of the heart” of Pascal can be traced back to the doctrine of the Greek *noûs*, but mediated by the Jewish-Christian tradition and transformed by a series of attempts to reconcile this with him. In this sense, it is possible to speak, as Duhem speaks about *the natural classification*, of a tradition of thought which is inherited along the passage of time, but that does not pass in the same way from generation to generation, but amending and fused with other forms of thought. As well, seems that the Greek *noûs* was inherited and transformed into the notion of “heart” of the Jewish-Christian and the Jewish-Christian notion of “heart” was inherited and transformed into the Duhemian *bon sens*.

Epilogue

According to Stoffel, in the analysis of the task apologetic of the thought of Duhem, we must set the following distinction: there are explicit and implicit tasks. According with this specialist of the thought of Duhem, the apologetic task of the work of French physicist is of second type, an implicit task; that is to say, what motivates Duhem to develop its special conception of the physical theory is not the apology of the religion, but there are in this work an apology of the religion (Stoffel 1995). However, it seems that it is not possible to deny the existence of an apologetic task in the scientific and philosophical and historical work of Pierre Duhem. This inability to deny the apologetic task in the thought of the French physicist is sufficient to establish the possibility of qualifying his thought as or a “crypto-theology of Providence” or as having a hidden agenda.

The opposition between the scientific and apologetic reasons or motives of a system of thought, can also be derived from the perspective which there is such opposition in the science and religion’s very nature. This perspective part of determined notions of science and religion which allow us to set the opposition. Duhem seems to have been aware that the opposition between science and religion is based on these determined notions of both human aspects and, precisely, one of its objectives is to combat such notions of science and religion which establish an opposition between them. Instead, it proposes notions of science and religion that allow its integration. Duhem does not renounce – as positivism – neither the existence of a link between science and metaphysics nor cancels – like positivism – the cognitive reaches of metaphysics.

According to Duhem, science and metaphysics use the same methods and take the same starting point. But neither apply in the same way such methods nor did they take the same way such starting point. They apply the methods in accordance with the specific subject-matter of study of each discipline and the point of departure is taken according to each type of thought. In this way, the *sens commun* can be the starting point of the principles of the *sciences of raisonnement* and the *bon sens* can be the starting point for the hypothesis of the *experimental sciences* and the *esprit de finesse* can be the starting point of philosophy, of religion and of the articulation between science and metaphysics. For this reason, the spirit of fineness, in its different forms, play a role of paramount importance in the system of thought full of the multifaceted thinker of Bordeaux. *Bon sens* that comes from a tradition that goes back to the Greek *noûs*

notion and arrives to our days.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 127-139

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Duhem's Legacy for the Change in the Historiography of Science: An Analysis Based on Kuhn's Writings

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Abstract:

What is the contribution of Duhem's work to the modern historiography? His interpreters have been discussing this question and ordinarily have recognized that the main aspect in his extensive work is connected with his research of medieval science. It has become customary to speak of the "discovery of medieval science" as his foremost historiographic achievement. This paper aims to discuss some aspects of Duhem's historiography more for its promotion of a new historical perspective than for its results. Duhem's legacy for modern historiography can be investigated from the characteristics that mark this new perspective, as regarded by Thomas Kuhn.

Keywords:

Pierre Duhem; Thomas Kuhn; historiographic revolution; new historiography of science

127

Received: 30 March 2017. Reviewed: 10 May 2017. Accepted: 30 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.12>

Introduction

Pierre Duhem is commonly regarded as a pioneer in the study of medieval contributions to the development of modern science.² For many of his interpreters, his extensive historical work has generated a true revision of scientific development, to the point of speaking of a historiographic revolution. However, in *The structure of scientific revolutions* (SSR), precisely when announcing an ongoing revolution in the historiography of science and citing the names of some influential historians, Kuhn does not mention Duhem. It is only in an article published a few years later – "The history of science"³ –, in which he globally examines the development of this field of study, that Duhem is remembered for his contribution to the modern historiography of science. In this work, I try to show that although Kuhn does not include Duhem's name among the 'new' historians nor does he refer to any of his works in SSR, Duhem's work is strongly present through its influence on some of the historians that Kuhn cites.

Initially, I begin by identifying the citations Kuhn makes of Duhem's name in his published work and by briefly mentioning some considerations of interpreters who have tried to establish relationship between Kuhn and Duhem. Next, I will try to indicate, according to Kuhn, the fundamental distinctive features between

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² For example, see Koyré (1973 [1966], 61) and Butterfield (1966 [1949], 27).

³ Published in 1969. Reprinted in his *The essential tension* (1977).



an older historical tradition and the emerging new historiography of science. I expect to demonstrate how these features were already present in Duhem's work and how his influence can be felt in his followers' works. Finally, considering that the rise of a new historiography of science is undeniably due to Duhem's legacy, and that the central features of this new historiography mark his historical production, I will try to understand why his name was entirely neglected by Kuhn in his study of scientific revolutions.

Duhem in Kuhn's Work

In a work published to mark the 50th anniversary of the publication of SSR, Brad Wray (2015, 168) says that although an author may be influenced by more sources than he cites, "citations are a good place to start looking, in order to understand the influences" on him. Curiously, he affirms that "Kuhn does not cite some of the sources that clearly influenced him".⁴

If Kuhn does not mention Duhem in SSR, he does so in some of his other works. In his first article⁵ – "Robert Boyle and structural chemistry in the Seventeenth Century" – Kuhn cites Duhem in two notes. In the first, he (1952, 12) considers that Duhem, among other historians, explores a simplification "which treats all atomisms as mere particulate theories" and although he bears in mind that this simplification had illuminated portions of the history of Chemistry, it had also been misleading. In the second, Kuhn (1952, 13) says that "Duhem sees Boyle's application of the corpuscular theory of matter as the first source of 'the notion of a simple substance such as that provided by Lavoisier and his contemporaries'".⁶

It is interesting to note that Kuhn, in this article, contests almost all of the history of Boyle's role in chemistry in the seventeenth century, maybe with the exception of Marie Boas's studies. And he cites a number of historians in his 90 notes, many of which are cited in SSR.⁷

It is also in notes that Duhem's name appears in *The Copernican revolution*, Kuhn's book published in 1957. In the introductory part of his bibliographical notes, Kuhn asserts that Duhem's *Le système du monde* could have been used very often in the composition of *The Copernican revolution*, but that it was only consulted "for special topics" (1970b [1957], 284).⁸ When Kuhn (1970b [1957], 286) indicates bibliographies for the chapters, he refers to Duhem's *Le système* for research on Arabic and medieval European astronomy.

In the same manner as in the article on Boyle, the bibliographical notes in *The Copernican revolution* are very extensive and anticipate the mentions of many scholars cited in SSR.⁹ It is also important to note that both the article on Boyle and the book on the Copernican revolution are works on the history of science and, although Kuhn traces some historiographic considerations in those works, he did not do so in relation to Duhem.

In fact, Kuhn presents a historiographic analysis in his (autobiographical) preface to SSR. There he writes about his immersion in the study of the history of science and about a group of historians that "has shown what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today" (Kuhn 1996 [1962], viii). He mentions the names of Alexandre Koyré, Émile Meyerson, Hélène Metzger and Anneliese Maier and their works that were "particularly influential" to a new historical perspective. By describing the functions of the history of science, as bequeathed by tradition,

⁴ Although the focus of Brad Wray's discussion is different from the one here intended, his analysis of citations made by Kuhn in SSR is informative: "Kuhn cites 127 different sources in the first edition of *Structure*, with a total of 206 citations. An analysis of these sources suggests that, even though *Structure* profoundly influenced scholarship in history, philosophy, and sociology of science, Kuhn drew mostly on work in the history of science" (Wray 2015, 168).

⁵ See Kuhn (2000, 291).

⁶ This view provided by Duhem in *Le mixte et la combinaison chimique* (1902), according to Kuhn, was dominant among the historians of science. So far as he knew, only Marie Boas would have explicitly indicated (until then) that Boyle did not believe in the existence of elements – a thesis defended by Kuhn in his article.

⁷ Among others, E. Meyerson, E. Burt, M. Boas, H. Kopp, J. R. Partington, D. Mckie, H. Butterfield, H. Metzger, A. Koyré and R. Hooykaas are cited in SSR.

⁸ Kuhn cites six out of ten volumes of *Le système du monde*. The seventh volume was published only in 1956, shortly before Kuhn published his book.

⁹ A. Koyré, H. Butterfield, E. J. Dijksterhuis, A. C. Crombie, E. Burt, M. Clagett, A. Maier and M. Boas are examples. In SSR, Kuhn's own *Copernican revolution* is cited half a dozen times.

he announced a change that represents the beginning of a “historiographic revolution”, of which representative historians

have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinions of that group and other similar ones from the viewpoint – usually very different from that of modern science – that gives those opinions the maximum internal coherence and the closest possible fit to nature (Kuhn 1996 [1962], 3).

Compared with the writings of historians of the older historiographic tradition “these historical studies suggest the possibility of a new image of science” (Kuhn 1996 [1962], 3). Kuhn presents the SSR as an essay about this image emergent from the “new historiography”.

Some years later, in an article published in 1968 – “The history of science” – Kuhn speaks again of a “new historiography” or, in a correlative way, of a “modern historiography of science” (Kuhn 1977). In this work, Duhem's name is mentioned twice. Firstly Duhem is positioned in a historiographic tradition that was more philosophical in its objectives. His writings are remembered alongside Whewell's and Mach's as those in which the “philosophical concerns became a primary motive for creative activity in the history of science” (Kuhn 1977, 106). Kuhn's second mention of Duhem is more expressive, because it occurs when he is writing about factors that have contributed to the historiographic change and mentions “another decisive event on the rise of the contemporary profession” (Kuhn 1977, 108):

Almost a century after the Middle Ages had become important to the general historian, Pierre Duhem's search for the sources of modern science disclosed a tradition of medieval physical thought which, in contrast to Aristotle's physics, could not be denied an essential role in the transformation of physical theory that occurred in the seventeenth century. Too many of the elements of Galileo's physics and method were to be found there. But it was not possible, either, to assimilate it quite to Galileo's physics or to Newton's, leaving the structure of the so-called Scientific Revolution unchanged but extending it greatly in time. The essential novelties of seventeenth-century science would be understood only if medieval science had been explored first on its own terms and then as the base from which the “new science” sprang. More than any other, that challenge has shaped the modern historiography of science (Kuhn 1977, 108).

In this article, Kuhn cites Duhem's *Études sur Léonard de Vinci*¹⁰ in the bibliography, among about 60 other works that support his argument. Here, Duhem's work seems very significant in the formation of a modern historiography of science.¹¹ So, the question that may be raised is: If Kuhn considers the rehabilitation of Middle Ages science to be significant to the emergence of a new historiography of science, why does he not cite Duhem among so many other scholars in his main book?

This question invokes the attempts of some interpreters to establish a relationship between Duhem and Kuhn. Agassi (2002, 409), for example, in his review of the posthumous collection of essays by Kuhn, published in 2000 as *The road since structure* (RSS), asserts that “Kuhn ignored his debt to Duhem while respecting his leading followers (286-287)”. He indicates pages 286 and 287, where Kuhn mentions the names of philosophers and historians of science, such as Koyré, Meyerson, Metzger and Maier. Agassi

¹⁰ *Études sur Léonard de Vinci* (1906-1913). The first and second volumes have as subtitle *Ceux qu'il a lus et ceux qui l'ont lu*, the third, *Les précurseurs parisiens de Galilée*. Hereafter cited as *Études*.

¹¹ Kuhn cites, among others, A. Koyré, M. Boas, H. Butterfield, M. Daumas, H. Guerlac and H. Metzger. All these historians appear in SSR related to some aspects of the emergent new historiography of science.

(2002, 409) ironizes the fact that Kuhn remembers Duhem in RSS only because of the fact that he invented a term.¹²

Stanley Jaki (1987) and John Worrall (1995) also discuss the rapprochement between Duhem and Kuhn's views. The former, one of the greatest scholars of Duhemian work, by suggesting that Duhem made many contributions to the philosophy of science, claims it to be surprising to a judicious reader of Duhem that there is not a single reference to him in SSR (Jaki 1987, 370).¹³

John Worrall does not limit himself to presenting similar aspects between the two philosophers and historians of science, going so far as to say that there is

nothing of real relevance to this particular issue in *The Structure of Scientific Revolutions* that was not raised already in Duhem's *The Aim and Structure of Physical Theory*. Indeed many of the Kuhnian theses that have created such a stir in philosophy of science seem at root to be (often rather less clear) restatements of Duhemian positions (Worrall 1995, 77).

Those considerations can even be suggestive, but they are very fast and general. Moreover, they are presented with a focus on the philosophy of science and not on the historiography of science, which is the scope that interests me. In this sequence, I try to provide some material to help bring Duhem closer to the new historiography of science, from the characterization of historiographic change, as expounded by Kuhn.

From the Older to the New Historiography of Science: Duhem's Spot

When Kuhn recognizes an ongoing historiographic revolution in SSR, he exhibits the state of change in the historical perspective. Traditionally, the history of science was conceived as the discipline that recorded the successive increments of scientific technique and knowledge while at the same time registering the obstacles that have inhibited their accumulation. Under this conception, according to Kuhn (1996 [1962], 2), the historian had two main tasks: to "determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented" and to "describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text". This perspective is particularly found in science textbooks, in which discarded theories are considered unscientific¹⁴ and what is relevant is the identification of the individual contributions, the place and the date they occurred.¹⁵

In the ongoing historiographic revolution – "still in its early stages" (Kuhn 1996 [1962], 3) – a group of historians have begun to put together other kinds of questions. "Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time" (Kuhn 1996 [1962], 3). Kuhn's example is about the study of Galileo's contributions. Some historians no longer sought Galilean contributions in relation to modern science, but sought to understand it in their own context, that is, in "the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences". And, as says Kuhn, they also insisted upon studying the conceptions of these thinkers from a viewpoint that is very different from that of modern science, that gave those conceptions "the maximum internal coherence and the closest possible fit to nature" (Kuhn 1996 [1962], 3).¹⁶ In doing so, they presented works that provided a completely divergent image of science from the one supplied by writers in the older historiographic tradition.

¹² In RSS, Kuhn (2000, 235) says: "When I entered history of science, it was customary, largely due to the influence of Pierre Duhem, to speak of 'medieval science' and I often used that highly questionable phrase myself".

¹³ Jaki (1987, 370) cites works by Cardwell and Beauregard, who explore the comparison of similarities between Duhem and Kuhn, noting that the matter is dealt with politeness and fear by the former, and discretion by the latter. His considerations are limited to a note.

¹⁴ Kuhn (1996 [1962], 99) explores the example of the "much-maligned phlogiston theory".

¹⁵ "When oxygen was discovered?" is Kuhn's example (1996 [1962], 2), by which he refers again to the history of the development of chemistry.

¹⁶ Alexandre Koyré's writings are considered here as, "perhaps", the best example of this new way to investigate the past of science.

From these brief considerations, it is possible to extract some important features that distinguish the work of some historians from those that represent the older historiography. Those who were promoting the revolution showed that the history of science (1) could and should be more than a repository for anecdotes or chronologies, (2) can no longer be conducted by the debates about priorities and (3) was based on an attempt to understand the past in its own terms. These features are interrelated and have implications in other aspects that, as we shall see, would constitute the modern history of science. Now I will try to show that those features are fundamental in Duhem's work.

Duhem: A Source for the New History of Science

Duhem's work in no way resembles the old manuals of the history of science, which are a repository for more of the anecdotes or chronologies. It is a repository of ideas, discussion and reflections. If we compare it with, for example, Sarton's, the differences are enormous, although Sarton had also been a historian devoted to the study of medieval science.¹⁷ Crombie (1959, 164), by the way, helps us think in this comparison in his review of two of Sarton's works, by saying that "Sarton was a man of facts rather than ideas", that "his most substantial contribution was to the bibliography of early science" and that "he did not work with the philosophical and analytical approach to the history of science such as is now, in the hands of younger scholars, throwing so much light on the development and character of scientific thinking".¹⁸ We have here the suggestion of a change in the history of science and we can suggest that the "younger scholars" were, like Crombie, the historians that, according to Kuhn, are making the revolution.

In Duhem's work, biography or bibliographical considerations are justified by the insertion of new, previously unknown characters in the history of science. Such is the case, for example, for his *Études sur Léonard de Vinci*. Before speaking of Albert of Saxony's influence on Leonardo da Vinci, Duhem dedicates a section to explain who Albert of Saxony, a name hardly pronounced in the history of science, is. In the beginning of his *Études*, Duhem (1984 [1906], v.1, 1) states that the history of science is misrepresented by two prejudices, so similar to each other that they could be taken as one: that scientific progress occurs through sudden and unforeseen discoveries and that the works of genius men have no precursors at all. He insists that the great discoveries

are almost always the result of a slow and complicated preparation, chased in the course of the centuries. The doctrines professed by the most influential thinkers come from a multitude of efforts, accumulated by a series of obscure workers. Those who we are accustomed to call creators, Galileo, Descartes, Newton, did not formulate any doctrine that was not bound by the innumerable lines to the teachings of those who preceded them (Duhem 1984 [1906], v.1, 1-2).

This passage is only one example among many other of Duhem's manifestations against the "eureka-moment"¹⁹ notion of a scientific discovery. He invariably criticizes the history of science that celebrates only the great discoveries and suggests the innovative character of his work that includes unknown contributions. The preface to Maire's work (1912) is a good example of Duhem's discussion about the difficulties involved in trying to determine priorities in the history of science:

There is a fine line between a scientific discovery and the personality who made it. In many circumstances, time quickly dissolves it. Sometimes, over the centuries, treatises and manuals continue to link the inventor's name to a mathematical proposition; to the law of physics, the name

¹⁷ In the introductory chapter of his most famous work, Sarton (1927) recognizes the need to consider the medieval science, but from an evaluative perspective very different from Duhem's. Kuhn (2000, 282) suggests the mention of his name as an example of "history of manuals". See also Preston (2008, 80-10) and Pinto de Oliveira and Oliveira (forthcoming).

¹⁸ It is worth noting that, in spite of his criticism, Crombie points out the importance of Sarton's work, "a devoted pioneer". Similarly Clagett (1957, 321) considers that Sarton's writings do not present a discussion of ideas and scientific activities and that his approach to the history of science is basically bibliographical and classificatory.

¹⁹ This expression is used by McEvoy (2010, 32) to refer to the vision of scientific discovery as a "single event of individual labor".

of the one who first enunciated it. One names: the theorems of Apollonius, the principle of Huygens. But, except some curious scholars, who then wants to carry out some research into the one who bears that name? When and where did he live? Who was he? By what sequences of meditations and essays did he come to know that truth for which he had not been entirely forgotten? These are questions that we never imagine to ask, that we do not suffer at all by seeing them unanswered (Duhem 1912, I).

Duhem places himself in a distinct position from that which prevailed in the traditional history of science. He insists on presenting a different view, according to which the historical analysis of a discovery or scientific creation is fairly complex because there is no way to establish an exact moment of its occurrence, to indicate a single name as being responsible for it, without incurring injustice and inaccuracy. "No scientific discovery is a creation *ex nihilo*", says Duhem (1912, III), and if that is truth, then

we have to explore a singularly extended domain whenever we wish to retrace the history of a discovery. It will not be enough (it's quite the opposite, actually) to meditate on the writings of the one to whom that discovery is commonly attributed. We have to search, read, compare the books of all those who, more or less directly, have been the auxiliaries of that person: the precursors who had prepared the new idea; the collaborators who assisted the inventor; the opponents who forced him to define, clarify and consolidate his thoughts; the successors who highlighted the latent fertility of this thought. We will have to review those of whom our author has spoken, those with whom he spoke and those who spoke of him (Duhem 1912, VIII).

Today, Duhem's insistence upon this question may seem exaggerated, but it is important to note the context in which he puts his argumentation. As indicated by Kuhn (1977, 106), the oldest traditions of history of science had produced "little significant historical research" before the nineteenth century. In other words, in his text of 1912 (as well as in his previous works) Duhem was writing against a point of view that was still dominant in his day, when heroic biographies were in vogue.

Duhem was aware of the innovative character of his work in relation to the traditional history of science. This can be observed, for example, in the preface to *Les origines de la statique* (1905), where he draws the reader's attention to the novelty of the content of his work, which would be singular in relation to other historical texts on the subject. The perspective presented there changed the history of static, which would entail a new ordering and characterization. On the one hand, Duhem stresses this innovation with enthusiasm, and on the other hand critically regrets classical history, which ignored the Middle Ages contributions to mankind (in science and art).

Duhem's comprehensive and investigative attitude in relation to texts of the past, however, is not only evident in relation to works and manuscripts, previously ignored by other historians that had studied medieval science.²⁰ This attitude is already present in a 1894 article, "Quelques réflexions au sujet de la physique expérimentale", in which Duhem discusses his need to understand theories of the past in his own terms. Duhem writes:

If the theories admitted by this physicist [who is investigated] are those we accept, if we agree to follow the same rules in interpreting the same phenomena, then, we speak the same language and can understand each other. But it is not always so; it is not so when we discuss the experiences of a physicist who does not belong to the same school as we do; it is not so, above all, when we discuss the experiences of a physicist fifty years, a century, two centuries apart. It is necessary, then, to establish a correspondence between the theoretical ideas of the author and ours, and, through

²⁰ According to interpreters, Duhem disclosed the medieval science during the writing of the first tome of *Les origines de la statique*. See, for example, Brenner (1990, 144) and Martin (1991, 147). Leite (2015, 28) writes about this discovery as "the 'historiographical turn' [which] occurred in a very specific way: it was the (re)discovery of Medieval manuscripts, forgotten by tradition, in which the historian glimpsed contributions that announced the modern static, which made him produce a genuinely historical work".

symbols that we accept, reinterpret what he interpreted through symbols he accepted. If we succeed in doing this, the discussion of his experience will be possible (Duhem 1987 [1894], 176).²¹

The discussions on conceptual change are recurrent in Duhem's work. While presenting the history of theories, Duhem provides elements for the interpretation of these theories in their contexts and one of the aspects that he is attentive to is the elucidation of the meaning of scientific concepts in specific contexts. The following passages, extracted from his discussion about Aristotelian physics, are examples:

The meaning of the word *movement* takes, in Aristotle's language, an extreme extension; it does not absolutely have the narrowness it has in modern physics in which it designates only the movement by which a body is transported from one place to another, the *local movement*.²²

[...]

What Aristotle calls movement in a straight line is what modern geometers name *translation movement*; all points of the moved body describe, at the same time, equal and parallel lines. The circle movement considered by the Stagirian is what we call the *rotation movement around an axis* (Duhem 1988 [1913], 160-171).²³

The attempt to understand a scientific theory of the past in its own terms led Duhem to give a distinct view from the one provided by other historians. I mentioned above (note 14) Kuhn's reference to the phlogiston theory as an example of a theory considered unscientific by the older history of science. It is worth noting that Duhem, though he had briefly discussed this theory, sought to review the dominant history about it in his day. In doing so, he postulated the need to

read Stahl's, his master Becher's, some of his predecessors', his contemporaries', his successors' writings. Now it is not at all distracting to search the yellowed and dusty pages of the old treatises of "Chemistry", in which the kabbalistic form of language confounds no less than the strange antiquity of thoughts, the germ of an idea which had to grow one day and produce our science. [...] we wish to become attentive inquisitors of the old scientific texts (Duhem 1916, 7).

For Duhem (1916, 6-7), "the victory of the oxygen theory over the phlogiston theory had in no way had the characteristics attributed to it": the inventor of phlogiston did not deserve the epithet of "mystic alchemist" and Lavoisier's victory was not "a victory of positivism over mysticism, of materialism over spiritualism". He announces one of his conclusions about the history of Chemistry in the foreword: Stahl's chemistry, in fact, contributed to Lavoisier's chemistry.

Duhem's attempt, manifested in many portions of his work, to provide a review of the past of science is closely related to some of the main features of the new historiography of science. In the following sequence, I discuss the influence of Duhem's historical analysis on some of the scholars chosen by Kuhn as his main inspirers.

Duhem and his Followers

Agassi (2002, 409) rightly notes that Kuhn acknowledged his debt to the main followers of Duhem in *The road since structure*. Koyré, Meyerson, Metzger and Maier are names presented there as those who brought forward, says Kuhn (2000, 187), "a sort of history, and an approach to history" that he admired and which he "encountered fairly early".

Kuhn invariably mentions Koyré's *Études galiléennes*, published in 1939, as an example of promising historical writing. In the introduction to this book, Koyré (1966 [1939], 11) writes: "Fortunately, it is no longer

²¹ The same passage appears in Duhem (1989a, 241).

²² Kuhn (2000, 17) also discusses the meaning of "motion": "When the term 'motion' occurs in Aristotelian physics, it refers to change in general, not just to the change of position of physical body. Change of position, the exclusive subject of mechanics for Galileo and Newton, is one of number of subcategory of motion to Aristotle".

²³ Other passages about "movement" can be found in Duhem (1988 [1913], 161, 171, 208; 1992 [1903], 10; 1989b, 466).

necessary to insist on the interest of the historical study of science. It is no longer even necessary, after the masterly work of a Duhem, of an Émile Meyerson; after those of M. Cassirer and M. Brunschvicg, to insist on the philosophical interest and fecundity of this study". The importance of Duhem's work to Koyré is not only manifest in a general way, but runs throughout the book. Besides praising Duhem's "masterly" work, Koyré (1966 [1939], 16) recognized that the history of scientific thought of the Middle Ages and the Renaissance became better known "thanks to the admirable works of Duhem".

In fact, Duhem's work is mentioned dozens of times in *Études galiléennes*, especially *Études sur Léonard de Vinci*, a source for discussion and argumentation. Although Koyré disagrees with some Duhemian interpretations, his recognition for his work is clear. In his "Rapport final" to the International Colloquium held in Paris in 1952, in commemoration of the 500th anniversary of Leonardo da Vinci's birth, Koyré writes:

A curious impression: that of the presence of a person, of a thought, of a work which was scarcely mentioned, which, even by those who have done so, has not been discussed, but which, as suggests by G. Santillana by the title of his communication – "*Leonardo da Vinci and those he did not read*" – seems to dominate us, or at least direct our work. It is in fact due to the admirable, but highly contestable work of Pierre Duhem and his *Études sur Léonard de Vinci - Ceux qu'il a lus et ceux qui l'ont lu*, that the problem of interpretation of Leonardo's personality and scientific work is set. (Koyré 1953, 237)²⁴

In another text about Leonardo, Koyré (1973, 100) echoes again the Duhemian historical perspective when he writes that, in order to suitably position Leonardo in the history of science, it is necessary to "confront him with his predecessors, his contemporaries and his successors."

Koyré's *Études galiléennes* was certainly one of the works that influenced Kuhn and it is worth remembering his example in SSR of studies about Galileo that focused on the relationship between that scientist's views and "those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences" (1966, 3). By the way, Anneliese Maier's researches into the precursors of Galileo have in Duhem's work an important secondary source.²⁵

It is common knowledge that historians of medieval science have contested several of Duhem's interpretations. Contemporary interpreters agree with their criticisms which, in most cases, relate to the discussions about the origin of modern science.²⁶ Maier, for example, writes:

Pierre Duhem, who must be credited with having opened up this new field of medieval studies, viewed fourteenth-century "physics" predominantly through the eyes of a natural scientist. He looked for the first glimmerings in the past of later discoveries without paying much attention to the intellectual milieu in which this "physics" belonged and without which it cannot be really understood. Since then much has changed, and scholars have for some time been treating this chapter of intellectual history and the history of science like all others, that is, as the history of ideas [...]. But despite these changes, the old controversy still arises about whether and to what extent the *physics* of the fourteenth century anticipated the theories of later classical mechanics [...] (Maier 1982 [1960], 146).

As we can see, Maier accuses Duhem of not paying attention to the context investigated, that is, of not following his own recommendations. Now, it is interesting to note that the discussion he makes, according to her, remained controversial. This corroborates the view that Duhem, instead of worrying about dates and chronologies, posed other types of problems for historians of science. Maier (1982 [1960], 77) discusses, for example, the approach of the theory of impetus as an "anticipation of the system of mechanics based on the law of inertia". Discussed by Duhem, "who first drew attention to the scholastic theory [...] the problem has been discussed repeatedly".

²⁴ I discuss the differences between Duhem and Leonardo's other interpreters in another work. See Oliveira (2016).

²⁵ The title of his work – *Die Vorläufer Galileis* – already establishes the bond with the Duhemian *Études*.

²⁶ David Lindberg (2007, 358-359) considers that Duhem's followers, such as Anneliese Maier, Marshall Clagett, and Lynn Thorndike, drew a more careful history of science. See also Brenner (1990; 1997).

Herbert Butterfield (1966 [1949], 27), who also writes about the importance of the theory of impetus, asserts: "the work of Duhem in the field that we have been considering has been an important factor in the great change which has taken place in the attitude of historians of science to the middle ages".²⁷

Another example of a "follower" of Duhem is Hélène Metzger. In spite of her work being focused on the history of chemistry, a field in which Duhem wrote very little, her writings suggest interesting parallels between them. By considering that her work distances itself from others dedicated to the history of chemistry, Metzger (1969 [1923]), provides a series of distinctive features between her work and other historians', explaining to the reader her motivations. Inclusion of scientists and works previously disregarded, as well as the non-insertion of biographies, are some of them.²⁸ She wrote: "Most of our predecessors, in fact, have reduced their work to establish who have been the craftsmen of the discoveries of which science can boast" (Metzger 1969 [1923], 11). In a note, she considers Duhem as an "exception", pointing out that he "unfortunately only accidentally touched the history of chemistry".

It is worth noting that she refers to the discussion of priorities in history as "irritant"²⁹ and adopts the Duhemian view that the "hypotheses are not the product of a sudden creation, but the result of progressive evolution".³⁰ Her work does not contemplate "the succession of abrupt revolutions" that altered chemical theory, but "the slow evolution" that it underwent by the work of many minds (Metzger 1969 [1923], 9).³¹

A further aspect of Metzger's work, reminiscent of Duhem's attitude, concerns her considerations about the need of the modern historian to look at the changes that have occurred since that period of time and refer to antecedents, material conditions and conceptual change.³² She insisted on the need to pay attention to the differences between the context of her day and that of the investigated one, signaling her effort to describe past theories, "as they should appear to the studious disciples of their masters" (Metzger 1969 [1923], 342). For example, with respect to the history of phlogiston theory, she states that her aim "was to reconstitute the whole of the Stahl's doctrine as it appeared at the time of its elaboration, without worrying about the oversimplifications or modifications which might have altered its aspect with regard to posterity" (Metzger 1930, 5).³³

Metzger, as well as Koyré, Maier and others³⁴ recognized the importance of Duhem's work to the development of their own. In a different way, they had his writings as a source for discussing problems that were gradually changing the historical research of science. They were Duhem's followers in the face of the "older" – or, as said Duhem, of the "classic (*classique*)" or "senseless (*insensée*)" history of science (Duhem 1906, 278). And as much as his followers had disagreed with his analysis and conclusions, the role he played in promoting new and unsuspected researches is undeniable. Duhem's followers were, like Duhem himself, changing the history of science with their revisions.

It is worth noting Bernard Cohen's (1987, 56-57) account of his "painful experience of showing" Koyré "that he had made a factual error in one of his publications." Koyré would have been "chagrined and sad, terribly annoyed with himself". But, after a moment's pause, would have replied: "If Duhem had never made any mistakes, we would have had no great jobs to do. We have lived on his mistakes". Such a statement

²⁷ It is important to remind that Butterfield's *The origins of modern science* was considered "admirable and influential" and a "pioneering synthesis" by Kuhn (1977, 35 and 109).

²⁸ Metzger (1930, 11) states that she avoided "especially any anecdotal or picturesque details" concerning the work of chemists in her historical investigation. She demonstrates disagreement with Sarton about the importance of providing biographies.

²⁹ See Metzger (1935, 9). Dumas (1951, 1) also finds "irritant" the problem stemming from historical discussions about the priority of scientific discoveries. Kuhn indicates his work (1996 [1962], 53) as an "indispensable recent review [about oxygen's discovery], including an account of the priority controversy".

³⁰ Metzger (1969 [1923], 155) cites section II, Chapter VII of *La théorie physique*. Besides it and Duhem's works about Chemistry, she cites his *Le système du monde*.

³¹ Kuhn, in 1952, had already mentioned Metzger's (1930 and 1923) and Meyerson's (1951) analysis as "more acute", which showed the "chemical revolution as proceeding not from a sudden break [...] but through an almost continuous extension and elaboration of the peripatetic and iatrochemical concepts" (Kuhn 1952, 14-15).

³² See Metzger (1969 [1923], 81, 342-343; 1935, 22), and specifically on conceptual change, see Metzger (1969 [1923], 61, 205; 1935, 13 and 19).

³³ Kuhn (1996 [1962], 100, note 3) considered in SSR that "[t]he fullest and most sympathetic account of the phlogiston theory's achievements" is provided by Metzger in his *Newton, Stahl, Boerhaave et la doctrine chimique* (1930).

³⁴ Marshall Clagett and Alistair Crombie are certainly other examples.

highlights the Duhemian legacy for the promotion of historiographic change, a change that took place in different degrees among the makers of the revolution that Kuhn identified in its early stages in the early 1960s.

Final Considerations

It is important to remember that when Kuhn (1996 [1962], viii) wrote about the historians that were important in shaping his “conception of what the history of scientific ideas can be”, he noted that he was “increasingly” questioning “a few of their particular historical interpretations”. It is thus his article on Boyle that led Kuhn (1952) not only to analyze different historical views, but also to try to provide a completely new view about the subject. This is clear when, some decades later, Kuhn (2000, 291) wrote about his first article: “It is, I think, a very good article – it’s totally unreadable because I thought I had to persuade a very learned group of historians of chemistry out there. And what I gradually discovered was that nobody knew nearly as much this problem [the notion of element in Boyle’s work] as I did”.

If it is true that Duhem’s followers disputed many of their historical interpretations, it is also true that Kuhn made reservations to their works. Koyré’s work is a good example. In a text published in 1970,³⁵ Kuhn writes again about the historiographic revolution, now discussing its stages, and for him, in that context, as notes Pinto de Oliveira (2012, 119), Koyré was not “fully a new historian of science” yet.

Thus, if on the one hand, Koyré criticized Duhem for committing exaggerations in his studies on medieval science, on the other hand Kuhn questioned “how Koyré could have failed to discuss the role played by the observation of pendulums in Galileo’s argument, commenting ‘That is no trivial slip, and it illustrates something else about Koyré. He did exaggerate the universality of his insights, and he did make mistakes, very occasionally egregious ones’” (Pinto de Oliveira 2012, 118)³⁶.

For many historians, Duhem’s work is mentioned because of his researches on medieval science and this is not different with Kuhn’s work. In an interview in 1995, when asked which authors would have played a role in shaping his thinking, in addition to mentioning the customary names of Alexandre Koyré, Arthur Lovejoy, Émile Meyerson, Hélène Metzger, Kuhn (1995, 13) states: “In relation to Duhem, I have maintained principally his idea that, to understand the transition from ancient physics to modern physics, one cannot economize the medieval physics”.

We know that the discovery of scientific medieval contributions has significantly altered the historical narrative of scientific development. History, as wrote Harcourt Brown, “is the product of historians; its categories remain fluid as new outlooks and emphases produce new evaluations. [...] As the work of, for instance, Pierre Duhem has progressed and been absorbed, the perspective has changed, and much of sixteenth-century science has lost its glamor” (Brown 1960, 42).

Brown’s analysis, which is focused on the change of vision in relation to the Renaissance, can, by extension, be applied to the scientific revolution. If it is true that the Duhemian view runs counter to the Renaissance conception as a period of sparse productivity, after the darkness of the Middle Ages, for many historians, it also diminished the merit of seventeenth-century scientists and, therefore, diminished the grandeur of scientific revolution. This conception is clear in Koyré’s thought, according to which Duhem denied the occurrence of revolutions in science³⁷.

As far as I can see in his work, Kuhn most likely conceived the Duhemian work in a distinct way from his *maître*.³⁸ Incidentally, the content of one of the bibliographical notes on a text by Koyré, “Le vide et l’espace infini au XIVe siècle”, is noteworthy here:

An attack on Duhem’s absurd statement that modern science begins with edicts of Bishop of Paris in 1277 against impossibility of void, etc. Documented by detailed study of some 14th century writings about the void showing clearly that they don’t take a modern position, etc. Useful, but considerably

³⁵ Review of *Metaphysics and measurement – Essays in the scientific revolution*, published by Koyré in 1968.

³⁶ Kuhn (1977, 35, note 3) presents some caveats to Butterfield’s *The origins of modern science*.

³⁷ See Koyré (1973, 172; 1966, 15-16). I discuss this subject in another work. See Oliveira (2012).

³⁸ Kuhn (1977, 21) referred to Koyré in these words: “the man who, more than any other historian, has been my *maitre*”.

vitiated by straw-man it attacks. Really fails to see whether there is an effect of the edicts [...] (Kuhn *apud* Oliveira 2012, 239).

It could be suggested that Kuhn did not cite Duhem in his book about scientific revolutions because of Duhem's thesis of continuity. Under this assumption, it would make no sense that Kuhn quoted an author who would supposedly deny the occurrence of revolutions in science at a time when revolutions studies were the order of the day. *Prima facie*, this could be a good reason. But, when analyzing the context, I do not think this is a satisfactory reason, because of the opposition between continuity and discontinuity views, and that the notions of revolution and evolution have no place in the SSR, not even being mentioned by Kuhn in the terms proposed by some of his interpreters. Also, the transition from older to new historiography involves many nuances, and the causes by which historians would represent more or less the new or the old historical tradition depend on what would be considered relevant to the discussion.

I have tried to identify some features of new historiography according to Kuhn's writings and, based on them, show that Duhem was a real contributor to historiographic change. But, the question that can be asked is: when did the change to which Kuhn refers in SSR begin? Kuhn's review of Koyré's work allows us to temporarily locate what he called the "historiographical revolution": a movement that began with Koyré himself. Kuhn (1970a, 67) writes: "More than any other single scholar, Koyré was responsible for the first stage of the historiographical revolution". But, as we know, a revolution is not the work of a single man. Kuhn identified in SSR a group of historians. It was a group of younger scholars that was presenting a "philosophical and analytical approach to the history of science" (Crombie 1959, 164).

Mary Hesse's review of SSR corroborates Kuhn's identification with a group: "My own impression is that Kuhn's thesis is amply illustrated by recent historiography of science and will find easier accessibility among historians than among philosophers" (Hesse 1963, 286). In fact, when we observe the works Kuhn cites in SSR, we can see that most of them were published in the 50's. His more cited masters had works published since 1930. We may also recall that in 1957, Kuhn was among more than seventy participating historians of the University of Wisconsin Congress, critically discussing problems in the history of science. Among them, there were some of those cited in SSR, such as Mashall Clagett, Rupert Hall, Giorgio de Santillana, A. C. Crombie, Derek J. S. Price, Henry Guerlac, Charles C. Gillispie and Marie Boas.³⁹ In different degrees, among other historians, they were making the historiographic revolution. And Kuhn saw himself as a participant of it. Retreating to Duhem to seek the sources of modern historiography could leave the structure of the so-called historiographic revolution (as writes Kuhn about the scientific revolution) "unchanged but extending it greatly in time".

So, it is likely that Kuhn did not cite Duhem in SSR simply because Kuhn was announcing an ongoing change at a time when there was a group sharing a new perspective, which echoes "the heroic times of Pierre Duhem", a man of "astonishing energy and knowledge" (Koyré 1973 [1966], 61), but which was promoting a new sort of history in a pioneering attitude. So much so that Kuhn, when analyzing later on the development of the history of science, granted a substantial role in the formation of the new historiography to Duhem. Perhaps, in this moment he was in better conditions to analyze the change in the field, since the writings evoked by Duhem's work, such as those of Dijksterhuis,⁴⁰ Maier, and especially Alexandre Koyré, became models which many contemporaries of Kuhn aimed "to emulate".⁴¹

In any case, what matters is that since Kuhn did not link Duhem to the historiographical revolution in his most famous book, we can try to do so, both through his favorite historians' writings and through his reflections on history and philosophy of science. My attempt as a reader (perhaps less concerned with analyzing the monumental work of Duhem and more with investigating the relationship between his and Kuhn's works) has been to better understand such a stimulating part of the development of the history of science.

³⁹ See Clagett (1969 [1959]).

⁴⁰ Dijksterhuis, a historian not cited in SSR, has his *Mechanization of the world picture* (1961) considered "magistral" by Kuhn (1977, 132). As we can see, Kuhn was updating his references in 1968.

⁴¹ P. Omodeo (2016, 74-75), by mentioning Kuhn's recognition in relation to Duhem's work, writes: "The reference to the alleged success of Duhem's school is prescriptive. Kuhn counted himself as one of the 'contemporary emulators' of the medievalist".

Acknowledgements

I would like to thank José Carlos Pinto de Oliveira and Fábio Leite for their helpful comments on an earlier version of this paper. I would like to thank also Baruana Calado and Christopher Beames for revisions.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 140-156

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem

Poincaré and Duhem: Resonances in their First Epistemological Reflections

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Abstract:

The object of this article is to show a certain proximity of Duhem to Poincaré in his first philosophical reflections. I study the relationships between the scientific practices of the two scholars, the contemporary theoretical context and their reflections. The first part of the article concerns the changes in epistemological consensus at the turn of the century. The second part will be devoted to Poincaré's reflections on the status of physical geometries and physical theories, as they appear in his texts written around 1890. Then I analyze the first reflections of Pierre Duhem on physical theory, in particular his thesis of the hypothetical/symbolic character of physical theories and his criteria for selecting good theories, partly associated with his ideal of physical theory; the whole set of considerations, highlighting the Poincarean inspiration.

140

Keywords:

Pierre Duhem; Henri Poincaré; *fin-de-siècle* physics; conventions

Received: 13 April 2017. Accepted: 10 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.13>

Introduction²

In his text “Quelques réflexions au sujet des théories physiques” (1892) Pierre Duhem wrote:

We are not alone in professing the ideas we have just set forth, and if there is an opinion which we are pleased to be able to invoke in support of ours, it is certainly that of the analyst who has written the following: “The mathematical theories are not intended to reveal to us the true nature of things; this would be an unreasonable claim. Their sole object is to coordinate the physical laws which experience teaches us, but which without the help of mathematics we could not even state”. (Duhem 1892, 165; quoting Poincaré 1889a, I)

These reflections immediately follow Poincaré's first reflections on the status of geometries and on physical theories, published around 1890. The explanation of this agreement expressed by Duhem is one of the purposes of our article, which aims to compare the first epistemological theses of the two *savants*-

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² I thank Olivier Darrigol and Fábio Leite for stimulating discussions.

philosophes. This will illuminate the history of the debate between the two (Brenner 2003, chapter III). This debate is reflected in an opposition that is explicit in *The aim and structure of physical theory* (1906); it manifests itself in the refusal of Maxwellian physics by Duhem and in his lack of interest in the achievements of atomism, associated with Lorentz's theory of electrons, the development of experimental microphysics (gas discharges, X-rays and radioactivity), black-body theory (Planck, Einstein), works on Brownian motion (Smoluchowski, Einstein and Perrin) and the Maxwell-Boltzmann kinetic theory; whereas Poincaré, after 1893, took an interest in atomistic theories and gave important contributions. But this dissonance must not erase the common ground of their reflections, which is related to their judgment on the state of theories around 1890.

The complex evolution of physics in this period is diffracted in the individual epistemological reflections of the *savants-philosophes*, who judge theories in situation. This is because Maxwell, Mach, Hertz, Boltzmann, Poincaré or Duhem try to integrate a variety of motifs: their perception of theories, their actual and personal scientific practice, a broader questioning inspired by the philosophical tradition, which imposes long lasting questions. The first reflections of Poincaré and Duhem, made before the rise of the physics of electrons and ions, have similarities which can be understood according to the contemporary state of physical theories, at a time when the Laplacian tradition, attached to a mechanistic reductionism based in the conception of the center-of-force atoms, is defeated by thermodynamics and electromagnetism, two domains to which Poincaré and Duhem are interested as researchers. The recognition of the hypothetical nature of theories, theoretical pluralism, stylistic differences between the French tradition and the physics of models, the complex nature of inter-theoretical relations and the relationship between theory and experience, constitute a common ground for questioning.³

The first part of our article concerns the changes in epistemological consensus at the turn of the century. The second part will be devoted to Poincaré's reflections on the status of physical geometries and physical theories, as they appear in his texts written around 1890. Then we analyze the first reflections of Pierre Duhem on physical theory, in particular his thesis of the hypothetical/symbolic character of physical theories and his criteria for selecting good theories, partly associated with his ideal of physical theory; the whole set of considerations, highlighting the Poincarean inspiration. The relationships between the scientific practices of the two scholars, the contemporary theoretical context and their reflections will drive this investigation.⁴

Changes in Scientific Consensus

Between the last years of the nineteenth century and the beginning of the 1910s, experimental access to the intimate structure of matter favored a consensus on the relevance of atomistic hypotheses of statistical mechanics. The French - notably Poincaré, Becquerel, Curie, Langevin and Perrin - contributed to this evolution, associated with the physics of electrons and ions, new radiations and Brownian motion.

In 1905, the *Société Française de Physique* invited H. A. Lorentz to speak on "Thermodynamics and kinetic theories". He distinguishes between two kinds of theories in mathematical physics. There are those which seek to "penetrate the intimate mechanism of phenomena" and those which, using certain general principles, establish relations between quantities "directly accessible to observation". Given the state of contemporary research, the fields of application of the two theories, which are taken as examples, are different and each one proves itself powerless where the other makes it possible to reveal relationships (Lorentz 1905, 533-534). This same pluralistic perspective can be found in Jean Perrin's book *Les principes* (1903), dedicated to thermodynamics. In the preface, Perrin states that molecular hypotheses correspond to the "deductive method", which "consists in imagining a priori for matter a structure whose direct perception still escapes our imperfect senses, and such that its knowledge would make it possible to deduce the

³ Poincaré was a member of the jury of the (second) doctoral thesis of Duhem (1888); the only known correspondence between the two is of this period and without epistemological interest (Poincaré 2007, 157-158). Around 1890, Poincaré saw Duhem as the great French specialist in thermodynamics (Poincaré 1892a, XIX, 233, 321-338, 366; 1892c, 63).

⁴ In the context of studies on Duhem, I reconstruct Poincaré's reflections and the French scientific context of the second half of the century in an unusual manner and that sheds another comparative light on their contributions; contrast with (McMullin 1990; Maiocchi 1990; Stoffel 2002).

sensible properties of the universe" (Perrin 1903, VII). Perrin emphasizes the heuristic value of this method, which makes it possible to follow "a perfectly logical march"; but he goes further:

It seems to me that we still have the right to attribute to the molecules, atoms or corpuscles, a greater reality. And I do not fall back into metaphysics. I do not cease to forget that sensation is the only reality. This is the only reality, on the condition that all possible sensations are added to current sensations. (...) Moreover, and precisely at the moment when the interest and the legitimacy of their method were under attack, the atomists have proved it again by striking discoveries, of which the corpuscular theory has succeeded in making a harmonious whole. It seems therefore reasonable in all respects to regard the debate as settled by the reconciliation of two methods [inductive and deductive] which are by no means incompatible. (Perrin 1903, IX-X)

But this consensus, increasingly favorable to atomism and rendering energetism untenable, does not characterize the situation around 1890.

John Heilbron, in his study on *fin-de-siècle* physics, admitted the existence of a minimum epistemological consensus, which he calls descriptionism. This includes the common aspects of the reflections of Mach, Hertz, Poincaré, Duhem and Boltzmann, made in the late nineteenth century and inspired by Maxwell and Kirchhoff (who limited the role of physical theory to a "description" in his lectures in mechanics of 1875). Among the philosophical inspirations Heilbron refers to Kant, with his "objective idealism" which rejects access to the thing-in-itself, and the positivism of Comte. This consensus reflects the tensions between mechanics (and the associated reductionist ideal) and thermodynamics and electromagnetism, and also the success of Maxwellian physics (consecrated with Hertz's experiments on the existence of electromagnetic waves in 1888) and the legitimization of theoretical and methodological pluralism, including the British physics of models, accepting the use of analogies and a degree of inconsistency among models and among theories, justified by fertility. From a sociological and institutional point of view, it reflects a defensive position against those who criticized science from religious and philosophical points of view and undermined its reputation in an industrial society that finances research institutions and promotes a new professional class.⁵

The term "descriptionism" favors a phenomenological perspective; but it hides the plurality of viewpoints on physical theories, including the persistence of atomistic beliefs. For example, in the French case, these are very present in the discussion that follows the publication in 1895, in the *Revue générale des sciences*, of an article by Wilhelm Ostwald, on the defeat of materialism in the sciences. The translators have called it "The defeat of contemporary atomism." It is primarily the mechanistic reductionism (matter and movement) that Ostwald criticizes, based on his vision of thermodynamics. In his reply, Alfred Cornu, Vice-President of the *Académie des Sciences*, renews his credo in favor of a mechanistic reductionist conception based on notions of material points and reciprocal actions. Marcel Brillouin, on his reply, advocates methodological pluralism and individualism, pointing out the success of mechanistic theories in chemistry and of the mechanical wave theory.⁶ As Olivier Darrigol has remarked, Heilbron's thesis is interesting in that it tries to identify a consensus, but this consensus is not achieved around a phenomenological perspective of physical theories. It rather promotes their hypothetical nature, which is clearly present in the title of the book *Science and hypothesis*, which includes texts written by Poincaré until 1900 (Darrigol 2016). The epistemological reflections of Poincaré and Duhem, made during the 1890s, are also a sign of this epistemological consensus, which precedes that which will be formed at the beginning of the 20th century and which will also include atoms.⁷

⁵ Indeed, many physicists, in their practice, abdicated sophisticated epistemological reflections and adopted a realistic spontaneous epistemology (Heilbron 1982, 56).

⁶ On this debate see: (Bensaude-Vincent and Kounelis, 1991; Abrantes 1985, 130; Príncipe 2008, 240-243).

⁷ One of the central figures of this movement of recognition of the hypothetical nature of physical theories is Helmholtz (Schiemann 2009).

Poincaré's Initial Reflections: Conventions, Pluralism and Simplicity

Henri Poincaré, who built his initial reputation in mathematics, is one of the principal responsible for the revival of French mathematical physics, being recognized at the turn of the century as one of the best theoretical physicists. His courses in mathematical physics, initiated in 1885, were very successful, being translated quickly into German. The prefaces of a few of them contain important philosophical reflections to which Duhem will react, on the one hand, by showing his agreement with this mathematician who has become a paladin of the physics of principles; of another recognizing Poincaré as the central figure of the introduction in France of a British style of doing the theoretical physics which he will criticize.

Poincaré: Conventions and Geometry

The first epistemological reflections of Poincaré concern the status of metric geometries. Poincaré knows the work of Bernhard Riemann, for whom the curvature of the space in which we live must be determined by empirical measurements, those of Hermann von Helmholtz for whom the axioms are a posteriori being possible a sensible intuition of the three geometries that allow the free movement of rigid bodies, and those of Marius Sophus Lie, where continuous groups provide a rigorous mathematical elaboration of Helmholtz's researches.⁸ In 1887, in his seminal article "Sur les hypothèses fondamentales de la géométrie", Poincaré questions the origin of Euclid's postulate of parallels and the presence of synthetic a priori judgments in mathematics. On the status of the axioms of geometry, Poincaré considers three options: they are facts of experience, or analytical judgments, or synthetic a priori judgments. He argues that none of the three options is valid and states:

Geometry is nothing else than the study of a group and, in this sense, (...) the truth of Euclid's geometry is not incompatible with that of Lobachevsky's geometry. (...) We chose from among all the possible groups a particular group to report the physical phenomena, as we choose three axes of coordinates to report a geometrical figure (...) the chosen group [the Euclidean] is only more convenient than the others and one cannot say that the Euclidean geometry is true and the geometry of Lobachevsky false. (Poincaré 1887, 215)

The general philosophical significance of this article is elaborated in an article of popularization published in 1891 in which it is shown that the ontological claims of some empiricists and the Kantian framework of the transcendental aesthetics are not acceptable. Poincaré points out that a dictionary can be constructed between terms of the geometry of Lobachevsky and terms of ordinary geometry, which makes it possible to translate the theorems of the first into theorems of the second. He summarizes his discussion by saying that geometric axioms are disguised conventions or definitions - they result from a free decision of the mind, motivated by experience (Poincaré 1891, 773). The word "convention" is used with two meanings: due to the consistency and inter-translatability of (pure) metric geometries, one can not assign preferential validity to one of them (except for simplicity reasons); the second meaning refers to the choice of a physical geometry which involves a package of coordination rules with empirical definitions. Physical geometry involves mechanics, thermodynamics (measurement standards) and optics (postulation of the rectilinear propagation of light rays). The geometry belongs to a more elementary level than the other domains (non-homogeneous or stratified holism) (Friedman 1999, 74, 80-1): the test using the parallax of the stars admits that the light rays are straight lines; but a result apparently contrary to Euclidean geometry would best be interpreted by modifying the laws of optics: "Needless to add that everyone would regard this solution as more advantageous." The choice of conventions is therefore not arbitrary, because it is based on a constraining intersubjectivity. Poincaré therefore believes that "a geometry can not be more true than another; it can only be more *convenient* [commode] (...) Euclidean geometry is and will remain the most convenient" (Poincaré 1891, 774).

⁸ On his sources of inspiration see: (Giedymin 1977; Poincaré 1891, 769; Heinzmann 2001; Darrigol 2007).

The Mathematical Physics Courses: Pluralism and the Method of Comparisons

One of Poincaré's first courses (1885-1886) is dedicated to pure kinematics and mechanisms, a traditional subject which could have helped to understand the illustrative mechanical models typical of British approaches. The course at the Sorbonne in the first semester of 1887-1888 is devoted to the mathematical theories of light, an area in which there was a clear de facto pluralism. The alternative between molecular ether and continuous ether was known since the 1830s, considering the compatibility of George Green's theory with continuous ether. Poincaré compares six competing theories and shows the equivalence of these theories despite the different physical hypotheses of departure.⁹

After the assertion, which Duhem will cite (see introduction above), that mathematical theories do not reveal to us the "true nature of things", Poincaré notes that the ether is only a convenient hypothesis, neither true nor false. The several theories of mechanical ether are all "equally plausible". To confine oneself to one of them would produce a blind confidence; the most instructive is to compare them. He notes that the molecular hypotheses, typical of French theories, "play only a secondary role. (...) I borrow from molecular hypotheses only two things: the principle of conservation of energy and the linear form of equations which is the general law of small movements (...) this explains why most of the conclusions of Fresnel remain unchanged when we adopt the electromagnetic theory of light [that of Maxwell]" (Poincaré 1889a, III). In the "Conclusions", Poincaré reinforces his instrumentalist point of view:

Besides, we cannot complain of being unable to make a choice [among the rival theories of the ether]. This impossibility shows us that mathematical theories of physical phenomena are to be regarded only as instruments of research (Poincaré 1889a, 398-399).¹⁰

Poincaré also teaches the theories of capillarity (the molecular theories of Laplace and Gauss), and the theories of elasticity, about which he distinguishes between molecular theories and phenomenological theories:

There are a great number of theories of elasticity. They can be reduced to two classes: in the first class we will class theories based on molecular hypotheses; in the second, those whose authors have sought to free themselves from all hypotheses on the intimate constitution of bodies; these latter theories are generally based on thermodynamics. (Poincaré 1892c, 27)

Poincaré points out that the two methods lead to the same equations, but that the molecular hypotheses are speculative (Poincaré 1892c, 62, 64). In the preface to his *Thermodynamics* he seems to be more explicit about the demise of molecular explanations:

Abandoning the ambitious theories of forty years ago, encumbered by molecular hypotheses, today we are seeking to build upon thermodynamics alone the whole edifice of mathematical physics. (Poincaré 1892a, V)

The course of the second semester of 1888-1889 is dedicated to theories of electrodynamics and to Maxwell's electromagnetic theory of light; after that, Poincaré will deal with the theory of Helmholtz and the experiments of Hertz. He will continue with his method of comparisons, showing, for example, that Maxwell's theory is a special case of that of Helmholtz (Darrigol 1993, 215, 222; 1995, 5-8); he notes that comparisons (mathematical ones) must not make us forget the distinct physical senses attached to the theories:

Hertz considers that the very substance of Maxwell's ideas lies in the equations he obtains, and that a theory may be regarded as equivalent to that of Maxwell, provided that it leads to the same

⁹ On the history of ether theories see Schaffner (1972).

¹⁰ The underdetermination of the theories of the ether is here the result of the linear character of the equations (Poincaré 1889a, 398-400); this will favor Poincaré's skepticism towards ether, and his preference for the views of Hertz, who abolished the ether (Darrigol 2000, 356).

equations. Thus Helmholtz's theory contains, as a special case, that of Maxwell, and yet Maxwell would not have accepted this interpretation, in which actions at distance still play a part. (Poincaré 1892b, VI)

In spite of his admiration for Helmholtz, Poincaré will recognize with Hertz that Helmholtz's theory is contrary to the unity of the electric force; this unitary conception based on considerations of symmetry (and not of experimental origin) justifies Poincaré's preference for Maxwell (Abrantes 1985, 208-212; Darrigol 2000, 355). From the 1890s, Helmholtz's theory will no longer be considered as an alternative to Maxwell's, despite Duhem's efforts (Atten 1992, 10-11 and 444-448).

Maxwell's *Treatise*: Its Fundamental Idea, National Styles

In the preface to his lectures on the theories of Maxwell (1889), Poincaré wants to show that the *Treatise* contains a fundamental idea, despite the fact that "the English scholar does not seek to construct a single, definitive and well-ordered edifice"; in fact "it seems rather that it raises a large number of temporary and independent constructions, between which communications are difficult and sometimes impossible" (Poincaré 1890, VIII). He notes the advantages of this method, which he attributes to Maxwell:

We must not, therefore, flatter ourselves with avoiding all contradiction; (...) indeed, two contradictory theories may, provided they are not mixed up, and that they do not seek the substance of things, may be both useful instruments of research, and perhaps reading Maxwell's *Treatise* would be less suggestive if it had not opened to us so many new divergent paths. (Poincaré 1890, IX)¹¹

According to Poincaré, this appearance of fragmentation conceals the fundamental idea of the *Treatise*. It corresponds to a profound change in the concept of mechanistic reduction of phenomena:

Maxwell does not give a mechanical explanation of electricity and magnetism; he merely demonstrates that this explanation is possible. (...) If a phenomenon includes a complete mechanical explanation, it will include an infinity of others which will also give a good account of all the peculiarities revealed by experience. (Poincaré 1890, VII, XIV)

This theorem, stated in Maxwell's *Treatise* (Maxwell 1873, II, §831), is demonstrated in detail by Poincaré, using the Lagrangian formalism. The generalized coordinates correspond to observable/measurable parameters. These can be related to an unobservable molecular coordinate system. There are an infinity of such systems which, by transformation of variables, allow us to obtain the same Hamilton function, $T + U$. This theorem justifies the attitude of those who find the "complete explanations" (typical of the ideal of the Laplacian program) unnecessary, especially the speculations on the ultimate structure of the ether. But the opposite attitude remains valid:

Among all these possible explanations, how can we make a choice for which the help of experience is lacking? Perhaps a day will come when physicists will lose interest in these questions, inaccessible to positive methods, and abandon them to the metaphysicians. This day has not come; man does not resign himself so easily to ignore eternally the substance of things. (Poincaré 1890, XV)

Then Poincaré remarks how ontological preoccupations about the nature of the substance or the inner mechanism are alien to the spirit of Maxwell's fundamental idea:

The same spirit is found throughout the work. What is essential, that is, what must remain *common to all theories*, is brought to light; anything that would fit a particular theory is almost always ignored. The reader thus finds himself in the presence of a form almost empty of matter which he is at first tempted to take for a fleeting and elusive shadow. But the efforts to which he is thus condemned

¹¹ On the interpretation of Maxwell by Poincaré see Darrigol (1993, 216-7, 220-223).

compel him to think, and he ends by understanding what was often a little artificial in the theoretical ensembles he once admired. (Poincaré 1890, XVI)

The epistemological value of the theorem is emphasized by Poincaré, who sees in it an explanation of the indecision between rival theories present in historical cases:

The preceding is confirmed by the history of all the parts of physics; in optics, for example, Fresnel believes the vibration perpendicular to the plane of polarization; Neumann regards it as parallel to this plan. We have long sought an "experimentum crucis", which enabled us to decide between these two theories, and we could not find it. All these facts are easily explained by the properties of the Lagrange equations which I have just recalled. (Poincaré 1890, XIV)¹²

The explanations that postulate unobservable quantities are therefore a cause of underdetermination of theories. Also, the theorem highlights the abstract dynamics, favored by some of the British physicists.

Among the old theoretical ensembles we find the molecular physics of Laplace. Poincaré establishes a close relationship between the tradition of French mathematical physics and the difficulties in understanding Maxwell's works:

The first time that a French reader opens Maxwell's book, a feeling of uneasiness, and often even mistrust, mixes at first with his admiration. (...) Why do the ideas of the English scholar have so much difficulty in acclimatizing among us? It is no doubt that the education received by most enlightened Frenchmen disposes them to taste precision and logic before any other quality. The ancient theories of mathematical physics gave us a complete satisfaction in this respect. All our masters, from Laplace to Cauchy, proceeded in the same manner. Starting from clearly stated hypotheses, they deduced all the consequences with mathematical rigor, and then compared them with experience. (Poincaré 1890, V-VI)

Poincaré emphasizes here the style of presentation, the logic and precision favored by the training of French physicists. He also recognizes the persistence of the Laplacian program, which contains an ontology of center-of-force atoms, the conception of an unobservable matter, "which have only purely geometric qualities and whose atoms are nothing but mathematical points subject to the laws of dynamics" (Poincaré, 1890, VI).

Simplicity and Coordination of the *Rapports Vrais*

Poincaré seems to favor a physics in which the "differential equations deduced from experience can be put into the Lagrangian form" (Poincaré 1890, XII); and he questions the merit of the complete mechanical explanations, since as soon as one is sure of having the Lagrangian version, Maxwell's theorem guarantees the existence of a myriad of such explanations, which takes away their value. But it is more complicated than that. Poincaré indeed values the unifying role of atomist hypotheses – for example, in 1892 he points out that Helmholtz's (1858) theorems on a perfect liquid, implying that the vortex rings "must retain their individuality," inspired William Thomson (1867) to conceive an atomic theory of matter based on a universal perfect liquid, which would allow "a mechanical explanation of the universe" (Poincaré 1893c, 2). In addition, while Poincaré valorizes the Lagrangian approach, he questions his universality by showing that the second principle remains rebel even to this more phenomenological mechanistic approach.¹³ Indeed, from 1893, Poincaré will favor the statistical approach of Maxwell and Boltzmann (Príncipe 2008, 293-334). His conception of mathematical physics is rather flexible, favoring the critical and comparative appreciation which presupposes methodological pluralism and a kind of suspension of judgment.

¹² Poincaré alludes to the experiences of Wiener (Poincaré 1891b; Langevin in Collectif 1914, 73).

¹³ Admitting that Thermodynamics can be presented according to the Hamiltonian formalism, Poincaré believes to have demonstrated that no function of the state of a system governed by Hamilton's equations can be constantly increasing – the Clausius principle is incompatible with that of the Least Action (Poincaré 1899b; Príncipe 2008, 279-293; Príncipe 2014, 135-137).

However, Poincaré's pluralism, like that of Maxwell, is not a variety of relativism that would fragment the body of physical theory because, beyond the differences of individual and national style, Poincaré postulates an intersubjectivity based on constitutive principles (mathematics being a constitutive language from this point of view) and on principles of convenience (in the sense of Kant) that allows us to determine which theories are the simplest and most harmonious. For example he remarks:

Our choice can therefore be guided only by considerations in which the share of personal appreciation is very high; there are however solutions that everyone will reject because of their quirkiness and others that everyone will prefer because of their simplicity. (Poincaré 1890, XII)

In the preface to his *Thermodynamics*, Poincaré emphasizes the role that considerations of simplicity have in the construction of theories. Poincaré affirms that our mind is endowed with a faculty which is the condition of possibility of science: the faculty of generalizing the empirical data. It allows us to satisfy our need for order and harmony and at the same time allows us to foresee. The laws are formulated "after relatively few experiments and which present certain divergences". Since "every proposition can be generalized in an infinite number of ways", the choice of the general law is made according to our criterion of simplicity, by obeying "a necessity to which the human spirit cannot escape" (Poincaré 1892a, VI, VII). It is simplicity that favors the acceptance of the principles of thermodynamics:

The imposing simplicity of the principle of Mayer [energy conservation] also contributes to affirm our faith in it. In a law deduced immediately from experience, such as that of Mariotte, this simplicity would appear to us rather a reason of mistrust, but here it is no longer the same, for we see elements, disparate at first glance, to be arranged in an unexpected order and to form a harmonious whole. (Poincaré 1892a, VIII)

Poincaré also remarks the advantage of teaching the historical course of "long groping by which man arrives at the truth (...) We shall note the important role played by various theoretical or even metaphysical ideas" (Poincaré 1892a, V).

Poincaré believes that the synchronic and diachronic comparison of theories allows the sedimentation of laws, which he will later call true relations [*rapports vrais*] (Poincaré 1900, 1168; 1902a, 292-3). Theories coordinate physical laws; for example, "the laws of optics and the equations which translate them analytically (...) will remain true, at least as a first approximation" (Poincaré 1889a, I-II), (Poincaré 1892b, VI). In his *Thermodynamics* he says: "The accuracy of physical laws is always limited by observation errors. But at least they pretend to be first approximations and we hope to replace them gradually by more and more precise laws" (Poincaré 1892a, XIII). Poincaré understands the complex nature of theories, which derives in large part from the multiplicity of inter-theoretical interrelationships, but he believes in the human capacity and need to reduce complexity in an unforeseen historical march towards the systematic unity of theories, which makes it possible to us to identify structures of true relations, subjects that he will deepen in his later texts, especially in those that will form *Science and hypothesis*.

Duhem: Physical Theories Faced with the Bankruptcy of Mechanics

In 1892, Duhem published his first major epistemological reflection, his opening lecture of the *Course of mathematical physics and crystallography at the faculty of sciences of Lille*, and also the first part of his *Commentary on the principles of thermodynamics*, with which he continued his research program whose aim was the creation of a "general theory of material transformations, which encompassed physical sciences" (Bordoni 2012, 11). The agreement with Poincaré is explained in the *Commentary*:

Every physical theory rests on a certain number of definitions and assumptions, which are, to some extent, arbitrary; it is therefore permissible to attempt to expound such a theory in a logical order; but to claim that he was given the only logical order of which it is susceptible would be an unjustifiable claim. (...) We are convinced that the principles of thermodynamics can be chained in a way other than that which we have adopted and yet also satisfactory, perhaps more satisfactory. (...) If the

question we have examined seems rather philosophical, let us be permitted to invoke (...) the interest shown (...) by an illustrious analyst [Duhem means (Poincaré 1892a)], for researches which concern the principles of thermodynamics. (Duhem 1892c, 270)

In this work, Duhem explicates and clarifies the basic hypotheses of thermodynamics, starting from a strictly phenomenological conception, notably by presenting an "axiomatic treatment of the first law of thermodynamics which is surprisingly good by present day standards" (Miller 1970, 229). Duhem calls "conventions" the introductory axioms, which will make it possible to obtain a mathematical expression symbolizing the transformation of a system in the presence of foreign bodies. He notes that the nature of this contribution of the action of foreign bodies to the energy of the system remains obscure. To penetrate its nature "is not the object of physics but of metaphysics" (Duhem 1892c, 290). The word "convention" appears here in the framework of a theory with principles, being associated with "axioms" and with the criticism of the hypotheses of a theory, themes present in Poincaré's reflections.¹⁴

Definitions and Hypotheses

Let us follow the considerations of Duhem's first epistemological text (Duhem 1892a). Duhem considers the classical Amperean idea that raw facts are organized by experimentation, which is the beginning of an ascending classificatory march that leads from facts to laws and from laws to theories (Braverman 2016, 71-72). A theory is constituted by a series of operations, the first being the definition of the quantities which symbolize the corresponding physical notions: for example, temperature symbolizes the notion of heat. The choice of the physical quantity is "to a high degree arbitrary", because "between these two ideas, being warm and temperature, there is no kind of natural relationship (...) Physical definitions constitute a true vocabulary (...) Definitions are a set of conventions matching a magnitude to each physical notion" (Duhem 1892a, 143-144). In the *Commentary*, Duhem shows how the construction of a physical notion, although starting from sensory experience, mobilizes the abstraction that corrects the logical imperfections that stem from the limited nature of the sensations associated with our organs:

This property of the bodies which we characterize by the words: to be hot, to be cold, to be more or less warm, our faculty of abstraction is soon going to attribute to it characters that sensation does not give us. (Duhem 1892c, 284)

The concept of thermal equilibrium, essential for the construction of the concept of temperature, presupposes the concept of an isolated system, which is an abstraction (Duhem 1892c, 274, 285), and results from generalization from vulgar observations. Once the "law of thermal equilibrium" is established (for an isolated system to be in equilibrium all its material parts must be equally warm) it "leads us to correct the data of our sensations (...) our sensations do not always inform us of the degree of heat of a body" (Duhem 1892c, 285-286). This is why Duhem emphasizes that experimental physics rises above empiricism (Duhem 1892a, 140).

Duhem considers that the correspondence which must be constructed between the notion of "being warm" and "temperature" can only be a partial analogy. It takes up the distinction between quantity and quality (Aristotelian distinction which is related to the Kantian distinction between extensive and intensive quantities); this distinction is present in the considerations of Maxwell, Mach and Helmholtz concerning the discussion of the concept of measurability, and in particular of that of temperature. Helmholtz (1887) believes that "intensive quantities, for which no concrete addition is known, could only be measured through a connection with extensive quantities" (Darrigol 2003, 519). Duhem judges that the establishment of this necessary connection introduces an arbitrary element. The "being warm" property is not a quantity because

¹⁴ In his justification of the phenomenological approach, he invokes a criterion of simplicity, in a passage of Poincaréan flavor: "In Physics, it is both impossible and useless for us to know the real constitution of matter. We are simply trying to conceive an abstract system that provides us with an image of the properties of bodies. To construct this system, we are free to represent a body which seems to us continuous either by a continuous distribution of matter in a certain space or by a discontinuous set of very small atoms. The first mode of representation, leading in all parts of physics to simpler, clearer, and more elegant theories, will be preferred to the second." (Duhem 1892c, 272)

it is not susceptible to addition; the correspondence between this property and an algebraic quantity is arbitrary because numbers have properties that do not correctly represent the properties of the corresponding physical notions: "We do not understand what it means (...) body A is seventeen times warmer than body B" (Duhem 1892a, 142). The characters that are required by the correspondence (to respect the zero law of thermodynamics and the transitivity of the relation "body A is warmer than body B") leave the temperature defined modulo a continuous and strictly increasing function. The material concretization of the correspondence depends on postulates defining what is a thermometer, admitting that a quantitative property of the thermometer depends only on temperature. Maxwell, Mach, Poincaré and Duhem thought that thermometers should be considered "as purely conventional means to identify and order thermal states" and, in addition, Mach and Duhem (...) believed that the confusion between quality and quantity belonged to the mechanical reductionism they both condemned" (Darrigol 2003, 519).¹⁵

After the first operation (definition of the quantities that symbolize the physical notions), a second operation is then implemented: hypotheses relate the physical quantities and by mathematical deduction, we obtain consequences that are tested experimentally (Duhem 1892a, 145).

Duhem does not believe possible the existence of theories without hypotheses and criticizes the "hypotheses non-fingo" of Newton and Ampère, the idea that theories can be deduced from experience alone:

What then has Newton done to formulate the law of universal gravitation? (...) He took as a hypothesis a proposition of which the experimental laws [the laws of Kepler] placed at the beginning of his theory are only particular consequences, exact or simply approximated. This is the general method employed by all theorists. To formulate their hypotheses, they make a choice of some of the experimental laws, the whole of which must be embraced by their theory; then by means of correction, generalization, and analogy, they compose a proposition of which the laws are exact or simply approximated consequences, and it is this proposition which they assume. (Duhem 1892a, 148)

The paths of theoretical invention are multiple and as soon as an hypothesis makes it possible to deduce a wide range of consequences, it is not necessary that it directly symbolizes the experience, although it is its relation to experience which gives it physical meaning. As in the case of the definition of quantities, there is arbitrariness in the choice of hypotheses. Duhem will be particularly concerned with the question of the criteria for choosing hypotheses, combating arbitrariness.

A first case of arbitrariness is the one resulting from the conventional choice of the definitions that allow the measurement:

In order to represent the same notion, one can in general make use of a multitude of extremely different magnitudes (...) the simple change of the definitions would already lead to changing the hypotheses [which would correspond to translate] the same hypothesis by means of different symbols, and these two statements of the same hypothesis in two different systems of symbols do not constitute any more two different hypotheses than the statements of the same proposition in French, Latin and Greek constitute three different propositions. (Duhem 1892a, 152)

This adjustment between definitions and hypotheses reminds one of Poincaré's reflections: Poincaré speaks of the intertranslability of metric geometries and the fact that the choice of a physical geometry (which makes it possible to measure lengths and angles) implies a set of rules of coordination with empirical definitions; Duhem notes that the conventional choice of definitions (which allow measurement) implies adjustments in the hypotheses, but that these adjustments are related to each other as translations in different languages of the same idea. Also their discussions on the concept of temperature (their common sources being

¹⁵ On the conventional character of the equality of temperatures in (Poincaré 1892a) see Darrigol (2003, 563). Darrigol shows that Helmholtz inspired the conceptions of Poincaré and Duhem on measurement; Duhem is individualized by his ideal of a physics of qualities, a neo-Aristotelian conception according to which qualities remain irreducible to quantity – "a property identified as a quality had to remain a quality for ever" (Darrigol 2003, 568), thematic preference (in the sense of Holton) to which Duhem remains faithful throughout his career. On the history of the concept of temperature see also (Chang 2004).

Maxwell, Mach and Helmholtz) exhibit the need for conventions in order to measure.¹⁶

The Good Theories

In a good theory, the consequences of the axioms form a complete and varied set, which demonstrates the ability of the theory to coordinate/symbolize experimental laws (Duhem 1892a, 145-146).

Duhem acknowledges the existence of different sources of indeterminacy of theories (Duhem 1892a, 149-151). Firstly, the hypotheses of a theory go beyond the simple symbolic translation of experimental laws, introducing modifications that are the work of the scientist's mind. Secondly, the presence of experimental error allows for competing theories, that yield different laws and yet agreed with experience, in the interval of experimental uncertainty. Thirdly, the extension of a theory may not be well known: a theory is designed to be applied in a certain domain, and its extension to a larger domain may not be appropriate. Fourthly, a theory is useful or good depending on the precision required in its applications – the simple gas law of Gay-Lussac, the generality of which has been invalidated by the experiments of Victor Regnault, may remain good for a chemist or an engineer.

Among these considerations, Duhem notes the role of experience in accepting or rejecting a theory. If there is a discrepancy between the expected consequence of the theory and "the methods of observation of which the theory accepts the control, the theory must be condemned" (Duhem 1892a, 151).¹⁷

The sources of indeterminacy allow us to understand the theoretical changes studied by the history of science. The value of a theory being relative (or conditional), a theory might be good and yet be replaced at the same time by a better one, either because the last one is capable of representing a wider class of laws, or because it is capable of represent the same laws with a greater degree of approximation. This substitution can be obtained either by a more continuous process, which maintains the hypotheses of the first theory by adding new parameters or some new hypotheses; or by a process which requires deeper modifications "which alter the definitions and assumptions upon which the first theory was based" (Duhem 1892a, 152).

According to the preceding considerations, the value of a theory depends on the examination of the extent of the domain of a theory, of experimental uncertainty, and of the concrete use of a theory (its more immediate instrumental character). Duhem proposes other more internal/logical criteria to make the choice between competing theories:

Logic leaves the choice of hypotheses free; but it requires that all these hypotheses be compatible among themselves, that they are all independent of one another; a theory has no right to invoke unnecessary assumptions; it must reduce its number to a minimum; it has no right to bring together consequences deduced from irreconcilable assumptions. (Duhem 1892a, 166; see also 169)

Until now, Duhem, in his overview of the criteria for evaluating the value of a theory, remains in the consensual plan of a certain good sense, as long as we accepts the central role of hypotheses and of the convenient definitions needed for measurement. This plan is abandoned when Duhem judges contemporary theories. Firstly, after citing the passage of Poincaré on the French style of presentation of the theories (see above), Duhem sees a weakness of the mind in the style of the *Treatise* of Maxwell (Duhem 1892a, 168), which implies a mixture of irreconcilable hypotheses -- a subject developed in a subsequent text on the English School (Duhem 1893); in the latter text, among his long considerations, Duhem does not tell us his opinion on what Poincaré considered the fundamental idea of Maxwell's *Treatise*, most probably because it is in harmony with the research program of Duhem: the Lagrangian approach valorizes a phenomenalist

¹⁶ There is a passage in which Duhem alludes to Helmholtz's considerations on non-Euclidean geometries (Poincaré refers them to the end of his article of 1891) and on arithmetic and the problem of measurement: "these profound researches on foundations of geometry and these meditations, so satisfying to the mind, concerning the origin of the axioms of arithmetic" [reference to *Zahlen und messen*, published in 1887] (Duhem 1893, 375).

¹⁷ I am the one who emphasizes. Experimental control is not therefore a simple and immediate action. In the *Commentary*, considering "absolute movement", Duhem points out that supplementary hypotheses are always associated with the assumption that a trihedron has its axes absolutely fixed (Duhem 1983c, 271), which prefigures the broader discussion of (Duhem 1894) and makes one think of Lakatos' protection belt; see Leite (2017, 145).

view in which mechanics has a more abstract structuring role. For Duhem there exists an ideal form of theory: the hypotheses of a theory must be the symbolic translation of experimental laws, in which case the theory is modified by a continuous process. When hypotheses move away from experimental laws, theories become more vulnerable to demolition (Duhem 1892a, 153). Implicitly, he sees an opposition between phenomenological theories and theories that postulate unobservables, which is typical of atomic mechanical theories. Duhem believes that this last class of theories has fulfilled its historical function and is in a state of rupture.

The Bankruptcy of Mechanical/Atomic Theories

Around 1870, in chemistry, atomic hypotheses were rejected by the adepts of the theory of equivalents. The atomist Adolphe Wurtz was opposed to Marcellin Berthelot and to Henri Sainte-Claire Deville. In this connection, two debates took place at the *Académie des sciences* (1876, 1877) between physicists and chemists; the first one had as a pretext the result of Kundt and Warburg's experiments on the specific heat of mercury vapor (the ratio between the specific heats obtained, $\gamma = 5/3 = 1.66$, corresponding to a monoatomic gas according to the predictions of the kinetic theory), the second concerned the law-hypothesis of Avogadro and the law of Dulong and Petit for specific heats (base of the atomic hypothesis in chemistry). The debate made it clear that the elite of French physicists was predominantly in favor of atomic hypotheses (Príncipe 2008, 190-200).

In 1892, in his article on atomistic hypotheses, Duhem took the side of the equivalentists, judging that atomistic hypotheses produced only "difficulties which had arisen from the presumptuous desire to take a classification for an explanation." Duhem cites in his favor the ideas of Sainte-Claire Deville (1818-1881) for whom chemistry must follow a method not "in the manner of geometrical concepts, but in the manner of naturalists", that is, a method of classification (Duhem 1892b, 452). In his *Lessons on affinity* of 1867, the mentor of Jules Moutier speaks of atomistic hypotheses as "contemporary tendencies to abstraction" to which we ought not to give reality:

Let us gradually do a work of classification that will be for a long time, that will perhaps always be, incomplete (...) But we must never rely on hypotheses that last only a moment (...) All the hypotheses accepted today will necessarily disappear from science. I make no exception, even for (...) the hypothesis of the luminous ether. (Sainte-Claire Deville cited in Duhem 1892b, 453)

This agreement means that Duhem has a unitary vision of chemistry and physics, inspired by a phenomenological methodology and translated by his notion of *natural classification*.¹⁸

Duhem recognizes that the ideal of mechanistic reductionism is a majority trend in France (Duhem 1892a, 153, 154; Príncipe 2015c). Here is a definition of mechanical theories:

To each physical notion, the theory had to substitute, as a symbol, a certain magnitude. This magnitude needs to present certain properties, the immediate translation of the characters of the notion which it symbolizes; but, apart from these characters, which in general are few in number, its definition remains absolutely arbitrary. In a mechanical theory, one imposes in addition to all physical magnitudes (...) the condition of being composed by means of geometrical and mechanical elements of a certain fictitious system; to all hypotheses, to be the statement of the dynamic properties of this system. (Duhem 1892a, 154)

¹⁸ Jules Moutier, Duhem's professor at Collège Stanislas (Paris), was a disciple of Deville. Duhem found in Deville the French pioneer of physico-chemistry: to build a chemical mechanics, based on thermodynamics, "it sufficed that Berthollet's main idea was revived and that it was irrefutably established that the laws governing physical changes and the laws governing chemical reactions are of the same nature. That was achieved by the work of Henri Sainte-Claire Deville" (Duhem 2002 [1899], 264). This text continues attributing to the "High priest of official science" – allusion to Berthelot – the responsibility for the difficulties in the development of physical chemistry in France; see also (Klein 1990, 53-54).

Duhem considers that a serious disadvantage of these theories is "the obligation to include in these definitions and hypotheses only a very limited number of notions of a determinate nature", and there is no guarantee that "all experimental laws can be symbolized by a combination, even very complicated, of mechanical concepts alone" (Duhem 1892a, 156, 157).

This question is natural in the theoretical context of that time. Duhem illustrates his judgment on the impasse to which the ideal of mechanical theory leads by three contemporary examples: the theories of the ether, those of heat and those of electricity. The first example is that invoked by Sainte-Claire Deville and which has aroused the reflections of Poincaré in the preface to his lectures on the theories of light. The luminous ether has been constructed using the theories of elasticity, the ether being conceived by some as a continuous medium and by others as being formed of isolated atoms (Duhem 1892a, 155). Considering the mechanical theories of heat, Duhem alludes to the mechanical analogies formulated by Clausius (1871) and studied by Moutier around 1875 (Príncipe 2008, chapter 6), and those of Helmholtz (1884 and 1886) criticized in (Poincaré 1889). Duhem concludes, paraphrasing Poincaré, that they cannot "give a satisfactory account of the principle of Carnot" (Duhem 1892a, 157). According to Duhem, Maxwell, in treating the subject of electricity, formulates several contradictory theories, imagining mechanical media with very complicated properties, and that are incompatible with the well-established theories of hydrostatics and elasticity (Duhem 1892a, 156, 168).

Duhem expresses his conviction that "mechanical theories disappear from science one after the other" and this is due to the fact that "among the hypotheses upon which a mechanical theory rests, there are a great many which have no source in experience and which arise only from the demanding conventions arbitrarily laid down by the physicist" (Duhem 1892a, 157). He therefore suggests that, contrary to the case of those conventions that must be introduced with the definition of physical magnitudes, one must eliminate the conventions attached to hypotheses and which are only a consequence of the work of the mind of the physicist.

Conclusions

The object of the article is to show a certain proximity of Duhem to Poincaré in his first philosophical reflections.

The agreement with Poincaré concerns global aspects of physical theories. The theory represents an economy for the mind: "The theoretical science aims at relieving the memory and helping it to retain more easily the multitude of experimental laws" (Duhem 1892a, 140); it is intended to provide a systematic classification or representation of experimental laws, not pretending to provide "a metaphysical explanation of the material world" that would "contemplate the very structure of the world" (Duhem 1892a, 150, 158-159). However, like Poincaré, Duhem recognizes the importance of the interactions between metaphysics and physics (and their criticism), a recurring subject of his historical work (Leite 2013).

Theories are constructed from hypotheses, taken as the starting point of the mathematical deduction, and they do not have to be a mere translation of experimental laws. Like Poincaré, Duhem distinguishes between phenomenological theories and (mechanical) theories that postulate unobservable entities. Both value mathematical physics and its tool: mathematics. Mathematical analysis, Duhem writes, is "a necessary instrument for the construction of any physical theory (...) and the physicist must be able to use, if necessary, all the parts of this instrument". At the same time mathematical analysis deserves to be cultivated by itself because of its beauty, and because sometimes its internal improvements end up rendering service in other fields of physics (Duhem 1892a, 171-173).¹⁹

The points of convergence between Duhem and Poincaré are quite numerous, since more than half of the "Quelques réflexions (...)" illustrate this agreement (Duhem 1892a, 139-165). Duhem differs from Poincaré only in his appreciation of the various methodologies or styles of making theory (Duhem 1893; see Maiocchi 1992, 377-380); and he makes judgments that reflect his ideal of a more phenomenological theory

¹⁹ This is another resonance; Poincaré will say: "Mathematics have a triple goal. They must provide an instrument for the study of Nature. But that is not all, they have a philosophical aim, and, I dare say, an aesthetic aim" (Poincaré 1897, 857).

and his constant concern for global coherence and uniqueness of representation.²⁰

Although the comparison given in this article is limited to the early 1890s, let us point out some factors of the progressive separation of Duhem and Poincaré.²¹ In the following years, Poincaré frequented the neo-Kantian and republican milieu, becoming one of the collaborators of the *Revue de métaphysique et de morale* (a journal in which Duhem published only once, in 1916), see (Soulié 2009, 68, 222-225), (Príncipe 2015a et 2015b). It seems to me probable that Duhem's divergence from Poincaré's ideas, explicit in his later texts, is partly an effect of his rejection of neo-Kantianism, although Poincaré's interest in the progress of atomism may have played a more important role. Duhem frequented neo-Thomist circles hostile to Kantianism, see (Rossi 2006, 123, note 35). Poincaré's flexible epistemological views are in harmony with his anti-dogmatism, his opposition to clerical intolerance, his republican spirit in favor of the egalitarian ideal, free thought and the right to seek and to speak the truth.²²

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²⁰ Duhem expressed his initial admiration (see my introduction) for the philosophical reflections of Poincaré elsewhere; for example, in his intervention during the third International Catholic Scientific Congress in Brussels (September, 1894), where he was attacked because of his reflections on the relations between physics and metaphysics; Duhem considers that "metaphysicians should have a knowledge of physical theories, acquired by ten to fifteen years of first-hand experience, rather than by reading prefaces to physics textbooks, before seeking to define the relation of physics to metaphysics: "If you want to make the philosophy of sciences, be a Helmholtz or a Poincaré!" ["Si vous voulez faire la philosophie des sciences, soyez un Helmholtz ou un Poincaré!"] (Hielbert 2000, 318). Simultaneously, the two criticize each other's contributions to thermodynamics, see note 1 above and (Stoffel 2002, 344, note 54).

²¹ The word "convention" does not appear in *The aim and structure of physical theory*; It is also the case of the passage (Duhem 1892a, 152), cf. above end of 3.1, which brings him closer to Poincaré. Duhem wanted to eliminate all that could bring him closer to the geometrical conventionalism of Poincaré interpreted as a pragmatist vision.

²² See: (Mawhin, 2004, 11-13), (Toulouse 1910, 143-144). In the Dreyfus affair they took opposing parties (Stoffel 2002, 46).

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Transversal: International Journal for the Historiography of Science, 2 (2017) 157-159

ISSN 2526-2270

www.historiographyofscience.org

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Book Review

Pierre Duhem: Between Physics and Metaphysics

Víctor Manuel Hernández Márquez (Ed.) *Pierre Duhem: Entre física y metafísica*. Universidad Autónoma de Ciudad Juárez and Anthropos Press, 2016. 208 p. 14 € - ISBN 978-84-16421-36-7

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Received: 24 March 2017. Accepted: 12 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.14>

This book is structured by seven chapters written by six researchers from three different Universities: Fábio Rodrigo Leite y João Cortese from the Universidade de São Paulo, Brazil; Ambrosio Velasco Gómez from de Universidad Nacional Autónoma de México and Víctor Manuel Hernández Márquez (coordinator), Roberto Estrada Olguín and Roberto Sánchez Benítez from the Universidad Autónoma de Ciudad Juárez, Mexico.

Each of the authors develops their own analytical perspectives around the work of Pierre Duhem (1861-1916). Ambrosio Velasco seeks to show that the contemporary philosophy of science began from a fundamental criticism of the modern conception of scientific rationality proposed by Descartes (in his rationalist version) and by Newton (in his empiricist turn). Velasco contends that Duhem's contribution to this discussion is to have undermined several myths and dogmas, among them, the Cartesian idea that the rationality of knowledge is based exclusively on strict adherence to certain methodological rules and the Newtonian thought that observation, induction and experimentation are the fundamental procedures of the scientific method.

Although several authors discussed the relevance of the method of composition or synthesis developed by Newton, as J. F. Herschel and W. Whewell did it at the beginning of the 19th century, Velasco argues that the strong empiricist commitment of Newtonian methodology was never questioned on its "foundational basis". Indeed, in his classic work *La Théorie Physique. Son Objet, sa Structure* (1906), Duhem pointed out the inconsistencies of the methodology proposed by Newton in relation to the inductive generation of scientific hypotheses and the limitations of empirical testing methods. At the end of his chapter, Velasco analyzes the influence of Duhem on some contemporary philosophers of science, including Otto Neurath, Karl R. Popper, Thomas S. Kuhn, Larry Laudan and Imre Lakatos.

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According to Velasco, Popper's *Logic of Scientific Discovery* (1935) is a response to the problem of the empirical sub-determination of theories formulated by Duhem. Popper's response to this problem is twofold, (i) that scientific evidence is theoretically dependent and (ii) that scientific evidence is ambiguous. Indeed, Popper defended point (i) without recognizing Duhem's influence on the matter; while point (ii) was not explicitly addressed by Popper but only indirectly in recommending not to "save" the hypothesis in the face of a major refutation. However, Popper's recommendation has to do with certain adjustments – drastic or not (Quine, 1951, 43) – within the theoretical system in order to maintain some theoretical statements. It seems to me that Duhem's argumentation on the matter is more modest by merely suggesting that when there is any conflict with experience, what is refuted is necessarily ambiguous.

In relation to Duhem's influence on thinkers such as Kuhn, Lakatos and Laudan, Velasco contends that the main idea that these philosophers inherited from Duhem is that "philosophical interpretations of science must be based on the analysis of the history of science" (2016, 39). However, Velasco ends by arguing that, with the exception of Kuhn, Lakatos and Laudan resorted to the formulation of methodological meta-rules to ensure not only the rationality of isolated scientific theories; but also the rationality of the research traditions that constitute the very history of science. In other words, both authors ended up "sublimating" the rationality they criticized in order to submit history to its own methodological meta-rules.

In his work, João Cortese seeks to show the common elements between Blaise Pascal and Duhem. According to Cortese, one of the resources that scientists resort to is the use of analogy, which is perceived through the "spirit of fineness". However, Cortese argues that Duhem goes too far in his distinction between the spirit of fineness – which Duhem associates with the heart and the immediate intuition – and the spirit of geometry – tied to reason and deduction. Pascal, from whom Duhem inherits these two concepts, certainly does not conceive this distinction in this way. In particular, the spirit of geometry is not specifically related to principles and deductions.

As is well known, Duhem's conceptual separation between physics and metaphysics (origin of the title of this book) is not a positivist distinction between what makes sense and what does not. In fact, it is a distinction between two legitimate types of scientific knowledge, that is, if physics deals with the description of experimental laws, the task of metaphysics is to show the reason for those laws, says Cortese (2016, 48). And this is how the analogies allow us to understand that scientific development is a progressive transit towards the attainment of a natural classification. Thanks to the spirit of fineness, scientists can "realize" the analogies and the tendency towards the natural classification that science follows; even though, Cortese argues, scientists are not able to logically explain how this could happen (2016, 65).

In his work, Víctor Hernández delves into the role that the concept of 'analogy' has in Duhem's work in relation to intuition and deductive reasoning. Hernández contends that Duhem uses the analogy to solve the tension between physics and metaphysics, without dropping the idea that physical theory is autonomous of any metaphysical system. Hernández argues that there are two basic meanings of the concept of 'analogy', the first as a heuristic resource in the construction of theories and as a bridge between theoretical physics and experimental physics. The second, as a cosmological (or metaphysical) tendency of science that seeks the final explanation of things.

According to Hernández, physics is confined to a set of mathematical claims deduced from a small number of principles that seek simple, complete, and exact representations of experimental laws. However, when logic is insufficient to elaborate this mathematical representation, scientists draw analogies (2016, 81). In these heuristic stages of science, when there are no clear methodological rules, analogy constitutes, according to Duhem, a "sure and fruitful method." Finally, Hernández points out that the contingent use of analogy in Duhem is different from that of Ernst Mach, for whom analogy occupies a "more prominent place in science" (2016, 84).

The most provocative intervention is that of Fábio Rodrigo Leite who argues that the logical analysis of scientific theories shows that it is not possible to obtain any kind of definitive or true knowledge due, among other reasons, to the fact that truth is not guaranteed *a posteriori* by the physical phenomena nor *a priori* by the claim of the universality of scientific statements. According to Leite, the value of science for Duhem is merely practical, that is, science has no relation to the "ultimate causes" that metaphysics studies. So, metaphysics functions as a regulatory idea that allows the "convergence" of science, avoiding relativism.

Leite proposes a Duhemian taxonomy that distinguishes, on the one hand, between metaphysics and cosmology and, on the other hand, between experimental and theoretical physics. In his essay "Physique et métaphysique" of 1893, Duhem accepted the model based on the notion of efficient causality

which allows the transit between physics and metaphysics. However, later, in his 1905 essay "Physique de Croyant", Duhem gave up the possibility of a causal transition from one to the other, replacing it with the notion of 'analogy'. Besides, the religious theme that Duhem left aside in 1893 is included, along with the theme of 'faith', in 1905.

Experimental physics studies three phases, namely, the fact finding, the discovery of its laws and the construction of theories. By other side, there are three degrees of our knowledge of the world, namely, the first degree refers to isolated and even confused facts collected by experience. The second degree is constituted by the knowledge of the purely experimental laws obtained by induction. According to Leite, what we may call the "first" Duhem certainly wavers about the certainty we can get from the general laws obtained by induction (2016, 92-93); while the "second" Duhem confers an absolute degree of certainty to induction. The third degree is obtained through theoretical hypotheses whose terms lack any kind of reference.

Leite's idea is that, although the theoretical laws depend on the laws of common sense, they are not determined by the latter, given that, in this epistemic stage, scientists "are free to choose – in the way that best suits them – their favorite representations" (2016, 96). It is worth noting that raw data from the first stage do not "depend" on theoretical knowledge; but "constitute" it. Certainly, the interpretation developed by Leite is close to the notion of 'hard core' of the scientific research programs developed by Imre Lakatos. In Leite's words: "Maintaining induction at the level of the laws of common sense allows Duhem to establish an immovable basis upon which all empirical knowledge can be erected" (2016, 97).

In order to gain access to the essential knowledge of inanimate matter, Duhem argues that we must begin with the study of effects (of which physics is responsible) and its causes (of which metaphysics is responsible). So, the study of physics precedes the study of cosmology and thus, physics can dispense with metaphysics and be founded autonomously. Note that this cause-effect thesis creates some tension with the Duhemian notion that metaphysics functions as a regulatory idea that allows the "convergence" of science and avoids relativism. That is, as a regulatory idea, physics cannot "do without" metaphysics, especially thinking, as Leite shows us, that physics and metaphysics have a common point in experimental data. For this tension to fade, we have to take into account the evolution of Duhem's thought that transits from these Thomist terms of cause' and 'effect', towards a more constructive version that emphasizes the dialogue thesis.

The last part of the book consists of three historical studies. Roberto Estrada makes a historical inquiry about the origin and nature of the notion of "saving the phenomena" in science. As is well known, the problem is that different hypotheses may be equally suitable to represent the same phenomenon. Estrada argues that it is not at all clear when exactly this concept was formulated for the first time. The only certainty is that it began to be used at the beginning of the Christian era. In his work, Roberto Sánchez also makes a historical inquiry around Duhem's studies on Leonardo Da Vinci. In general, Sanchez traces the sources from which Leonardo developed his scientific ideas, as well as the way in which the thought of this genius influenced the development of certain aspects of the science of his time.

Finally, Víctor Hernandez outlines the possible relationships between the philosophical positions developed by Louis Couturat and Duhem. Hernandez holds that, since an analogy is a type of inductive argument, there are at least two reasons why Duhem's demonstration by *reductio ad absurdum* of the principle of mathematical induction would not have been accepted by Couturat. The first reason is that the proof developed by Duhem ignores the achievements of the new mathematical logic of his time. The second is that such proof must show that the principle of induction is analytic in the sense that it accomplishes with purely logical concepts and axioms. Indeed, Duhem's emphasis on logic within physical theory brings him closer to English contemporary logicians, says Hernandez (2016, 190), forcing us to reexamine the nature of his supposedly "conventionalist" stance based in the Duhemian hypertrophy around the spirit of fineness to the detriment of the spirit of geometry.

In general, the book offers a clear line of research that serves as the guiding thread along the seven chapters, expressed accurately in book's title: Pierre Duhem: between physics and metaphysics. Although each one of the texts approaches the subject from a different angle, it is possible to appreciate certain dialogue between the authors. The depth and conceptual clarity with which each author develops his arguments, shows us that each one of the texts is well documented. The book exhibits an expository cadence that is the result of the thematic coherence demanded in a text written for specialists in the subject.

Transversal: International Journal for the Historiography of Science, 2 (2017) 160-162

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem – Book Review

Duhem, Pierre. *La théorie physique : Son objet, sa structure*. – New edition [Online] / Presentation and editing by Sophie Roux. – Lyon : ENS Éditions, 2016. – 297 p. – (Bibliothèque idéale des sciences sociales). €4,99 e-ISBN: 9782847888348.

Reviewed by:

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Received: 10 April 2017. Accepted: 05 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.15>

It was in 1981– thus during the same year as his dissertation defense, one year prior to doing the same for *Σύζειν τὰ φαινόμενα* (1982), and six years before his book *Duhem: Science et Providence* (1987) – that Paul Brouzeng (1938-2012) finally furnished Francophone readers, after a wait of more than sixty years, with the first complete reprint (and, incidentally, the first anastatic one) of the second edition of *La théorie physique* (1914). It was enriched by an introduction of eleven pages, a very succinct bibliography and an onomastic index, which must have misled many readers since it, in fact, only covered the text of *La théorie physique* itself and not that of the two articles added by Duhem in his second edition. Considering the fact that this reprint of *La théorie physique* is still and ever available at *Vrin Bookshop* (both in hardcover and paperback formats), it is worth assessing any additional value which may be afforded it by Sophie Roux's new online edition, other than the fact that, as is the case with all electronic publications, it offers readers the considerable advantage of being able to search the entire text, thereby addressing the aforementioned shortcoming with respect to Brouzeng's edition.

This contemporary edition distinguishes itself by furnishing (in decreasing order of importance): 1) a comprehensive introductory essay, which is both concise and synthetic, entitled *Lire « La théorie physique » aujourd'hui* (23 pages); 2) a summary of around 15 lines at the beginning of almost every chapter (Chap. 3 of Part 1 being an odd exception), thus deftly bringing the progression of the Duhemian arguments (p. 1) to the fore; 3) over fifty notes – often biographical, sometimes particularly enlightening (p. 112, n. 2) and erudite (p. 248, n. 85) – added by this editor and serving to comment upon the 230 pages of text (it is, however, regrettable that the notes of both Duhem and the editor were carried over to the end of each chapter instead of being, more conveniently, placed at the bottom of the relevant page; 4) the typographical emphasis of certain quotations, the modernization of units of measurement, and the (unreported) correction of some errata; and 5) a more comprehensive bibliography than that of Brouzeng.

In her introductory essay, Sophie Roux astutely proposes to retrace the reception of *La théorie physique* in the 20th century, and thus to explain to the reader why this renowned work was so little-read and largely misunderstood for so long, and therefore why the time has come to read it in its entirety and its authenticity. In order to achieve this, she identifies three stages within this reception. The first takes us from

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the genesis of this work to its first reception in France (1892-1940), emphasizing the part that was due to its complex stance (against the positivists and equally against the neo-Thomists), to its religious convictions and its scientific choices (against atomism and against relativity) in light of Duhem's lack of influence during this period. The second, which is undoubtedly more original, analyses the reception, still in France, of this historic Duhemian work by comparing it to that of Alexandre Koyré (1940-1970). Even if such a comparison may seem appropriate, the proposed ideas themselves are certainly not: it will surely retain the interest of the specialists, without necessarily obtaining their full approval. Finally, the third stage (1950-1985) leads us initially to the German-speaking countries (with the Vienna Circle), then on to the Anglophone countries (with post-positivism), and deals with the social, political and religious “decontextualization” of the work, all of which afford a better global understanding from a contemporary perspective. Aside from the overall accuracy of the ideas expressed, the entire text is compelling due to its conciseness, clarity and the quality of expression.

Even if, as we have just observed, the reading of this introductory essay undoubtedly reflects the editor's ability to successfully meet the challenge of composing an introduction to a book – especially one as eager to flee its misleading labelling as Duhem's most renowned work – unfortunately, this examination also reveals that it may hold little interest for those seeking attention to detail. Indeed, Duhem died in 1916, and not in 1917 (p. 12 et p. 14); Brouzeng's forename was 'Paul' and not "Pierre" (p. 6); *La théorie physique* first appeared as various installments in the *Revue de philosophie* and not in the *Revue des questions scientifiques* (p. 7); Duhem was not elected “Corresponding Member in the Physics section of Academy” in 1913 (p. 12), but rather ‘Corresponding Member’ in the Mechanics section in 1900, and ‘Non-Resident Member’ in 1913; even if “Marcellin” is indeed a forename (p. 12), in Berthelot's case, his is the variation ‘Marcelin’; the name of the great French mathematician is spelt ‘Hermite’ and not Hermitte (p. 12); read ‘Octave Manville’ rather than “Octave Mandeville” (p. 12); P. Humbert's book came out in 1932 and not 1933 (p. 12); it was not to P. Humbert that Duhem was replying, upon the occasion of the appointment of a Chair for the General History of Science at the *Collège de France*, regardless of his possible return to Paris as a theoretical physicist (p. 12), but to E. Jordan; Humbert's text is, in this context, merely a quote from Jordan's (cf. E. Jordan, *Pierre Duhem*, in *Mémoires de la Société des sciences physiques et naturelles de Bordeaux*, 1917, p. 16); Duhem's book of 1902 was called *Les théories électriques de J. Clerk Maxwell: Étude historique et critique* and not *Théorie historique et critique [sic] de J. Clark [sic] Maxwell : étude historique et critique* (p. 25); the review dedicated to *La théorie physique* by G. Lechalas has a title, namely *M. Duhem et la théorie physique*, and was published in 1909 in *L'année philosophique* rather than in 1910 in a journal entitled *l'Année de philosophie* (p. 26). Consulting our Duhemian bibliography (unmentioned) would undoubtedly have avoided many of these errors.

Similar inaccuracies are also, naturally, to be found in her Duhemian text annotations: the *Revue des questions scientifiques* never went on to be called “*Revue scientifique*” (p. 75, n. 12) for the simple reason that there was already a review of that title in existence, as is evidenced by F. Mentré's article which is clearly mentioned (p. 248, n. 79); the fundamental letter which Duhem penned to his friend J. Récamier cannot be categorically assigned to the year 1906 (p. 116, n. 68) – we mentioned it previously as having been written “undoubtedly after 1906” (J.-Fr. Stoffel, *Le phénoménalisme problématique de Pierre Duhem*, p. 79), and, at present, we can safely date it to around 1915, which makes it coeval to Duhem's frame of mind at the time of *La science allemande*; lastly, his 1911 book, which apparently marks the apotheosis of his scientific work according to our fellow scholar, is entitled *Traité d'énergétique ou [and not « et »] de thermodynamique générale* (p. 249, n. 94).

Displeasing in the context of an introductory essay, this lack of attention to detail becomes a great deal more problematic when one's primary objective is editing a text. Even if the body of the text seems to have been accurately reproduced within this current edition of *La théorie physique*, one certainly has grounds to mistrust the transcription of Duhem's own footnotes. Indeed, beyond the rather inexplicit statement that “some additions [were] made [by the editor] to the bibliographical references” (p. 5), it should be understood that these notes were instead extensively revised (and not just complemented) in order to render them both more precise and more in line with current bibliographic standards. Unfortunately in the present case, this objective, although commendable in itself, proves rather difficult to achieve for three main reasons: 1) Duhem's text is often severely altered to the point where the valuable information it contained is lost (for example, the reference number given to a letter in an edition of his correspondence, or the title of a chapter or section referred to specifically), mistakes appear where there were none (at the risk of raising

suspicious that Duhem, since he had made errors in his references, may not have hesitated to refer books he had never consulted), and this without even systematically rectifying the erroneous references present in the Duhemian text; 2) the reader is kept in the dark as to the changes effected and as to their extent, since these are neither explicitly stated nor typographically documented; 3) lastly, these alterations, incoherent as they are, do not seem to adhere to any form of systematic implementation resulting from clearly predefined principles. In deference to those who pay attention to the footnotes – and especially to those for whom it is their primary focus of study – the text of the Duhemian footnotes should have been faithfully transcribed, as well as the bibliographical references systematically checked, before distinctly claiming to offer a more complete version. Instead, in her eagerness to improve and modernize the Duhemian bibliographical references, the editor provides the French translation of city names, Gallicizes authors' names, reduces forenames to their initials, and transforms publications years, which were furnished in Roman numerals, into Arabic numerals. By effecting all these modifications, one naturally runs the risk of introducing errors. Here are a few examples (bearing in mind that we indeed checked each one to be quite sure that they were neither necessary nor appropriate): “MDXCVI” becomes “1615” (p. 75, n. 14); “MDCXXVI” becomes “1636” (p. 245, n. 14); “MDCLI” becomes “1606” (p. 246, n. 20); and “MDLVII” becomes “1556” (p. 247, n. 51).

These errors, resulting from the author's commendable desire to amend the Duhemian notes, are evidently compounded by those due to inaccurate transcription. To further illustrate this point: “*Essai sur la théorie physique*” instead of “*Essai sur la notion de théorie physique*” (p. 75, n. 11); “t. I” instead of “t. IV” (p. 75, n. 13); “*Lectures on Molecular*” instead of “*Notes of Lectures on Molecular*” (p. 114, n. 23); “Syperum” instead of “Sygerum” (p. 245, n. 9); “1558” instead of “1588” (p. 245, n. 6); “1646” instead of “1640” (p. 247, n. 57); and “XXVII” instead of “XXXVII” (p. 247, n. 65).

Finally, we would like to specify that this review was based on the version directly transmitted to us, i.e. the version dated 2 September 2016 – it is therefore possible, and even desirable, that some or even all of these detected errors have since been corrected.

In conclusion, while we invite all those interested in Pierre Duhem, both the historical figure and his philosophy, to read Sophie Roux's introductory essay, we do urge those for whom the accuracy of the text is primordial, to consider whether it may be worth adhering to Paul Brouzeng's classic edition, especially considering the fact that no link between these two editions is provided in this editor's contemporary version.

Transversal: International Journal for the Historiography of Science, 2 (2017) 163-165

ISSN 2526-2270

www.historiographyofscience.org

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Dossier Pierre Duhem – Book Review

Bordoni, Stefano. *When historiography met epistemology: Sophisticated histories and philosophies of science in French-speaking countries in the second half of the nineteenth century.* – Leiden; Boston: Brill, 2017. – x, 335 p. – (History of modern science; 2). €149,00 ISBN: 9789004315228

Reviewed by:

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Received: 10 April 2017. Accepted: 03 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.16>

Dedicated to a book which has long been considered a classic, and which, from the *Traité de l'enchaînement des idées fondamentales dans les sciences et dans l'histoire* (1861) by A.-A. Cournot to *L'évolution des théories physiques du XVII^e siècle jusqu'à nos jours* (1896) by P. Duhem, takes us on a tour of 35 years of intellectual history, this review offers three objectives. Firstly, to present the author's broader arguments. Secondly, considering that, on the one hand, its contents are not immediately apparent (at least not from its Table of Contents) and that, on the other hand, the method used consists in providing (while remaining as faithful to the text as possible) a critical interpretation and commentary on the selected publications, to provide a brief introduction to the authors and the themes addressed. Lastly, owing to its publication within a dossier specifically dedicated to P. Duhem, to further explore the main arguments and ideas, which occupy nearly a third of the work, centered around this illustrious scholar.

French historical epistemology can be defined as the conviction whereby a genuine and authentic historical perspective is seen as essential in order to establish a constructive dialogue between science and philosophy, and in order to construct an epistemology which better conforms to the reality of scientific approach. According to the traditional view adopted chiefly by A. Brenner and C. Chimisso, it originated, depending upon the chosen emphasis, either during the last decade of the 19th century with the works of H. Poincaré, P. Duhem and G. Milhaud (A. Brenner), or during the 1930s and 1940s with G. Bachelard as the key figure in this case (C. Chimisso).

Without disputing the importance of the discussions conducted at the end of the 19th century, the point of this book consists in correcting the chronology that we just mentioned: this historicization of the epistemology or, to put it differently, this onset of a mutual engagement, both profound and sophisticated, between historiography and epistemology occurred during the 1860s, thus in the second half of the 19th century and not, as is commonly stated, at the extreme end of the 19th century, and certainly not during the first half of the 20th century. Consequently, it becomes instantly evident that H. Poincaré and P. Duhem, rather than constituting the starting point (A. Brenner) or even representing part of its ancestry (C. Chimisso), should be seen as a destination point (albeit provisional), which is particularly true for Duhem in as much as

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he personifies the fulfillment and consolidation of a project which had hitherto been conducted by A.-A. Cournot, Cl. Bernard and E. Naville.

However, this is not to suggest that the sole interest of this work is to postpone the starting point, by a couple of decades, of this intellectual movement, which, by promoting the belief that science is a complex historical and philosophical process instead of considering it as simply ruled by logic and/or experience, resulted in the progressive substitution of a more sophisticated history of science and a more critical epistemology of the scientific approach, for naïve historical reconstructions and simplistic, dogmatic epistemological concepts. Indeed, beyond the mere question of chronology, it involves, on the one hand, recognizing the qualities of a tradition (inspired by Pascal) designed to find the right balance between a naïve and dogmatic ‘scientism’ and an ineffective scepticism and, on the other hand, promoting the idea that — in opposition to the normative and simplistic epistemologies that we have grown used to — the revival of such an aspiration is not only desirable but still possible. Let’s discuss each of these two assertions.

Regarding the first (reviving Pascalian inspiration), in this work one might find the prominence of Pascal’s personage surprising, especially in contrast to the influence that I. Benrubi intended to attribute to Kant. Aside from the arguments traditionally put forward to account for this Pascalian presence — the discovery of the original manuscripts of *Pensées* and the effectiveness of his ideas in counteracting the prevailing scientism — the author introduces the idea that, during the last few decades of the 19th century, the debate about determinism and reductionism took place within a context (modern science being of sufficient maturity) which allowed for this illustrious 17th century thinker’s voice to be heard, but within a situation where such subversive ideas could only remain inaudible.

As to the second (the resurgence of this inclination towards a refined and well-balanced stance), this is enabled by the author himself. His conviction that the history of scientific thinking is not only characterized by a progressive accumulation of knowledge and by the occurrence of scientific revolutions, but also by a third component: the “buried memories”, namely, the historical process by which isolated research projects, or even broad intellectual traditions, entirely disappear from prevailing considerations despite their heuristic fecundity, only to reappear at a future time in a slightly different form and within a new historical context. Recognizing that this submersion is precisely what had happened to the sophisticated thinkers he studied, St. Bordoni suggests that their intellectual trends had nonetheless been revived by such diverse figures as A. Koyré, N. R. Hanson and Th. Kuhn. Through this philosophy of history, which we would readily describe as typically Duhemian due to its mixture of optimism and unpredictability, the author suggests that studying intellectual life during the second half of the 19th century, is not only about providing a means to better understand that of half of the 20th century, but also about preserving a precious heritage that should not be definitively condemned to perpetual imprisonment in the annals of history.

Having covered the general theme, let us briefly consider the contents of the book. After a lengthy introduction, aimed at, on the one hand, outlining the broader context of this era marked by a profound transformation of life, being as much material as intellectual, and, on the other hand, presenting the historiographic theme which is to be developed upon throughout the work, the first six chapters are devoted to two fundamental questions which, between the beginning of the 1860s and the middle of the 1890s, animated many scientific discussions among French thinkers largely influenced by scientism. These initially dealt with determinism (considered from the perspective of reconciliation with human free will) and reductionism (conceived as, firstly, legitimately bringing the social sciences back to the model represented by the natural sciences and, secondly, reducing the natural sciences themselves to the archetype represented by classical mechanics). In this book, the reason for such detailed discussions on determinism and reductionism is that they clearly demonstrate the conflict between a naïve scientism and the emergence of newer and more sophisticated historical and philosophical reconstructions. The first three chapters, which are devoted to reductionism, include a substantial section on the refined (yet largely overlooked, since premature) ideas of A.-A. Cournot — undoubtedly one of the author’s preferred scholars, along with E. Naville and P. Duhem — but also include those of Cl. Bernard, as well as É. Boutroux’s radical yet minority antireductionism and, conversely, the reductionism of J. Soury whose personal and intellectual evolution seems representative of that of his entire era. Determinist discussions are the subject of three consecutive chapters chronicling, as central figures, J. Boussinesq with his multidisciplinary, original research program, and the philosopher and theologian E. Naville with his flexible and dynamic epistemology. Since the emergent intellectual movement was characterized by a new awareness of the historical and philosophical complexity of the scientific process, it is to be expected that this realization would give rise, aside from those

originating in traditional historiography, to further research in the history of science conducted according to a new historiographic framework. Chapter 7 is also devoted to the history of science, with three personalities corresponding to three different stages: M. Berthelot typifying a radical and naïve historiography of Comtean inspiration, which is however somewhat tempered by the collective nature of its endeavour; P. Tannery typifying a history of science based on multiple skills and largely devoid of any hagiographic or ideological perspective; and lastly, G. Milhaud typifying an attempt to summarize the erudite research of his predecessor, and to combine history and philosophy of science. Since Duhem personifies, through the original ideas he developed within the context of his firsthand experience in scientific research, the very essence of the sophisticated alliance between science, history and philosophy, this study naturally culminates in two chapters dedicated to him. Finally, it is worth noting the existence of an “epilogue”, which, although of somewhat heterogeneous content, is nonetheless invaluable.

We would like to conclude this review by focusing more specifically on the role attributed to P. Duhem. Having previously evoked the fact that Duhemian *physics* issued from the scholars who preceded him (*Taming complexity: Duhem's third pathway to thermodynamics*, 2012), St. Bordini — as foreseen — continues his enquiry by examining those elements of Duhem's scientific philosophy which are ascribable to his numerous philosophic and scientific forefathers. From the onset we notice the emergence of an intriguing and distinct behavioural difference: there is Duhem-the-physicist who never hesitated to acknowledge the existence of the relevant scientific tradition from which he drew his inspiration, and then there is Duhem-the-philosopher who never explicitly referred to the scholars and philosophers who, just prior, had entertained similar beliefs and ideas. This difference is all the more astonishing since the appearance of the scientific tradition, from which it is inspired, and that of the philosophical tradition, from which it could have been inspired, are contemporary. Among the various causes which could account for this difference, St. Bordini points to the fact that Duhem relied primarily on the dual influence of Aristotle and Pascal, far more than on that of his immediate predecessors. In order to summarize this dual influence — and most certainly worth a direct quotation here — the author formulates the following magnificent explanation: “Duhem found in Aristotle the awareness of the complexity of natural processes, and he found in Pascal the awareness of the complexity of scientific practice” (p. 241). Lastly we would like draw the readers' attention to the ideas advanced with respect to natural classification (a particularly fragile concept), and to a certain Duhemian deviation (jeopardizing his sophisticated philosophy of science by succumbing to the appeal of a more comfortable finalism).

By positioning itself chiefly in relation to the historiographic theories advanced by I. Benrubi, A. Brenner et J. Renn, by distrusting the legislators of scientific practice, and by urging us not to idealize the adjectives ‘naïve’ and ‘sophisticated’, which it fortunately coined itself, this book constitutes, on the whole, a veritable and most welcome plea in favour of recognizing the complexity of the natural world, as well as the no less important historical and philosophical complexity of the scientific approach which is called to investigate it!

Transversal: International Journal for the Historiography of Science, 2 (2017) 166-203

ISSN 2526-2270

www.historiographyofscience.org

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Article

A Development of the Principle of Virtual Laws and its Conceptual Framework in Mechanics as Fundamental Relationship between Physics and Mathematics

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Abstract:

Generally speaking, *virtual displacement* or *work* concerns to a timely idea according to which a motion of a certain body is not the unique possible motion. The process of reducing this motion to a particular magnitude and concept, eventually minimizing as a *hypothesis*, can be traced back to the Aristotelian school. In the history and philosophy of science one finds various enunciations of the *Principle of Virtual Laws* and its *virtual displacement* or *work* applications, i.e., from Aristotle to Leibniz's *vis viva*, from Maupertuis' *least action* to Euler and Lagrange with calculus of variations (statics and dynamics) to Lazare Carnot's mechanics. In this case study, I will demonstrate that a particular approach used by Lazare Carnot is original by explaining within the historical context of rival approaches such as the development of the *Principle of virtual Laws* (also known as the *Principle of virtual velocity* or of *virtual work*). I will also discuss Carnot's *geometric motion* as one of the possible but invertible movements applied to *virtual displacement* as employed in his theories of machines and collisions. I will then go on to explore how the originality of an *invertible motion* within his mechanical and, in general, mathematical research program permitted Carnot to introduce a new way of structuring a scientific theory and making mechanics, with respect to the Newtonian paradigm, to scholars and his students of the *École polytechnique de Paris*.

166

Keywords:

Virtual laws; virtual velocities; Aristotle; de Nemo; Tartaglia, *gravitas secundum situm*, Newtonian paradigm, Lagrange's mechanics, Geometric motion, Mechanics and collision theory, Lazare Carnot, Relationships between Physics and Mathematics, Foundations of Physics.

Received: 13 January 2017. Reviewed: 20 March 2017. Accepted: 30 March 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.19>

Introduction

A history of the *Principle of virtual laws* (velocities, work) asserts, i.e., that even if one agrees that the *principle of virtual work* exist prior to all laws of mechanics (and not all scholars agree with this thesis), and could therefore be derived from the *principle of virtual work*, in the end, the fact that the *principle of virtual*

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work is self-evident cannot be accepted. In other words, one cannot accept it as a mere principle. Therefore, proof is necessary; or, a reduction of a theorem of another approach to mechanics, or an attempt to provide a more convincing version. Consequently, the main challenge of proving a *proof* of the *principle of virtual work* sparked a heated debate, especially with Vittorio Fossombroni (1754–1844; Fossombroni 1794) and in France where Lazare Carnot (Carnot L 1786, 1803a), Fourier (Fourier 1798; 1888–1890, 475–521), Ampère (Ampère 1806) and Poinsot (Poinsot 1838; see also Poinsot 1806) provided major contributions. In effect, a difficulty was linking the problem to Newtonian laws and obtaining its formal validity. Initially, this principle was independent from Newtonian laws, which were generally concerned with an isolated particle (or the systems derived from it). The *Principle of virtual work* also deals with extended systems of bodies² which include constraints in an essential way.³ These given forces are constraining reactions that are not included in the classical Newtonian scheme because they are unknown *a priori* (Lagrange 1788, pt II, IV). It was on the principle of least action that young Lagrange concentrated his attention.

Before beginning with the following scientific and historical overviews explanation, just few lines to clarify that the discussion will focus on: a) the history and philosophy of sciences, precisely on the historical epistemology of physics (mechanics) and its relationships with mathematics; and b) a mechanical system subject to constraints. In particular, the latter is a standard physical system⁴ today which deals with movement (kinematics), equilibrium (statics), applied physical forces (dynamics) and in some circumstances, applied mechanics, including machines, machinery and micro elements. The constraints (also *vincula*) result in a restriction on the freedom of movement of a mechanical system, i.e., of particles. The constraints are presented as mathematical physics relationships between coordinates and force. Thus, considering these aspects of a mechanical system, I am going to develop historical and epistemological research, which relies upon historical and historiographical (Kragh 1987) sources. My aim is to suggest *history and philosophy of science as historical epistemology of science* (Renn 1995; Renn and Damerow 2010).

Related to this, Lazare Carnot presented a new mechanics system without axioms and metaphysical entities. When he worked on machines, he analyzed the efficiency of machines using the *Principle of virtual laws* and reasoning about a collision model of mechanical interaction. Despite his fundamental works (Carnot L 1786, 1803a,b, 1813) and his notable political career (Dhombres and Dhombres 1997), Lazare Carnot appears as minor figure in comparison with lead scientists such as Newton, D'Alembert, Lagrange and Fourier. This is a general conviction, which deserves to be discussed. By investigating historical and epistemological aspects of science foundations, particularly focusing on the relationship between physics and mathematics, we open to the possibility of discovering new insights.

The Structure of the Paper

In order to discuss the originality of the *Principle of virtual work* adopted and used by Lazare Carnot in his mechanics (and geometry), an early historical account about the development of this principle is necessary. Therefore, the paper is structured as follows:

- Early approach: *Principle of virtual laws* by Aristotle (virtual velocities in *Problemata mechanica*⁵) and applications by de Nemore and Tartaglia (*Science of weights, virtual velocities, virtual displacements*).

²A difference exists with Euler's reasoning on fluids (Euler 1757, 286): the partial derivatives in Euler's equations (Euler's fluids) are applicable to compressible as well as to incompressible flow. It consists of an application of either an appropriate equation of state or assuming that the divergence of the flow velocity field is zero, respectively.

³ Some of this historical material is clearly dealt with Darrigol's *Physics and Necessity* (Darrigol 2014, 80-84).

⁴ Generally speaking and nowadays, a physical system is defined as the portion of the physical universe chosen for physical investigations and mathematics modeling.

⁵ Attributed to Aristotelian School. I remark, in any case, that the *equilibrium* is never cited in *Problemata mechanica*. On that a remark is only reported by the commentator (Aristotle 1955 [1936], 350, ft. a). See also Duhem 1905-1906, I, 240-270.

- Late approach: mechanics in context, principle of *virtual work* by Lagrange (*analytical mechanics*).
- A case study on the relationship between physics and mathematics: Lazare Carnot (*equilibrium and movement, machines, collision theory*).

For sake of brevity,⁶ a description of the different mechanical traditions in the seventeenth and eighteenth century are not presented in the details. Readers interested in previous works of mine can refer to: Pisano and Capecchi 2013, Gillispie and Pisano 2014, and most recently, Pisano and Capecchi 2015. However, taking in to account the objective of the paper, the extended part at the beginning on the *Principle of virtual work/displacements* and subsequent parts are largely discussed. For sake of brevity and the aims of this paper, I do not go into to the huge literature around both Bernoulli-Varignon-D'Alembert conceptualisations (parallelograms, energies, forces, velocities, theorems/laws, dynamics/mechanics as empirical science, geometry, etc.: i.e., see Bernoulli 1686, 1703, 1742; Varignon 1725; D'Alembert 1758, 1755 [1743]) and the debate on its proof belonging/not belonging to the Newtonian (paradigm) laws (i.e., see Sommerfield 1951, 1952).

A Scientific Overview of the *Principle of Virtual Laws*

In order to define the principle of virtual law in scientific terms, a short overview follows.

In modern terms, to define the *principle of virtual work*, one can specify that a *displacement* is possible if it is compatible within the fixed constraints. In addition, it is *virtual* if it is compatible within the constraints even if they are moving. Specifically, *virtual displacement* δs of a point is any arbitrary infinitesimal change in the position of the point consistent with the constraints imposed on the motion of the point. This displacement can be only imagined. Limiting ourselves to the case of time-independent constraints, we can also derive a *possible displacement* (i.e., a rotation as well). If the *virtual displacements* are mathematically independent variables, they are also arbitrary.

The *principle of virtual work* – which arises in the application of the *Principle of least action* (hereafter discussed) to the study of forces and movement of a mechanical system – is a law of mechanics for which there is not any generally accepted epistemological and ontological account: it can be seen both as a principle and as a theorem to be proven. The *principle of virtual work* is used in statics for a solution of a special class of problems involved in a system in equilibrium. Generally speaking:

The necessary and sufficient condition for equilibrium of a mechanical system without friction is that the virtual work done by the externally applied forces f is zero.

$$\sum f_i \cdot dr_i = 0$$

On the whole:

- a) The necessary and sufficient condition for the equilibrium of a particle is zero virtual work done by all working forces acting on the body during any virtual displacement δs consistent with the constraints imposed on the particle;

⁶ The *Principle of Virtual Laws*, argument of this paper, is a fundamental part of my lines of research. In ca. two last decades, I met it in several studies of mine on the relationship between physics and mathematics in history and philosophy of science such as my main subject of inquiring. This paper is an advanced-structured reorganization of a short self-sufficient interlude and spots-ideas of the *Principle of Virtual Laws* presented in previous publications of mine. In advance, I warmly thank the publishers. Generally speaking I dealt with it in *Conceptual and mathematical structures of mechanical science between 18th and 19th centuries* (Pisano and Capecchi 2013), *Lazare and Sadi Carnot: A Scientific and Filial Relationship* (Gillispie and Pisano 2014, 353-356; 375-381), *Tartaglia's science of weights* (Pisano and Capecchi 2015, 224-225), *The Emergencies of Mechanics and Thermodynamics in the Western Society during 18th-19th Century* (Pisano and Bussotti, 412-413). Thus, to deeply discuss and to present a structured history of the principle of virtual laws from Aristotle to Lazare Carnot – the cited publications – as cases study – necessary parts are cited from them as a self-citation. Particularly, I thank Springer (Dordrecht) for courtesy and permission.

- b) The necessary and sufficient condition for the equilibrium of a rigid body is that the virtual work done by all external forces acting on the particle during any virtual displacement δs consistent with the constraints imposed on the body is equal to 0.

Particularly, if the *principle of virtual work* is applied to a system of rigid bodies (i.e., mechanism), then no *virtual work* is done by internal forces, by reactions in smooth constraints, or by forces normal to the direction of motion. The virtual work is done by reactions when friction is present.

In statics, it is possible to calculate a force F implied in an equilibrium state concerning, e.g., a crank-slider mechanism in the position given by a certain known angle. For example, by knowing essentially a) the position of points of actions of applied forces implied in a given mechanical system, b) the coordinates-position (α, β, γ) of the crank (and distances and others data of the problem) then, according with application of the *Principle of virtual work*, an immediate equilibrium equation can be written using $\delta\alpha$, $\delta\beta$ and $\delta\gamma$.

Early Times

Based on two main traditions of the *principle of virtual work*, Aristotle (384-322 BC) deals with *virtual velocities* and Jordanus de Nemore (fl. 12th–13th) deals with *virtual displacements*. Particularly in Aristotle, small forces can move great weights:

[On the lever, Problem 3]. Why is it that small forces can move great weights by means of a lever, as was said at the beginning of the treatise, seeing that one naturally adds the weight of the lever? For surely the smaller weight is easier to move, and it is smaller without the lever. Is the lever the reason, being equivalent to a beam with its cord attached below, and divided into two equal parts? For the fulcrum acts as the attached cord: for both these remain stationary, and act as a centre. But since under the impulse of the same weight the greater radius from the centre moves the more rapidly, and there are three elements in the lever, the fulcrum, that is the cord or centre, and the two weights, the one which causes the movement, and the one that is moved; now the ratio of the weight moved to the weight moving it is the inverse ratio of the distances from the centre. Now the greater the [image] distance from the fulcrum, the more easily it will move. The reason has been given before that the point further from the centre describes the greater circle, so that by the use of the same force, when the motive force is farther from the lever, it will cause a greater movement.⁷

The main Aristotelian *simplicity hypothesis*, concerning virtual work and correlated *Principle of Last Action* (see the notable Feynman's lectures on the subject: Feynman 1963), was based on the actual movement of a body as a natural motion, tending to minimize the motion of a particular material body (i.e., one can see Aristotelian inertia). In Jordanus⁸ de Nemore's mechanics (de Nemore 1565a [1533]), also known as the principle of virtual displacement, a physical system (e.g., masses subjected to forces) is in equilibrium state if and only if the (forces-)weights are inversely proportional to their virtual displacements. In his words:

If two weights descend along diversely inclined planes, then, if the inclinations are directly proportional to the weights, they will be of equal force when descending [idem force – equilibrium]⁹.

⁷ *Problemata Mechanica* 850a 30 in Aristotle 1955 [1936], *Mechanical Problems*, 3, 353. Renn, Damerow and McLaughlin 2003.

⁸ See also Jordanus de Nemore in Tartaglia's edition (Tartaglia 1565b; see also Clagett and Moody). Moreover, both *Elementa Jordani super demonstrationem ponderum* (1229) and *Liber de ratione ponderis* (fl XIII c.) show an interesting proof of Archimedes' law of the lever, *Quaestio Sexta* (*Liber de ratione ponderis*, in Tartaglia's *Jordani Opusculum de Ponderositate* edition: Tartaglia 1565b, 5(–6), line 13), by means of the application of the *principle of virtual work* and where the fall of geometric directions of *displacements* is considered vertical by Jordanus de Nemore. Moreover, one can also see Jordanus de Nemore's *Suppositio Sexta* (Tartaglia 1565b, 1, line 13; see also *Liber de ratione ponderis* edited by Clagett and Moody, 174–175) where one can read that a body is able to raise another lighter body if a lever is utilized, that is, like an embryonic engine.

⁹ "Quaestio decima. Si per diversarum obliquitatum via duo pondera descendant, fueritque declinationum et ponderum una proportio eodem ordine sumpta, una erit utriusque in descendendo [idem force – equilibrium]" (Tartaglia 1565, 7, line 1). See also Tartaglia 1554, Quesito XV–Def. XII; Quesito XVI–Def. XIII, 84, line 7 (Pisano and Capecchi 2008, 2010; Capecchi and Pisano 2010a, 2010b; Pisano 2009c, 2008).

Particularly Jordanus de Nemore¹⁰ studied *principles of virtual displacement* in the following way (Tartaglia 1565b, *Quaestio Sexta*, pp 5–6; see Fig. 1):

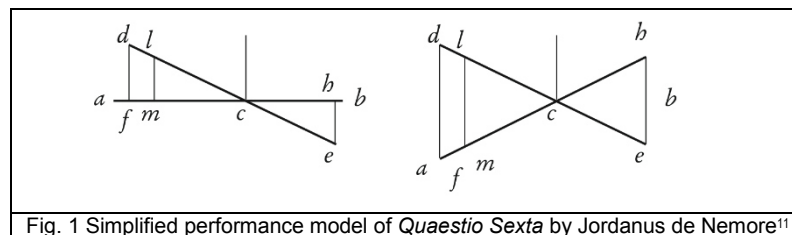


Fig. 1 Simplified performance model of *Quaestio Sexta* by Jordanus de Nemore¹¹

The *principles of virtual displacement*, adapted to force(–weight) and lever, claim that a (virtual) rise h of a body p placed on an arm of the lever should correspond to a (virtual) lowering H of a body P placed on the other arm of the lever, so that the relation $ph=PH$ should be valid. In short, two facts related to Jordanus de Nemore’s statement (*Ibidem*) should be noted:

- a) No reference to the time during *virtual displacement* is considered.
- b) De Nemore’s principle suggests two trends. If the body goes up, instead going down, then the *virtual work* is negative.

Therefore, the principle proposed by Jordanus de Nemore is a principle of equivalence, or conservation, which is not in a condition of equilibrium. In other words, the equilibrium condition must be obtained for *reductio ad absurdum* (as below discussed). The examination of the proof of the law of the lever reported by de Nemore, in his *Liber de ratione ponderis* (*Jordani Opusculum de Ponderositate* edition: Tartaglia 1565b, pp 5–6), addresses this elegant method in order to improve the theory and make it more profound. Nevertheless, we should note that the *principle of virtual displacement* is not mentioned explicitly; it is part of a proof. For instance, the proof of an inclined plane obtained by implementing the *principle of virtual displacement* is an emblematic reasoning and thanks to that principle, the procedure is correctly analysed for the first time. For an application of *Principle of virtual work* within *Science of weights* and related to the lever we have: let us consider a lever where two bodies having mass P and Q are applied at the end points of said lever. These bodies are inversely proportional to the length of the arms.

This statement is commonly expressed by $M=Fd$, where F is, e.g., one of the two force–weights ($F=mg$, related to one of the masses), d is the distance between the two force–weights and the fulcrum, and M is the consequent turning force¹². Based on the principle of equivalence, mass P can be substituted by a body having mass equal to Q , at distance q from the fulcrum of the lever, and placed on the same side of P , since $Qq=Pp$ should be valid. In this way, thanks to the *principle of sufficient reason*¹³. Nevertheless, this situation logically implies that the lever was also in an equilibrium state before substituting mass P with mass Q . In order to better introduce the *principle of virtual work*, let us examine an application of Jordanus de

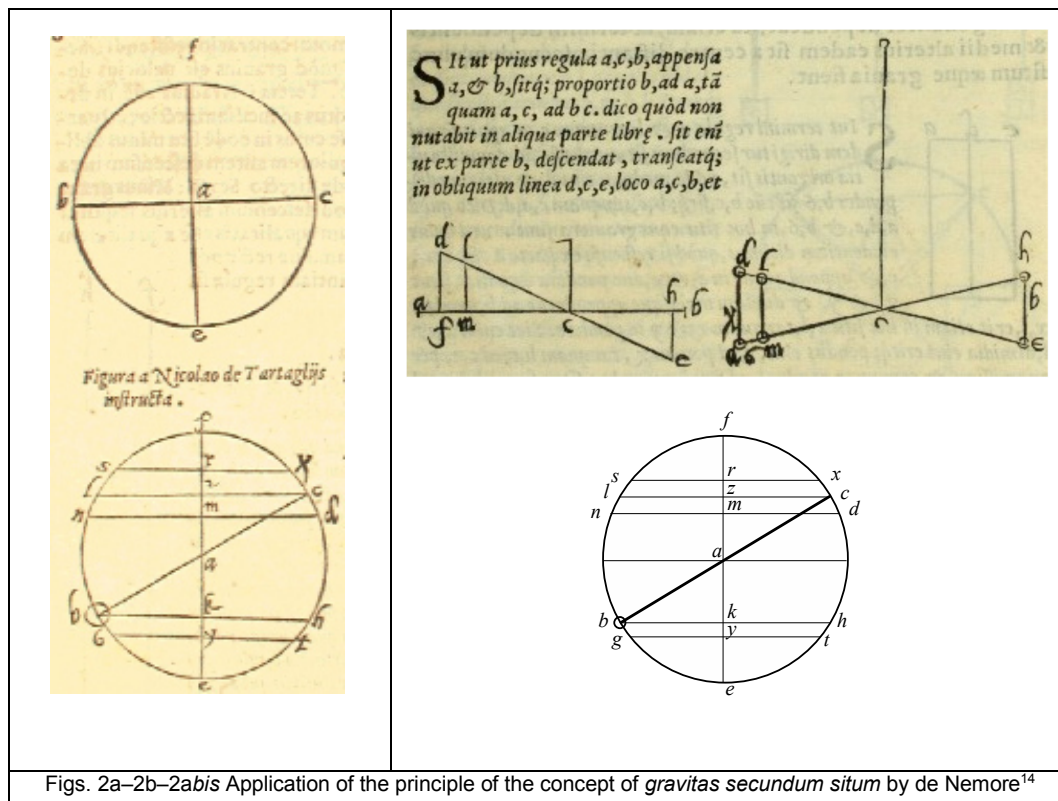
¹⁰ Jordanus de Nemore’s ideas will have a foundation in René Descartes’ (1596-1650) algebra.

¹¹ Tartaglia 1565b, 5–6. See also Pisano and Capecchi 2015, 224.

¹² I precise that M is the moment (or rotation). It is not the *Momentum*. The moment generally corresponds to a measure of an effect caused by a physical quantity around a certain axis. The *Momentum* (Galileo 1890–1909, II, 159–161) is a physical property. Nowadays, in classical mechanics is the product of the mass and velocity of an object: $p=mv$. On *Momentum* see also Galluzzi (Galluzzi 1970) and Galluzzi and Torrini (Galluzzi and Torrini 1975–1984).

¹³ Where, i.e., anything that happens does so for a reason no state of affairs can be obtained, and no statement can be true unless there is sufficient reason why it should not be otherwise. This principle is implicitly used until the born of the modern statics. For sake of brevity I do not discuss the evident importance of this principle reminding to the reader to so extensive secondary literature. I only precise that the bases of the first Archimedes’ law (*Equilibrium Plane*, Archimedes 2002) are mainly two: 1) epistemological one related with two equal weights et equal distances – that is *principle of sufficient reason* – and 2) the two weights are in equilibrium (Capecchi and Pisano 2010a).

Nemore's reasoning (Pisano and Capecchi 2007; Capecchi and Pisano 2007, 2008, Pisano and Capecchi 2013):



Figs. 2a–2b–2abis Application of the principle of the concept of *gravitas secundum situm* by de Nemore¹⁴

Let us now consider de Nemore (and Tartaglia's) application to the principle to *gravitas secundum situm* (positional gravity). Let us assume that the body having mass P moves down along the arc of the circumference (Fig. 2c) from point h to point z , thereby describing the arc of circumference hz . The path traced by P , not along the arc of circumference, but along the diameter of the circle (Fig. 2c), is $h'z'$. Additionally, let us assume, e.g. that the body having mass P , in another circumstance moves down along the arc of circumference $ak=hz$ (Fig. 2c) where the path traced along the diameter of the circle is ok' . Therefore, since $hz=ak$, we obtain that

$$h'z' < ok'$$

so the fall of the body having mass P along the diameter (Fig. 2c) is more "oblique" because it travels over a shorter segment ("less direct path"). Of course, *oblique*, means that body has more/less *gravitas secundum situm* with respect to another body-position along the circumference; it depends on the geometry of the problem. Here, for my aim related to the Carnot case study, it is no longer necessary to deal with it¹⁵. Just to mention "Ingenious reasoning, but wrong" like Marshall Clagett (1916–2005) claimed in his *The science of mechanics in the middle ages* (Clagett 1959, 76, line 18). In other words, Jordanus de Nemore – in *Elementa Jordani super demonstrationem ponderum* and partially also in *De ratione ponderis* – improperly applied the concept of *positional gravity* (*gravitas secundum situm*) when he reasoned upon displacements along a circumference (Capecchi and Pisano 2008; Pisano 2011).

¹⁴ As reported by Tartaglia in *Jordani opuscolvm ponderositate* (published *posthumous*, 1565): Tartaglia 1565b, pp. 3–5, p. 7. The figures should be read from left to right. 2abis is a simplified performance model of concept of *gravitas secundum situm* by Jordanus de Nemore. (Source: Public domain, Biblioteca Viganò, Brescia, Italy)

¹⁵ For an extensive account see Pisano and Capecchi 2015.

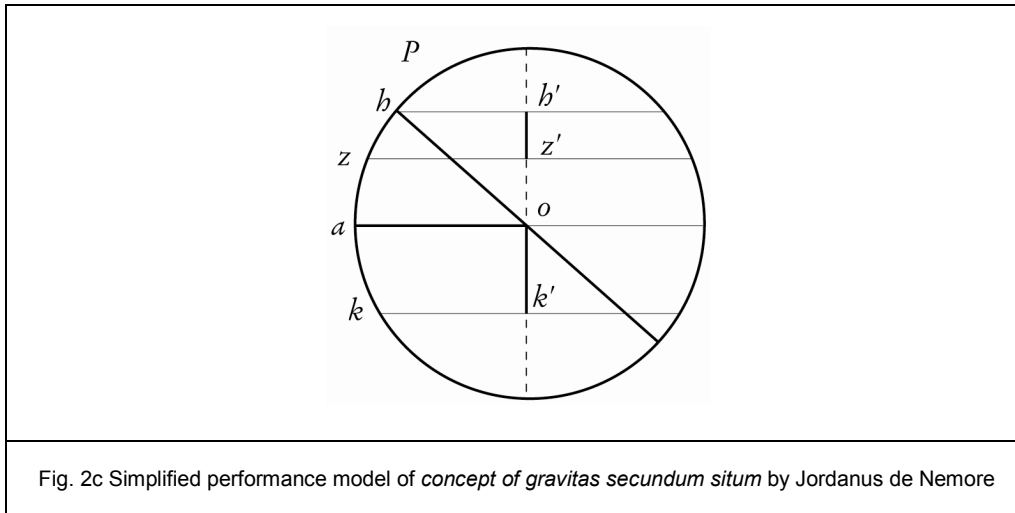


Fig. 2c Simplified performance model of concept of gravitas secundum situm by Jordanus de Nemore

The reasonings (Pisano 2007) exposed by Jordanus de Nemore in both *Liber de ratione ponderis* and in *Iordani Opvscvlvm de Ponderositate* (Tartaglia 1565b, 3–5, see also 7), and by Tartaglia in *Quesiti ed inventioni diverse* (Tartaglia 1554, pp 89–93) are very interesting. Let us look at some examples.

In the *Suppositio V*¹⁶ in *Iordani Opvscvlvm de Ponderositate* edited by Tartaglia (and in *Liber de ratione ponderis* as well) Jordanus (thus Tartaglia) reasons that a rectilinear segment intercepted along with the vertical component of the arc's virtual path suggests the *principle of virtual displacement*. Nevertheless, it is easy to verify that this reasoning is only valid if vertical segments are used. If one considers the entire diameter as the lever's beam and considers small arcs (ca. infinitesimal) along the circumference, then the difference of their vertical projections (at limit values) is zero. Therefore, the displacements become the circumference and mutually them. Therefore, vertical components are also equal. Let us examine some details of Tartaglia's (or de Nemore's) reasoning in *Quaestio sexta of Iordani Opvscvlvm de Ponderositate* (Tartaglia 1565b, 5) and in *Propositione V and VI of Quesiti ed inventioni diverse* (Tartaglia 1554, XXXII–XXXIII, Props V–VI, pp 89–91, XLI, Prop. XIII, 96–97). Tartaglia edited Jordanus de Nemore's reasoning on the equilibrium of a lever using principles of virtual work. Let there be a lever *ACB* (Fig. 3) having fulcrum *C* and, at the end of it, *A* and *B* are applied to two bodies *a* and *b* (Fig. 3). The following relation is therefore valid:

$$b : a = AC : BC \quad (1)$$

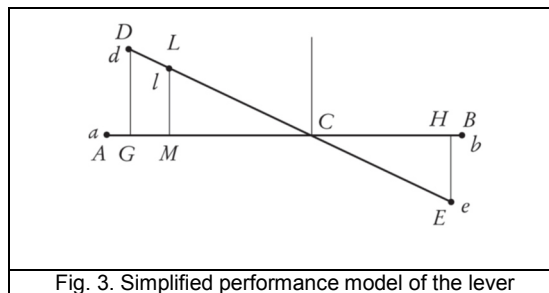


Fig. 3. Simplified performance model of the lever

Therefore, the lever is in an equilibrium state. From an epistemological point of view, with regard to Fig. 3, an *ad absurdum* proof is based on the following two main assumptions:

- 1) Based on common experience, in a physical lever system the masses (or force-weights) are inversely proportional to the length of the arms. Therefore, it assumes the following configuration: (a) equilibrium or (b) non-equilibrium. For situation (b) a necessary and logical

¹⁶ “Quinta: Obliquiorem autem descensum, in eadem quantitate minus capere de directo [A more oblique descent is one which, in the same distance, partakes less of the vertical]”. (Tartaglia 1565b, 3, line 12). See also *quarta suppositio*, “Quarta: Secundum situm grauius esse cuius in eodem situ minus obliquus descensus” [It is heavier in position when in that position its path of descent is less oblique]. (Tartaglia 1565b, 3, line 10).

condition follows: one of the two masses at the end of the lever produces an inclination of the lever toward its side.

- 2) If either of the masses descends, then it raises a mass equal to itself which is distant from the fulcrum, creating a displacement equal to that of its descent.

Let us describe, e.g., by *reductio ad absurdum*, equation (1) assumed in the above-cited configuration (b) at point 1): the lever is not in an equilibrium state. This is the *ad absurdum* hypothesis.

Therefore, in that condition of non-equilibrium, the lever should necessarily incline toward one of two sides. Let us suppose that it inclines toward *B* (Fig. 3). In this way, the physical system lever-masses gets the new configuration *DCE* (Fig. 3). If we consider the same masses of previous configuration *ACB*, ($d=a$ in *D* and $e=b$ in *E*), then it is possible to draw the perpendicular *DG* on *AC* in descent. Therefore, by the same method of drawing, the rise of the perpendicular *EH* on *CB* is possible. It is clear that both the descent and rise along the arcs of circumference

$$AB \quad \text{and} \quad DE ,$$

and rectilinear displacements

$$\overline{AB} \quad \text{and} \quad \overline{DE}$$

are considered¹⁷. In Fig. 3, due to the law of the similitude of triangles *DCG* and *ECH*, one can obtain:

$$DC : CE = DG : EH \quad (2)$$

Moreover, the following relation should also be valid:

$$DC : CE = b : a \quad (3)$$

Therefore

$$DG : EH = b : a \quad (4)$$

At this point, the author considers a point *L* on segment *DC*. The proof assumes an *ad hoc* procedure: point *L* is made to be symmetric to *E* with respect to fulcrum *C*. In *L*, a mass *l* equal to mass *b* is added. Here, it seems that Jordanus de Nemore would like to verify what happens to an imagined (virtual) motion of a certain mass in a certain point of beam during the previous configuration. In this sense, rise *e* until *B* corresponds to descent *l* until *M*. In other words, thanks to the geometric construction (and an *ad hoc* procedure) of the problem, one can obtain that $LM=HE$. Again, the law of the similitude of triangles establishes the following relation:

$$\frac{LM}{DG} = \frac{CL}{CD}$$

Therefore

$$DG : LM = b : a .$$

Since $l=b$, the following conclusion is obtained:

$$a : l = LM : DG$$

Therefore, masses *a* and *l* are inversely proportional to their vertical (opposite) displacements toward the elevated side. In effect, these displacements concern the reasoning upon the virtual rotation of the lever. At

¹⁷ Let us note that these segments are physically the vertical components of the displacement vector.

this point, Jordanus de Nemore¹⁸ reasons “Therefore, what suffices to lift a to D , will suffice to lift [mass] l through the distance LM ”¹⁹. Since $l=b$, and, also $LC=CE=CB$ equilibrium is obtained. In this reasoning, since b should not be (“sufficient”– ly) able to displace l of a quantity LM which is equal to the symmetric of b , then b , due to a logical consequence, should not even be (“sufficient”– ly) able to displace a of a quantity DG . Nevertheless, for (Eq. 1) and since it is not possible to presuppose it *a priori*, the lever cannot assume configuration DCE . It is evident that the proof is based on an Aristotelian approach, even though certain aspects differ. In fact, with respect to the Aristotelian approach, 1) which considers the displacements along circular arcs, de Nemore also reasons on *virtual* rectilinear paths; 2) in Supposition IV – “It is heavier in position when in that position its path of descent is less oblique”²⁰ – he considers the descent along an inclined plane. Thus, according to Giovanni Vailati²¹ (Vailati 1896–1897, 15; see also 1–25) Jordanus de Nemore deals with, albeit in an embryonic stage, the problem of the inclined plane (afterwards Tartaglia corrected some aspects). 3) The third instance follows. Initially, it seems that an *ad absurdum* proof was not necessary –it could have been adopted only for the sake of simplicity. That is to say, *virtual displacement* could be considered a difference of positions, or the mathematical displacements of a and of b and not as an effect of certain forces (at the time, the concept of force was not well physically and mathematically defined). In order to do so, a more explicit reasoning²² than Eq. (4) on the virtual displacement is necessary. By using the aforementioned quote: “Therefore, what suffices to lift a to D , will suffice to lift [mass] l through the distance LM (Tartaglia 1565b, 6, line 3). In effect, Eq. (4) effectively establishes a criterion of equivalence applied to the lever. In other words, the work by a certain Force–weights for a body a

$$F_p^a$$

and considering a certain displacement ($DG=rise$), is equal to work accomplished by the same Force–weights for a body b

$$F_p^b$$

to displace the same quantity ($LM=rise$), the body b symmetrically positioned to a with respect to the fulcrum. From a mathematical point of view (Fig. 1) and by considering virtual displacement extended to the principle of virtual work, one can obtain:

$$L^a = (F_p^a \cdot DG) = (F_p^b \cdot LM) \quad (5)$$

The two works, thanks to different orientations of the displacements, have opposite signs, and are equal if they are considered in absolute value. Therefore, the total work is null. In this sense, the third cited proof is in conflict with Aristotle’s claim that there is a reason to claim the equality in the Equation (5). In fact, the total work is null²³. Therefore, equilibrium is assumed. It should be noted that Tartaglia’s reasoning (Tartaglia 1554, 89–91) upon *gravitas secundum situm* in *Quesiti et inventioni diverse* is similar to Jordanus de Nemore’s reasonings in *lordani Opvscvlvm de Ponderositate*.

¹⁸ Let us remark that Tartaglia (Tartaglia 1554, Q. XXXII, Prop. V, 90–91) cited the non–exactness of the Aristotelian rule but he does not cite the previous correction made by Jordanus de Nemore.

¹⁹ “Quod ergo sufficit attollere a in D , sufficit attollere l secundum LM .” (Tartaglia 1565b, 6, line 3). In other words: “to displace a mass p to height h is equivalent to displacing a mass $q=p/k$ to height hk , whatever is k ”. This statement is, in some way, connected with the principle of the impossibility of perpetual motion.

²⁰ “Quarta: Secundum situm gravius esse quando in eodem situ minus obliquus est descensus” (Tartaglia 1565b, 3, line 10).

²¹ In this regard, Jordanus de Nemore – using some variants on the positional gravity – also applied (*Liber de ratione ponderis*) this concept to the angular lever, reasoning upon the equilibrium of an inclined plane.

²² This reasoning was criticized by Simon Stevin. The latter was opposed to it, since it was absurd to reason on the fact that a *situation of equilibrium* could be derived from a *situation of motion* (Capecchi and Pisano 2007, 2010; Pisano 2010b; Radelet de Grave 2007, 1996).

²³ On the development of engines, recently: Pisano and Bussotti 2015b.

Late Times

Explicit comments regarding the principle of virtual work are also reported by Galileo in *Le Mecaniche* (Galileo 1890–1909, II, 155–191) and in *Discorsi intorno alle cose che stanno in sù l'acqua* (Galileo 1890–1909, IV, 3–141). Particularly in this latter manuscript, Galileo clearly attributed the law of virtual velocity to Aristotle (Aristotle 1955 [1936], 847a 10–15, 847b 10, 329–332) also adding that the idea of the principle of virtual work was born thanks to the observation of the motion of points, which rotate along a circumference. Galileo also dealt with the law of virtual displacement in more than one situation²⁴.

Recent notable historical studies consider Isaac Newtonian's contributions (as a paradigm in history), as expounded in his 1687 masterpiece *Philosophiae naturalis principia mathematica* (Newton 1803 [1686–7]; Pisano and Bussotti 2016a,b), as the climax of classical science and mechanical scholars of the Enlightenment added little to it. The tendency today is to realize that this analysis is misleading and that this period, far from being a dark century, was filled with fundamental contributions which helped to establish the majority of mechanical concepts. On the other hand, the change in the role of practical mathematics in the understanding of the world (Henry 2011, 193–196; Westman 1980, Jardine 1988; Høyrup 1994) is also discussed (Capecchi and Pisano, 2010a, 2010b, 2007, 2008; Pisano and Capecchi 2013).

It is known that a great amount of Newtonian mechanics – under certain physical standpoints – only allowed the study of the motion of material points free in space with an incompletely developed mathematical apparatus which was based on calculus and methods by several scholars at that time, too. That is one of the main reasons for the emergence of the next commented editions (Bussotti and Pisano 2014a, 2015a). In fact:

The mathematization of many problems posed prematurely by Newton in the *Principia* became possible (e.g., in the moon theories of Euler, Clairault and d'Alembert developed in the 1750s) thanks to calculus which was neither Leibnizian nor Newtonian: it would be better to call it “Eulerian calculus.”²⁵

Furthermore, problems related to systems of constrained points remained unapproachable, as did the study of continuum bodies both rigid and deformable. Moreover, Newtonian science had to face, mainly on the Continent, people who were reluctant to follow his reasoning; Cartesianism was still dominant and the religious metaphysics behind his work were not well respected (Bussotti and Pisano 2013; Panza and Malet 2006). This situation promoted a profound innovation in dynamics as formulated by Newton. Newton's main concepts, that of force, remained dominant among scientists but their interpretation changed and a different approach based on work and energy became a serious contender. Competition was not based on a more or less appealing ontology but on a simpler or more complex mathematical formulation. Before beginning any considerations regarding XVIII century mechanics, it is essential to refer to Newton's own mechanics, which should be ideally located in the XVII century. Only in this way will it be possible to identify the distinctive features of Lagrange's account (Bussotti 2003) Euler, Carnot's (Gillispie and Pisano 2014) mathematics (Alvarez and Dhombres) and mechanics (Capecchi 2011, 2012). It will become clear that in what is referred today as Newtonian mechanics, only a few aspects can be recognised as Newton's proper mechanics, as defined in his *Philosophiae naturalis principia mathematica*. Newton assumed the following laws of mechanics which he referred to as laws, probably to stress that he considered them to be of an experimental nature (Newton 1803 [1686–7]):

DEFINITION III. *The vis insita, or innate force of matter is a power of resisting, by which every body, as much as in it lies, endeavours to preserve in its present state, whether it be of rest, or of moving uniformly forward in a right line.*²⁶

Axioms; or Laws of Motion. Law I. Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon; *Law II:* The

²⁴ Galileo 1890–1909 II, 240–242, IV, 68–69; VIII, 310–331, 329–330. See also: Pisano 2016, 2010a, 2009a,b,c; Pisano and Bussotti 2012, 2015a,b,c.

²⁵ Guicciardini 1999, 259. Author's italic.

²⁶ Newton 1803 [1686–7], I, 2; author's *italic* style and Capital letters.

alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed; *Law III*: To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.²⁷

These laws are quite familiar to a modern reader even though some particularities²⁸, both formal and substantial, do not go unnoticed, mainly for Law II. First, the famous formula $f = ma$, commonly known as Newton's second law, is differently proposed – with respect to today's formula. Mass is not named explicitly but is absorbed in the quantity of motion and in the end no reference is made to acceleration. Scrutiny shows that impressed force, apparently, the only element identifiable in the second law, cannot be identified with the modern concept of force. Indeed, integration of the law of motion, considered in the modern mathematical sense as $f = ma$, over a finite interval of time produces:

$$m\Delta v = \int f dt$$

where the first part is the variation of the quantity of motion, or according to Newtonian terminology, the alteration of motion. A comparison of the analytical expression just obtained with Law II of motion shows that what Newton calls force must be equal to

$$\int f dt$$

Newton chose the word *force* to indicate a founding quantity of dynamics, but he did not connect it to any of the concepts known as *force* today. Newton's concept of force is quite far from the concepts force introduced by previous scientists such as Descartes (Schuster 2000, 2013a, 2013b; Bussotti and Pisano 2013) and Torricelli (Capecchi and Pisano 2007; Pisano 2009b). This concept today is scarcely used and is not referred to by this name; the most common name for

$$\int f dt$$

that is the impulse of force f .

In *Scholium*, which followed the three laws of motion, Newton wrote about the force of gravity as an example of a force acting continuously. Stated *verbatim*:

When a body is falling, the uniform force of its gravity acting equally, impresses, in equally particle of time, equal forces upon that body, and therefore generates equal velocity; and in the whole time impresses a whole velocity proportional to the time.²⁹

That is, the total variation of velocity is proportional to the total force, which is proportional to time. In *Philosophiae naturalis principia mathematica* the whole force can also represent the intensity of an impulse and the action of continuum force, like gravity, for instance, usually described as a sequence of impulses, divided by a constant time step Δt , which approaches zero (as the sequence of impulses) goes to infinity. *Nevertheless, what kind of foundational and logical problems still remained unsolved?* Leaders of classical mechanics in the Enlightenment age, if we do not consider masters Newton and Leibniz, who were more part of the XVIII century, were Jean d'Alembert (1717–1783), Joseph Louis Lagrange (1736–1813), Leonhard Euler (1707–1783), Lazare Carnot (1753–1823), Pierre–Louis Moreau Maurice de Maupertuis (1698–1759), Jakob Bernoulli (1655–1705), Johann Bernoulli (1667–1748), Daniel Bernoulli (1700–1782),

²⁷ Newton 1803 [1686–7], I, 19-20; *Italic style* and capital letters belong to the author.

²⁸ For sake of brevity I remind discussion on Newtonian studies to my recent papers (Pisano and Bussotti 2016a,b; Pisano 2014, 2013a,b; Bussotti and Pisano 2013, 2014a,b). Particularly, I am working with (Paolo Bussotti) to a huge editorial project about rethinking the whole *Newtonian Geneva Edition* (4 vols.): critical translations from Latin to English and related commentaries for the Oxford University Press.

²⁹ Newton 1803 [1686–7], I, 22.

and Jakob Hermann (1678–1733). Each of them provided a contribution which marked the development of mechanical science. There are different, not completely distinct, points of view regarding the historical development of mechanics: the kind of mathematical approach, the kind and logical state of principles, the model of force. In the following section I will briefly present these different points of view.

At the beginning of the XVIII century Newton was surely seen as one of the most prominent mathematicians and physicists but not as the one who established dynamics in its final form. But it is known that Newton's mechanics were considered unsatisfactory by many scholars from both epistemological and ontological points of view, mostly because of his introduction of forces acting at a distance, which were considered occult entities. More fundamentally, for scientists, Newtonian mechanics was considered limited essentially to the motion of material points free in space, unsuitable for solving problems raised by the technology of the time. As an example of the opinions of the period, below are some comments by Daniel Bernoulli and Euler:

Theories for the oscillations of solid bodies that up to now authors furnished presuppose that into the bodies the single point position remains unchanged, so that they are moved by the same angular motion. But bodies suspended at flexible threads call for another theory. Nor it seems that to this purpose the principles commonly used in mechanics are sufficient, because clearly the mutual dispositions of points are continuously changing.³⁰

But as with all writings composed without analysis, and that mainly falls to be the lot of Mechanics, for the reader to be convinced of the very truth of these propositions offered, an examination of these propositions cannot be followed with sufficient clarity and distinction: thus as the same questions, if changed a little, cannot be resolved from what is given, unless one enquires using analysis, and these same propositions are explained by the analytical method. Thus, I always have the same trouble, when I might chance to glance through Newton's *Principia* or Hermann's *Phoronomiam*, that comes about in using these, that whenever the solutions of problems seem to be sufficiently well understood by me, that yet by making only a small change, I might not be able to solve the new problem using this method.³¹

Therefore, Newtonian laws alone were not sufficient for understanding all laws of motion as more fundamental mechanical laws were necessary. Generally speaking, from an epistemological point of view, the problems faced by XVIII century scientists were less demanding than those faced by Newton concerning the search for general laws, but this does not mean that they were any simpler. They concerned, for example, the search for the oscillation centre of a rigid body and the study of the vibrations of a chain or a thread. The search for the centre of oscillation was quite a relevant and difficult problem. It was equivalent to finding the length of a simple pendulum within the same period. The problem was substantially solved by Christian Huygens (1629–1695) in his *Horologium Oscillatorium sive De motu pendulorum ad horologia aptato demonstrationes geometricae* (Huygens 1673) by means of the first formulation of the theorem of living forces (*vis viva*). Jakob Bernoulli revisited the subject in 1713, with a completely different and promising approach in his paper *Démonstration générale du centre de balancement et d'oscillation, tirée de la nature du levier* (Bernoulli Jakob 1703). His study addresses the roots of both D'Alembert's principle and the angular momentum equation. Johann Bernoulli also faced these problems beginning at the end of the XVII century and published his considerations in *Opera Omnia* (Bernoulli Johann 1742a; 1742b; see also Radelet de-Grave 2009) where he first introduced the concept of angular acceleration, a very relevant idea. Scientists such as Euler, D'Alembert Johann and Daniel Bernoulli studied the problem of the vibrating chain. Among the problems that busied the minds of XVIII century scientists was the study of the motion of bodies on mobile surfaces, such as the motion of a heavy body upon an inclined plane which moves on a plane without friction. Johann Bernoulli (Bernoulli Johann 1742b) studied the motion of a material point from a Newtonian approach by introducing constraint reactions within external forces, referring to them as immaterial forces, since they were outside the bodies in contact. It should be noted that the assimilation of constraint reactions to ordinary forces was quite common in statics, but in dynamics the problem was much more complex conceptually because reactions were endowed with activity. Euler, who developed principles of mechanics which facilitated the introduction of constraint reactions, tried to avoid their explicit use as

³⁰ Bernoulli D 1733, 108.

³¹ Euler 1736, *Praefatio*, (Author's *italic* style and Capital letters). See also: Euler 1774 [1773]; 1749; 1752 [1750].

much as possible. To conclude, in the solution of the various problems, a unique principle was not referred to and it was sought in analogies with problems which had already been solved.

In the mid-eighteenth century, thanks to the brilliant minds of Euler and Johann Bernoulli, some general principles confirmed the living forces theorem and the minimum action principle. On the living forces theorem after Huygens and Leibniz, were works by Johann and Daniel Bernoulli, D'Alembert and notably Lagrange.³² The living forces theorem was only correct for problems limited to one degree of freedom, because it gave only a scalar equation. More interesting but equally incomplete was the minimum action principle. It can be attributed to Pierre de Fermat (1601–1665) who in 1662, in his studies on the refraction of light, wrote a letter addressed to de La Chambre (“Sunday, January 1, 1662”) and drew on a theological and moral principle, namely, “[...] nature *always acts along the shortest paths* [...]”³³ (Fermat 1891–1922, vol II, CXII [D., III, 5r.] 458). However, Maupertuis gave it the name and extended it to mechanics in various steps. The final step was referred to in his paper *Les lois du mouvement et du repos déduits d'un principe métaphysique* of 1746:

Because of so many people who worked about I hardly dare to say I have discovered the principle on which all these laws are based: which extends also to elastic Bodies, from which the movement of all physical corporeal depends.³⁴

That is the principle of *least quantity of action* so wise, so worthy of the Supreme Being, et to which Nature looks so constantly joined, that it observes it not only in all its changes, but also in its permanence. *In the impact of Bodies, the Motion distributes so that the quantity of action, which derives from the change, is the least possible.*³⁵

178

Euler tried to give the least action principle a more precise formulation, however did not reach a general formulation, leaving to metaphysics the decision of whether or not it could be assumed as a general principle or if it could be determined within the laws of mechanics. It was Lagrange in *Application de la méthode exposée dans le mémoire précédent à la solution des différents problèmes de dynamique* (Lagrange 1870–1873 [1762], I, 363–468) who provided a proof of the least action principle based only upon the laws of mechanics, without any metaphysical traces. Despite some important successes in the solution of various problems and the existence of some general principles, a general feeling of disappointment prevailed among the scientists of this period which gave rise to an effort to find simpler and more general principles. This effort began to be fruitful in the second half of the XVIII century and was led by Euler and Lagrange to a nearly finished form, respectively, of *vectorial* and *analytical* mechanics.

Lagrange's results on his studies of the least action principle were published in *Application de la méthode exposée dans le mémoire précédent à la solution des différents problèmes de dynamique*, and represented the perfecting of its formulation and his convincing proof. The use of the principle, however, is possible only for what should be referred to today as conservative systems. In such a case, by the addition of the living force theorem, any (discrete) dynamical problem could be solved. However, even before *Application de la méthode exposée dans le mémoire précédente à la solution des différents problèmes de dynamique*, Lagrange thought of a principle which was more general than the least action principle. In a letter to Euler on November 24th 1759, Lagrange³⁶ wrote about having composed elements of differential calculus and mechanics to develop the true metaphysics of his principle. In *Recherches sur la libration de*

³² Johann Bernoulli, *Theoremata selecta pro conservatione virium vivarum demonstranda et experientia confirmanda* (Bernoulli Johann 1727), Daniel Bernoulli, *Remarques sur le principe de la conservation des forces vives pris dans un sens général* (Bernoulli D 1750 [1748]). D'Alembert's *Traité de dynamique* (D'Alembert 1758 [1743]; see also Hankins 1970; D'Alembert 1751–1780; 1767) and Lagrange's *Recherches sur la libration de la Lune* (Lagrange 1764). On that and a development of mechanics between 17th and 18th century is previously published (Pisano and Capecchi 2013, 97–121). On the history of mechanics (Truesdell 1968a,b) and also history of thermodynamics (Truesdell 1970, 1976, 1980) Truesdell's works (sometime considered by someone *surpassed*) are – in any case – important examples of historical criticism and reflections.


³³ Author's *italic* style.

³⁴ Maupertuis 1746, 286. (Author's *italics* style).

³⁵ *Ibidem*.

³⁶ Euler's Correspondence with Joseph Louis de Lagrange. In: *Opera Omnia*. Series IV, Vol I, retrieved via: <http://eulerarchive.maa.org/correspondence/correspondents/Lagrange.html> *Idem* letter was previously edited in Lagrange 1892, Tome XIV, pp 170–174.

la Lune (Lagrange 1764), for the first time, Lagrange obtained dynamical equations of motion with the aid of a new law of Mechanics, the principle of virtual work:

<p style="text-align: center;">1764</p>  <p style="text-align: center;">RECHERCHES SUR LA LIBRATION DE LA LUNE, Dans lesquelles on tâche de résoudre la Question proposée par l'Académie Royale des Sciences, pour le Prix de l'année 1764.</p> <p style="text-align: center;">I.</p> <p>ET écrit a pour objet d'examiner les différens mouvements apparens, ou réels que la Lune peut avoir autour de son centre. Je suppose d'abord que cette Planette à une figure quel- conque; & je cherche le mouvement qu'elle doit recevoir de l'action de la terre & du soleil. Quoi qu'un très- grand Geomètre ait déjà donné des méthodes & des formules générales, qui peuvent aisément s'appliquer à la recherche dont il s'agit ici, néanmoins il m'a paru plus commode de reprendre la question en entier, & de</p> <p style="text-align: right;">A</p> <p style="text-align: right;">Prix de l'Acad. Tome LX.</p>	<p>III.</p> <p>“There is a principle generally true in Statics, according to which, if a system of whichever bodies or points, each of them subjected to powers [forces], is in equilibrium and if somebody gives the system a small arbitrary motion, for which each point covers an infinitesimal space, the sum of the powers multiplied each of them for the space covered by the point to which it is applied, in the direction of this power, will be always = 0 [zero].³⁷</p> <p>[In the same article “III” he used the following formulas:]</p> <p>Dans la question présente, si on imagine que les lignes X, Y, Z, R, R', deviennent, en variant infiniment peu la position de la Lune autour de son centre, $X + \delta X, Y + \delta Y, Z + \delta Z, R + \delta R, R' + \delta R'$, il est facile de voir que les différences $\delta X, \delta Y, \delta Z, \delta R, \delta R'$, exprimeront les espaces parcourus en même tems par le point a dans des directions opposées à celles des puissances $\alpha \frac{d^2 X}{dt^2}, \alpha \frac{d^2 Y}{dt^2}, \alpha \frac{d^2 Z}{dt^2}, \alpha \frac{T}{R^2}, \alpha \frac{S}{R'^2}$, qui sont sensées agir sur ce point; on aura donc, pour les conditions de l'équilibre, l'équation générale.</p> $\int \left(\alpha \frac{d^2 X}{dt^2} X - \delta X + \alpha \frac{d^2 Y}{dt^2} Y - \delta Y + \alpha \frac{d^2 Z}{dt^2} Z - \delta Z + \alpha \frac{T}{R^2} X - \delta R + \alpha \frac{S}{R'^2} X - \delta R' \right) = 0.$ $\left[\sum f_i \cdot \delta u_i = 0 \right]$
<p>Fig. 4 Frontispiece of the <i>Recherches sur la libration de la Lune</i> and his formulation³⁸</p>	

The progress of the Lagrangian formulation (Pisano 2014, 2013a) of the principle over that of Johann Bernoulli in 1715 was significant in many ways. Lagrange’s formulation was stated in a clearer way for a system of bodies, because only virtual displacements congruent with constraints were used, and also because the principle was embedded in the newly established *variational* calculus. At any rate, the principle

³⁷ Lagrange 1764, 5 (Author’s Capital letters). See also Lagrange’s arguments on the same topic in the *Théorie de la libration de la lune*: “[...] 1. Le principe donné par M. d’Alembert réduit les lois de la Dynamique à celles de la Statique; mais la recherche de ces dernières lois par les principes ordinaires de l’équilibre du levier, ou de la composition des forces, est souvent longue et pénible. Heureusement il y a un autre principe de Statique plus général, et qui a surtout l’avantage de pouvoir être représenté par une équation analytique, laquelle renferme seule les conditions nécessaires pour l’équilibre d’un système quelconque de puissances. Tel est le principe connu sous la dénomination de *loi des vitesses virtuelles*; on l’énonce ordinairement ainsi : *Quand des puissances se font équilibre, les vitesses des points où elles sont appliquées, estimées suivant la direction de ces puissances, sont en raison inverse de ces mêmes puissances.* Mais ce. principe peut être rendu très-général de la manière suivante.” (Lagrange 1870–3, V, 15; Author’s *italics* style and Capital letters; see also *idem* arguments in Lagrange 1764, 8).

³⁸ Lagrange 1764. Nowadays Lagrange’s equations are called *Symbolic Equation of Dynamics*. In other passages Lagrange try to remark and generalize his conclusions in a note: “IV. Scholie. Le principe de Statique que je viens d’exposer n’est, dans le fond qu’une généralisation de celui qu’on nomme communément le principe des vitesses virtuelles, & qui est reconnu depuis longtem[p]s par les Géomètres pour le principe fondamental de l’équilibre. M. Jean Bernouilli est le premier, que je sache, qui ait envisagé ce principe sous un point de vue général & applicable à toutes les questions de Statique, comme on le peut voir dans la Section IX. de la nouvelle Mécanique de M. Varignon, où cet habile Géomètre, après avoir rapporté, d’après M. Bernouilli, le principe dont il s’agit, fait voir par différentes applications, qu’il conduit aux mêmes conclusions que celui de la composition des forces”. (Lagrange 1764, 6. (Author’s Capital letters)). Source: Google Books – Public domain.

of virtual work alone was not sufficient for founding dynamics. It had to be associated with another principle of dynamics, thanks to D'Alembert. The interpretation of D'Alembert's principle as provided by Lagrange has become classic, although it has little to do with the original interpretation (Fraser 1983): accelerating forces (ma), with their signs reversed, balance applied forces.

[IV ...] the Principle of Statics I am introducing, combined with the Principle of Dynamics due to M. D'Alembert gives a kind of general formula which contains the solution of all problems relative to the motion of bodies.³⁹

In other terms:

$$\sum f_i \cdot \delta u_i - \sum m_i a_i \cdot \delta u_i = 0$$

The principle of virtual work (also known as the principle of least action) has the peculiarity of being expressed by a unique symbolic equation. This is a *variational* equation and can be stated without paying particular attention to the choice of reference systems. Since this is quite automatic, it is possible to avoid solving most of the necessary geometrical problems that arise when writing the equation of motion in the Eulerian style. It should be noted that the virtual work equation allows for an easy solution to the problem of constraints: it is sufficient to use virtual displacements congruent with them. In this way, a static as well as a dynamic problem are solved by means of a kinematic study. This still implies the need for some geometric considerations. Lagrange avoided these with a typical trick in his calculus of variations: the virtual displacements were considered free from any constraints that were added in their analytical form to the *variational* equations. For instance if $f(u_1, u_2, \dots, u_n) = 0$ is a constraint equation, the *variational* problem has the form:

$$\delta W(u_1, u_2, \dots, u_n) + \lambda \delta f(u_1, u_2, \dots, u_n) = 0$$

where W is the virtual work and it is a function of u_1, u_2, \dots, u_n , now called Lagrangian multipliers, to which the meaning of constraint reactions can be associated.

Lagrange perfected his approach in *Mécanique analytique*⁴⁰ (Lagrange 1788), where in the introduction, he emphasized the absence of any geometric considerations.

One will find no figure in this work. The methods I will expose do not require neither constructions nor reasonings of mechanical or geometrical nature, but only algebraic operations which develop regularly and uniformly.⁴¹

In what follows, having present the constraint equations among the coordinates of various bodies, which are given by the nature of bodies, the variation of these variables will be reduced to the smallest number, so that the resulting variations are completely independent each other and absolutely arbitrary. We then will equate to zero the summation of all terms concerned with these variations; and we will have all the equations necessary to find the motion of the system.⁴²

As a final comment it must be noted that although the Lagrangian principle of virtual work is usually associated with the first edition of *Mécanique analytique* (1788), its elements were provided completely in *Recherches sur la libration de la Lune*. The other question is that of the principle justification. To this end, the two *Mécanique* editions (1787, 1811) tried to offer a satisfactory proof that was considered too weak by many scientists of the time, as will be shown further ahead. Evangelista Torricelli (1608–1647), in his *Opera geometrica* (Torricelli 1644) claimed to have established a rational criterion for equilibrium, playing a fundamental role in mechanics and in the history of mechanics (Capecchi and Pisano 2007). It can undoubtedly be considered the origin of the modern statement of the principle of virtual work.

³⁹ Lagrange 1764, 8. (Author's Capital letters).

⁴⁰ This is the original title of the 1788 edition. Next editions (1815, 1853) refers to *Mécanique analytique*.

⁴¹ Lagrange 1788, vi.

⁴² Lagrange 1788, 197.

Two heavy bodies linked together cannot move by themselves unless their common centre of gravity does not descend.⁴³

With regard to Torricelli's principle, one can also consider John Wallis (Wallis 1693) and Pierre Varignon's (1654–1722) (Varignon 1725) assumptions, which allowed rigorous mathematical physics formulation. This was aimed at founding all statics upon an easy geometric principle: the composition of forces. In this sense, it is also alternative to the *principle of virtual work*. Let us note that in his letter to Johann Bernoulli (1667–1748), Varignon also dealt with the concept of *virtual velocities*, as components of *virtual infinitesimal displacements* towards the direction of the forces (Bernoulli J 1742, II). After Bernoulli, the most significant contribution to the development of the *principle of virtual work* is probably thanks to Vincenzo Riccati (1707–1775) who tried to establish it based on simple principles easily accepted by his contemporaries, introducing *Principles of actions* in *Dialogo di Vincenzo Riccati della compagnia di Gesù* (Riccati 1749) and in *De' principi della meccanica* (Riccati 1772).

The Relationships between Physics and Mathematics in Lazare Carnot's Mechanics

Lazare Carnot's mechanics is an operative type of mechanics (Gillispie and Pisano 2014) and presents a strong attitude to Leibniz's ideas,⁴⁴ *theoretical physics must explain facts with facts*. The mathematics introduced is that which is absolutely necessary, adapted to represent a previously established physical argument. The theory is independent from the (physical and mathematical) concept of absolute space, typical of previous predictive mechanics (see Table 1). For example, in Lazare Carnot's works⁴⁵, the solutions for the equations of motion are *velocity* and *quantity of motion* (Gillispie and Pisano 2014, chaps 2–4, 11). The concept of space for Carnot is concerned with the finite volume of a system. Time is also different from typical Newtonian time. For example, for Carnot, time is not absolute and does not have continual variations. It has only one dualistic variation: before and after. In this regard, it can be noted that by avoiding the use of Newtonian absolute time and space, Carnot's science also omits the use of physical quantities as non-finite mathematical variables, which in common theoretical physics are fundamental for the infinitesimal calculation of the variations of certain physical quantities. Therefore, from the very beginning their theories did not contain abstract notions such as absolute space or force-cause. Lazare Carnot's mechanical theory was limited to algebraic and trigonometric equations (because in this theory the types of equations of the invariants of motion are to be solved with velocity only). Lazare Carnot made this attitude clear in his second book, *Principes fondamentaux de l'équilibre et du mouvement* (1803a) when he stated that all scientific (and mathematical) notions can only come from experiments.

⁴³ "Praemittimus. Duo gravia simul coniuncta ex se moveri non-possent, nisi centrum commune gravitatis ipsorum discenda. (Torricelli 1644, Liber primus De motu gravium naturaliter descendendum, 99, line 4". Author's italics). [English translation is mine]; Pisano and Capecchi 2010; Capecchi and Pisano 2010, 2009c; Pisano and Capecchi 2007).

⁴⁴ In particular, one can see the concept of collision (adopted by Lazare Carnot) presented by Leibniz in his *Dynamica de Potentia et Legibus Naturae Corporeae* (Leibniz 1849–1863, II, sectio III, proposition 1–18, pp 488–507) and the early concept of potential energy (*Ivi*, II, sectio I, 435). E.g., Lazare Carnot introduced an advancement of potential energy in his theory of motion applied to machines (Carnot L 1803a, pp 36–38). On the Leibnizian background in Lazare Carnot, one can also see the famous correspondences in 1677 (*Ivi*, VI, 81–106) between Leibniz and Honoré Fabri (also Honoratus Fabrius, 1607–1688). For a first panoramic view on Leibniz and his dynamics, see Pierre Costabel's (1912–1989) works (Costabel 1960). For the most complete (works and letters) series of Leibniz's mathematical writings, see Eberhard Knobloch's VIII edition for "Berlin-Brandenburgische Akademie der Wissenschaften Leibniz-Edition, Reihe VIII" (Leibniz 2009–), Bussotti 2015. Particularly on Leibniz, in the occasion of his anniversary, see also *The Dialogue between Sciences, Philosophy and Engineering. New Historical and Epistemological insights. Homage to Gottfried W. Leibniz 1646-1716* (Pisano, Fichant, Bussotti and Oliveira 2017; Bussotti and Pisano 2017).

⁴⁵ I remark the general scientific and filial project assumed by Lazare and Sadi Carnot (Pisano 2010) on mechanical and heat machines theories (Pisano 2012). As above mentioned, I refer and use recent own material around Lazare Carnot's science (Gillispie and Pisano 2014) under Springer permission as above and below reported.

Table 1. Carnot's physical–mathematical–geometrical approach

Main concepts	Lazare Carnot (1786; 1803a; 1813)
Space and time	No absolute and infinite Newtonian space and time
Physics–mathematics relationship	Only physical
Geometry–physics relationship	Independence from position in space
Basic concepts	Velocity, Quantity of motion, limited mechanical Work
Mathematical approach	No local and infinitesimal variables
Main mathematical-geometrical technique	Geometric motion

To be precise, in Lazare Carnot's words:

Following this idea ["to avoid metaphysical notion of force" and... to use "the theory of communications of motions"⁴⁶] we will soon see, as I previously mentioned, the necessity of turning to the experiment, and that is what I did, without neglecting to support myself with reasonings that can confirm it in the most plausible way, using or generalizing the results per induction. At times I even used the name of the force in the vague sense of which I spoke above [...].⁴⁷

[...] *Primitive ideas concerning the matter, the space, the time, the rest, the motion, etc.* 7. The first rule to establish in such delicate research on the laws of nature is to only admit notions so clear that they can comprise the bounds of our logic. We must therefore reject the definitions of *matter*, *time*, *space*, *rest*, and *motion* as expressions that are impossible to express with more clear terms, and the ideas that these expressions produce in us primitive ideas outside of which it is impossible to construct. But once these expressions are admitted, we will easily see that which is a body, speed, a motive force, etc. 8. The body is a given part of matter. 9. The apparent space that a body occupies is called its *volume*; the actual space that this same body occupies, or its real quantity of matter, is called its *mass*. When the body is such that equal parts of its volume always correspond to equal parts of its mass, we say that it has a uniform *density*, or that it is equally *dense* in all of its parts; and the relationship from mass to volume, or the quotient of one times the other, is called the *density* of this body. But if unequal masses correspond to equal volumes, we say that the density is variable and for each particle of matter, we call *density* the volume of this particle divided by its mass, or rather, the last reason of these two quantities. The empty parts or gaps lodged between the parts of the matter, and that make the volume or apparent space greater than the actual space are called *pores*.⁴⁸

[On the concept of force in the theory]. [...] in my opinion, no rigorous proof of the parallelogram of forces is possible: the mere existence of the *force* in the announcement of the proposition is able to make this demonstration impossible for the nature of things in itself. "It is extremely difficult", as Euler said, "to reason on primary principles of our knowledge [...]". This obscurity disappears in the second way [theory of motion] to conceive the mechanics, but another inconvenience appears; that is the fundamental principles that in the first way [theory of forces where cause produces motion] are established such as axioms in favor of the metaphysical expression [...] that is to say, [...] force, are, in this second case [theory of motion], nothing less than self-evident propositions, and in order to establish them, we need to include the recourse to the experience.⁴⁹

In the Newtonian paradigm until Laplace's physics (Fox 1974; see also Pisano and Gaudiello 2009) and generally speaking for mathematical physics, one can see a strong use of geometry and mathematics (e.g., differential equations). Thus, a mathematical result is obtained.⁵⁰ *But, what about from physical point of view*⁵¹?

⁴⁶ Carnot L 1803a, XVI, line 5.

⁴⁷ Carnot L 1803a, XVI, line 10.

⁴⁸ Carnot L 1803a, 6–7, line 1. (Author's *italics*).

⁴⁹ Carnot L 1803a, xii–xiv, line 17.

⁵⁰ In the 1816 Laplace pointed out that the speed of sound in air depended on the heat capacity ratio and corrected Newton's surprising error (Biot 1858, 1–9, 1802, 173–182).

⁵¹ E.g., the second Newtonian law is not a strictly physical equation. It is – in modern term – a second order differential equation that would interpret (physically) the law of motion. It does so by a mathematical–physical equation, which, of

Newton gave, in the second book of his *Mathematical Principles of natural Philosophy* the expression of the speed of sound: how he achieves this is one of the most remarkable features of his genius.⁵²

When the temperature of the air is raised, at constant pressure, only part of the heat is used to produce that effect [to raise the temperature]: the other part, which becomes latent, serves to increase its volume. This latter part of the heat is liberated when the air is reduced to its primitive volume by an increase in pressure. When two air molecules come close together in a vibration, the heat released raises their temperature and tends to radiate out into the nearby area; but if this happens very slowly relative to the speed of vibration, we can suppose that the amount of heat remains the same [for the two molecules]. Thus, as the two molecules approach, they meet a resistant force, first, because since their temperature being supposed constant, their [forces of] repulsions augment in inverse proportion to their distances; and second because the latent caloric which develops increase their temperature. Newton only considered the first of these causes of repulsion; but it is clear that the second cause must increase the speed of sound, since it increases the pressure. By entering it in the calculation, I come to the following theorem: "The real speed of sound is equal to the product of the Newtonian formula times the square root of the ratio of the specific heat of air at constant pressure of the atmosphere and at different temperatures, to its specific heat at constant volume".⁵³

Newton's calculation gave 968 (920–1085) English feet per second (Newton 1803 [1686-7], 371–372), which is ca. 20% shorter than the value of the speed of sound and later 979 English feet per second appeared (Newton 1714, 343–344). It may have been convenient for the experimental data of the time but it was undoubtedly too low of a value⁵⁴. In effect, the adiabatic compression of the air, which results in a local rise in temperature and pressure, was also taken into account.

Laplace's investigations in practical physics were confined to those carried out jointly with Lavoisier on the specific heat of various bodies from 1782 to 1784. It should also be noted that this is a similar technique to the one Émile Clapeyron would use in 1834 to reformulate Sadi Carnot's theory, but he would not succeed in doing so with his theorem (Clapeyron 1834, 153–190). Lazare Carnot considered the mathematical technique with the differential to be inaccurate but he did not believe in caloric (Carnot L 1990). In fact, he considered infinitesimal analysis (Gillispie 1971, ft. 1, 12, 1979, pp 251–298, § 13, 256) to be a very clear mathematical apparatus in and of itself, which varies with continuity by means of concrete variables. However, for differentiated variables in the previous technique, the mathematical problem is the opposite: the aim is to determine function Q by using an abstract calculation. Therefore, as Lazare Carnot explains in a footnote in *Principes fondamentaux de l'équilibre et du mouvement* (Carnot L 1803a, 11, ft. 1), infinitesimal analysis is not suitable in these cases. A different type of mathematics is necessary in which geometry acquires a greater importance.⁵⁵ Lazare Carnot's mathematics select geometric motions, which by definition, admit their opposites.

Lazare Carnot⁵⁶ is usually considered to be the foremost author who claimed that the empirical nature of mechanics was both theoretical and mechanical (Gillispie and Pisano 2014). Lazare Carnot expressed his view of mechanics in the introductory parts of *Essai sur les machines en général* (Carnot L 1786) and *Principes généraux de l'équilibre et du mouvement* (Carnot L 1803a). This is what Carnot wrote in his *Essai sur les machines en general*:

course, one cannot establish experimentally, as instead can be done by dynamometer to measure magnitudes in a static equation, e.g., Hooke's law (Pisano 2007, 2009a, 2009b).

⁵² Laplace 1816, 238, line 7; author's *italics* and Capital letters.

⁵³ Laplace 1816, pp 238–239, line 24. (Author's quotation marks).

⁵⁴ Finn 1964, ft. 19, 8; Newton 1999, pp 772–778.

⁵⁵ On mathematical and geometrical standpoints see: Chemla 1990, 1998; Nabonnand 2010, 2011.

⁵⁶ For the biography see: Gillispie 1970–1980 III, 70–79. On Lazare Carnot's mechanics see the fundamental Gillispie's works: Gillispie 1971, 1976, Gillispie and Youschkevitch 1979.

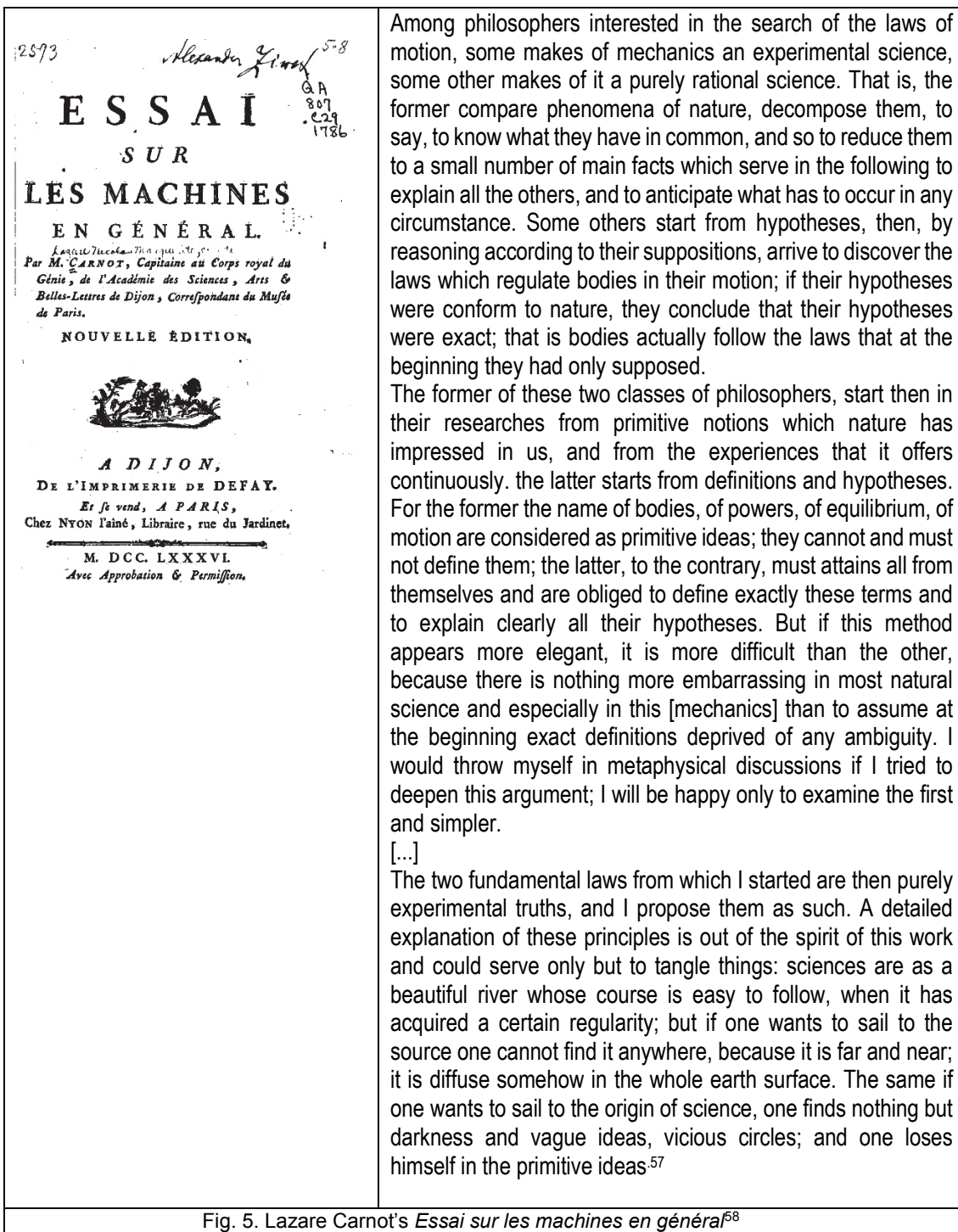


Fig. 5. Lazare Carnot's *Essai sur les machines en général*⁵⁸

In the first part (Carnot L 1786, 104–107) Carnot declared his preference for the analytic approach and in the second part he declared the two principles assumed in *Essai sur les machines en général* (the action and reaction and the conservation of *momenta* in the impact) empirical laws. In the other work, for the introduction of *Principes généraux de l'équilibre et du mouvement* he reasserted his empiric idea:

Ancients established as an axiom that *all our ideas come from senses*; and this is no longer object of dispute [...].⁵⁹

⁵⁷ Carnot L 1786, 104–107.

⁵⁸ Carnot L 1786. Source: Google Books – Public domain.

⁵⁹ Carnot L 1803a, 2. Author's *italics*.

But he also expressed the opinion that the laws of mechanics can be considered as either empirical or fully rational:

3. This notwithstanding sciences do not derive in the same way their basis from experience. Pure mathematics derive from them less than all the others; then mathematical physic sciences, then physical sciences. [...].

4. It would be certainly satisfactory, in each science, to be able to decide the point where it breaks off to be experimental and becomes rational; that is to reduce as much as possible the number of possible truths we must obtain from the experience and when accepted are sufficient with the sole reasoning to follows all the branches of the science. But this seems to be too difficult. If one wants to go up too much one will venture to give dark definitions ad vague and scarcely clear proofs. There are less drawbacks to obtain from the experience more items of information than those strictly necessary. [...].

It is thus from experience that men derived the first notions of mechanics. This notwithstanding the fundamental laws of equilibrium and motion [...] appears from one hand so natural to reason, and from the other hand they manifest themselves so clearly by means of the most common facts, that it seems difficult to say that is from one instead than from the other that we derive the complete conviction of these laws.⁶⁰

Carnot specified the role analysis plays in the establishment of these laws, which are referred to as hypotheses:

72. Now it has to establish upon given facts, and upon other observations which we still could have, *some hypotheses* [italic is ours] which are constantly in accord with these observations and which we can assumes as general laws of nature.⁶¹

It is not necessary to have concerned phenomena hypotheses which are unrelated to each other:

73. My objective is not to reduce them [the hypotheses] to the smallest number; it is enough for me that they were consistent and clear enough [...] but they are the most suitable to confirm the principle [the experimental facts], by showing that they are, as to say, nothing but the same truth which says all the same under different forms.⁶²

Carnot assumed seven hypotheses which are summarized in the following Table 2.

Table 2. Some of Carnot's hypotheses (Carnot 1803a)⁶³

1	Once at rest a body cannot move by itself and once put in motion it cannot change neither its velocity nor its direction by itself (<i>Ivi</i> , 49).
3	When many forces, either passive or active, equilibrate themselves, each of this force is always equal and opposite to the resultant of all the others (<i>Ivi</i> , 49).
5	The action that two bodies contiguous exerts each other by impact, pressure or tension, does not depend in any way by their absolute velocity, but only by their relative velocity (<i>Ivi</i> , 49).
7	When bodies who impact are perfectly hard or perfectly soft, the proceed always together after the impact; that is according to the straight line of their mutual action [...] (<i>Ivi</i> , 50).

In modern didactic presentations of classical mechanics, force is considered as varying continuously. This was not the case in the XVIII century where the impact of bodies was also considered very important. The relevance of impact was mainly due to the dominant atomistic conception of matter where impact among atoms was the only way to transmit forces. According to Newton's *Opticks* (Newton 1730) the ultimate

⁶⁰ Carnot L 1803a, 3–5.

⁶¹ Carnot L 1803a, 46–47.

⁶² Carnot L 1803a, 47.

⁶³ Adapted from Pisano and Capecchi 2013, 113.

constituent of matter was small bodies not completely deformable or hard. Due to this property, it was natural to argue on a rational basis that when two hard bodies of equal mass and opposite velocity collide they cannot help but stop. When they touch, they must stop because of their impenetrability and they then remain at rest because there is no reason for a rebound. Some scientists however did not accept the hard body model, and among them were Johann Bernoulli and Euler who agreed with Leibniz (Bussotti 2014, *forthcoming*). In particular Euler divided bodies in more or less soft (*mollioribus corporibus ut cera vel argilla*) and elastic (*elastica*) (Euler 1738 [1730–1], 161). Independently of its foundation any formulation of mechanics had to address and explain global phenomena where variation of force and motion occurs either continuously or through an impact.

On one hand, Euler, who decided to base mechanics on force acting continuously, considered impact as a continuous process: when two bodies collide, they are deformed and exchange forces which continuously vary in time, even if this happens in a very short time interval. The knowledge of the way the matter warps allows for the discovery of the law of (continuous) force the bodies exchange and for the study of the effect of the impact with the law of a mechanics where force is a continuous quantity, even when bodies are extended and not simple material points. On the other hand, D'Alembert and especially Lazare Carnot, who based their mechanics on impact, considered the continuous variation of force to be due to a sequence of infinitesimal impacts.

On Lazare Carnot's Concept of Work

There are two ways to deduce mechanics from its principle. The first is to consider it as the theory of forces, that is the causes which impress motion. The second is to consider mechanics as the theory of motion in itself.⁶⁴

Lazare Carnot preferred the second way. However, he was not against the term *force* which he used quite often, sometimes with a technical meaning: “[...] will call moment of activity, consummated by this force in a given time, the sum of moments of activity consummated by it at every instant [...]”⁶⁵, sometimes following common sense, others even intending the meaning of work. Carnot maintained that as far as the motion of a machine is concerned, force is not the most important concept because the effect it produces also depends on the way it is applied. Carnot used the concept of work to take this way into account (Gillispie and Pisano 2014). He was not the first to do this, but he was the first to emphasize it and give it an operational meaning as a foundation of mechanics, especially for applied mechanics (Gillispie 1976, 1979). The term he used to indicate work was *moment of activity*:

XXXII. If a force P moves with a velocity u and the angle formed by P and u is z , the quantity $P \cos z \, u \, dt$, where dt is the element of time, is called *moment of activity* consumed by force P during dt .⁶⁶

The total moment of activity during a finite interval of time T is given by⁶⁷:

$$P \int u \, dt \, \cos z$$

Lazare Carnot could quite easily formulate, as a corollary, a fundamental result of his mechanics: the conservation of work:

*Corollary V. Particular law concerning the Machines whose motion changes by imperceptible degrees. X LI. In a Machine whose motion changes by imperceptible degrees, the moment of activity in a time given by soliciting forces, is equal to the moment of activity, exerted at the same time by resistant forces.*⁶⁸

⁶⁴ Carnot L 1803a, xj.

⁶⁵ Carnot L 1786, 65–66. Author's *italics*.

⁶⁶ Carnot L 1786, 65–66. Author's *italics*. See also: *Ivi*, pp 96–97.

⁶⁷ Carnot L 1786, 66.

⁶⁸ *Ivi*, pp 75–76. (Author's *italics*).

Lazare Carnot considered the *production of work* to be produced by mechanical machines⁶⁹ (Oliveira 2014, Chaps. V-VI, 118–171). The f_i -forces are only important when they are linked to δs_i -displacements of bodies. In his father's mechanical theory, the *production of mechanical work* occurs with the transference of motion from one body to another, both being constrained bodies.

Preface. Although the theory here presented is applicable to all issues concerning the communication of motions, *Essay on machines in general* was given as title of this pamphlet; first of all, because they are mainly the Machines that are considered the most important argument of mechanics; secondly, because no particular machine is dealt with but we only deal with properties which are common to all of them.⁷⁰ [...] XXXII. If a force P moves having velocity u , we call z the angle formed by u and P , the quantity $Pudt\cos z$, where dt is the element of time, will be called *moment of activity* consummated by the force P during dt ; that is the *moment of activity* consummated by a force P in an infinitesimally short time, is the product of this force, orientated such as its velocity, and the path that the point, [fds] where this force is applied, does in an infinitesimally short time. I will call *moment of activity*, consummated by this force in a given time, the sum of *moments of activity* consummated by it at every instant [...] ⁷¹. [...] we come back specifically to the second way [theory of motion] of looking at the problem, that is to say, that mechanics are nothing else than the theory of the laws of the communications of the motions.⁷² [...] The first method [theory of forces where cause produces motion] offers much more ease; so it is, as I mentioned here above, almost generally followed. Nevertheless, I adopted the second [theory of motion] as I already did in the first edition; because I wanted to avoid the notion of metaphysics of forces, to leave undistinguished the cause and the effect, in short, to bring everything to the only theory of communication of motions.⁷³

However, it should be noted that the analogy does not go any further.⁷⁴ To be more precise, according to Lazare Carnot, an action of every force with the weight force can be reproduced. Due to the *communication of motion* (Carnot L 1803a, xij–xvj), in the end, one can also theorize on the work it completes as well as the idea *en général* of producing a new physical situation based on the impossibility of perpetual motion. Lazare Carnot studies a mechanical machine in general based on the fundamental affirmation that *perpetual motion is impossible* and the independence from the working substance, bodies and mechanism:

[...] everyone repeats that in Machines in motion time or speed is always lost when force is gained [...].⁷⁵

⁶⁹ On the role played by science and technique/machines within history of science see recently Pisano and Bussotti 2015b, 2015a.

⁷⁰ Carnot L 1786, iij, line 1. Author's *italics*.

⁷¹ Carnot L 1786, 65–66, line 2 Author's *italics*.1; see also pp 96–97. Author's *italics*.

⁷² Carnot L 1803a, xij, line 4. Author's *italics*.

⁷³ Carnot L 1803a, xv–xvj, line 24. Author's *italics*. See also 1803a.

⁷⁴ An analogy between mechanical and heat machines should be noted. If in thermodynamics Q is analogous to f , since neither are state functions f must be substituted by potential $\Delta V=f\Delta s$, while Q must be substituted by entropy, which however has a different formula $\Delta S= \Delta Q/t$. (Thomson 1851, I, 175–183; see also Clausius 1850, vol 155, 368–397; 500–524). Moreover, it should be also noted that in the second case, it is not a special physical distance but it is temperature–range, $\Delta t \neq 0$. Sadi Carnot wrote this at the beginning of the discursive part of *Réflexions sur la puissance motrice du feu* and repeats it several times as well as at the end of the demonstration of his celebrated theorem (Carnot S 1978, 38): *work can be obtained every time there is a difference in temperature between which heat passes*. Thus, it is possible to note a common way of conceiving work in comparison with special and heat motions (Pisano 2010; Gillispie and Pisano 2014).

⁷⁵ [...] tout le monde répète que dans les Machines en mouvement on perd toujours en temps ou en vitesse ce qu'on gagne en force [...]. (Carnot L. 1786, vi, line 14; see also *Ivi*, viii, line 20).

[...] The reflections I propose on this law [lvi, vi, op. cit.] lead me to say something about perpetual motion and I will show not only that every machine which is aborted must stop, but I will assign the very instant when this must occur.⁷⁶

[...] But, I repeat, this Trial only concerns machines in general; each of them have their own particular properties.⁷⁷

[...] we compare these different efforts regarding the agents that produce them, because the nature of the working substance cannot change the forces they must exert to fulfil the different objects for which the Machines are intended.⁷⁸

[...] LVII. what is finally the veritable purpose of moving machines? [...] *the machines in motion, always lose time and velocity, what is they gained in force.*⁷⁹

[...] LXII. We can conclude from that which we have just said regarding friction and other passive forces, that perpetual motion is absolutely impossible, using it to produce only bodies which are not solicited by any motive forces and even heavy bodies [...].⁸⁰

[...] It is therefore evident that we must absolutely give up the hope of producing that which we call perpetual motion if it is true that all of the motive forces that exist in nature [...].⁸¹

Table 3. On the way of conceiving vincula and the production of work

Lazare Carnot (1780; 1786)

The work as a *product* of a mechanical machine; *vincula* bodies.

Mechanical vincula: $M \gg m$

(*Principle of virtual work*). Systems of bodies, non–infinitesimal points, but global and with *vincula*.

More than one body having infinite mass cannot be a machine: no work from *vincula*, only.

It is impossible to link (in a direct way) different potential systems to produce work freely (impossibility of perpetual motion).

108. When a body acts on another one it is always directly or through some intermediary body. This intermediate body is in general what we call a machine. The motion that is lost at every moment in each of the bodies applied to this machine is partly absorbed by the machine itself and partly revised by the other bodies of the system but as it may happen that the subject of the matter is only to find the interplay of the bodies applied to the intermediate bodies without the need to know the effect on the intermediate bodies, we have imagined, in order to simplify the question, to ignore the mass of this body, however keeping all the other properties of matter. Hence the science of machines has become a sort of isolated branch of mechanics in which it is to be considered the mutual interplay of different parts of a system of bodies among which there are some that, lacking the inertia as common to all the parts of the matter as it exists in nature, withheld the names of machines. This abstraction might simplify in special cases where circumstances indicating those

⁷⁶ “Les réflexions que je propose sur cette loi [Carnot L 1786, vi], me conduisent à dire un mot du mouvement perpétuel, & je fais voir non–seulement que toute machine abandonnée à elle–même doit s’arrêter, mais j’assigne l’instant même ou cela doit arriver.” (Carnot L 1786, ix, line 16).

⁷⁷ “Mais, je répété, cet Essai n’a pour objet que les machines en général; chacune d’elles à ses propriétés particulières [...]” (Carnot L 1786, x, line 14). After a re–elaborated work (1781) a publication (nowadays lost) appeared on 1783. The next edition (Carnot L 1786) is the only one consultable.

⁷⁸ “[...] l’on compare ces différents efforts dans égard aux agents qui les produisent, parce que la nature des agents ne peut rien changer aux la nature des agents ne peut rien changer aux forces qu’ils font obligés d’exercer pour remplir les différents objets auxquels sont destinées les Machines [...]” (Carnot L 1786, 62, line 2). After a re–elaborated work (1781) a publication (nowadays lost) appeared on 1783. The next edition (Carnot L 1786) is the only one consultable.

⁷⁹ “LVII. Quel est donc enfin le véritable objet des Machines en mouvement? [...] *les Machines en mouvement, on perd toujours en temps ou en vitesse ce qu’on gagne en force.*” (Carnot L 1786, 88–89, line 24 (Authors’ *Italic style*)).

⁸⁰ “LXII. On peut conclure de ce que nous venons de dire au sujet du frottement & autres forces passives, que le mouvement perpétuel est une chose absolument impossible, en n’employant, pour le produire, que des corps qui ne seroient sollicités par aucune force motrice, & même des corps pesants [...]” (Carnot L 1786, 94, line 16).

⁸¹ “[...] Il est donc évident qu’on doit désespérer absolument de produire ce qu’on appelle un mouvement perpétuel, s’il est vrai que toutes les forces motrices qui existent dans la nature [...]” (Carnot L 1786, 95, line 33).

bodies for whom it was proper to neglect the mass to make it easier for the objective, but we easily know that the theory of machines in general has become much more complicated than before because then this theory was confined in the theory of motion of bodies as they are offered to us by nature, but now it is necessary to consider at the same time two kinds of bodies, one kind as actually existing, the other partially deprived of its natural properties. Now it is clear that the first problem is a special case, since it is more complicated than the other so that by similar hypotheses, we easily find the laws of the equilibrium and of motion in each particular machine such that the lever, the winch, the screw, resulting in a blend of knowledge whose binding can be hardly perceived and only by a kind of analogy; this must necessarily happen as we will resort to the particular figure of each machine to show the property which is common to it and to all the others. Since these properties are the ones we have mainly seen in this first section, it is clear that we will be able to find them only by putting aside the particular forms. So let us start by simplifying the state of the issue by ceasing to consider the system bodies of different natures; finally giving back to machines their inertia it will be easy afterwards to neglect the mass in the result, we will hold the possibility to consider it or not, and therefore the solution of the problem will be general and easier at the same time.⁸²

It should be noted that in Lazare Carnot's theory, it is implicit that many bodies with infinite mass, or constraints alone, do not form a machine (Carnot L 1786, 58–59) and therefore never produce work. Following this analogy one can clearly affirm, with the same reasoning as before, that it is impossible for connecting constraints *in a way only directed at different a t thermostats*. That is to say a machine to run (*produced work*) by letting heat pass without restrictions–dissipations. In other words, the reflection on the old experiment of the exchange between two bodies inside a calorimeter cannot show how work is produced. In fact, to produce work, other intermediary mechanisms are necessary in addition to thermostats in order to adequately utilize the transference of heat between the two temperatures. This is the *second argument ad absurdum* that unites the (implicit) development of the two theories, according to their common model of theory based on a problem.

The principle states that the total virtual work performed by all the forces acting on a system in static equilibrium is zero for a set of infinitesimal *virtual displacements* from equilibrium. The infinitesimal displacements are virtual because they need not be obtained by a displacement that actually occurs in the physical system. The virtual work is the work performed by the virtual displacements, which can be arbitrary and are consistent with the constraints of the system. Its common mathematical expression is:

$$\delta W = \sum_i F_i^{(a)} \delta s_i = 0$$

The theory of mechanical machines may be based on the *principle of virtual work*, and thought of as a consequence of the principle of the impossibility of perpetual motion, e.g., applied to machines and constraints: *it is impossible that the reactions of the constraints on the actions of the bodies, which make up the machine, produce positive work*. In other words, it is impossible for forces of bodies of constraints to produce work:

$$\sum_i R_i ds_i \leq 0$$

On Lazare Carnot's *Principle of Virtual Laws*

Lazare Carnot established an approach to science which was different from the common paradigm of his time (Dhombres and Dhombres 1997). The generalization of Lazare Carnot's *Principle of virtual work* is historically very important because it precedes Lagrange's approach in his *Mécanique analytique* (Lagrange 1788).

Lazare Carnot began by stating his principles, which he also referred to as laws, to underline their empirical content. In this regard, Charles Coulston Gillispie (1918–2015) observed:

⁸² Carnot L 1780, § 108.

He [Lazare Carnot] did achieve a greater clarity, most notably in the passages defining geometric motion: Any motion that, when imparted to a system of bodies, has no effect on the intensity of the actions that they exert or can exert on each other in the course of any other motions imparted to them, will be named geometric [“DÉFINITIONS” (Carnot 1803a, § 136, 108). Neither in the 1780 memoir nor in 837 the *Essai sur les machines en général* had Carnot adapted his concept of geometric motions from the principle of virtual velocities. In the *Principes fondamentaux de l'équilibre et du mouvement*, however, he went on to recognize the analogy between such motions and that principle in the use Lagrange made of the latter⁸³

They are only two principles in *Essai sur les machines en général* which become seven in *Principes généraux de l'équilibre et du mouvement* as we have already seen.

First law: *the reaction is always equal and contrary to action.*

Second law: *when two hard bodies act on each other, because of an impact and pressure, that is because their impenetrability, their relative velocity, immediately after the impact, is always zero.*⁸⁴

The first law states that all bodies which change their state of rest or motion always do so due to the action of another body. All bodies resist their change of state. Referring to this resistance Carnot uses the term “inertia force”, defending himself, for example, from Euler who considered it to be a confusing concept. This was because of the union of the contrasting ideas of activity (force) and passivity (inertia), which still prevailed in applied mechanics. For Carnot, the inertia force was “the result of the present motion and of a motion equal and opposite with respect to that which it must have in the subsequent instant” (Carnot 1786, pp 60–61).

The second law concerns hard (or completely soft) bodies. Carnot was convinced that this law put aside elastic bodies. He declared this openly and justified it by assuming that the behaviour of elastic bodies can be re-conducted to that of hard bodies considering the former as composed of many small hard bodies connected by springs. It is clear that Carnot’s is a forced justification because the way to quantify elasticity has yet to be clarified. By applying his principles to a system of free hard bodies, or to a system of bodies connected by rigid and insensible rods, Carnot obtained a first principle of mechanics which had the following form:

$$\sum mVU\cos Z = 0$$

This was named the “first fundamental equation of mechanics”. Here m is the mass of the corpuscles of the system, V is the true velocity after the impact, U is the lost velocity (such that $W = V + U$ is the velocity the mass would have before the impact) and Z is the angle between V and U . At this point Carnot introduced the concept of geometric motion.

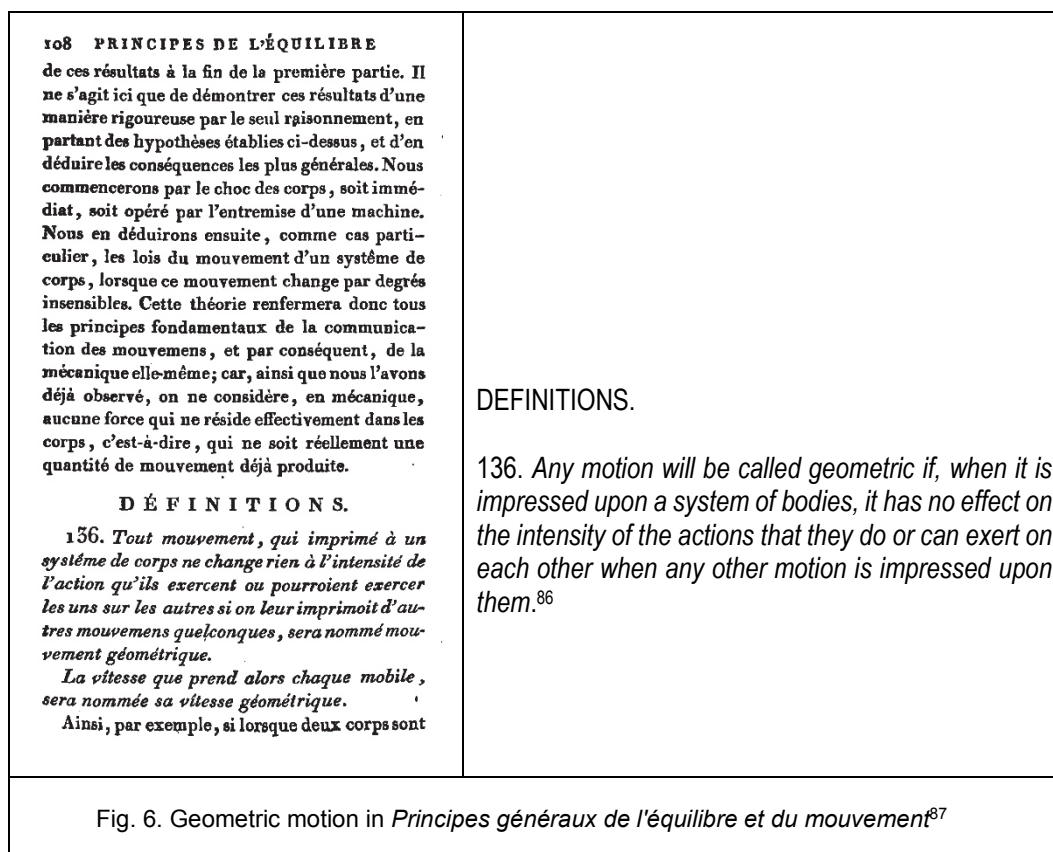
XVI. [...] *if a system of bodies sets out from a given position, with an arbitrary movement, but yet of such [a nature] that it is possible to make it take another in every respect equal and directly opposite; each of these movements will be named a geometrical movement [...]*⁸⁵

⁸³ Gillispie and Pisano 2014, 72, line 23.

⁸⁴ Carnot L 1786, pp 21–22. Author’s *italic*.

⁸⁵ Carnot L 1786, 28. Author’s *italic*. See also *Ivi*, 29–34, 41–45. See also Carnot L 1808a, 212; 1808b.

In *Principes généraux de l'équilibre et du mouvement* the definition is slightly different:



The first definition is purely geometric: geometric motions are reversible motions congruent with constraints. In the second definition there is also a reference to mechanical concepts, because the word “action” calls for concepts like force or work. From the examples Carnot gave in the Essay, it appears that geometric motions can also be infinitesimal (see the example referred to in the note on 26). The same holds true for *Principes généraux de l'équilibre et du mouvement* (see theorem IX, 130). From an operational point of view the finite or infinitesimal nature of geometrical motion makes no difference because what Carnot used is velocity u associated with the geometric motion, called geometric velocity. It can be said that the modern concept closest to geometric motion is that of virtual velocity, which today is not often distinguished from virtual displacement. Carnot gave great emphasis to geometric motions, considering their introduction as one of his major contributions to mechanics:

The theory of *geometric motions* is very important; it is as I have already noted like a mean science between ordinary geometry and mechanics [...] This science has never been treated in details, it is completely to create, and deserves both for its beauty and utility any care by Savan[t]s.⁸⁸

With the aid of geometric motion, the first fundamental equation of motion can be rewritten in a more meaningful and expressive way. It is easy for Carnot to show that the first fundamental equation remains valid when the true velocity is V after the impact is substituted by the geometric velocity u , to obtain:

$$\sum muU\cos z = 0$$

⁸⁶ Carnot L 1803a, 108. Author's *italics*. See also: Gillispie 1971, 43.

⁸⁷ Carnot L 1803a. Source: Google Books – Public domain.

⁸⁸ Carnot L 1803a, 116. Author's *italics*.

Now z is the angle between u and U . Carnot called this equation the “second fundamental equation of mechanics” and noted that by varying u among all the possible geometric motions one can obtain all the equations needed to find the lost motions U of all masses. In this way, Carnot solved what seems to be his main problem: given the initial velocities V of a system of masses to find the final velocities W after the impact. Indeed, when U is known, the final velocities are given simply: $W = U + V$. Unfortunately, this problem has little practical value because one rarely has to address the impact of hard bodies, simply because hard bodies do not exist, not even in an approximate way. To pass from the ideal dynamics of impact to the more realistic dynamics of force varying continuously, Carnot provides a passage which is difficult for us to understand: Carnot passed from the expression of motion lost in the impact to that of motion lost by imperceptible degrees and identifies mU (the lost motion) with the force F , which can now be read with the modern meaning.

The tension of treads, or the pressure of a bar, expresses equally both the effort which is exercised on the machine and the quantity of motion who itself loses because of the reaction he tries: if so one call F this force, this quantity F will be the same thing as what is expressed by mU in our equation.⁸⁹

In addition, we can then write the second fundamental equation “F” (Carnot L 1786, 32) as:

$$\sum Fucosz = 0$$

Which is, in fact, the equation of virtual work as given by Lagrange. Particularly, Lazare Carnot also dealt with the principle of virtual work and by means of *geometric motion* (in modern terms, virtual velocities), canonically formulated the principle of virtual velocities in a fundamental theorem (Carnot L 1786, § XXXIV, 68–69). In effect, since his theory of geometric motions coincided with velocities and not with displacements, this allowed Lazare Carnot to avoid, in the formulation of the principle of virtual work, infinitesimal displacements, which could have produced some scientific embarrassment with respect to his assumptions (Carnot L 1813). Furthermore, for the principle of virtual velocity related to any (general) mechanical machine, one can claim that the (forces–) weights that balance each other are reciprocal to their virtual velocities. Incidentally, the two conceptually different approaches/formulations can be mathematical equivalents using the concept of virtual motion as key reasoning.

Lazare Carnot formulated the principle of virtual work by beginning with his law of collisions (Carnot L 1786, 1803a) and without (generally speaking) using classical Newtonian forces. Particularly, Lazare Carnot used the principle of virtual work to discuss and define the conditions of equilibrium of the forces applied to the bodies.

General principle equilibrium and of motion in machines

XXXIV. *Whatever is the state of repose or of motion in which any given system of forces applied to a Machine, exists, if we take it all at once assume any given geometric motion, without changing these forces in any respect, the sum of the products each of them, by the velocity which the point at which it is applied will have in the first instant, estimated in the direction of this force, will be equal to zero.* That is to say, by calling F each of these forces (l), u the velocity which the point where it is applied will have at first instant, if we make the Machine assume a geometric motion, and z the angle comprehended between the directions of F and of u , it must prove that we shall have for the whole system $[\sum]Fucosz=0$. Now this equation is precisely the equation (AA) $[\sum]Fucosz=0$ (Carnot L 1786, 63, line 15)] found (XXX) [lvi , 60] which is nothing else in the end but the same [second] fundamental equation (F) $[\sum]muUcosz=0$ (lvi , 32, line 6)] presented under another form. It is easy to perceive that this general principle is, properly speaking, nothing else than that *Descartes*, to which a sufficient extension is to be given, in order that it may contain not only all the conditions of the equilibrium between two forces, but also all those of equilibrium and of motion, in a system

⁸⁹ Carnot 1786, 65–66.

composed of any number of powers: thus the first consequence of this theorem will be the principle of *Descartes*, rendered complete by the conditions which we have seen were waiting in it (V)⁹⁰.

The aim was to obtain a mathematical expression of its invariant, or the efficiency of a heat machine with respect to all possible kinds of working substances. Therefore, it was necessary to obtain invariants with regard to the efficiency and reversibility of a mechanical machine.

We will now note that in the traditional mechanical theory of hard bodies, the *principle of virtual work* formally defines the condition of *equilibrium* of the forces that act on the bodies in order to produce work:

*Corollary II. General principle of equilibrium in weighing Machines. XXXVI. When several weights applied to any given Machine, mutually form an equilibrium, if we make this Machine assume any geometric motion, the velocity of the centre of gravity of the system, estimated in the vertical direction, will be null at the first instant.*⁹¹

Lazare Carnot, having the mathematical formula for the *principle of virtual work*, studies the theoretical conditions that translate the practical conditions of equilibrium and also obtains his invariants with regard to the efficiency and reversibility of mechanical machines.

His *first* (Carnot L 1786, 32) and *second* equations (lvi, 33) generalized for multi-body systems are:

$\sum muU \cos(\langle \vec{U}, \vec{u} \rangle) = 0$	(E)	m = mass of the body
$\sum mVU \cos(\langle \vec{U}, \vec{V} \rangle) = 0$	(F)	W = velocity before interaction
		V = velocity after interaction
		$U = W - V$
		u = arbitrary geometric motion

In short⁹²:

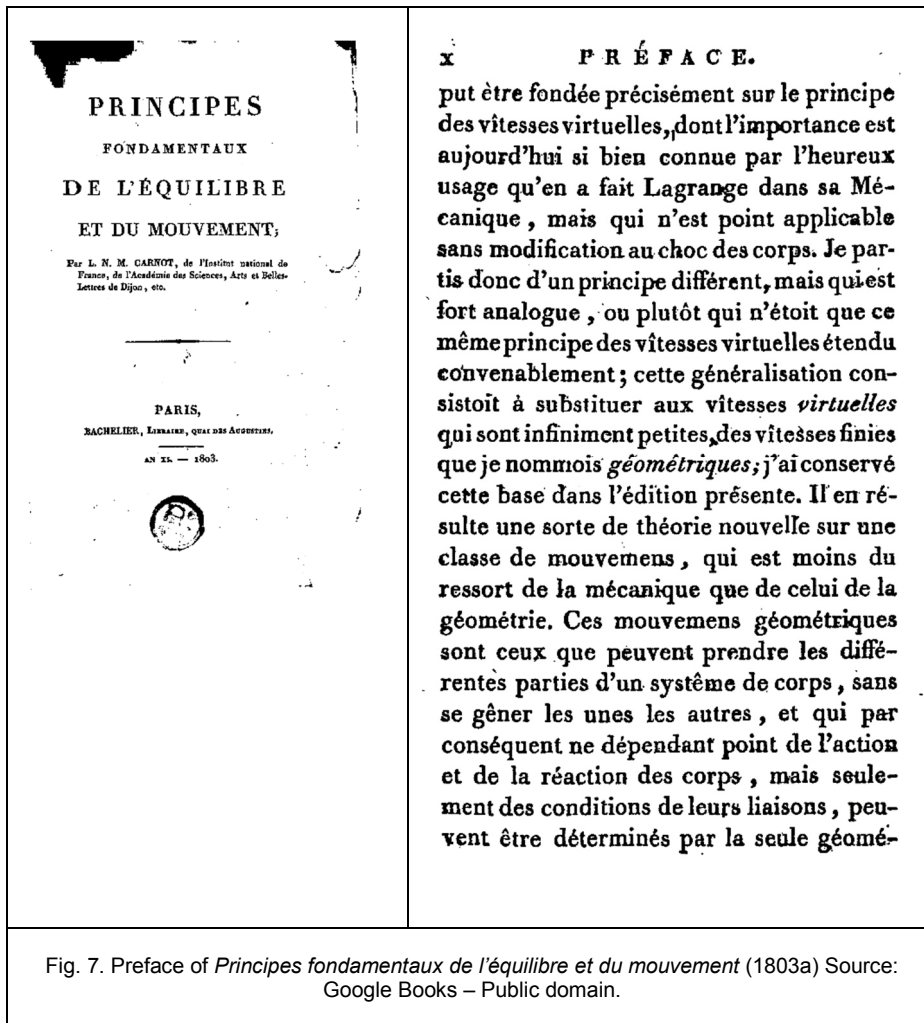
- The mass of the parts of a machine.
- Global magnitudes, abstracting from the mass of the mechanism.
- Kinematics first, then dynamics, and statics is a special case of dynamics.
- A theory of machines concerns a theory of the *communication of motions*.
- A machine is a connected system of (hard) bodies.
- The connections between the bodies constrain the *communication* of motion of the bodies.
- The theory of interaction–collisions by means of insensible degrees (e.g., see Carnot L 1803a, § 293, pp 261–262) as the result of a sequence of infinitesimally small percussions.

In order to complete our discussion on the use of the *principle of virtual work* in the two Carnots' theories, in the following section we summarize Lazare Carnot's reasoning upon his laws of conservation, mainly included in both *Essai sur les machines en général* (Carnot L 1786) and *Principes fondamentaux de l'équilibre et du mouvement* (Carnot L 1803a).

⁹⁰ Carnot 1786, § XXXIV, 68–69 and footnote "(I)". (Author's *italics* and Capital letters).

⁹¹ Carnot L 1786, 71, line 1. (Author's *italics*). See also Carnot L 1803a.

⁹² An extensive discussion is in: Gillispie and Pisano 2014.



A law of conservation for plastic bodies – even though he called them *hard bodies* – can be written in the following way:

$$\sum_i m \vec{U}_i = 0$$

m_i = mass of i -th body [for isolate system]
 U_i = velocity lost (by that body) during the collision
 W_i = velocity before interaction
 V_i = velocity after interaction
 $\vec{U}_i = \vec{W}_i - \vec{V}_i$

Therefore, using the hypotheses of *parfaitement élastiques*⁹³ bodies (Carnot L 1803a, 105), one obtains:

$$\sum_i m_i \vec{U}_i \vec{V}_i = 0$$

V_i = velocity after interaction is the same for all of them
 Generalization for all bodies using a n -elasticity index.

Now, by using certain calculations, the *law of conservation of kinetic energy* for soft bodies is obtained:

$$\sum_i m_i \vec{W}_i^2 = \sum_i m_i V_i^2 = 0$$

⁹³ Lazare Carnot proposed a generalization from plastic bodies to all bodies by means of an *ad hoc* index. (See below). He presented it in *Essai sur les machines en général* (Carnot L 1786 pp 15–22) and in *Principes fondamentaux de l'équilibre et du mouvement* (Carnot L 1803a, pp 103–106, pp 131–146). The generalization is differently presented in the two cited books. More specifically, the first reports an inverse procedure with respect to second.

Following Lazare Carnot's reasoning presented in both his books (Carnot L 1786; 1803a), at this point, by introducing *geometric motions* (Carnot 1786, 28–30) starting from the previous *law of conservation*, one can write:

$$\sum_i m_i \vec{U}_i \cdot \vec{u}_i = 0$$

m_i = mass of the i -th body
 U_i = velocity lost (by that body) during the collision
 u_i = velocity called "mouvement géométrique"⁹⁴

Here, it is unproblematic to recognize the extension of the *principle of virtual velocity* to the collision of several bodies using

$$\vec{u}_i = \text{const.}$$

With following the proof, which we omit for the sake of brevity, we can write:

$$\sum_i m_i \vec{U}_i \cdot \vec{u}_i = 0; \rightarrow \vec{u} \sum_i m_i \vec{U}_i = 0$$

By considering the u -arbitrariness, we can write:

$$\sum_i m_i \vec{U}_i = 0$$

With

$$\vec{U}_i = \vec{W}_i - \vec{V}_i$$

In short:

- A theory of interacting bodies by means of collisions.
- A collision is a basic phenomenon. In particular, continuously accelerated motion is obtained as a limiting case of a system driven by a series of pulses.
- Newton's second law is replaced by Lazare Carnot's second fundamental equation for a system of n -bodies.
- Due to the arbitrariness of u_i , it can be assumed constant, that is to say the same translation of geometric uniform motions of all bodies is adopted.

By considering another *ad hoc* geometric motion,

$$\vec{u}_i = \vec{\omega} \times \vec{r}_i$$

e.g., the rotation of the system with angular velocity around a fixed axis, and using the properties of the triple product and the arbitrariness of " $\vec{\omega}$ ", he then wrote his two main *laws of conservation* as *invariants of motion*:

⁹⁴ In practice, u is physically the velocity of any *geometric motion*. However, it is a mathematically indeterminate variable and each specification produces (in the equation cited in the running text) an equation applicable to the physical system considered.

$$\sum_i m_i \vec{W}_i = \sum_i m_i \vec{V}_i \quad \text{Law of conservation of the total-quantity-of-motion}$$

$$\sum_i m_i \vec{r}_i \times \vec{W}_i = \sum_i m_i \vec{r}_i \times \vec{V}_i \quad \text{Law of conservation of the total-angular-momentum}$$

Concluding Remarks

The role played by the *Principle of virtual laws* within *classical mechanics* is not easily definable and theoretically it does not appear to be crucial (from a technical standpoint). One can think of *rational mechanics* while avoiding problems⁹⁵ and not allowing for the theory to take on unresolvable problems. In the mechanics applied to rigid bodies, that is, the relationship between physics and its mathematical interpretation, the principle of virtual laws can be used powerfully to solve certain *vincula* problems. Therefore, generally speaking, in modern times it does not appear critical within scientific research. The *vincula* are taken into account by introducing the *constraint reactions* such as *auxiliary unknowns* that are then eliminated by substitution during the solution of the single static problems; and with friction constraints, one can easily provide adequate *constitutive laws*. In *continuum mechanics*, its role is very important since the solution is simpler if one adopts particular values of the mathematical expression of the *principle of virtual laws*, such as using the *method of finite elements*. From a historical point of view it is of utmost importance. It had a long historical process beginning with ancient Greek science (Pisano and Capecchi 2015, chap. 2) until the Aristotelian⁹⁶ and Archimedean mechanical approaches.

Based on some of Lazare Carnot's crucial discussions (Carnot 1786, 28–30), the *geometric motion*⁹⁷ essentially expresses a non-mechanical interaction. Lazare Carnot defined these motions as *invertible*: *a motion assigned to a physical system of interacting bodies is geometric if the opposite motion is also possible*. The result is a *possible motion*, but it is not always *invertible* (e.g., the motion of a sliding ring on a rotating rod). Therefore, one should add the hypothesis of *invertibility* for obtaining the concept of *geometric motion*. Conversely, a *geometric motion*, when integrated, gives an *invertible motion*. At this point, for *vincula* independent of time, a *geometric displacement* is equivalent to a *virtual invertible displacement* (but not vice versa). On the contrary, only a *possible displacement* that is *invertible*, produces, after its derivative, a *geometric motion*. In this sense, we note that initially, the geometric motion is a kind of uniform motion moving on the whole physical system when one considers the equivalence of the state of rest and the state of uniform motion. Consequently, by using double negative sentences (Cf. Gillispie and Pisano 2014, chap. 7) one can write:

$$\neg[(v = 0) \neq (v \neq 0)]$$

Finally, the previous discussion on the *principle of virtual work* and in Lazare's work is, in any case, limited to the two different mathematical approaches with respect to adopted physics. Lazare Carnot himself introduced the reader to a new level of describing physical phenomena (Carnot L 1803a, x), that is to say, the *principle of virtual work*. In this sense, a new kind mechanics – with respect to Newtonian mechanics – would be born thanks to *l'organisateur de la victoire*.

⁹⁵ By the way many assumptions and equations derived by it (i.e. Lagrange, Hamilton, Hamilton–Jacobi).

⁹⁶ In the history of mechanics, if one avoids the theory of centres of gravity (Pisano 2007), a modern interpretation of the principle has its general roots into Aristotelian mechanical school (Pisano and Capecchi 2015, chap. 2).

⁹⁷ Carnot L 1786, pp 28–34, pp 41–45; see also Carnot L 1780, § 113; Gillispie 1971, Appendix C, § 113, 308–309.

Acknowledgments

I thank anonymous referees for their valuable remarks, which have been of great help. I also want to express my warm gratitude to John Schuster and Paolo Bussotti for our deep discussions. I am delighted and greatly honoured to have had them as readers for their valuable comments as well. Finally, I thank *Rights and Permissions Springer Nature* for its kind authorisation.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 204-225

ISSN 2526-2270

www.historiographyofscience.org

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Article

Michael Scot and the Four Rainbows

Tony Scott¹

Abstract:

We apply a physical and historical analysis to a passage by the medieval scholar Michael Scot concerning multiple rainbows, a meteorological phenomenon whose existence has only been acknowledged in recent history. We survey various types of physical models to best decipher Scot's description of four parallel rainbows as well as a linguistic analysis of Scot's special etymology. The conclusions have implications on Scot's whereabouts at the turn of the 13th century.

Keywords:

Meteorology; multiple rainbows; optical dispersion; middle-ages

Received: 17 February 2017. Reviewed 26 April 2017. Accepted: 12 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.18>

204

Introduction

The rainbow is an impressive and fascinating natural phenomenon that is observed when sunlight interacts with raindrops in the air and involves the splitting of white light into its constituent colors. The splitting of light into its colors is also observed when white light passes through a prism. This latter effect has been investigated and explained by Isaac Newton and arises from optical dispersion.

Most of us associate the rainbow with the familiar single bow in the sky. Yet there are occasions in which four, or even more, rainbows are observed. The first, to the best of our knowledge, written record to that effect appears at the beginning of the thirteenth century and is due to Michael Scot, as quoted by Lynn Thorndike:

It should be known that *four* bows, and maybe more, can be formed at once, at slight distance apart. And when so many are formed, people seeing them are much astounded, and then it is a sign of a small gathering of clouds in air, and those which are there are for the most part *rare and subtle in substance*. And that is a sign of *very little or no rain*, and they do not produce thunder but break into fragments and vanish. And such clouds appear very *zalla* and low and mountainous (*montuose*). Black and thick clouds do not generate so many bows, also produce thunder and rain (...) (Thorndike 1965, 69)

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In the quote above, the term “zalla” is italicized (and will be subject to examination in the section entitled “Possible Source of Unknown etymology”). The quote originates from Michael Scotus, the Latinized form for Michael Scot (1175-1235 CE) (Thorndike 1965, Burnett, 1994, 2,101-126). Scot became part of history and legend as the court “astrologer” of Frederick II (Abufalia 1988, Benoist-Méchin 1997), ruler of the Holy Roman Empire. Although well viewed by the papal authorities around 1227, Scot would acquire the sinister reputation of a magician and wizard and would be condemned to the inferno in Dante Alighieri’s epic poem, *The Divine Comedy* (albeit “rescued” much later in Sir Walter Scott’s poem *Lay of the Last Minstrel*). Michael Scot and his contemporary Fibonacci were members of the court of Frederick II and would play their part in transmitting much of the scientific knowledge of the Muslims (largely from Moorish Spain) into Europe (largely Italy and Sicily), thereby planting many of the seeds of the Italian “Renaissance” (Haskins 1927, Burnett, 1994, 2,101-126).

In Thorndike’s work, reference is made to a 1959 edition of a book by Carl Boyer on rainbows (Boyer 1959), in which it is stated that “the quaternary rainbow arc is not known to appear in nature”. Though Thorndike’s book on Michael Scot was excellent, Thorndike was not a physicist and was not therefore in a position to evaluate this claim. As a matter of fact, four rainbows may indeed form simultaneously in the sky. Tertiary and quaternary rainbows in particular were spotted in 2011 by Michael Theusner (Theusner 2011) as reported by the BBC (Palmer 2011). If these recently observed quaternary rainbows are indeed related to the four bows described by Michael Scot (as illustrated in Figure 1), then Scot reported, in the 13th century CE, a sighting of a natural phenomenon not fully understood until the 20th century and not fully demonstrated until the 21st century! In particular, as discussed in this article, scholars in Scot’s time did not believe in the possibility of formation of more than two bows in the sky. The precociousness of Scot’s testimony is the subject of this article as it begs, amongst other things, the question as to the origin of the four observed bows.



Fig. 1 - Four Rainbows, an artistic impression.
Courtesy Zhao Jingying, Taiyuan, China.

This would be not the first time that scholarly experts had misread or underestimated records left by Michael Scot. Haskins for instance had examined a very detailed description in Latin concerning the medical case known as “Mary of Bologna” and dismissed Scot’s record as a “calcified fibroid tumor” (Haskins 1927, 274). In the early 1970s however, the exacting detail of Scot’s description enabled a new and different medical diagnosis: this was a very rare case of miscarriage or “spontaneous abortion”, not followed by immediate expulsion, of *twin embryos*, dead at different dates and calcified (O’Neill 1973, 77-811, O’Neill 1974,125-9). In short, Scot had actually recorded, back in the 13th century, a rare medical case and this has not been fully appreciated until the 20th century! We note in addition that a recent analysis on the Fibonacci numbers (Scott & Marketos 2014) suggests that Scot may have played a role in the formulation of the Fibonacci

sequence in Leonardo de Pisa's book "Liber Abaci", a further indication that a conclusive understanding of Michael Scot and his contributions is yet in the making.

In order to understand the physical origin of Scot's reported observation, we present, in the following, the physics of the rainbow. We also discuss physical models that give rise to multiple rainbows while at the same time keeping in mind the context of the era in which Scot lived and worked. We therefore present, in addition to findings from scientific analysis, cultural, historical and even linguistic evidence, in an effort to unravel the mystery of Scot's sighting.

Primary and Secondary Rainbows

A "primary" rainbow, and a fainter "secondary" rainbow parallel to the first bow are shown in Figures 2 and 3. The primary rainbow results from a single internal reflection of refracted light inside a raindrop. In this case, light is refracted as it enters the raindrop, it is then reflected off the back of the drop, and is finally refracted as it leaves the drop (Figure 4). The color on the outside of the primary rainbow is red, leading through to violet on the inside. The secondary rainbow is formed from rays that are reflected twice within the drop before leaving the drop.

The angle of refraction depends on the frequency of radiation. This effect is known as dispersion and causes sunlight to split into its constituent colors on entering the raindrop. Red light is refracted by a smaller angle than blue light as it enters the drop and red rays turn through a smaller angle than blue rays on leaving the raindrop. Consequently, light in the primary rainbow is spread, with a maximum intensity at an angle of $40^\circ - 42^\circ$ (About Rainbows – UCAR, 2013) (see Figure 3). The angle of 42° corresponds to what is called an *anti solar point* which is the imaginary point on the celestial sphere exactly opposite the Sun. The anti solar point is the center of rainbows and can be easily identified: on a sunny day, it is located at the shadow of one's head. Secondary rainbows, caused by two total internal reflections of sunrays before they leave the raindrop, appear at an angle of $50 - 53^\circ$. As a result of the second internal reflection, the colors of a secondary rainbow are inverted compared to those in the primary bow, with blue on the outside and red on the inside. As some light nevertheless escapes the drop at each internal reflection, the secondary rainbow is fainter than the primary because a) the intensity of the light that is transmitted is smaller in this case (due to the two internal reflections) and b) the secondary rainbow itself is spread over a greater angle in the sky.

A more thorough study of the interaction of sun rays with raindrops requires knowledge of the physics of *reflection, refraction and dispersion*. The law of reflection is easy to understand and was already known in the middle-ages. The path of any ray hitting the drop can be determined using *Snell's law* and simple trigonometry. Dispersion on the other hand arises from the dependence of the angle of refraction on the frequency (wavelength) of radiation.

The law of refraction was first accurately described by Ibn Sahl, of Baghdad, in the manuscript "On Burning Mirrors and Lenses" in 984 CE (Rashed, 1990, Wolf, 1995). This law would not be rediscovered in Europe until the 1600s and then credited (1621 CE) to Willibrord Snell. Snell's law states that:

$$\frac{\sin(\theta_1)}{\sin(\theta_2)} = \frac{n_2}{n_1} \quad (1)$$

where n_1 and n_2 are the indices of refraction of adjacent media, labeled 1 and 2. This is illustrated in Figure 5. We note that Ptolemy, a Greek living in Alexandria, Egypt, in the second century CE (Harland 2007) had found a relationship involving the angle of refraction, which however was inaccurate for angles that were not small. Ptolemy's empirical law was obtained partly as a result of fudging his data to fit theory (Weinstein 1996-2007).

When light travels from a medium with a higher refractive index, such as water or raindrop, to one with a lower refractive index, such as air, if the angle of incidence is large enough, Snell's law requires that the sine of the angle of refraction be greater than one. This is not possible, and the light in such cases is reflected by the boundary, a phenomenon known as *total internal reflection*. The largest possible angle of incidence which still results in a refracted ray is called the critical angle; in this case, the refracted ray travels

along the boundary between the two media, with the angle of refraction being equal to 90° .

The process of *total internal reflection* is illustrated in Figure 6 for adjacent media with indices of refraction equal to 1 (e.g. air) and 1.5 (e.g. glass). A similar situation is encountered when a ray of light moves from water to air with an angle of incidence of 50° . The refractive indices of water and air are approximately 1.333 and 1, respectively, and Snell's law (eq. (1)) gives us the relation:

$$\sin \theta_2 = \frac{n_1}{n_2} \sin \theta_1 = \frac{1.333}{1} \cdot \sin (50^\circ) = 1.333 \cdot 0.766 = 1.021, \quad (2)$$

which is impossible to satisfy for this angle of incidence. The critical angle θ_{crit} is the value of θ_1 for which θ_2 equals 90° :

$$\theta_{crit} = \arcsin \left(\frac{n_2}{n_1} \sin \theta_2 \right) = \arcsin \frac{n_2}{n_1} = 48.6^\circ, \quad (3)$$

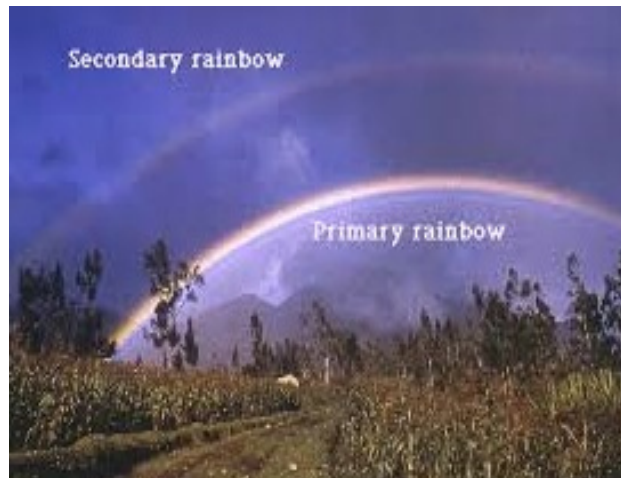


Fig. 2 - Primary and Secondary Rainbows. Courtesy University of Illinois Guide to Atmospheric Physics.

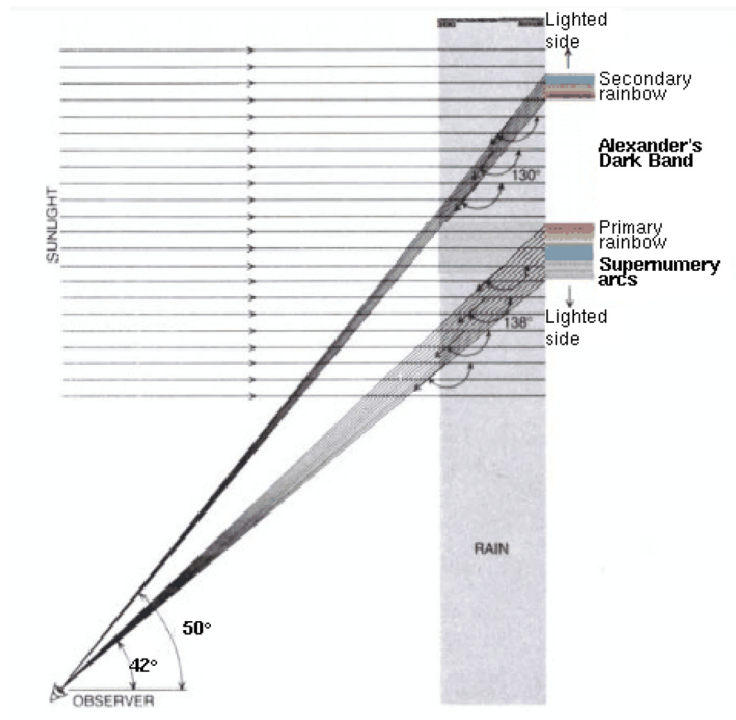


Fig. 3 - Angles of Elevation of Primary and Secondary Rainbows. Courtesy H. Moysés Nussenzweig, Scientific American, 1977.

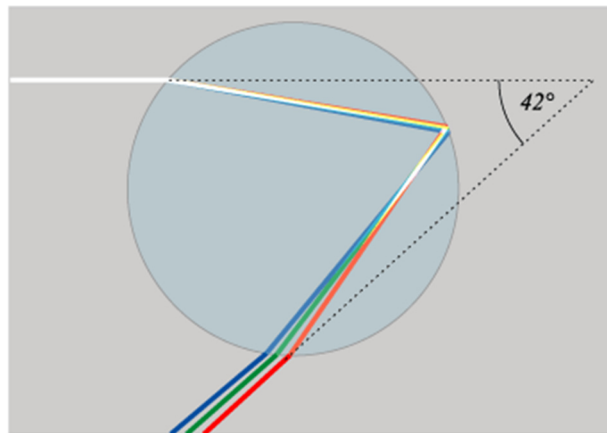


Fig. 4 - Processes that result in the formation of a primary rainbow: dispersion (as light enters and leaves the drop) and total internal reflection. Courtesy of KES47 of Wikipedia Commons, 2010.

which is very large even larger than the anti solar angle of 42° . Thus, contrary to popular belief, the light at the back of the raindrop does *not* undergo total internal reflection and some light does emerge from the back. (This is important to remember when dealing with successive reflections and consequently multiple rainbows.) It should be noted that light coming out the back of the raindrop does not create a rainbow between the observer and the sun because spectra emitted from the back of the raindrop do not have a maximum of intensity, as the other visible rainbows do, and thus the colors blend together rather than forming a rainbow.

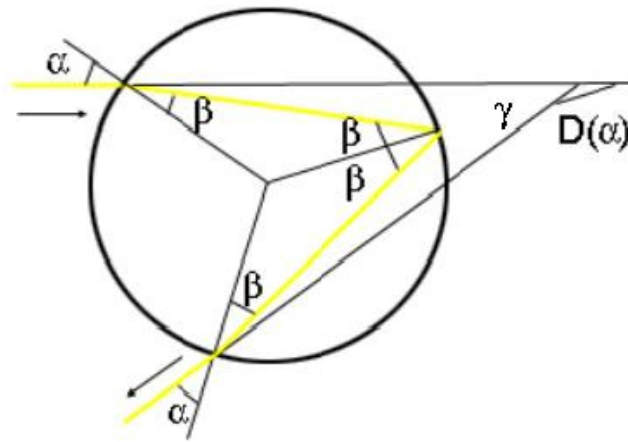


Fig. 7 - Angle of deviation

Interestingly enough, in 1266 CE a contemporary of Scot, Roger Bacon, measured the angle of the rainbow cone for the maximum elevation of the rainbow as 42° (Hackett 2013). In this measurement, probably achieved with an astrolabe, Bacon advocates the skillful use of instruments in an experimental science. However, Bacon's knowledge of the rainbow was understandably limited. For instance, like Aristotle, he attributed the rainbow solely to reflection and not refraction. The anti solar angle of 42° shown in Figure 3, which matches the total angle (later denoted γ in Figure 7), between incident and reflected waves shown in Figure 4, can be derived from the deviation $D(\alpha)$. According to the ideas of Rene Descartes (Descartes 2001, Osler, 2008, Tipler, 2004) as illustrated in Figure 7, $D(\alpha)$ is given by the formula:

$$D(\alpha) = (\alpha - \beta) + (180^\circ - 2\beta) + (\alpha - \beta) = 180^\circ + 2\alpha - 4\beta. \quad (4)$$

From Snell's law of eq. (1):

$$\frac{\sin \alpha}{\sin \beta} = \frac{n_{water}}{n_{air}} \quad (5)$$

where n_{air} is equal to 1.

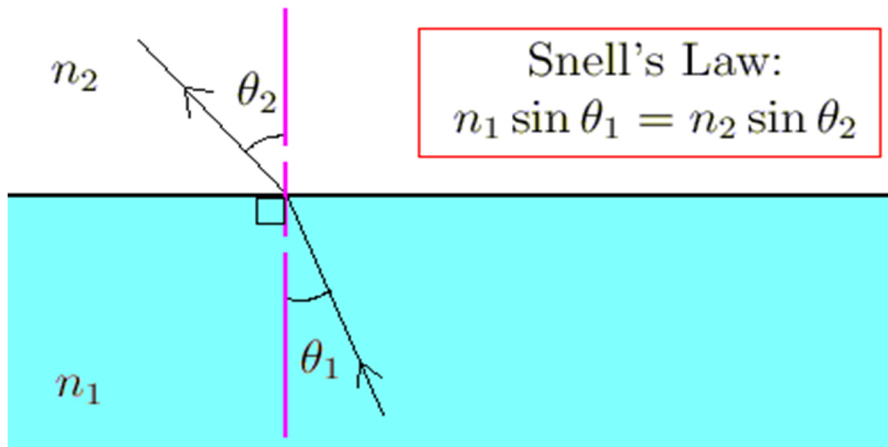


Fig. 5 – Snell's Law of Refraction. Courtesy math.ubc.ca

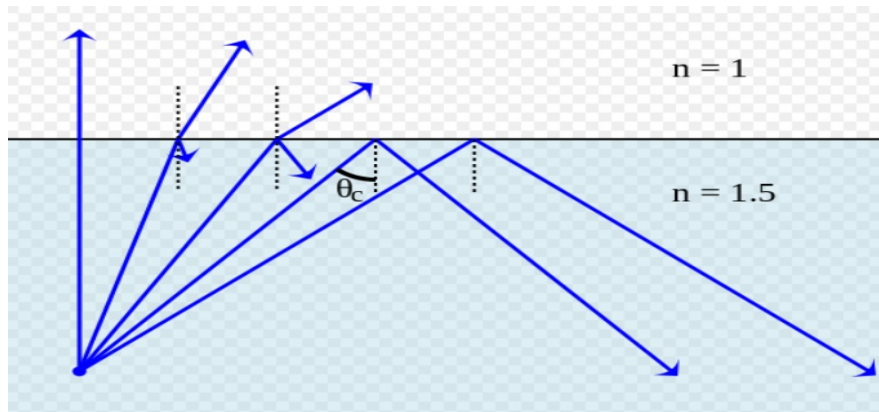


Fig. 6 – Demonstration of refraction and total internal reflection, when the angle of incidence exceeds a critical angle. Courtesy Lasse Havelund, Wikipedia Commons, 2009.

Solving for β , one obtains:

$$\beta = \arcsin \frac{\sin \alpha}{n_{water}},$$

which is then substituted back into eq. (4). The rainbow is produced by rays around this minimum deviation. Solving $dD(\alpha)/d\alpha = 0$ for α to find α_m , the value of the angle α for which D is a minimum, we obtain:

$$\alpha_m = \arccos \sqrt{\frac{n_{water}^2 - 1}{3}}.$$

Taking the index $n_{water} = 1.33$ for a particular frequency in the red, we obtain $\alpha_m = 59.58^\circ$, $\beta = 40.42^\circ$, and $D(\alpha) = 137.48^\circ$. Now if an emerging rainbow ray from a droplet meets one's eye, this means that this ray

makes an angle $\gamma = 180^\circ - 137.48^\circ = 42.52^\circ$ which is indeed the angle of 42° at the anti-solar point as shown in Figure 4.

The rainbow's spectacular aspect, its colors, were explained by Newton. In 1666 CE, Newton showed that white light being refracted in a prism is split up in its constituent colors. The color scattering is due to the index of refraction being dependent on the wavelength (and hence the color) of radiation. Each color in the sunlight thus produces its own bow. A rainbow is a collection of these bows, each slightly displaced from the rest. Newton worked out the angles of the red bow, $42^\circ 2'$ and the violet bow, $40^\circ 17'$. This gives a rainbow spread of $1^\circ 45'$. This would have been the width of the rainbow if the sun rays were parallel. As a matter of fact, the sun disk has a diameter of half a degree. Taking this into account, Newton concluded that the width of the rainbow should be 2 degrees and 15 minutes, a value that agreed nicely with Newton's own measurements. Note that although Newton's original used a corpuscular theory rather than a wave theory, the results of his model nonetheless carry through to a wave theory (Blay 2001, Shapiro, 2002) and his corpuscular model is "rescued" by today's modern particle-wave duality and the quantization of electromagnetic light waves known as photons (Tipler 2004).

Types of Multiple Bows

In addition to the primary and secondary rainbows, four bows may also form in the sky. These are classified as follows:

1. Reflected Double Rainbows
2. Supernumerary Rainbows
3. Quaternary Rainbows

In an effort to associate the four bows reported by Scot with one of the cases above, we will first review each of these cases in the following sub-sections.

Reflected Double Rainbows

A double rainbow (consisting of a primary and a secondary rainbow) and reflections on a sufficiently large shallow body of water, such as a lake or the water near a calm seashore, can produce four bows. In the *reflected rainbow*, the sunlight is first deflected by the raindrops, and then reflected off the body of water, before reaching the observer. A *reflection rainbow* is produced when sunlight reflects off a body of water before reaching the raindrops. It should be noted however that, in either case, the four bows are never parallel, as seen in Figure 8. Due to the combination of requirements, a reflected double rainbow is not very common. The reflection rainbow appears above the horizon. It intersects the normal rainbow at the horizon, and its arc reaches higher in the sky, with its center as high above the horizon as the normal rainbow's center is below it. Six (or even eight) bows may also be distinguished (Atmospheric Optics 2013, Nordvik, 2007). Reflected double rainbows have been cited and are more common than the tertiary or quaternary rainbows that will be discussed later.



Fig. 8 – Reflection rainbow: bows reflected on a body of water.
Courtesy of Lawlnut, i.imgur.com, 2012.

The Scottish Western Isles are favorable for the formation of reflection bows. The prevailing warm south westerlies from the Atlantic Ocean bring frequent showers of fine rain interspersed by skies of exceptional purity whose sunlight is reflected in the many bays and inlets. Since Scot originated from South Scotland, it is possible that he was aware of reflection rainbows. In his description, he made a point of emphasizing that the four bows are “at slight distance apart” indicating they are likely *parallel*. This is not the case for reflection rainbows, as the generation of four bows from reflection rainbows would involve a reflected double rainbow making up two sets of parallel bows that *intersect* each other as shown in Figure 8. Scot’s precise and meticulous descriptions, as demonstrated in the case of Mary of Bologna suggests that Scot was aware of the distinction. This excludes the possibility that reflected double rainbows are the origin of Scot’s observation.

Supernumerary Rainbows

A supernumerary rainbow also known as a “stacker rainbow” is not observed frequently. It consists of several faint rainbows on the inner side and within the primary rainbow, and very rarely also outside the secondary rainbow. It is not possible to explain supernumerary rainbows using classical geometric optics, as these are caused by the interference of light waves, a phenomenon that has been investigated in detail by Thomas Young. In 1803, Thomas Young showed that waves from two wave sources (e.g. two holes in a pier in a bowl of water) interfere constructively and reinforce each other, creating crests or troughs, or interfere destructively and cancel out. Young pointed out that the supernumerary bows could be caused by constructive and destructive interference of sunrays which have followed different paths through the raindrop, if the difference between the distances traveled by these rays is equal to an odd number of half wavelengths (destructive interference) or an even number of wavelengths (constructive interference).



Fig. 9 – Over Niagara Falls. Courtesy A. Bierstadt (1830-1902).



Fig. 10 – In Fife, Scotland. Corinne Mills, 2011.

Examples of Supernumerary Rainbows

Figures 9 and 10 provide examples of supernumerary rainbows over the Niagara falls and in the region of Fife in southern Scotland. Since Scot came originally from southern Scotland, perhaps even from the area of Fife itself, which are regions abundant in rainfall, it is possible that he may have observed supernumerary rainbows and that the four bows in his description perhaps can be classified as a supernumerary rainbow.

Tertiary and Quaternary Rainbows

Unlike primary and secondary rainbows, that can be observed in a direction opposite to the sun, it is also possible (but rare) to observe two faint rainbows in the same side of the sky as the sun. These are the *tertiary* and *quaternary* rainbows, appearing on the opposite side of the sky to the familiar rainbow arc, at about 40° from the sun (for tertiary rainbows) and 45° (for quaternary rainbows). A tertiary rainbow is formed by light that has suffered three total internal reflections inside the raindrop, whereas a quaternary rainbow by light that has suffered four total internal reflections within the rain drop (Figure 11). It is difficult to observe these types of rainbows with the naked eye not only because of the sun's glare, but also because the intensity of the n^{th} bow decreases dramatically as n increases.

Theoretical possibilities for multiple and higher-order rainbows were described by Felix Billet (1808-1882 CE) who depicted angular positions up to a 19th-order rainbow, a pattern he called a "rose of rainbows" (Billet 1868, Walker 1977, 138-144, 154). In the laboratory, it is possible to observe higher-order rainbows by using extremely intense and well collimated light produced by lasers. Up to the 200th-order rainbow was reported by Ng et al. in 1998 using an argon ion laser beam (Ng *et al.* 1998).

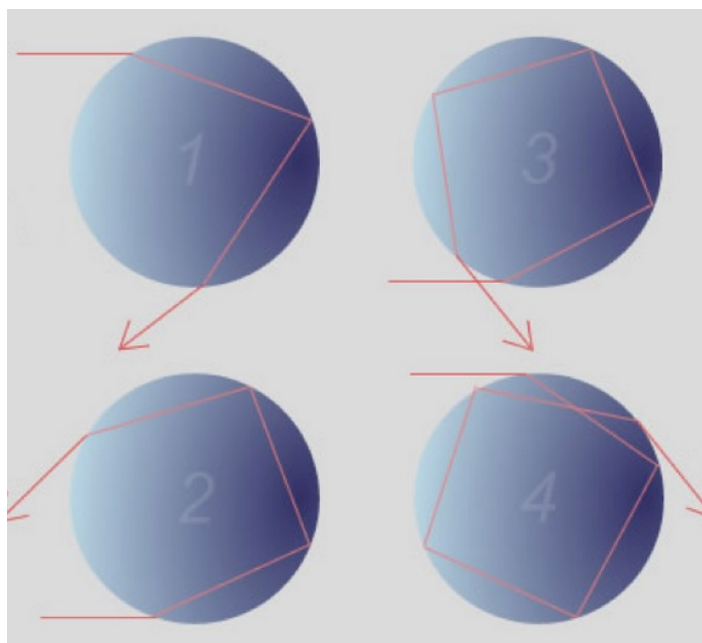


Fig. 11 – Formation of four rainbows from one, two, three and four successive total internal reflections in water droplets suspended in air. Rainbows formed from one and two total internal reflections appear on the opposite side of the Sun, those formed from three and four total internal reflections on the same side as the Sun. Courtesy Robbie Gonzalez, io9.com, 2011.

Raymond Lee (Lee & Fraser 2001, chap. 8), a meteorologist at the US Naval Academy, combed 250 years of scientific literature for recorded evidence of tertiary rainbows: he found just five examples. The conditions under which those five sightings occurred and a recipe for spotting tertiary rainbows has been published in *Applied Optics* (Grossmann 2011). It has been suggested that such rainbows could be spotted against dark clouds after a storm. Evenly sized drops were also a requirement.

The *Arbeitskreis Meteore*, the German association for the observation of atmospheric phenomena, went hunting for the tertiary rainbow and Michael Grossmann found one following a storm in Kaempfelbach, in south-western Germany (Grossmann 2011). Because the effect is so faint, a number of shots had to be taken and superimposed. A digital enhancement known as “unsharp masking”, was also required to reveal the tertiary rainbow.

Soon after, another rainbow hunter, Michael Theusner (Theusner 2011), caught another tertiary rainbow and its adjacent quaternary counterpart near Bremerhaven in northern Germany - after processing the images in the same way. The rare conditions that lend themselves to a nearly visible tertiary or quaternary rainbow, along with the processing required to make them apparent, means that amateur sky gazers are unlikely as ever to catch sight of one.

Tertiary and quaternary rainbows are so elusive because the intensity of the n^{th} bow decreases dramatically as n increases. A calculation within the Descartes model using Fresnel equations indicates that a secondary rainbow is about 2.4 times less intense than the primary rainbow (Calvert 2003). However, dispersion and the wave nature of light are not considered in this calculation. A more refined model using the theory of diffraction was derived by the Mathematician and Astronomer George Biddell Airy in the 1820s. Airy was able to express the intensity of the scattered light in the rainbow region in terms of a new mathematical function, then known as the *rainbow integral* and today called the Airy function and explained the dependence of the intensity of the colors of the rainbow on the size of the water droplets (Airy 1838, Airy, 1849).

Modern descriptions of the physics of the rainbow are based on Mie scattering (solutions to Maxwell’s equations describing the scattering of electromagnetic radiation by a sphere), a body of work published by Gustav Mie in 1908 (Mie 1908). Neither the mathematical form of the Airy function nor the more complex models used to explain intensities of multiple rainbows will concern us here, as these are far beyond the means of Scot and his period. It suffices that the $(n+1)^{\text{th}}$ bow is less intense than the n^{th} bow

and consequently the observation of tertiary and quaternary rainbows is *extremely* rare indeed.

Note that the four bows, consisting of two sets of two bows on each side of the sun, as photographed by Theusner, differ from Scot's description of four bows at slight (visual) distance from each other. The same applies for the reflected double rainbow. The supernumerary rainbow might seem to come closest to Scot's description but it is usually not so spread out in the sky. A historical and cultural analysis is needed here.

Known Rainbow Models at the Time of Michael Scot

Around 300 BCE, in his *Meteorology* (Aristotle 1984), Aristotle presented the first explanation for the formation of the rainbow. He attributes its formation to clouds on a hemisphere resting on the circle of the horizon reflecting sunlight to the observer where the angle is equal (to some constant angle – see Figure 13) and was the first to explain the rainbow's circular shape and the fact that the rainbow is not located at a definite place on the sky, but is seen in a certain direction.

The angular gap between the primary and secondary rainbow, illustrated in Figure 3, is Alexander's (dark) band, named after Alexander of Aphrodisias who first described it in 200 CE (Lee & Fraser 2001, 110-111). The Aristotelian theory of the rainbow made no allowance for refraction. The primary bow was believed to be caused by reflection, at a dewy cloud, of rays of sight from the eye which then were bent back towards the sun. The fainter bow was assumed to be made in the same manner. Reflection in the latter case takes place at a portion of the cloud much higher than that causing the primary bow. As the assumed reflection causing the second bow took place more obliquely at a greater distance from the eye, it seemed natural that its colors should appear paler. A third bow would have to be caused by a reflection from clouds placed at a still greater altitude.



Fig. 12 – Example of Quadruple Rainbow
Courtesy Robbie Gonzalez, io9.com, 2011.

From Boyer (Boyer 1987, 141), (Aristotle [Works of Aristotle] 1931)

Three rainbows or more are not found because even the second one is fainter, so that the third reflection can have no strength whatsoever and cannot reach the Sun at all.

Consequently, tertiary and quaternary rainbows simply do not exist in Aristotle's model. According to Raymond L. Lee and Alistair B. Fraser, "Despite its many flaws and its appeal to Pythagorean numerology, Aristotle's qualitative explanation showed an inventiveness and relative consistency that was unmatched for centuries." (Lee & Fraser 2001, 109).

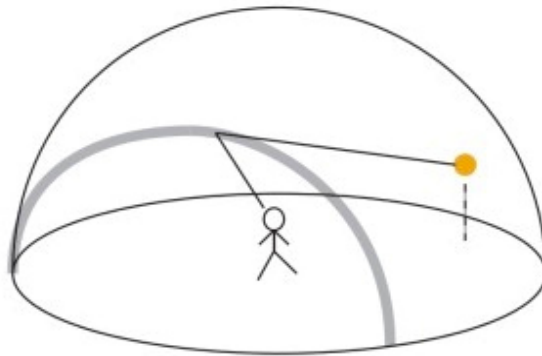


Fig. 13- Aristotle's rainbow model: clouds on the sky hemisphere reflect rays of sight. Courtesy of "The Aristotelian Rainbow: From Philosophy to Computer Graphics" by Jeppe Revall Frisvad, Niels Jørgen Christensen, and Peter Falster, Technical University of Denmark, ACM, 2007.

Scot had recovered the knowledge of Aristotle's rainbow in part thanks to Muslim scholars of the middle-ages such as Averroes (see e.g. (Topdemir 2007)) and Avicenna. Carl Benjamin Boyer described Avicenna's ("Ibn Sina") theory on the rainbow as follows:

Independent observation had demonstrated to him that the bow is not formed in the dark cloud but rather in the very thin mist lying between the cloud and the sun or observer. The cloud, he thought, serves simply as the background of this thin substance, much as a quicksilver lining is placed upon the rear surface of the glass in a mirror. Ibn Sina would change the place not only of the bow, but also of the color formation, holding the iridescence to be merely a subjective sensation in the eye. (Boyer 1954)

Significant developments in the middle-ages concerning scientific explanations for the rainbow include the contributions of the Persian physicist and polymath Ibn al-Haytham (also known as Alhazen; 965-1039 CE) and especially Kamal al-Din al-Farisi (1267-1319 CE), who lived later than Scot though. Farisi gave the first satisfactory explanation of the rainbow and had "proposed a model where the ray of light from the sun was refracted twice by a water droplet, one or more reflections occurring between the two refractions", also verified experimentally (O'Conner & Robertson 1999). Nonetheless, rainbow models at the time of Scot address the causes of the bows themselves or the source of their colors but never question the maximum possible number of bows in the sky i.e. two at most!

It is therefore seen that Scot's observation of four bows is outside of the body of thought concerning rainbows in both the ancient Greek as well as Muslim scholarships in his time. In such a case, where could his reported observation of four bows originate from? Presumably, from the places Scot lived and worked, Scotland or the wider area around Toledo, Spain.

Multiple Bows and the Tuareg

To the best of our knowledge, occurrences of four bows are not very common. If one considers the wider area in which Scot lived, such descriptions are only encountered in the Tuareg, a Berber people with a traditionally nomadic pastoral lifestyle and the principal inhabitants of the Saharan interior of North Africa. Mahmoudan Hawad (born 1950), a contemporary Tuareg poet and author (Chaker & Claudot-Hawad 1989) has observed parallel bows in the Sahara-Sahel region of Aïr, a triangular granitic mountainous region located in north central Niger, as recounted by his wife H el ene Claudot-Hawad (Claudot-Hawad 2002). He had witnessed multiple rainbows during his younger days in this region and was aware of Tuareg lore on the subject. These parallel rainbows were observed after thunder where the rain is able to appear at a distance. The Sahara-Sahel has a range of microclimate changes caused by the close proximity of deserts, oases, and green mountainous regions. Today, while the Aïr mountains are largely bare of vegetation, the dry wadi river valleys (known by the Hausa term "Kori") dissect the mountains, channel and hold rainwater in "gueltas" (stone pools) as shown in Figure 15, creating oases. Hot springs are found in the mountains, as are ancient rock carvings, a testimony to a much lush vegetation in the distant past. The sight of rainfall at a distance is possible from these mountains. In the desert, the rain can appear in the form of numerous flash floods of intense and short duration.



Fig. 14 - Tuareg in Sahara Desert. Courtesy PhotoBucket.

The Tuareg have been in North-Africa for thousands of years. William Langewiesche claimed their ancestors were the warriors with chariots pictured in Sahara rock art (Langewiesche 1997) which is abundant in the Aïr region. In the past few centuries the Sahara-Sahel region has faced drought (Brahic, 2012). Currently the Sahara Desert is apparently increasing in size and the region faces serious ecological issues (Schmidt, 2001). In the time of Scot however, one would expect a greater abundance of rain and a higher frequency of rainbow formation.

Given the endless precariousness of rain water, it is no coincidence that the rainbow, which is associated with rain, appears in the Tuareg mythology. It would further make sense for the Tuareg to discern patterns and divination techniques based on rainbows, in an attempt to make predictions of rainfall.

In the Tuareg language, the word for the rainbow is "tezzel ader" which means "(she) stretches the leg". In their cosmogony, the rainbow is the very picture of the metamorphosis ("tebedya"). It appears when, after lightning and thunder, rain cannot fall. This abortion of the storm is dangerous, disrupting the harmony

between the earth and the sky. While turning into a rainbow, the multicolored snake “stretches the leg” above an anthill (symbol of the world below) and the wasted energy of the storm creates the curve of a transient universe. This third ephemeral world is capable of pacifying the irregularity instituted by the absence of rain, a negation of the exchange relations between the two antagonistic and complementary parts of the universe, earth and sky. The absence of rain resembles the condition of “very little or no rain” in the description of Scot. Further, cloud formations in these regions match Scot’s description of “low mountainous clouds”.



Fig.15 – A *guelta* near Timia, in central Air, provides water throughout the year in an otherwise dry region. Courtesy Wikipedia Commons, 2006.

Possible Source of Unknown Etymology

At this stage, we are left to come to grips with the meaning of the word “*zalla*” mentioned by Michael Scot in his description of the multiple rainbow. We also mention other unknown terms by Michael Scot, in particular, two words to describe musical string instruments namely *ineba* and *senphonium* which “are not found in dictionaries and seem peculiar to Scot” (Thorndike 1965, 12) as part of a list which included the viola, psaltery, lute and harp. Michael Scot had his own etymologies, a natural complement to his translation activities. Nobody operates in a vacuum and it is unlikely that Michael invented these words entirely on his own and it therefore becomes important to understand his historical and geographical location, namely the Toledo School of Translation in the 12th and 13th centuries (eq. see (Kann 1993, Universidad Castilla 2012, Wightman 332, 1953)).

According to J. Wood-Brown, Michael Scotus was helped in his translations by a Jew named “Andrew” (Brown 1897)². Although the name “Andrew” is in doubt, there can be no doubt that Michael Scotus was helped by Jews who knew Hebrew, Arabic and Spanish when he made his translations of scholarly

² It has been conjectured that Andrew was a convert to Christianity. It is curious at any rate that the name given him was that of Scotland’s patron saint.

works from Arabic to Latin while in Toledo. Under the leadership of King Alfonso X of Castille (1221-1284 CE - known as “the wise”), Sephardic Jewish scientists and translators acquired a prominent role in the School. They were highly valued by the King because of their intellectual skills and mastery of the two languages most used in the translations: Arabic and Spanish (Muñoz Sendino 1949, 15).

Alfonso's nephew Juan Manuel wrote that the King was so impressed with the intellectual level of the Jewish scholars that he commissioned the translation of the Talmud as well as the Kabbalah (Castro y Calvo 1947, 2). Amongst these Jewish scholars, we cite the personal physician of King Alfonso himself, Yehuda ben Moshe ha-Kohen (Proctor 40, 1945, González, 1998) and especially members of the Ha – Levi family, such as Judah Halevi - actually Yehuda Ben Shemuel Ha-Levi (1075-1141 CE) a Jewish physician and poet (see e.g. (Kaplan 1993, 405-407)) - as well as Meir ben Todros Ha-Levi Abulafia (c. 1170-1244 CE) (Kohler *et al.* Jewish Encyclopedia, 1906) a major Sephardic Talmudist and authority on Jewish religious law and contemporary of Michael Scot. When Michael Scot was in Toledo, the school of translation was under the direction of the Archbishop Rodrigo Jimenez de la Rada who played a key role in the war against the Almohads and at the battle of Las Navas de Tolosa in 1212 CE (Pick 2004).

Kabbalah and its numerology system called “Gematria” was very popular in Michael's time and place. This is an ancient system which consists in assigning a number to each Hebrew letter and then summing these numbers for a given word. It is believed that identical numerical values bear some relation to each other or bear some relation to the resulting number itself. For clarification, it is not, by any means, our goal to convince the reader of the validity of Kabbalah, only that it was used in many studies in locality and time of Michael Scotus and is relevant to his etymology. Quite the contrary, we agree with the assessment of the Mathematician Barry Simon that e.g. there is, as yet, no real proof of a so-called “Biblical code” (Simon 1998). Rather, Hebrew as a language, has particular linguistic properties: when viewed as a mathematical basis for a language, it is *overcomplete*. For example, the words “Michael” and “Samuel” both end in “-el” which means God and so each Hebrew letter has a meaning in itself. Thus, when the original Hebrew words are constructed as compounded symbols, where each individual letter has meaning, it is not surprising that the Gematria system can produce a consistent complementary meaning to the word itself. In this context, Kabbalah with Gematria are considered as linguistic *etymological tools*, nothing more. This approach is not so unorthodox if one considers that Gematria has been used for coding (e.g. Atbash) and encryption. Concerning Michael's appreciation for music, according to “Music and Kabbalah” by Matityahu Glazerson (Glazerson 1996, 23)

When a person has a connection between the physical world and the spiritual world, he has a desire to sing. Singing is the result of the natural world (the number seven, the ז) joining with its higher root (the א). It is written in the book *Livnat HaSapir* (2 Kings, 3:15) that the reason a baby is pacified when he is sung to is because the singing reminds him of the root of his נשמה (*neshama*, soul), i.e. the spiritual world from whence he came.

The א represents the כתר (*keter*, crown), the highest sphere and the source of all the spheres. כתר, in Kabbalah, is called פלא עליון (*peleh elyon*, the sublime wonder). The word פלא contains the same letters as the word אלה. It is interesting to note that the numerical value of the word מוסיקה (*musika*, music: 40 + 6 + 60 + 10 + 100 + 5) is 221, which is the same numerical value of the word ארך, the term used for the sphere כתר. Even though מוסיקה is not actually a Hebrew word, our Sages also sometimes gave numerical values to foreign words. The reason is based on the principle that all languages are derived from and have a connection with Hebrew, the holy language.

Such notions are not accepted by many today and an analysis based on Jewish Gematria may seem unorthodox but it would certainly be acceptable by Michael Scot in his time and place especially as he was surrounded by languages such as Hebrew and Arabic in the city of Toledo. He appreciated etymology, numerology as well as music. Accordingly, our analysis for the three words of Michael Scot is as follows:

senphonium: looks very much like a Latinized version of an old Hebrew word “Sumponia” or “Cumponyah” סומפניא and corresponds to the Greek word “symphonia” meaning “symphony” (in modern Hebrew סימפוניה) or “symphonos” meaning “harmony”. However, Michael Scot used this word to describe



a string instrument, possibly a lyre (Thorndike 1965,12). The Hebrew word “Sumponia” is mentioned in the Book of Daniel (Dan 3:5), and corresponds to a bag-pipe:

“that when you hear the sound of the horn, pipe, lyre, trigon, harp, bagpipe, and every kind of music, you are to fall down and worship the golden image that King Nebuchadnezzar has set up.”

However, there is another view that it is not a musical instrument, but rather the collective of instruments in harmony (Encyclopedia Judaica v. 12, 563). If we apply Glazerson’s reasoning with Jewish Gematria, the Hebrew letters of “Sumponia” have respectively the numbers from right to left (i.e. from ס to א in סוּמְפוּנְיָא) are respectively 60, 6, 40, 80, 50, 10 and 1 which add up to 247. This is the same number as for “zemer” זִמֵּר which means singer and according to Glazerson relates to “neshama” (soul) as mentioned in the earlier quote and represents the “highest part of the spirit within man” (Glazerson 1996, 55). With both definitions as an instrument and harmony, this seems a likely source for Michael Scot’s word “senphonium”.

ineba: This word is much more difficult as it is not a Spanish word nor a Hebrew or Arabic word *per se*. Nor can we find it amongst the Berber Languages like Tamazight nor Tamasheq (the ancient language of the Tuareg). It is none of the Hebrew words string instruments for lute or harp such as e.g. “nevel” נֶבֶל nor “kinnor” כִּנּוֹר. It is thus a challenge. The Hebrew closest to it is “Annaba” אַנְנָבָה which also sounds very much like the Arabic word, a portal city in modern-day Algeria (which used to be called “Hippo Regius”, the birthplace of Pope Augustine, a major influential Church authority in Michael’s time and thereafter). The vowels “i” and “e” do not register in Jewish Gematria indicating a Latinization process. If we apply Jewish Gematria on the remaining letters “n”, “b” and the ending of the word נ, ב and ה, we get respectively 50, 2 and 5 which add up to 57. Michael Scot was fascinated by multiples of 7 but this number falls short of 77 which represents the “perfection of the number seven” according to Matityahu Glazerson (Glazerson 1996, 22) as found multiple times in the Bible. Michael Scot certainly knew Latin and would have realized that “ineba” would cognate with the Latin word for inebriation. This suggests an instrument used for light-hearted situations rather than solemn religious music. Though decidedly we have no actual proof, it nonetheless becomes tantalizing to think that Michael Scot may be actually describing the *guitar* which reached Europe by way of Spain and existed in various forms in Spain and throughout North-Africa. However, this remains a suggestion which we do not insist on, especially as it does not appear in the description of rainbows. This example is shown only to see how far we can push this analysis even for such an elusive example.

Zalla: “Zalla” is very similar to the Arabic “Allah” which not only means God but also the “whiteness of consciousness or light it is sometimes symbolized as clouds”. The term Allah is derived from a contraction of the Arabic definite article al- “the” and “ilāh” “deity, god”, to “al-ilāh” meaning the sole deity. This is similar to the Hebrew word “Elohim” i.e. deity and also means sky. A word which phonetically sounds similar is the Hebrew word “tzillah” צִלָּה which means shadow, shade, umbra, or darkness which translates into the Arab word “vallah”. Thus, it seems, the lightness of the cloud can be mitigated, tuned and even negated linguistically. It’s gematria value (from right to left) is 90, 30, 5 which add up to 125 and this number has a special meaning in Kabbalah though we are no longer dealing with music. There are the 125 “spiritual degrees that complete the correction of one’s soul” which are associated with “Rashbi”, the author Kabbalah’s chief work i.e. the Zohar (Laitman 2008, 9). Michael’s word “zalla” appears elsewhere (Thorndike 1965, 69) where he attributes colors of the rainbow to the color or darkness of a cloud following a quasi-Aristotle reasoning:

“These variations occur according to the varied dispositions of the clouds, which receive such impression from the sun’s rays, wherefore a fiery cloud makes a red color; a thin one of little substance, white; a *zalla* cloud, purple or blue and black or quasi-green or black like oil.”



Thus, the “zalla” cloud color seems to widely range from blue and green to black. The colors described here match the colors of the multicolored snake in the mythological Tuareg description.

Verdict

We finally present our answer to the question: what kind of multiple rainbow was Scot describing? Our answer is that it is a description from some form of weather divination lore generated and influenced by multiple eye-witness accounts of multiple rainbows over the ages. The observed phenomena were likely supernumerary rainbows and possibly the occasional reflection bow and quaternary bow. What leads us to this conclusion is as follows. Firstly, he was clearly reporting accounts from other people not his own personal observations. Secondly, just as the properties of the biblical manna do not perfectly match its proposed identifications such as the resin of the Tamarisk tree (Jewish Encyclopedia 1906) or the honeydew of certain insects, etc...., Scot's description does not fit perfectly into any of the three given categories of multiple rainbows. In form, Scot's parallel rainbows greatly resembles supernumerary rainbows but these are not localized around e.g. waterfalls, rather they located in the sky much like a quaternary rainbow. Scot's description is thus from third-party or rather mythological sources though, as is often the case, with a basis in reality.

Discussion and Implications

Michael Scot's record of multiple rainbows does not originate from Muslim scholarship of his time, in which knowledge on rainbows was hardly more advanced than that of Aristotle, but rather from observations either in Scotland, where he lived at a young age, the or in Sahara-Sahel and in particular the Aïr region, where the mountainous settings and weather conditions favor a more frequent realization of multiple rainbows. Such conditions do not materialize in Christian Europe which our conventional knowledge associates with Scot: Spain, Sicily and the rest of Italy, France and even Germany - given that Germany was part of the Holy Roman Empire under the jurisdiction of his patron Frederick Hohenstauffen II. If this specialized and rare piece of information does not originate from Scotland, it is highly possible that access to it has been gained through contact with the Tuareg. These desert roaming nomads are the only tribe in the wider region in which we place Scot that have a culture in which the rainbow has a central role. The Tuareg have a mythology surrounding the rainbow. Established in a wide region in North Africa over thousands of years, with a perennial concern for rain water, the Tuareg have further developed a system by which to make predictions based on the rainbow and have a “divination” technique based on multiple rainbows. Mythologies concerning rainbows exist in many cultures but only Hawad's description of Tuareg lore matches the description of Scot.

So, how did the Tuareg knowledge concerning the four bows reached Scot? The Sahelian kingdoms were thriving during the middle-ages because their wealth came from controlling the Trans-Saharan trade routes across the desert, especially the slave trade within the Islamic world. Their power came from having large pack animals like camels and horses that were fast enough to keep a large empire under central control and were also useful in battle. Camels could travel in valleys, often dry river-beds called “wadis” (or “oueds”) and ultimately at a much faster rate than horses could travel over long distances in Europe. These Sahelian kingdoms supported several large trading cities in the Niger Bend region, including Timbuktu, Gao, and Djenné. The distance between Morocco and Béjaïa (Algeria) where Fibonacci resided could be bridged in a matter of months.

Given in part the connection between Michael Scot and Leonardo Fibonacci concerning the Fibonacci numbers (Scott & Marketos 2014) and the known historical record that Leonardo Fibonacci had been stationed in Béjaïa, located on the coast of modern-day Algeria, as part of a Pisan trade colony, with sea trade routes all around the Mediterranean, the outcome of this analysis implies that Michael Scot would have ventured into North-Africa, at least as far as Morocco (which is not that far from Toledo) and quite possibly southward where he would have learned about multiple rainbows and the lore surrounding them from actual contact with the Tuareg people. There does not appear any other way by which Michael could have obtained this very specialized and extraordinary bit of information as it does not appear in Muslim scholarship or the models of Aristotle at any time. Mythologies surrounding rainbows exist in many cultures

but only Hawad's description of Tuareg lore matches the description of Michael Scotus. Moreover, quaternary rainbows are *extremely rare* and only a people established in a given region over thousands of years with a perennial concern for rain water would develop a belief system by which to make predictions based on e.g. the number of parallel bows in the sky. Nor is there any other group apart from these desert roaming nomads that have such a mythology surrounding the rainbow. Michael Scot might have been aware of supernumerary or reflected rainbows from Scotland but only the Tuareg have a "divination" technique based on multiple rainbows and were relatively near his whereabouts in Toledo, Spain (or Sicily for that matter).

Possibly, a trek to North-Africa would have also included a meeting with Leonardo Fibonacci concerning quite possibly the translations by Gérard of Cremona of the works of Al-Khwārizmī and Abū-Kāmil used by Fibonacci himself for his book *Liber Abaci* (Scott & Markeos 2014). Although, Béjaïa lies north of the Air mountains, we do not claim that Michael Scot actually reached this region, nor Béjaïa itself, only that he had communications with Leonardo de Pisa and the Tuareg tribesmen (which means he had to reach Morocco at least).

Apart from the question of where, there is also the question of when. Even though Leonardo Fibonacci and Michael Scot were part of the court of Frederick II, each of them was much older than Frederick and of the same generation with respect to each other. Michael Scot was not part of Frederick's court until after 1223-4 CE being the dates Pope Honorius tried to get Michael a position in Cashel, Ireland (which Michael subsequently refused). Note that the chronology of J. Wood Brown (Brown 1897) is incorrect: Michael Scot was not in Sicily before traveling to Spain: it was the other way around. The first official translation and recorded date of Michael Scot was in 1217 CE (Thorndike 1965) and a conjecture made by Charles Burnett suggests that he might have been in Spain as early as 1200 CE or maybe even earlier as a young man (Burnett 1994, 101-126). The first version of Leonardo's *Liber Abaci* was written in 1202 CE while the second version of Leonardo Fibonacci's famous book was dedicated to Michael Scot in 1227 CE. Thus, both scholars, Michael Scot and Leonardo Fibonacci were active long before they were part of the court of Frederick II and could have met any time between 1200 CE and 1217 CE.

The French writer and historian Henri Daniel-Rops once said that history is always a conjectural science. For example, it is amazing how much science and applied science in archeology can debunk some of the most conventional notions. Even though, there is no historical record, the record of multiple rainbows by Michael Scot indicates that his reputed thirst of knowledge starting in early boyhood (Thorndike 1965) would have made him venture into "enemy territory", namely the world under Islamic jurisdiction around the time of the Crusades. The outcome of the present work suggests an even greater penetration into the world under Islamic jurisdiction on the part of Michael Scot than previously thought. The contradictory nature of the Tuareg observation of multiple rainbows and the known Science in his time could very well have prompted Michael Scot to realize that the knowledge of the ancient Greeks and the Muslims was imperfect and that experimentation was needed.

Acknowledgements

I would like to thank H el ene Claudot-Hawad for relating the memories of her husband Hawad concerning the description of multiple rainbows in the Sahara-Sahel region. Special thanks to Kshama Zingade of Near.co, Rick Gould, Johannes Grotendorst, Pan Marketos, Yair Zami of the University of Ben-Gurion in the Negev, Israel, Lucille Gear, Susan Peppiatt, Howard Pederson, Marc Moyon, David Harper, Daniel Foor, John Carosella and Awo Falokun for invaluable help in confirming some of the information herein. Special thanks go to the dear departed Carlos Klimann of l'INRIA in France who brought me to the French library known as the *Maison des Sciences de l'Homme* and introduced me to the wonderful work of Charles Burnett and his colleagues, on Michael Scot, and also, to H el ene E. Hagan, author of *The Shining Ones*, for introducing me to H. Claudot-Hawad.

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Transversal: International Journal for the Historiography of Science, 2 (2017) 226-232

ISSN 2526-2270

www.historiographyofscience.org

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Article

Galileo and the Medici: Post-Renaissance Patronage or Post-Modern Historiography?¹

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Abstract:

At the beginning of the eighties of the last century, the issue of “patronage” began to arouse scholarly interest and gained importance. Galileo became a test case: his importance, and the importance of patronage – and that of the Medici in particular – go beyond the historical junction of the scientific revolution and have corollaries in the more general attitude to science and knowledge. This case furnished a new line of research for the historical sociology of science. As far as Galileo is concerned, my claim is that the new trend belongs to post-modern historiography, rather than to post-Renaissance Medici patronage.

Keywords:

Galileo; Medici; patronage; post-modern historiography

226

Received: 13 February 2017. Accepted: 03 April 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.17>

At the beginning of the eighties of the last century, the issue of “patronage” began to arouse scholarly interest and gained importance. Galileo became a test case: his importance, and the importance of patronage – and that of the Medici in particular – go beyond the historical junction of the scientific revolution and have corollaries in the more general attitude to science and knowledge. This case furnished a new line of research for the historical sociology of science. As far as Galileo is concerned, my claim is that the new trend belongs to post-modern historiography, rather than to post-Renaissance Medici patronage.

In 1985 the late Richard Westfall published an interesting article in *Isis* under the title “Galileo and the Telescope,” arguing that Galileo’s main concern then was not so much astronomy as the telescope’s capacity to ensure his own future at the Tuscan court (Westfall 1985). Westfall lamented that quite generally, the history of science had been excessively dominated by nineteenth-century concerns. He suggested drawing more on seventeenth-century ideas, whereby “the subtle alchemy of patronage transmuted an object of science into an *objet d’art* to amuse and flatter a prince” (Westfall 1985, 15). He concluded that patronage could well have been the most pervasive institution of pre-industrial society, as well as an avenue leading us into the fruitful social history of the scientific revolution. He thereby offered a solution to a rarely mentioned historical problem: today scientists are academics or employees in industry, and both kinds of

¹ This article is based on a lecture held at the Bar-Hillel Colloquium for the History, Philosophy and Sociology of Science, Jerusalem, on March 9, 2005.

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scientists scarcely existed prior to the scientific revolution (Segre 2015). What, then, was their economic base? Answer: they were private pensioners of rich and powerful patrons.

Westfall's claims are interesting – although doubtful – and they deserve serious consideration. His challenge was welcome. As one result, meticulous investigations of Galileo's ascent to the Tuscan court were conducted, albeit not until twenty years after his article had appeared. To use the contemporary jargon, historians of science and of the sociology of science tried to reconstruct the "strategies" involved, such as "microphysics of patronage" and "self-fashioning of a client versus his patron," flavoured by a variety of "practices" drawn from realms such as etiquette, rhetoric, art, mythology, and even emblems.³ The conclusion was that science would not have evolved the way it has but for the kind of patronage that Galileo had inaugurated. Some historians went so far as to claim that in Galileo's case and in Early Modern Europe in general, patronage and science were more or less coextensive.

Two difficulties throw doubt on these views. The first is historical: In the Galilean case, patronage ultimately played a relatively restricted role, if any, in the advancement of science; his great contributions were quite independent of the patronage that he enjoyed. The second difficulty is philosophical: patronage – particularly when offered by a potentate to a courtier – came with the demand for a measure of conformity. How can this be reconciled with science's demand for freedom of thought, which is essential for scientific innovation? Indeed, even Galileo suffered pressure to conform, and it took a strong character and a brilliant intellect to overcome it. And so it is tempting to suggest that scientists are under pressure to conform, and only those who overcome this pressure have any chance to innovate. This is a romantic, Kuhnian view that scarcely squares with the complex and varied historical record (Kuhn 1996 [1962]).⁴ Without belittling the importance of patronage, let me call for caution; occasionally it may have been overemphasized following an attempt to conform to a certain historiographic trend.

Galileo and Patronage

At the beginning, Galileo's move to the Tuscan Court, his acceptance of a patronage, was advantageous to both sides, and to the Medici even more than to him. In a period of utter decline, the presence of Galileo at their court enhanced their prestige: they could present themselves as patrons of the new, emerging science and pursue the traditional cultural policy of their dynasty. But this was no more than a temporary aura and a luxury that they could dispense with. In the long run, his presence at court did not produce any particular advantage either to the Medici or to Tuscan culture.

Galileo's benefit was mainly financial; the Medici granted him enough leisure to concentrate on his scientific work with no teaching duties. His remuneration, incidentally, was paid not by the court but by the University of Pisa – a fact that raises a question concerning the extent and nature of the Medici patronage (Galilei 1890-1909, 233-264). Independently of the source of money, Galileo got all he asked for and more: in addition to good financial support and no teaching duties, he was able to take advantage of all the services a court could offer. Furthermore, he was totally free to proceed with his work – an exceptional situation as we shall soon see, especially in Tuscan post-Renaissance patronage.

Nevertheless, the outcome was disastrous. In 1616 the Catholic Church prohibited Galileo from teaching heliocentrism, and in 1633 the Roman Inquisition sentenced him to life imprisonment. The Grand Duke of Tuscany was only able to offer him his carriage to go to Rome and to put at his disposal the services of his embassy. History cannot rest on subjunctive conditionals, but these have their role to play. So let us note that all this might not have happened had Galileo remained a well-paid civil servant of the relatively strong and independent Republic of Venice. Moreover, much of Galileo's contribution to science was made prior to his return to Tuscany: his major work, the *Dialogue* of 1632, is essentially a popular presentation of previous thoughts, admittedly on the highest literary level, but still scientifically not very innovative. Even

³ A substantial contribution to this literature is made by Mario Biagioli, *Galileo Courtier: The Practice of Science in the Culture of Absolutism* (1993). He describes Galileo's science as part and parcel of his career and self-fashioning at the Tuscan court. The book has been debated, battles have been fought, and the History of Science has moved on. The inclination to conform, however, is always present.

⁴ Kuhn allows scientific leaders to be sufficiently nonconformist to break the framework occasionally, but "normal" scientists conform both in following the paradigm and in switching allegiance to a new one when told to. See Segre (2016).

without Galileo's campaign, the new astronomy would have established itself thanks to the contributions of great thinkers such as Kepler, Huygens, and Newton – and possibly in a less traumatic way. Taking Galileo as a test case, then, scales down the importance of patronage.

Why, then, give so much importance to patronage? To answer the question, one should consider a broader historical and historiographical context. As far as history is concerned, a look at the development of the Medici patronage could be helpful, and this requires extending the discussion from history of science to history of art, and more specifically to history of art patronage.

The basic question is:

What Was the Purpose of the Medici Patronage?

Any question about patronage can be too ambiguous to receive a clear answer, as the very concept of patronage is both broad and ambiguous. Collins *English Language Dictionary*, for example, defines it generally as “help and financial support given by someone to a person or group,” to enhance enterprises such as science, art, or culture (Collins 1987). Consider then, for instance, a later case – that of Luigi Galvani. After resigning his chair at the University of Bologna to avoid taking an oath of loyalty to the Napoleonic Cisalpine Republic, Galvani was sheltered by his brother. Can we call this patronage? And can one call the salary that the University of Pisa grudgingly paid Galileo “court patronage”?

The issue gets even more complex if one considers that the Medici patronage began in the fifteenth century, lasted three centuries, and involved the arts and letters at first and the sciences only later. Although the literature describing patronage is enormous, interestingly it overlooks, to the best of my knowledge, the question of whether the stipend that the University of Pisa paid Galileo was a patronage proper. It seems this literature even overlooks the basic question of the purpose of this stipend or similar ones.

Even with no expertise in the history of the Medici or the history of art, it is not hard to notice that the style of patronage varied in different times and under different rulers. It is likewise not hard to notice that the tradition of patronage began as a successful private enterprise and developed into a less successful state project. The first members of the Medici family to rule Florence were bankers whose motivation was protecting their finances. The complex structure of the Florentine *comune*, which some historians call a “League of Mafia families,” needed – *inter alia* – good terms with artists and their guilds (Trexler 1980, 27).

The first famous Medici “godfather,” in the first half of the fifteenth century, was Cosimo the Elder, a high-style businessman and a shrewd politician who promoted the arts in an enlightened way. He gave his clients total freedom, at least occasionally, even if this meant the deliberate overriding of current morality. An example of this is Donatello's “David,” which he commissioned in 1434, and which was the first life-size nude to be cast in bronze since Classical times, with a playful, sensuous, and androgynous body.

Cosimo instituted his patronage for diverse reasons. It seems he had religious motives in addition to his personal taste and his interest in local politics: as his earthly enterprises were not always spotless, he may have hoped to redress the balance with pious deeds. One of his major sponsorships was the restoration of the Dominican Cloister of San Marco in Florence. In its cells one can still admire Beato Angelico's wonderful, meditative frescos, and Cosimo's own cell testifies to his spiritual concern.

The heritage of Cosimo the Elder reappeared in his legendary grandson, Lorenzo “the Magnificent,” who supported artists and men of letters both in his own personal interest and in that of the state.

On the personal level, Lorenzo was a learned individual. He enriched the Medici libraries with rare manuscripts, collected rarities, and sponsored crafts neglected by traditional patrons. On the political level, Lorenzo was more ambitious than his grandfather and endeavored to win for Florence and its scattered territories the cultural leadership of Italy. He used art and artists for diplomatic and propaganda purposes, and strengthened ties with other princes and states by offering artistic advice and art objects and by recommending artists.

Yet just half a century ago the leading French historian André Chastel argued, under the provocative title “*le mythe de la Renaissance: age d'or et catastrophes*” (“The Renaissance myth: age of gold and catastrophes”), that Lorenzo's time had ran far less smoothly than his legend suggests. The quality of intellectual production at the time was lower than that of the earlier Renaissance. Also, Lorenzo's cultural enterprise seems to have been excessive at times and a burden on the family's and state's finances (Chastel

1492 – raised Savonarola to power and forced the Medici family into temporary exile.

Interestingly, Savonarola came from the very San Marco Cloister that Cosimo the Elder had restored, and his Dominican followers in San Marco initiated a reactionary, anti-humanist, and later anti-Copernican trend directly related to Galileo's trial.⁶ The first serious challenge to Galileo and to Copernicanism originated in San Marco in 1614. And this is but one example of the unpredictability of the results of patronage.

Incidentally, the Florentine decline at the end of the fifteenth century is depicted in a clear manner, with a nuance of sadness, in Botticelli's later works. This decline is the reason why leading high Renaissance artists from Tuscany, including Michelangelo and Leonardo, had to try their luck elsewhere. A century later, much the same happened to the young innovative mathematician and academic Galileo, who went to Padua.

Thus, it seems clear that at least part of the aura surrounding Lorenzo's patronage is an artificial production – his own or that of later historians. Moreover, during the period in which Italian rulers were competing with each other to raise their prestige and embellish their cities and palaces, European nations were taking shape. It was an epochal change that heralded the modern era. Fine arts could contribute little to assist the Italian principalities to keep up with these developments. Niccolò Machiavelli foresaw all this at the beginning of the sixteenth century and suggested political remedies.⁷ He offered the Medici his advice, together with his good offices, when they returned from exile; they maltreated him and shoved him aside.

That is the picture of Tuscany at the beginning of the sixteenth century. During this century that is described as post-Renaissance, the Medici became the absolute rulers of most of the Tuscan territory, received the titles "Duke" and later "Grand Duke," and hoped, at times pathetically, to obtain a royal crown. They deluded themselves that culture could be instrumental in achieving this ambition, and this opened an interesting new chapter in their patronage: post-Renaissance patronage.

The main aim of the first Tuscan Duke, Cosimo I, was to emulate the great European powers, Spain in particular (Forster 1971; Segre 1991a, 7-9, 144-145). Cosimo I, unlike Cosimo the Elder and Lorenzo the Magnificent, was no intellectual. His goal was not so much to encourage culture as to use it in his effort to glorify Tuscany and his own self. He introduced a complex art of patronage which was carried on by his heirs and lasted over a century.

Briefly, Duke Cosimo I raised culture to the status of a major official state project. This included financing Tuscan cultural institutions (universities and academies in particular), and at the same time putting them under strict state control so that they could serve political purposes. Cosimo I also used culture as a means for the prevention of possible opposition.⁸ Among the projects that he invested in as methods for the glorification of his dynasty, which fitted well into the absolutist extravagance of his day, were botanical gardens that he opened and new university chairs that he established to attract leading scholars. He tried in vain to bring Andreas Vesalius, the leading anatomist and physician to Charles V, to the University of Pisa.⁹

Most importantly, as far as historiography is concerned, Cosimo I allowed an artistic genius, Giorgio Vasari, to supervise the state's artistic interests. One of Vasari's undertakings at court was to glorify Tuscan culture and art under the patronage of the Medici. His celebrated *Lives of the Artists*, considered the beginning of modern history of art, was soon criticized for paying too much attention to Tuscan art and neglecting art produced elsewhere on the peninsula.¹⁰ Vasari used his literary ability to inflate and spread

⁵ Cf. Melissa M. Bullard's outstanding *Lorenzo the Magnificent. Image, anxiety, politics and finance* (1994).

⁶ One of the main anti-Copernican figures in this trend was Giovanmaria Tolosani (c. 1470-1549). See Camporeale (1986).

⁷ In *The Prince (Il Principe)*, written in 1513 and published for the first time in 1532). Chapter 24 is "an exhortation to liberate Italy from the barbarians," expressing the wish that Italy become a great European nation.

⁸ For Cosimo and the academies, see Cochrane (1983). For Cosimo's reform of the University of Pisa, see Marrara (1965).

⁹ For a detailed biography of Vesalius, see O'Malley (1964, 203) on Charles V and Vesalius. See also Galluzzi (1980) arguing that the Medici's patronage of science, just as in other domains, has been mystified: p. 194 on Vesalius.

¹⁰ Carlo Cesare Malvasia (1616-1693), a Bolognese historian of art, for instance, in his *Le pitture di Bologna* (1686, 1-2), criticized Vasari, among other things, for neglecting non-Tuscan artists, particularly from Bologna, see Malvasia (Reprinted 1969).

the myth of the Medici as enlightened patrons.¹¹

Incidentally, Vasari played a posthumous part in the creation of the Galileo myth (he died in 1574 when Galileo was ten years old) as Galileo's influential follower and earliest biographer, Vincenzo Viviani, adopted his hagiographical style of writing in his *Vita* of Galileo in 1654 (in Galilei 1890-1909). One sentence of his biography is an almost exact copy of one from Vasari's *Life of Michelangelo* (Segre 1989; 1991a, chap. 7).

The image of Tuscan culture created by Vasari and the Medici post-Renaissance patronage in general may have been impressive, but it utterly failed to achieve their political aims. The country was sinking into irreversible political, artistic, and cultural decadence. Intellectuals and artists there were forced to work under strict control. This was naturally unpopular and harmful (Forster 1995).

This, then, was the state of affairs when Galileo joined the Tuscan court in 1610 – much tradition, some decadent splendor, and very little substance. Understandably, Grand Duke Cosimo II was very happy to have Galileo at court. As an exception to traditional, post-Renaissance Medici patronage, he imposed no restrictions on him. Yet, to repeat, even this did not help. Cosimo's mid-seventeenth century heirs tried to return to the traditional post-Renaissance policy of control, censoring the work of the Galilean followers whom they supported (Galluzzi 1980; Segre 1991b). But this, too, did not bring the desired results, as one of the last prominent members of the dynasty, Prince Leopold de' Medici, frankly admitted.¹²

All this renders very questionable the claim that patronage and science were at that time more or less the same thing. It requires imagination, or perhaps faith in somewhat speculative theories in sociology and anthropology, in addition to taking for granted past historical descriptions that have been exposed as more legend than truth. How was this possible?

From Post-Renaissance Patronage to Post-Modern Narrative

In the nineteenth century, as William Whewell's monumental *History of the Inductive Sciences* (1837) was giving much prestige to modern history of science, Galileo was still largely depicted as the mythical martyr and founder of experimental science. The only social aspect of his work considered was his trial, which suited the anticlerical feelings of the day (Segre 1998, particularly 393-396). Yet as the field developed, historians began wondering what criteria to adopt when choosing among historical facts. Also in the same century, the founder of positivism, Auguste Comte, invented the term "sociology" and Émile Durkheim, one of the leading thinkers who established this field, asserted that "social facts" (to use his term) are the basis for all human action. Among the countless historiographies suggested, the presentation of science as a social occurrence began gaining ground. Thomas Kuhn was perhaps most instrumental in establishing it in the second half of the twentieth century. Kuhn drew attention to the relevance of the social aspects of science and particularly to its professionalization.¹³ (It is nevertheless hard to view mathematicians such as Copernicus, Galileo, or even Newton as scientific professionals.)¹⁴

Historians of science became increasingly interested in the social developments related to science, and instead of chronological facts and discoveries, began speaking of "practices." Substituting practices for facts moved the discussion to the sociological domain, but the difficulties could not be surmounted. The hoary issue of experimenting, for instance, re-emerged, with the difference that instead of speaking of experiments, one spoke of "practice of experiments" and concentrated more on the experimenters than on the experiment itself.

The myth of Galileo as the founder of experimental science and martyr of science, like Viviani's hagiography of Galileo, or Vasari's myth of the artistic patronage of the Medici, all share one problem that post-modern terminology calls "conflict between science and narrative." This is the starting-point for the

¹¹ See Forster (1971) and Rubin (1995). Rubin's book is a detailed study of the composing of the two editions of Vasari's *Lives*: on Vasari's glorification of the Medici, see pp. 197-208.

¹² Prince Leopold de' Medici promoted the work of the Accademia del Cimento between 1657 and 1667. For an English outline of the work of this Academy, see Middleton (1971). For the prince's disappointment, see p. 316.

¹³ According to Kuhn (1996 [1962]) a scientific revolution is a change in paradigm, whereas science administrators decide what the next paradigm is.

¹⁴ Joseph Agassi, in his recent, masterly, *The Very Idea of Modern Science: Francis Bacon and Robert Boyle* (2013), presents, *inter alia*, modern science as an amateur movement.

French philosopher Jean-François Lyotard, who, in 1979, published his influential book on *The Postmodern Condition: A report on Knowledge*, in which he proposes a post-modern approach (Lyotard 1979 and 2004).

“Post-modern” is a concept even vaguer than “patronage.” The term was invented in the nineteenth century in relation to art and was adopted in different fields with different meanings. It distances itself from what it considers the monolithic approach of modernity, no longer pertinent in a culturally diverse and fragmented world such as ours. It has produced interesting results such as *Learning from Las Vegas* by Robert Venturi *et al.*, where, in a study of Las Vegas’ architecture, the authors show how a mixture of styles can be attractive (Venturi; Brown; Izenour 1977).

Lyotard and others express disillusion with the Enlightenment’s rationality and reject absolute standards and truth: knowledge can only be relative (knowledge and rationality are indeed relative, but not the truth.) They favour and advocate narrative without any meta-narrative (i.e. narrative without a theory of rationality). They consider science as strictly human, and therefore suffering from human bias; This is indeed true – and so the truth is not easily accessible.

Westfall’s article was well accepted in the new trend despite, or perhaps thanks to, the ambiguity of his claims. Assuming, for instance, that Galileo thought first of his career, as Westfall claims, this does not mean that patronage was as important or as pervasive as Westfall claims. Post-modern narrative can, however, digest, and even welcome, irrational and confused discourse. Westfall’s article received a prize from the History of Science Society and inspired many other works that emphasise the importance of the Medici.¹⁵ Incidentally, quite a few articles that were awarded prizes by the History of Science Society in the following years were clearly post-modern.¹⁶

Conformism, then, is the common denominator between patronage and post-modern historiography, or so it seems, and that is why a post-modern historian would exalt post-Renaissance patronage with a clear conscience. It still is difficult to agree, and it is still much more helpful to apply the more modest approach of formulating specific questions and attempting to answer them in their immediate context and discuss them as critically as we know how.¹⁷ Had historians posed the question, for example, of what was the purpose of the Medici patronage, or other related specific questions, they would be in a much better position to give a balanced judgement. And this would probably indicate that patronage had more incidental consequences than have been presented in the past thirty years.

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¹⁵ The article was awarded the 1997 Zeitlin-Verbrugge Prize by the History of Science Society.

¹⁶ An example of a post-modern formulation is Findlen (1993). The author suggests (p. 41) labeling Francesco Redi – a naturalist working at the Medici court in the middle of the seventeenth century – rather than a leading contemporary biologist, “a courtier who deployed the natural and human resources that his environment offered to shape experimental narratives that met the expectations of a patrician and largely court-based audience” (*sic*). Redi’s works and manuscripts still show him to be a leading biologist; yet Findlen’s description is an example of trendy, post-modern jargon.

¹⁷ As suggested by Karl Popper, for an historiography following Popper’s philosophy of science, see Agassi (1963; 1975). Agassi argues for the superiority of a Popperian approach compared mainly over the inductivist and conventionalist ones. This is further emphasized in Agassi’s collection of articles under the title *Science and its History: A Reassessment of the Historiography of Science* (2008).



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Transversal: International Journal for the Historiography of Science, 2 (2017) 233-237
ISSN 2526-2270

www.historiographyofscience.org

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Interview: Helge Kragh¹



Born in Copenhagen in 1944, Helge Kragh has been emeritus professor at the Niels Bohr Institute (Denmark) since 2015. He is a former professor of history of science at Aarhus University, University of Oslo and Cornell University. Kragh holds doctorates in science and philosophy. His publication list includes more than 600 items, written for specialists as well as the public. Most of his research is in the history of 20th century physics, chemistry, astronomy and cosmology, that is, in the history of the physical sciences since about 1800, but he has also contributed to the history of technology, science-religion studies, and for the special interest of *Transversal: International Journal for the Historiography of Science*, historiography of science. Kragh has been active in the organization of International History of Science and is a co-founder of the *European Society for the History of Science*. In 2008-2010 he served as president of this organization.

Prof. Helge Kragh at Lille (France) in July 2015

Interviewed by

Gustavo Rodrigues Rocha² in April 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.20>

Gustavo Rodrigues Rocha (GRR): You published a book titled *An Introduction to the Historiography of Science* in 1987 which is still to date a work unique in its kind. How did you come to write it?

Helge Kragh: At the time I was a high-school teacher of physics and chemistry, without any kind of training in history, historical method or history of science. But I felt a need to understand the history of science in its broader contexts and especially its historiography and relation to general history. I think the book was basically an attempt of self-education and to understand what is special about history of science and its methods. I read a great many books without finding one that satisfied my needs and so I decided to write one myself.

GRR: Many years after your book on historiography of science: what is the place of historiography of science in the history of science? Where is the current historiography of science heading towards?

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Helge Kragh: These are complex questions. For the last several decades there has been much interest in integrating history of science and general history, or coordinating the two, and today many historians of science are trained in history. I consider this a healthy development. There have been various trends, from social and cultural history over constructivist history to so-called contextualist history. There is probably no general trend or tendency in current historiography of science except that some kind of contextualism is characteristic for much history of science published in monographs and the more prestigious academic journals such as *Isis*.

GRR: You have been interested in the science-religion dialogue, being a board member of the Science-Theology Dialogue Forum, and having written fascinating books on these topics such as *Matter and Spirit in the Universe* (2004) and *Entropy Creation* (2008), and published compelling papers on subjects such as Pierre Duhem and Catholic faith, and natural philosophy, theology, and cosmology. Religion can play an important role in how people think about the world, is it okay if I ask you about the religious environment that you grew up in? What religion (if any) were you brought up in? Did your religious views change over time?

Helge Kragh: I was not brought up in a religious milieu but was (like most Danes) born into the Lutheran-Protestant church. Religion did not play much of a role and when I was in my early twenties I left the church; not because I became an atheist but just because of lack of interest and a certain dislike of organized religion as practiced in my country. My interest in religion is of relatively new date and mostly a result of my studies in history of science which showed how important Christian religion has been for the development of science (and at some stage also Islam). Especially after I turned toward history of cosmology I began thinking about religion in connection with, for example, the perennial question of the origin of the universe. Although I do not believe in traditional religious dogmas I have sympathy and respect for religious thought whereas I have no sympathy for hard-core atheism and materialism. Somehow, it seems to me, there must be something above and beyond the physical universe, a mystical spirit or divine principle. If this principle is called God, I believe in God. But this god has no interest at all in human beings. In a sense, my kind of religiosity is somewhat the same as the one Einstein expressed on various occasions.

GRR: You have also cosmology as one of your primary interests and have published an entire book on grand theories in physics, namely, *Higher Speculations* (2011). It is not uncommon for people to search in cosmology some insights on big questions or some sort of big picture. It's an inclination not far from theology for instance. How do you think the science-religion dialogue interest and your passion for cosmology (and maybe grand theories in physics) are all related in your works? Would you say you have a common motivation driving all these interests and somehow bringing them together? If yes, how so?

Helge Kragh: As mentioned, my interest in religion is indeed related to my interest in cosmology but mostly through the scientists and philosophers who have thought about the universe. To understand these thoughts and their relations to theological questions as they have developed since early Christianity, one needs to know about theology. I am not so naïve to believe that I can say something original about the origin of the universe, for example. But of course I have thought and written about it. I am rather sure that the ultimate origin of the universe cannot be explained in scientific terms. That is impossible. From this one cannot infer a creative divine being, however. And even if such a being existed (which is an appealing possibility) the God-hypothesis rests on faith and cannot possibly be justified scientifically. I share the belief of most experts that one cannot use science in the service of religion, nor religion in the service of science. By and large I am a supporter of what is called the "independence thesis" in the science-religion discussion.

GRR: You wrote a biography of Dirac published in 1990. You also wrote about 25 year later, after your first comprehensive and detailed account of Dirac's life and contributions to science, a second book about Dirac's legacy in cosmology and geophysics, namely, *Varying Gravity* (2016). How did you get interested in Dirac's scientific biography? What about Paul Dirac fascinated you so much?

Helge Kragh: I actually also wrote a third book on Dirac, a small and popular one called *Simply Dirac* and published in 2016. For a long time ago I became interested in the early attempts to formulate quantum mechanics in agreement with relativity, which led me to the Schrödinger equation and the Klein-Gordon equation; from there it was natural to examine the origin of the Dirac equation and the physicist who found it. At the time when I started my work on the biography, Dirac was still alive but I failed getting in contact with him. He was a remarkable scientist and a person whose life, science and mentality fascinated me. Apart from being a genius, he also had a peculiar personality. And of course, by following Dirac's career one also follows important parts of the development of modern physics. I found his cosmological hypothesis to be particularly interesting even if we know today that it is wrong. Another aspect of his work which attracted my interest was his idea of so-called beautiful mathematics in physics. But contrary to Dirac, I do not think there is much substance in the idea.

GRR: You were appointed professor of history of science and technology at Aarhus University, Denmark, in 1997, where you remained until very recently moving to the Niels Bohr Institute in 2015 (being there an emeritus professor ever since). Was this position at Aarhus University eagerly anticipated? Did they have a chair in the history of science and technology at Aarhus University before you arrive?

Helge Kragh: I had no connections to the History of Science Institute in Aarhus before I was employed in 1997, except that I served for one year as curator of an associated museum of history of science and medicine. But I had previously been associate professor at Cornell University, USA, and full professor of history of science at the University of Oslo, Norway. At Cornell I had an office next to the one of Hans Bethe, the famous physicist. Most of the time since my graduation, I had worked as a high school teacher. Aarhus University had since about 1965 had a chair in history of science, occupied by Olaf Pedersen who was a specialist in medieval and ancient exact sciences. It was his chair I took over in 1997, when I terminated my position in Aarhus. The chair has now been replaced by a professorship in science studies.

GRR: You published in 1999 what I consider to be the finest one single volume on the history of physics in the 20th century, namely, *Quantum Generations* (1999). Was this a long-term project? When did you start writing this volume? What were your motivations behind *Quantum Generations* (1999)? How was the reception of the book?

Helge Kragh: It was not my idea to write the book, but Princeton University Press wanted a book in connection with the turn of the century and they asked me to write it. So I wrote the book pretty quickly, it took me about a year's hard work I think. I rather liked it because I had myself missed a broad and comprehensive account of the development of modern physics. The book was quite successful and has been translated to five or six other languages, including Japanese and Chinese. It has also been used for courses in history of science. Some reviewers thought there was too much social history in it, while other reviewers found it to be too technical and internalistic; others again found that there was too much about quantum physics and too little about materials physics. But I could not please everyone. It is probably the most sold of my books, but the best one, in my own estimation, is *Cosmology and Controversy* from 1996.

GRR: You have also helped to found the European Society for the History of Science around the early 2000's (having been its president during the term 2008-2010). How was that? What main roles did you play in its foundation?

Helge Kragh: I had earlier served as assistant secretary for the International Union of History and Philosophy of Science (IUHPS, Division of History of Science), so I had some experience with organizational work. The idea of creating the European Society for the History of Science (ESHS) was due to the French historian Claude Debru, and not to me. But I supported the idea from the beginning and was active in the process that made ESHS a reality. For several years I had worked

as editor for the journal *Centaurus* which later became the official journal of ESHS. I was vice-president and then president for the organization which is today a rather strong and successful one with biannual conferences that attract many students and scholars. I attended the last one that took place in Prague in 2016.

GRR: You have recently published three more books on cosmology, namely, *The Weight of the Vacuum* (2014), *Masters of the Universe* (2015), and *Varying Gravity* (2016). What is your perspective on the present (and maybe future) state of the standard model of cosmology?

Helge Kragh: As a historian and not a scientist I don't need to have an opinion about current theories in physics and cosmology. But I can judge the theories from a historical point of view. The hot big bang standard theory is undoubtedly very impressive and probably true as far as it goes. The big bang is no longer a matter of debate, but one needs to keep in mind that the big bang is not the same as the creation or absolute beginning of the universe. While the standard model is reliable I am much less convinced by its extensions to the time regimes even closer to the magical moment $t = 0$. Inflation is not yet proved and pre-inflation scenarios seem very speculative. The same is the case with various theories of a universe before the big bang, although such theories cannot be ruled out. It is too early to say with certainty that the age of the universe is finite.

GRR: You have also been interested in historical and alternative models in cosmology, such as your study on the scientific controversy between big-bang theory and steady-state theory, as presented in *Cosmology and Controversy* (1999), and your recent reevaluation of Dirac's hypothesis of a varying gravity, as presented in *Varying Gravity: Dirac's Legacy in Cosmology and Geophysics* (2016). Don't you think that there is very little room today for proposing, investigating and researching alternative models in cosmology in the mainstream institutions? If yes, why do you think this is the case?

Helge Kragh: Yes, you are right, there is little room for alternatives of the standard model, but there is plenty of room for alternatives at or below the Planck timescale. More importantly, from a historical and sociological point of view it doesn't matter so much whether or not the alternatives are reasonable or not. The very fact that there are such proposals makes them of interest. I have a certain weakness for alternative ideas, not because I think they are valid but because they tell us something about science and the psychological state of scientists. The modern idea of the multiverse is controversial and it is precisely for this reason I am interested in it.

GRR: What have you been working on or involved in nowadays (books or papers you have been writing)? What may be your next projects or research interests?

Helge Kragh: I recently published a paper (in *Journal for Astronomical History and Heritage*) on Zwicky's and others' ideas of "tired-light" hypotheses designed to keep the universe static. And shortly there will appear a detailed investigation (in *Annals of Science*) of Bohr's hypothesis of energy non-conservation ca. 1930, including his somewhat desperate attempt to explain stellar energy production. For half a year ago I published a biography of a leading nineteenth-century Danish chemist entitled *Julius Thomsen: A Life in Chemistry and Beyond*. My main work at present is however the preparation of a collaborative volume of the history of modern cosmology to be published by Oxford University Press in 2018. I am co-editor together with Malcolm Longair, a British astrophysicist, and write some of the chapters. I am also working on the status of astrophysics and cosmology within the Nobel system until 1966, relying on new material from the Nobel Archive in Stockholm. This work is still in progress.

GRR: What are the areas worth investigating in the history of the physical sciences that you think is yet not well (or less) researched?

Helge Kragh: One area, which I have thought of for a long time, is a sociologically oriented analysis of the origin and development of modern cosmology. How did cosmology become a scientific discipline? When and why did people begin to identify themselves as "cosmologists"? When and

why did textbooks and specialized journals appear? Can one speak, even today, of a community of cosmologists? There exists this kind of social history for particle and solid-state physics, for example, but not for cosmology or even for astrophysics. I would not myself be able to write such a history, but in collaboration with a sociologist or social historian I probably would. I also think that there are interesting areas of a cross-disciplinary nature, especially modern geophysics, medical physics and astrophysics that deserve to be more and better cultivated. And within the chemical sciences there is not, to my knowledge, any good historical work on so-called computational chemistry. Finally, it would be of value to have a comprehensive study of the relationship between philosophy and the physical sciences in the period after about 1970. My guess is that the impact of professional philosophers upon physics in this period has been minimal, but I am not sure.

GRR: What would be your best advice for a historian of science (and especially a historian of physical sciences) in his or her early career today?

Helge Kragh: I find it difficult to come up with a good advice. In some sense he (or she) should work in established areas of history of science, on the other he should also try to come up with something original. From my own experience I would say that it is important to learn the craft of history as well as the content of one or preferably more sciences. One should not be too specialized but have the broader perspectives in mind.

GRR: Thank you so much!

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Transversal: International Journal for the Historiography of Science, 2 (2017) 238-241

ISSN 2526-2270

www.historiographyofscience.org

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Book Review

Galileo as a Critic of the Arts

PANOFSKY, Erwin. *Galilée critique d'art*. Transl. Nathalie Heinich. Bruxelles: Les impressions nouvelles, 2016. 112 pp. ISBN: 978-2-87449-417-8, 15 €.

Reviewed by:

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Received: 30 March 2017. Accepted: 12 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.22>

Once again, Erwin Panofsky returns to the publishing scene. In 2016, *Galilée critique d'art* was again published by *Les impressions nouvelles*. But, in fact, it is not just Panofsky's return. In the French-speaking world, his text was hardly ever published alone. It was almost always accompanied by either Nathalie Heinich's foreword or Alexandre Koyré's review, or by these two works whose considerations gained a weight almost equivalent to Panofsky's own text. On the one hand, Heinich elucidates, in the wake of Pierre Bourdieu, the fruitful method implied in the analyzes of the art historian. On the other, Koyré affirms and unfolds the reach of Panofsky's statements that surpass his place of comfort, those based on the field of the history of the sciences, in which Koyré is considered an authority. And this is how the texts of Heinich, Panofsky and Koyré configure what comes to us as the book *Galilée critique d'art*.

In this work, Panofsky presents us with a series of statements that, in any way, could be included in the foreseeable assertions. It is in the midst of a *disputatio* over the superiority of painting or sculpture, a field where Leonardo da Vinci once engaged, which he places the mathematical physicist Galileo Galilei. In describing him, he does not speak of physical and astronomical theories, but of artistic tastes, he speaks of a character who knew by heart the latin classics, who loved Ariosto and repudiated Tasso, who was a designer and profound connoisseur of painting - even more inclined to study it than mathematics - who was a close friend of the painter Ludovico Cigoli, and for this very reason he was involved in the battle between the partisans of the painting and the sculpture, initiated in century XV.

It is not, therefore, only the exposure of galilean knowledge in the fields of plastic arts, music and literature, but a live engagement, which showed a greater concern than victory over any quarrel. Since Leonardo da Vinci, no one but Galileo, Panofsky points out, provided original contributions in the discussion he unleashed in 1430. Such originality is shown above all in two conceptions issued by Galileo. The first, with no equivalent in the criticism of the sixteenth and seventeenth centuries, developed from the change of the target of valorization in the artistic sphere. Merit comes out of the nature of matter and goes into the effort of the artist. Undermining the classic argument of the partisans of sculpture, Galileo makes a remarkable reasoning: "Farther from the things to be imitated will be the means by which we imitate, the more admirable will be the imitation" (Panofsky, 2016, 32). The fact that sculpture shares the character of three-dimensionality with matter gives it no advantage over painting. "Artificial to the highest point, on the other hand, will be the imitation which represents the relief by its opposite, which is the plane" (Panofsky,

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2016, 31). Larger art is to represent pain not by its natural expression, the crying, but rather by singing. And even greater merit would be to represent it not by the voice that comes from a living being, but by the sound of an inanimate instrument, Galileo emphasizes.

But it is in the wake of this refutation that we find the argument on which the art historian is most concerned. In fact, unlike the italians, the dutchman Johan Albert Bannius, some frenchmen like Mersenne and Descartes, and his own father, Vincenzo Galilei, Galileo did not think that music and poetry were inseparable. So if his reasoning culminates in the greatness of the musician who expresses the secrets of the soul through an instrument that does not possess it, it is also because, for him, instrumental music could - and would be better if it were - to be dissociated from the text. It is then that Panofsky puts in correspondence the Galileo that still had to present with the celebrated mathematical physicist already known. "Galileo's insistence on demanding a clear separation of the values and processes which at the time were commonly held to be inseparable testifies to a critical purism in which one can see the true mark of his genius. Just as he preferred pure music-without words-to singing, and disdained song mixed with sobs or laughter, so he demanded that the quantity of quality be separated, and the science of religion, magic, mysticism and art" (Panofsky, 2016, 35). For this reason, he repudiated, in the field of literature, Tasso's allegorical poetry in the field of painting, Holbein's anamorphs and Arcimboldo's "double images," and in astronomy the discussion of the existence, whether profane or sacred, of the four satellites around Jupiter, a debate that unfolded after the revelation of his discovery.

However, this position of Galileo was not at all unusual. Panofsky outlines a movement of ideas between the Mannerism of the middle of the sixteenth century and the Baroque and Classicism of the seventeenth century. Between 1590 and 1615 approximately, a movement appeared that was contrary to the Mannerism that preceded it and partisan of the values of the Renaissance. Galileo shared with Cigoli, Carrache, Dominiquin, and Agucchi the same inclination to appreciate the clarity, the harmony, the smooth composition of the contours, the fusion of reality and idea, a taste that did not change throughout his life.

But, for Panofsky, this context of reaction contrary to Mannerism does not explain Galileo's inflexible stance. It is neither a "product of historical conditions," nor a reflection of a thought derived from a strictly scientific rationality. What explains the galilean position, not only in the face of Mannerism, but in the different fields of knowledge, are the constitutive principles of his thinking, the greatest of which is "critical purism."

Such is the core of Panofsky's thesis. His main and decisive argument in his favor presents itself with a solution to a problem especially troubling to astronomical historians: Galileo's silence in all his writings on Kepler's laws. Galileo's silence was a fact that should be explained because the mathematical physicist had many reasons not to do so. Partisan and intrepid defender of the copernican system, close to Kepler, Galileo knew, at least since 1612, that his "comrade-in-arms" had corrected and amplified copernicus's astronomical theory. Kepler's modifications, which replaced the circles with the ellipses in the description of the trajectories of the planets, explained in an undoubtedly better way the astronomical observations and later formed the basis of Newton's solution. Even with these theories that would greatly aid him in his defense of the copernican system, Galileo ignored them, repeating the same weaknesses present in the way Copernicus originally conceived them.

Panofsky's answer to the enigma of Galileo's silence against Kepler's laws was that "he seems to have dismissed these laws of his mind, by what might be called a process of automatic elimination" (Panofsky, 2016, 63), elimination resulting from the incompatibility between what Kepler presented and, above all, that dominant principle of his thought and imagination; "critical purism." Galileo saw in Kepler's ellipses the disturbance and confusion of Holbein's anamorphosis and Tasso's poetry. The ellipse was "a form in which 'perfect order' has been disturbed by the intrusion of rectilinearity" (Panofsky, 2016, 64 and 65). It was also a form widely used in Mannerism and expressly rejected by the Renaissance.

Far from the vulgar and predictable assertions, Panofsky attests in his book that "if one considers that the scientific attitude of Galileo influenced his aesthetic judgment, one is entitled to consider as much as his aesthetic attitude has influenced his scientific convictions; and more precisely, one can say that, as a man of science as well as a critic of art, he obeys the same types of controls" (Panofsky, 2016, 58). Due to the demand for clarity and separation between genders, Galileo could not accept the mixture between circularity and rectilinearity - for him, the ellipse was the fruit - nor the animism of Kepler present in his astronomy which, together with his attitude of not rejecting the quantitative data, allowed this to get rid of the fascination for the circularity, manifested in Galileo.

Here is Panofsky, but not entirely *Galilee critique d'art*. This begins with *Panofsky épistémologue* - it is followed by two translations: one from the letter of Galileo to Cigoli and one from "Monsignore Giovanni Battista Agucchi and his speech Del Mezzo" - and ends with *Attitude esthétique et pensée scientifique*. The book begins with the outline of a method, with Heinich, and is finalized with the outline of the fruits of Panofsky's analysis, with Koyré. By paying attention to the commentators, however, the reader feels a nuisance caused by a mismatch between the authors whose suggested ratio seems to be full agreement. Heinich claims that Koyré ventured where Panofsky did not allow himself. The art historian did not even say that Galileo's aesthetic tastes were guided by scientific positions, or the other way around. To do this, one had to have "the openness and curiosity of mind of a Koyré" (Heinich, 2016, 9). However, this author does not, at any moment, point out traces of Panofsky's hesitation in admitting relations of influence. Quite the opposite. For him, his intrepidity led him to break with the traditional image of Galileo scientist, and in fact his only mistake was not to employ it, as he does in his text, in his title. But this mismatch goes beyond the attestation of a lack of one or another author. It reflects two different readings of Panofsky's text.

For Heinich, the turbid nature of Panofsky's claims about the relations of influence between artistic and scientific positions is far from being a mistake. He expresses his adoption of another epistemological perspective, different from that which seeks to postulate relations of cause and effect. That is replaced by a perspective that shows "homologies, structural identities common to different fields" (Heinich, 2016, 12), indicates "what could be a sociology or an anthropology of culture which, in true research logic, would finally cease to be exhausted in deriving 'art' from 'society', 'science' from 'social', 'individual' from 'collective' and the 'tastes' from 'social interests'..." (Heinich, 2016, 12). That explains Panofsky's erudition, which is not "knowledge fetish." This author walks in the fields of music, literature, painting, physics, astronomy to highlight "the similarities, the affinities, the logical links to structural constants" (Heinich, 2016, 12), or "structuring schemes." Heinich follows the same reading presented by Bourdieu in his afterword to the *Gothic architecture and scholasticism*, where, if we admit Fugier Pascal's statement, he brings out, for the first time, his concept of *habitus*. Bourdieu recognizes it in Panofsky's "search for the locus of all forms of symbolic expression proper to a society and an age" (Bourdieu, 2007, 337). He refuses to "safeguard the rights of creative individuality and the mysteries of singular creation." Doing that is "to deprive oneself of discovering collectivity at the core of individuality in the form of culture. Language of Erwin Panofsky, of the *habitus* that makes the creator participate in his collectivity, of his time and, without his conscience, guides and directs his seemingly more singular acts of creation" (Bourdieu, 2007, 342). Heinich presents, in his text published for the first time in 1987, the same structuralist reading carried out by Bourdieu in 1961. For her, Panofsky had pursued his search for the structuring schemata outlined by Bourdieu. For that reason, he could not clearly trace a causal relationship between Galileo's aesthetic tastes and his scientific stance, which, in Heinich's view, has the great advantage of resolving a blatant contradiction present in Panofsky's text: according to the author, putting an aesthetic response to an epistemological question.

For Koyré, the so-called influence relationship is not questioned. What Panofsky really means when he speaks of Galileo's rejection of Kepler's discoveries is that "he rejected the Keplerian ellipses for the simple reason that they were ellipses ... and not, as it was, circles" (Koyré, 2016, 101). According to Koyré, Panofsky's great merit was, certainly, that he did not commune with Galileo's critical purism by approaching it in a way different from that of science, that is, of the spirit of new science, of opposition to natural places and geometrization of space. Proof of this excommunication was his attitude of not interpreting, like historians, the passages where Galileo clearly affirms "the obsession with circularity" in his spirit - like that of the beginning of his "Dialogue" - taking them *à la lettre*. It was in consequence of this that Panofsky was able to overcome another "obsession": that of the traditional image of Galileo.

Koyré insists on the misery of purism. And soon one sees that his criticism is not restricted to the author of the *Dialogue* or to the historians who have studied it, but it is something greater. In effect, Koyré concludes his text by stating: "Purism is a dangerous thing. And the example of Galileo, which is by no means unique, shows clearly that nothing should be exaggerated. Not even the requirement of clarity" (Koyré, 2016, 109). Since the 1930s, Koyré had already rejected the epistemology of the Vienna Circle, which asserted the need for clarity and purification of the scientific domain, a revival two years before the publication of his review of Panofsky at the time of his meeting with Philipp Frank, at a congress in Boston in 1954. As has been known, since that time, he had already demonstrated in his *Galileo studies* the impossibility of studying the evolution of scientific thought as an independent series - isolated from religious, metaphysical, and philosophical - expressing it later clearly in his *curriculum vitae*, written in 1951. If Koyré does not question Panofsky's statement about the influence of Galileo's aesthetic attitude on his scientific

conceptions, it is because, for him, there was nothing contradictory in putting, repeating, to an epistemological question, an aesthetic answer. Just as he did not believe it would be contradictory to present a scientific, religious, metaphysical, or philosophical answer to a scientific question. Koyré saw in the art historian, a strong ally in affirming the importance of "trans-scientific" ideas in the course of scientific thought itself. Corollary, Koyré also saw in Panofsky, a strong ally in affirming the indispensability of the historical reconstitution of the guiding elements of thought, since these are not situated in a previously established domain. Hence Koyré's exaltation of impure character is, open to possible cross-links, Panofsky's position. Hence feels at ease in merging the conclusion of the historian's text – which he reproduces - with the conclusion of his review, stating, as in the same voice of Panofsky, that "the ways of human thought are curious, unpredictable, illogical" (Koyré, 2016, 109).

Therefore, if it bears here to say what the *Galilee critique d'art* is, it may be said that it is a book similar to the mannerist anamorphosis so rejected by Galileo. If, looking from the front, it looks like a painting that only brings confusion, as we shift our glance, it lets us glimpse figures whose contours are proficient and clear. Face to face, the book seems a forced and confused mixture, since it brings a text circumscribed by two readings that point in different directions, each of which can be adopted by the reader. The confusion seems to grow when one notices that it is also possible to accept neither. It may be said that if, on the one hand, Koyré presented more of his own convictions than those on Panofsky, on the other, it can be said that Heinrich made basic considerations originally directed to a text different from that in question, and that *Galilee critique d'Art* has distinct characteristics of *Gothic architecture and scholasticism*. But if we shift our point of view towards the perspective which, with Bourdieu, Heinrich calls us, seeking to glimpse *Galilee critique d'art* in the whole of Panofsky's works, we perceive a solid interpretation of his apparently loose and performative methodology. It is interesting to notice, with Thomas Frangenberg, that a re-reading was carried out, both in the context of semiotics and in structuralism, of Panofsky's texts, and this author was identified as one of the forerunners of these disciplines. And finally, in shifting our point of view in the direction that Koyré points out - less in the search for Panofsky's methodological presuppositions and more in the search for the contributions of his specific analyzes on Galileo - we see above all the extension of the field of historical-philosophical investigation, in the 1930s. In admitting the influence of Galileo's aesthetic tastes on his scientific stance, Koyré adds within the trans-scientific ideas, situated in the realms of philosophy, metaphysics, and religion, and crucial in Study of the history of the sciences, those ideas coming from the field of the arts.

But, contrary to Galileo's taste, the incitement to the displacement of a point of view is a characteristic, rather, to be appreciated than the opposite. There is the wealth of *Galilée critique d'art*.

Transversal: International Journal for the Historiography of Science, 2 (2017) 242-246

ISSN 2526-2270

www.historiographyofscience.org

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Book Review

A Contribution to the Newtonian Scholarship: The “Jesuit Edition” of Isaac Newton’s *Principia*, a research in progress by Paolo Bussotti and Raffaele Pisano

Reviewed by:

Gustavo Rodrigues Rocha¹

Received: 13 March 2017. Accepted: 12 May 2017.

DOI: <http://dx.doi.org/10.24117/2526-2270.2017.i2.21>

The *Mathematical Principles of Natural Philosophy* (*Philosophiae Naturalis Principia Mathematica* in Latin), hereafter *Principia*, a three-volume *tour de force* written by Isaac Newton, and published in 1687, is the seminal work in the history of modern physics. American theoretical physicist and Nobel laureate, Steven Weinberg, remarked in his 1972 work on cosmology and gravitation that “all that has happened since 1687 is a gloss on the *Principia*” (*apud* Pask, 2013, 14).

The second edition of the *Principia* was published in 1713, and reprinted and corrected in 1714, incorporating a more comprehensive theory of the Moon, the motions of comets, and the precession of the equinoxes, and, at the end of the whole book, the famous general scholium. The third edition of the *Principia* was published in 1726. Newton made some additions to the third edition, including new explanations for the resistance of fluids in “Book 2” (which resumes “Book 1”, *De motu corporum*, “On the motions of bodies”), as well as a more detailed explanation for the Moon’s orbit and the role of gravitation, and, in “Book 3” (*De mundi systemate*, “On the system of the world”), new observations of Jupiter and the comets. The first translation into English was published in 1729, by Andrew Motte, based on the 1726 third edition of the *Principia*.

The so-called “Jesuit Edition,” which is the focus of Bussotti’s and Pisano’s project, was also based on the third edition of Newton’s *Principia*, published between 1739 and 1742, in four volumes, by the minim friars Thomas Le Seur (1703-1770), François Jacquier (1711-1788), both of whom were French priests, and Jean-Louis Calandrini (1703-1758), a Swiss mathematician. This edition includes several commentaries, explanations and addendums to the *Principia* that did not originate with Newton.

Newton’s masterpiece has ever since been the focus of studies and exegesis, translations and commentaries, interpretations and reformulations. The geometrical procedures of Newton were gradually replaced by analytical procedures. The editors of the “Jesuit Edition”, for instance, made use of the formalism

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developed by Leonhard Euler (1707-1783), outlined in his work *Mechanica* (1736), to rewrite *Principia's* propositions into differential equations. The works by Pierre Varignon (1645-1722), David Gregory (1659-1708), Guillaume François Antoine Marquis de l'Hôpital (1661-1704), Johann Bernoulli (1667-1748), Abraham de Moivre (1667-1754), John Keil (1671-1748), Jacob Hermann (1678-1733) and Willem Jacob's Gravesande (1688-1742) are among the first contributions to physics derived from the exegesis and expansion of the outcomes of the *Principia*.

In addition to the preceding historical editions, there are several modern editions of the *Principia*. The analytic historical exegeses of Newton's third edition by scholars such as Bernard Cohen (1914-2003), known for his works *Introduction to Newton's Principia* (1971) and *The Newtonian Revolution* (1980), and Alexandre Koyré (1892-1964), known for his work *Newtonian Studies* (1965), are among the most outstanding examples in the twentieth century. Peter and Ruth Wallis collected the immense quantities of new material relating to Newton up to the time of Cohen and Koyré in *Newton and Newtoniana 1672-1975, A Bibliography*.

The works by Brackenridge (1995), De Gandt (1995), Densmore (1995) and Dobbs (1976, 1991, 1995) stand out in the contemporary Newtonian historiography in the previous decades. In 2000 physicists and historians of physics, Michael Nauenberg, an expert in Hooke, Newton and Huygens, and Richard Dalitz, well known in the field of particle physics, edited the book *The Foundations of Newtonian Scholarships*. This book was the outcome of a symposium held at the Royal Society in London in 1997 which brought together many of the today's Newtonian scholars.

Newton's biography by Richard Westfall, *Never at Rest* (1980), "based on an extensive study of Newton's manuscripts, does not merely chronicle the events in Newton's life but illuminates almost every aspect of Newton's life and thought, providing a rich and valuable commentary on Newton's scientific achievement" (Buchwald and Cohen, 2001, xiv). In the field of translations and commentaries, the 1999 prominent translation of the *Principia* by Bernard Cohen and Anne Whitman should be highlighted and the study by the Indian physicist S. Chandrasekhar, *Newton's Principia for the Common Reader* (1995), attracts attention as one of the most recent reassessments of Newton's *Principia*.

There has been also a long-lasting tradition of popularization of the *Principia*. Francesco Algarotti's *Newtonianism for Ladies* (1737) was a landmark in the popularization of Newtonian philosophy, as well as Voltaire's *Éléments de la Philosophie de Newton*, first published in 1738 and then again in 1745 in a new edition that included a new section devoted to Newton's metaphysics. *Magnificent Principia: Exploring Isaac Newton's Masterpiece* (2013) by the British mathematician Colin Pask is another contemporary example, and *Reading the Principia* (1999) by the historian of mathematics Niccolò Guicciardi has also become a reference in the field of *Principia's* popularization. The study of the history of science and technology also has among its classic seminal texts exegeses of the *Principia*, such as *The Social and Economic Roots of Newton's Principia* (1931) by the historian and philosopher of science Boris Hessen. It is in the context of this tradition, and these efforts of exegesis of the *Principia*, that one could place the works and the project by Paolo Bussotti, researcher and professor at the University of Udine, and Raffaele Pisano, researcher and professor at the University of Lille 3.

Thomas Le Seur and François Jacquier were Catholic priests of the Order of Friars Minor – founded in the fifteenth century in Italy – but were mistaken for being Jesuits. The so-called "Jesuit Edition" (JE) is an extensively commented version of the third edition of the *Principia*, though not studied enough in this tradition of the *Principia's* exegeses. The historians Bussotti and Pisano have tasked themselves with filling this missing element in the Newtonian scholarship. The JE, after being written between 1739 and 1742, had several editions, such as the 1760 edition in Cologne, the 1780-85 Prague edition (which is a partial reedition of the JE), and a third edition (published first in 1822 and again in 1833) in Glasgow.

Based on the Glasgow edition, mathematician John Martin Frederick Wright corrected several mistakes found in the previous editions of the *Principia's* "Jesuit Edition," and published the corrections in 1833 as "Commentary on Newton's Principia". Bussotti and Pisano have as their starting point for their work this third corrected version of the *Principia's* "Jesuit Edition" which has not yet been translated into English from its Latin original version. Their project includes an English translation of the four volumes of this 1822 corrected JE and an additional introductory fifth volume. This project is to be concluded by 2020.

The authors, clarifying the relevance of the JE, remind their readers that, historically, the commentators of the *Principia* have tried to "1) explain Newton's propositions in a clearer manner than Newton did; 2) translate the properties given by Newton geometrically in more analytical terms; 3)

sometimes explain the development of physics, based on Newton's discoveries, after Newton" (Bussotti and Pisano, 2014a, 35-36). Le Seur, Jacquier and Calandrini accomplished these tasks magnificently.

Firstly, the authors of the JE added clarifying remarks for every single proposition by Newton in the *Principia*. Bussotti and Pisano remind their readers that there is no more meticulously commented edition of the *Principia* than the JE. Secondly, from the first half of the eighteenth century onwards, the work by Euler, *Mechanica* (1736), and the JE became the classic references to the readers of the *Principia* because Newton's formalism as expressed in his *magnum opus* became gradually outmoded and eventually obsolete. Pierre Varignon (1654-1722), Jakob Bernoulli (1654-1705), David Gregory (1659-1708), Johann Bernoulli (1667-1748), John Keil (1671-1721), Jacob Hermann (1678-1744), and Daniel Bernoulli (1700-1982) are among the first authors who translated Newton's results into completely analytical terms. Finally, the editors of the JE offered an encyclopedic summary of the developments obtained throughout the first four decades of the eighteenth century derived from the applications of Newton's calculations in the *Principia*. It is worth noting that, up to the present time, the JE remains very rarely commented by the secondary literature. Therefore, a reassessment of the *Principia's* JE is of enormous relevance to Newtonian scholarship.

According to Bussotti and Pisano the JE provides three fundamental pieces of information useful to "1) Understand Newton's mathematical techniques and physical results, 2) Get a clear idea of the development of physics and mathematical analysis in the 20-25 years after the publication of the third edition of the *Principia*, 3) Fully realize the profound difference between Newton's physical-mathematical approach and the approaches of his successors" (Bussotti and Pisano, 2014a, 37). The work of Bussotti and Pisano advances four areas of research. First, an investigation of the three personalities involved in the production of the JE. Why did the authors of the JE organize the many notes the way they did? Second, they explore the politics of science in the elaboration of the JE. Who conceived and initiated this gigantic project? What are the reasons and motives behind it? Bussotti and Pisano intend to rebuild the social environment inside which the JE was conceived, formulated, and developed, as well as to clarify the nature, purpose, and structure of those notes added by these commentators on Newton's *Principia*. Third, Bussotti and Pisano want to trace the changes and development in mathematics and the physical sciences during the first four decades of the eighteenth century, which led to a growing number of commentators who made Newton's approach and stylistic writing intelligible. Bussotti and Pisano want to verify whether there is a relationship or not between changes in mathematics and the content itself of the physical theories of the period. Finally, they devote part of their research to examine who was the target audience of the JE. "Who might have been the readers of the JE?", ask the historians of science Bussotti and Pisano, who promise to prepare in the coming years a series of papers presenting their results (Bussotti and Pisano, 2014b, 439-440).

The present paper, "*Philosophiae Principia Mathematica* 'Jesuit' Edition: The Tenor of a Huge Work," is divided into two parts. In the first part the authors outline the general structure of the JE, the personalities of the three commentators, and the role played by the JE in comparison with the three editions of the *Principia* between 1687 and 1833. In the second part they layout the ongoing editorial project of the translated and commented edition of the Glasgow version of the JE.

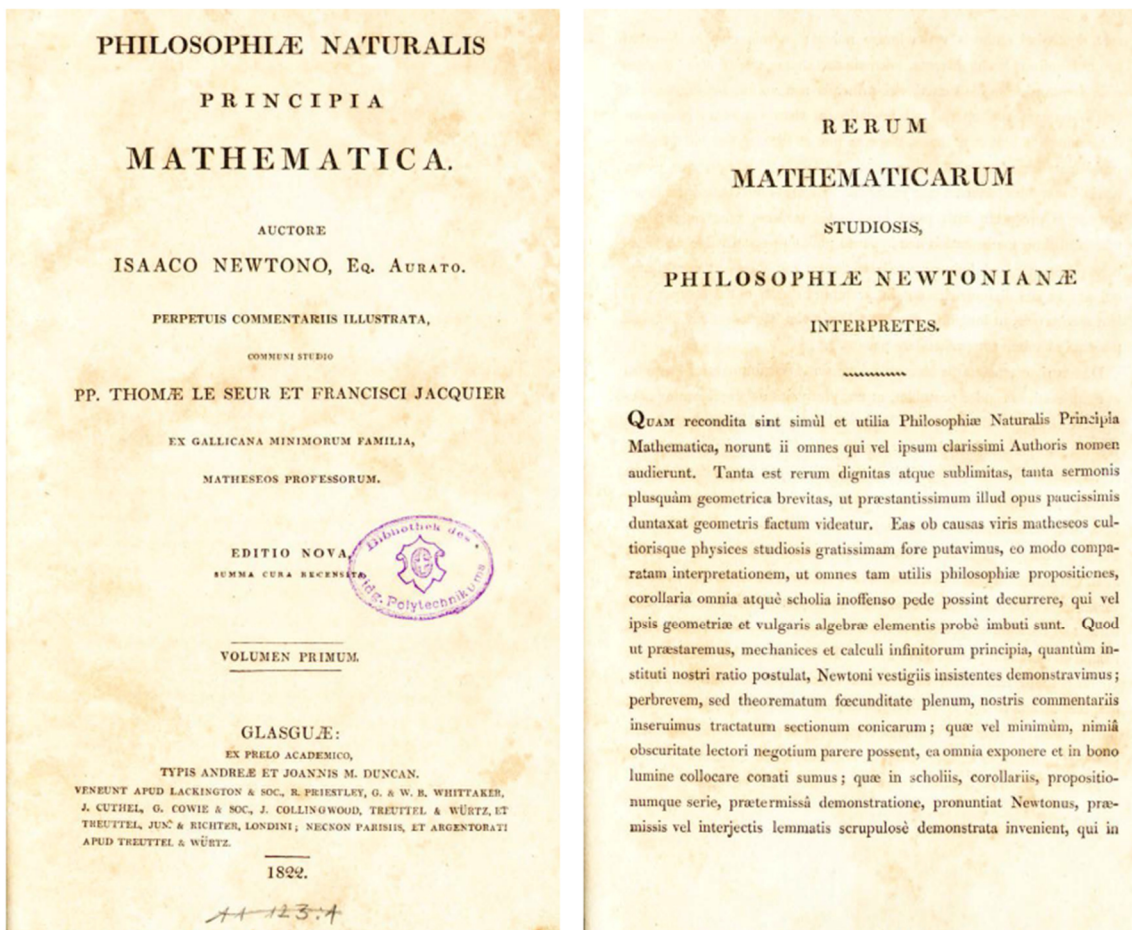
The first volume of the JE was published in 1739 and included the "Book 1" of the *Principia*. The second volume was published in 1740 and included the "Book 2" of the *Principia*. The third and fourth volumes were published in 1742. The third volume includes the first 24 propositions of the "Book 3" of the *Principia* and the fourth volume includes the remaining propositions and the *General Scholium*, a closing essay that Newton inserted into the second edition of the *Principia*.

There are four kinds of interventions, from a typographical point of view, found in the JE: 1) those notes which are directly referred to passages by Newton are specified by a letter within parenthesis, as in (a), 2) those notes which are not interpreted by the commentators as direct explanations of Newton's writing, but which represent either a clarification or an annexation by the commentators are signed by a number, as in 1), 2), 3), and so on, 3) treatises inserted by the commentators to introduce general problems found by readers of Newton's *Principia*, and 4) treatises by different authors about theories first found in Newton's *Principia* and then perfected and developed by them, e.g. works by Daniel Bernoulli and Leonhard Euler.

As it can be seen in the picture below, only the names of the French Catholic priests, Thomas Le Seur and François Jacquier, can be found in the title page. However, Le Seur and Jacquier acknowledged Calandrini's contribution in the end of the first book. The mistaken view that the authors were Jesuits, when in fact Le Seur and Jacquier were Catholic priests of the Order of Friars Minor, was first introduced by the typographer of the Glasgow edition. While they are mostly known for the JE of the *Principia*, they also wrote several other essays. Jean-Louis Calandrini for instance had been a Newtonian since his youth and then

focused on the physics on the colors for his thesis at the Academy of Geneva in 1722. He also studied and wrote about spherical trigonometry and infinite series, as well as maintaining an interest in botany, meteorology, and the problem of the aurora borealis.

Bussotti and Pisano illustrate the potential of the research that they have been doing by presenting in their paper an example that they consider to be paradigmatic in the commentaries introduced by the editors of the JE. The chosen example is found in “Book 1”, “section VII”, “proposition XL”, where Newton presents his solution to the inverse problem of central forces. Bussotti and Pisano argue that the example is a paradigmatic representation of the peculiarity of Newton’s geometric line of thinking and the standard manner by which commentators translate it into its analytical modern expression. Bussotti and Pisano emphasize how this example is important for the history of mathematics as it represents a transition from different thought traditions between that of Newton and his immediate successors. Bussotti and Pisano leave their readers looking forward to both their next results as well as their final editorial project. The coming developments of this important contribution to the Newtonian scholarship are to be eagerly anticipated.



Title page of the 1822 Glasgow “Jesuit Edition”. In: BUSSOTTI; PISANO, 2014, 36.

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