Galileo's Mathematization of Nature at the Crossroad between the Empiricist and the Kantian Tradition

Citation for published version:
Massimi, M 2010, 'Galileo's Mathematization of Nature at the Crossroad between the Empiricist and the Kantian Tradition' Perspectives on Science, vol 18, no. 2, pp. 152-188., 10.1162/posc.2010.18.2.152

Digital Object Identifier (DOI):
10.1162/posc.2010.18.2.152

Link:
Link to publication record in Edinburgh Research Explorer

Document Version:
Publisher final version (usually the publisher pdf)

Published In:
Perspectives on Science

Galileo's Mathematization of Nature at the Crossroad between the Empiricist and the Kantian Tradition

Michela Massimi
University College London

The aim of this paper is to take Galileo's mathematization of nature as a springboard for contrasting the time-honoured empiricist conception of phenomena, exemplified by Pierre Duhem's analysis in To Save the Phenomena (1908), with Immanuel Kant's. Hence the purpose of this paper is twofold. I) On the philosophical side, I want to draw attention to Kant's more robust conception of phenomena compared to the one we have inherited from Duhem and contemporary empiricism. II) On the historical side, I want to show what particular aspects of Galileo's mathematization of nature find a counterpart in Kant's conception of phenomena .

1. Introduction

Current philosophy of science has been characterised by a lively and ongoing debate between two main positions: realism and empiricism. The for-

This paper originates from another paper on Kant's view of phenomena, presented at the Royal Institute of Philosophy Annual Conference "Kant and Philosophy of Science Today" (UCL, 2–3 July 2007), and published in Massimi (ed.) *Kant and Philosophy of Science Today* (2008). I am very grateful to the audience of the Kant conference and in particular to Roberto Torretti, Michael Friedman, Hasok Chang, for very helpful comments. An earlier version of this paper was presented at the British Society for Philosophy of Science and at the &HPS1 conference in Pittsburgh (October 2007): I am grateful to the audiences there, especially Peter Achinstein, John Norton, Don Howard, Peter Machamer, Ernan McMullin, John Worrall for most thought-provoking and constructive criticisms. I am also grateful to Katherine Dunlop for helpful advice on the notion of postulate at Galileo's time, and to Domenico Bertoloni Meli for discussion on Galileo's axiomatization of the science of motion. Many thanks finally to the archivists of King's College Cambridge for access to the Keynes collection of Galileo's *Opere.*

Perspectives on Science 2010, vol. 18, no. 2

mer claims that the aim of science is to discover the truth about nature; the latter denies the very same possibility to discover truth, and defines the aim of science as that of 'saving the phenomena'. This is an area where integration with history of science can be particularly helpful in clarifying some methodological assumptions at stake in this debate. Discussions about the aims of science can indeed be illuminated by looking at what scientists believed and did as part of their scientific practices, and *whether* and *how* their attitudes towards theories and experiments may provide support for one interpretive line over another. An interesting example of such integration between history and philosophy of science on this specific issue of realism vs. empiricism, can be found for instance in Peter Achinstein's recent article on the diverse philosophical attitudes of Maxwell, Ostwald, and Duhem, as to the reality of atoms. By contrast with Maxwell's realism, Duhem exemplifies the empiricist position in claiming that "atomic theories are examples of theories about an unobservable material world underlying observable phenomena; (. . .) such theories cannot be established to be true or false by physical science" (Achinstein 2007, p. 370). Any scientific theory that goes beyond the observable phenomena in postulating unobservable entities (e.g. atoms) risks falling into the realm of "'metaphysical theories' postulating occult causes" (Achinstein 2007, p. 369), according to Duhem. No wonder, Duhem's judgment is not confined to atoms but extends to Aristotelian physics as well as Newtonian physics with its attractive force acting at a distance. As Achinstein points out:

> For Duhem empirical science begins with 'sensible appearances' (e.g., light), which can then be represented in what he calls an 'abstract and general form' (e.g., by means of mathematical rays subject to experimental laws of reflection and refraction). He distinguished this legitimate activity from the illegitimate one of inferring hypothesis about an unobservable reality underlying sensible appearances and their abstractions. (2007, p. 373)

Duhem's empiricist attitude is nicely illustrated, among other works, in a series of historical essays called *To Save the Phenomena* (1908), where he traced the philosophical tradition of saving the phenomena back to Plato and to what he called the *method of the astronomer.* For centuries, the aim of ancient Greek astronomy was, according to Duhem, to introduce hypotheses that could save the phenomena, as opposed to hypotheses that could give a true story about celestial bodies. This is because, as ancient astronomers knew well, there could be more than one hypothesis compatible with the available data (e.g., epicycles or eccentric circles):

> Very different hypotheses may yield identical conclusions, one saving the appearances as well as the other. Nor should we be surprised that astronomy has this character: it shows us that man's knowledge is limited and relative, that human science cannot vie with divine science. . . . In more than one respect, Proclus' doctrine can be likened to positivism. In the study of nature *it separates, as does positivism, the objects accessible to human knowledge from those that are essentially unknowable to man.* But the line of demarcation is not the same for Proclus as it is for John Stuart Mill. . . . By extending to all bodies what Proclus had reserved for the stars, by declaring that only the phenomenal effects of any material are accessible to human knowledge whereas the inner nature of this material eludes our understanding, modern positivism came into being. (Duhem [1908] 1969, pp. 21–2; emphasis added)

Not surprisingly, Duhem himself subscribed to this positivist / empiricist tradition. Indeed, after a detailed historical reconstruction of how the Platonic tradition passed on from Medieval Christian Scholasticism to Osiander's Preface to Copernicus' *De revolutionibus,* Duhem took a look at the turning point marked by Galileo's new sciences. According to Duhem, it was only with Galileo that the philosophical trend of 'saving the phenomena' stopped and reversed. By contrast with the method of the astronomer, Galileo introduced the *method of the physicist:* astronomy had to conform to reality, rather than saving the phenomena. This was the task that Galileo set for himself in the case of Copernican astronomy, and that led him to clash with religious authorities, who by contrast had welcomed Osiander's quasi-empiricist preface to *De revolutionibus.* Duhem famously concluded: "Despite Kepler and Galileo, we believe today, with Osiander and Bellarmine, that the hypotheses of physics are mere mathematical contrivances devised for the purpose of saving the phenomena. But thanks to Kepler and Galileo, we now require that they save *all the phenomena* of the inanimate universe *together*" ([1908] 1969, p. 117).

Latching onto Achinstein's analysis of Duhem's skepticism toward atoms, we can see that this is part of a much bigger philosophical picture Duhem subscribed to. Duhem was championing the empiricist tradition of saving the phenomena that many empiricist philosophers of science still nowadays support (see van Fraassen 1980; 1985; and 2006). According to Duhem's reconstruction, the empiricist tradition has a distinguished pedigree, going back to ancient Greek astronomy, Osiander, Bellarmine and modern positivism.[1] From a historical point of view, Duhem's so-called

---

[1] Duhem's historical reconstruction is of course open to question. For example, his analysis of Copernican astronomy has been disputed by Barker and Goldstein (1998), while

continuity thesis, that is, the thesis that there was no abrupt discontinuity in the transition from medieval to early modern science, has been reevaluated against Kuhnian historiography. From a philosophical point of view, Duhem's empiricism and his polemic with the realism he saw in Copernicus and Galileo, has also been re-appraised in all its conceptual nuances.[2] I am not going to enter into this literature on Duhem because the focus of this paper is Galileo. Galileo's mathematization of nature is historically at the crossroad of rival philosophical traditions about the aims of science. For scientific realists, Galileo was one of the fathers of the scientific revolution: he fought for the view that science gives us a true story about the way things are in nature. For Duhem and the empiricists, on the other hand, Galileo marks the end of the tradition of saving the phenomena but in a way, his was a Pyrric victory.

In this paper, I want to go back to Galileo's mathematization of nature as a springboard for contrasting this time-honoured empiricist attitude with another important philosophical tradition, which has been surprisingly overlooked by historians and philosophers of science. This is the tradition that goes back to Immanuel Kant and that is equidistant from the realist and the empiricist one. From a Kantian perspective, the aim of science is not to put forward true theories; nor is it to save the observable phenomena and suspend belief about unobservable entities (as empiricists maintain). Rather, from a Kantian perspective, as I want to suggest in this paper, the aim of science is to *constitute* phenomena according to the conditions of possibility of human experience. This is the transcendental task that Kant set for himself by asking "how is pure mathematics possible?" and "how is pure natural science possible?". Transcendental philosophy was Kant's answer to these questions, and the identification of what he called the "conditions of possibility of experience," namely those conditions of human cognition that have to be in place for us to have any experience of the world around us.

The key intuition behind Kant's view was to downplay the gap that for centuries empiricism envisaged between sensible appearances—intended as what *appears to our senses*—and an underlying unobservable reality that

---

his rather controversial judgment on Galileo has attracted renewed attention among historians and philosophers of science (see for instance Ariew and Barker 1990; Jaki, 1984; Martin 1982, 1991).

[2] For instance A. Goddu (in Ariew and Barker 1990, p. 312) tried to defend Duhem from stereotypical realist accusations and to show that "Duhem seems self-consistent in maintaining a *qualified* instrumentalism as regards physical theories and laws while holding a qualified, if regulative, realism as regards our ordinary experience and deeper metaphysical intuitions. This is, I believe, one of the sources of the confusion in discussions of Duhem's positivism and realism".

goes beyond our capacities of knowing. Kant would agree with Duhem that whenever we go beyond the sensible appearances towards an underlying unobservable reality, we can discover nothing and we can only lose ourselves in what Kant called the antinomies of reason. However, by contrast with Duhem, Kant had a more substantial notion of 'appearances' than *what appears to our senses.* Nor should Kant's claim that our knowledge is confined to phenomena be confused with phenomenalism. As I am going to clarify in section 2, Kant developed a new conception of phenomena, which finds a natural counterpart in some salient aspects of Galileo's mathematization of nature. Indeed, Kant's new conception of phenomena is patterned upon Galileo's in believing that science can reveal nature's lawfulness, against Bellarmine and followers.

The purpose of this paper is twofold. I) On the *philosophical* side, I want to draw attention to a (Kantian) more robust conception of phenomena than the one we have inherited from Duhem and contemporary empiricism.[3] II) On the *historical* side, I want to show some salient aspects of Galileo's mathematization of nature that find echoes in Kant's philosophical analysis. It is *neither* my purpose to enter into exegetical analyses of Kant, *nor* to engage with scholarly interpretations of Galileo. The literature on both sides is immense, and goes well beyond my competence and my purpose here. Instead, my more modest purpose is to illustrate the Kantian stance on Galileo's mathematization of nature, as an alternative reading of Galileo compared to the empiricist one exemplified by Duhem.

In my illustration of the Kantian stance on Galileo, I make use of Duhem's continuity thesis. I focus my analysis on Galileo's new science of motion (rather than on his defence of Copernican astronomy), and I show how in his demonstration *ex suppositione* of the law of free fall, Galileo used the force concept of 'impeto' or 'momento', which, although reminiscent of the Medieval impetus theory of Buridan and Oresme, is in fact as distant from it as from the later Newtonian dynamics. So, although most of current Galilean historiography shares Duhem's continuity thesis in highlighting the continuity with Medieval science, and especially with Archimedean hydrostatics, the philosophical readings of Galileo are not themselves necessarily Duhemian (one might even venture to say that historiography *underdetermines* the choice of philosophical readings).[4] In other words, one can endorse Duhem's continuity thesis at the historical level,

---

[3] I have addressed this issue already in Massimi (2007), and especially in Massimi (2008), from which this paper originates.

[4] This is not entirely surprising since even during Duhem's time, Duhem's historical and philosophical views were not perceived as necessarily connected. I thank an anonymous referee for pointing this out.

while rejecting his empiricism at a philosophical level. This is precisely my strategy in this paper.

In section 2, I briefly present Kant's view on phenomena, and how it differs substantially from the empiricist one. In section 3, I discuss Kant's stance on Galileo and why Galileo is repeatedly mentioned by Kant as an example of his "Copernican revolution" in philosophy. In section 4, I turn to Galileo himself and his demonstration of the law of free fall, from the early study in *De motu antiquiora* (1590) to *Discourses and Mathematical Demonstrations concerning Two New Sciences* (1638). By latching onto W. L. Wisan's scholarly interpretation of Galileo, I try to clarify what aspects of Galileo's mathematization of nature find their counterpart in Kant's conception of phenomena. This section touches on some delicate and rather controversial aspects of Galileo's scientific methodology, and tries to highlight the analogies with the Kantian analysis presented in section 2 and 3. The result of this combination of Kantian philosophy and Galilean history is to provide an alternative perspective to the well-established empiricist one on Galileo.[5]

2. Beyond Empiricism: Kant's New Conception of Phenomena

The original motivation behind Kant's *soi-disant* 'Copernican revolution' in philosophy was a sense of dissatisfaction towards the empiricist tradition that dominated British philosophy in the seventeenth and eighteenth century. Despite some important insights, such as David Hume's criticism of causation, for Kant the empiricists barred themselves from the possibility of explaining *how* our scientific knowledge of nature is *possible:*

> The famous Locke, . . . because he encountered pure concepts of the understanding in experience, also derived them from this experience, and thus proceeded so **inconsistently** that he thereby dared to make attempts at cognitions that go far beyond the boundary of all experience. David Hume recognised that in order to be able to do the latter it is necessary that these concepts would have to have their origin a priori. But since he could not explain at all how it is possible for the understanding to think of concepts that in themselves are not combined in the understanding as still necessarily combined in the object, and it never occurred to him that perhaps the understanding itself, by means of these concepts, could be the originator of the experience in which its objects are encountered, he thus, driven by necessity, derived them from experience

[5] A very authoritative Kantian reading of Galileo was offered by Cassirer (1906), (1927). The more modest aim of this paper is to latch contemporary Kantian perspectives in philosophy of science to the history of Galileo's demonstration of the law of free fall.

> (namely, . . . **custom**). . . . The **empirical** derivation, however, to which both of them resorted, cannot be reconciled with the reality of scientific cognition a priori that we possess, that namely of **pure mathematics** and **general natural science,** and is therefore refuted by the fact (Kant 1781/1787, B127–8; emphases in the original).[6]

Starting from what—echoing the sentence above—the neo-Kantian Marburg school in the nineteenth-century called the 'fact of science', the only way of explaining *how* scientific knowledge is *possible* is by assuming that it has (in part) an a priori source in our understanding. Empiricism precluded itself from the possibility of explaining the 'fact of science', and led to Humean skepticism at best.

Hence, the necessity to operate what Kant famously described as his "Copernican revolution" in philosophy, according to which "our representation of things as they are given to us does not conform to these things as they are in themselves but rather these objects as appearances conform to our way of representing" (Kant 1781/1787, Bxx). Instead of asking: 'how can we bridge the gap between scientific hypotheses and evidence?'; 'Do hypotheses give us a true story about the way things are in nature, or do they simply save the phenomena?'; Kant realised that the above questions are ill-posed, and, as such, bound to remain open. According to Kant, we should instead ask a different type of question: namely, how objects as they appear to us (as *appearances*) can conform to *our way of representing* (and not the other way around).

We should immediately avoid a possible misunderstanding. When Kant speaks of appearances that have to conform to our way of representing, 'appearances' should not be confused with things as they *appear to our senses* as empiricism would have it, and as still Duhem meant it, as we saw in the Introduction. For Kant, appearances are not perceptual states. Instead, for Kant, the possibility of perception is defined in terms of conformity to a set of a priori conditions of sensibility such as space and time. What is then given to us as appearances, for Kant, are *spatio-temporal objects* as given to the mind in empirical intuition (through space and time as a priori forms of sensibility). It is at this point that we have to mark an important distinction between appearances and phenomena.

At the outset of the *Transcendental Aesthetic* Kant defined an appearance as "the undetermined object of an empirical intuition" (A20/B34). Appearance refers then to a spatio-temporal object as merely given to the

---

6. As it is custom, A refers to the first edition of the *Critique of Pure Reason* (1781), while B refers to the second and substantially modified edition (1787). I shall henceforth use the Guyer and Wood (1997) English translation.

mind in empirical intuition and conceptually still "undetermined," not brought yet under the categories of the faculty of understanding. A phenomenon, on the other hand, is a *conceptually determined appearance,* namely an appearance that has been brought under the categories of the understanding. Kant gives a detailed analysis of this distinction in the Third Chapter of the *Analytic of Principles,* where he says that "appearances, to the extent that as objects they are thought in accordance with the unity of the categories, are called phenomena" (A249).

From a Kantian perspective then, we gain scientific knowledge of nature by subsuming appearances (i.e. spatio-temporal objects as given to our mind in empirical intuition) under concepts of the understanding (via schemata). Our scientific knowledge is confined to phenomena intended as *conceptually determined appearances.* Phenomena are not *what appear to our senses.* Kant's Copernican solution to the problem of explaining *how* scientific knowledge is *possible,* can be found—I want to suggest—in the revolutionary new conception of phenomena that he put forward in opposition to the empiricist tradition.

Kant introduced the notion of a priori to justify how we can have scientific knowledge (i.e. mathematical, geometrical and physical knowledge), which derives from experience and yet is apodictically certain. Scientific knowledge takes the form of 'synthetic a priori judgments', based on the interplay of what Kant called the faculty of sensibility (with its a priori forms of space and time) and the faculty of understanding (with its a priori categories and principles, such as causality, among many others). Of course, the Kantian notion of a priori has been at the centre of an ongoing debate since the beginning of the twentieth century, when with the advent of relativity theory and quantum mechanics, the apodictic certainty of Euclidean geometry and Newtonian mechanics, which Kant meant to justify with his notion of 'synthetic a priori judgments', has been proved false.

An entire generation of philosophers and physicists, from Hans Reichenbach to Ernst Cassirer to Hermann Weyl, engaged with the vexed question of how to salvage the salvageable in Kant's system, when the very foundations of the system proved to be on shaky grounds.[7] And as early as 1920, in an important book entitled *Theory of Relativity and A Priori Knowledge,* Hans Reichenbach made an important distinction between two possible meanings of the Kantian notion of a priori: i) a priori means fixed and unrevisable for all times; but it also means ii) *constitutive of the object of experience.* In the light of the scientific revolutions of the twentieth century,

---

[7] For an excellent analysis of how the reception of relativity theory was filtered through Kantianism, see Ryckman (2005).

Reichenbach concluded that while the first meaning of the Kantian notion of a priori is untenable (nothing is fixed and unrevisable in the history of physics, and we now have a better theory, relativity theory, which has replaced Newtonian mechanics that Kant thought to be the state of the art); on the other hand, the second meaning of the Kantian a priori maintains all its validity for modern physics. In other words, according to Reichenbach, the main function of the Kantian a priori was to make experience of nature possible, i.e. to lay down the *conditions of possibility* of our scientific knowledge of nature. This is what it means to be *constitutive of the object of experience.*

For instance, causality, which Kant took to be an a priori principle of the faculty of understanding (what he called 'the second analogy of experience') and which, following Hume, he believed is not in nature itself, but is instead projected onto nature by the human mind, can be regarded a priori in the second (Reichenbachian) sense. More generally, for Kant the analogies of experience are *constitutive principles* because they "seek to bring the *existence* of appearances under rules a priori" (A179/B221), that is, they determine how appearances can be ordered temporally. For example, we can order temporally the appearance of motion according to the succession of cause and effect. Kant saw causality instantiated in nature via moving forces as the causes of determined effects. In particular, he saw in Newton's impressed force F—as captured by Newton's second law—an expression of causality with respect to the accelerated motion imparted on a body as an effect.[8] Even if we now have a better scientific theory (general relativity) and no longer identify—as Kant did—causality with an impressed force F *causally responsible* for the acceleration of bodies as per Newton's second law ($F = ma$), nonetheless we can retain the idea that causality is a *constitutively a priori* element of our experience of nature: that is, it has to be in place for us to be able to have scientific knowledge of nature and to identify Humean empirical regularities as lawlike.

This is the meaning of the Kantian notion of a priori that in recent

---

[8] Kant identified Newton's three laws of motion with 'metaphysical principles of natural science', whose transcendental counterparts are the three analogies of experience (i.e. substance, causality and community) in the *Metaphysical Foundations of Natural Science,* chapter 3. Some philosophers (Friedman 1992a, 1992b) have interpreted this move as an attempt to secure the 'synthetic a priori' nature of Newton's law of gravitational attraction, which would be 'synthetic' because derived from observed motions of planets as recorded by Brahe and Kepler, and 'a priori' because of the transcendental back-up that causality provides as a principle of the understanding via the notion of an impressed force as per Newton's II law, from which the law of universal gravitation can be deduced. Other Kantian scholars (Allison 1994; Buchdahl 1969 and 1974) have questioned this interpretation of Kant in favour of a 'looseness of fit' between the transcendental apparatus, on the one hand, and Newtonian physics, on the other hand.

time Michael Friedman (2000; 2001) has stressed, building up on Reichenbach's original intuition. Friedman has defended what he calls *relativized a priori* principles as, on the one side, vindicating the Kantian idea of *constitutive* elements of a scientific framework, and, on the other side, as *relative* to it, and revisable during a scientific revolution. Hence a resultant form of 'dynamic Kantianism'. 'Dynamic Kantianism' is a fascinating attempt to reconsider and adapt Kant's transcendental philosophy to modern science, by disentangling it from the fortunes of Newtonian mechanics and Euclidean geometry, upon which it was originally patterned. Most importantly for the purpose of my paper, dynamic Kantianism provides the philosophical lens, through which we can look at the history of science and at the historical evolution of scientific theories in the same spirit that motivated Kant's original desire to answer the question of *how scientific knowledge is possible.*

In the following two sections, I give a slightly new twist to this Reichenbachian–Friedmanian notion of *constitutive a priori* by showing how it enters directly into what in my view is Kant's richer conception of phenomena. In Section 3, in particular, I show that Kant saw in physical forces in nature the expression of the *constitutive a priori* principle of causality. Building up on Friedman's analysis of the *Metaphysical Foundations of Natural Science,* I show how in an important work called "Transition from the Metaphysical Foundations of Natural Science to Physics" and published in the *Opus postumum,* Kant saw in the historical evolution from Galilean kinematics to Newtonian dynamics the search for moving forces as the *causes* of *appearances,* and hence ultimately as a way of transforming *appearances* into *phenomena,* qua objects of scientific knowledge. The implications of the constitutive a priori for Kant's conception of phenomena has not been analysed in the recent Kantian literature: and the goal of this paper is to elucidate precisely such implications.

Here below I clarify this Kantian epistemological insight by looking at one specific historical episode that Kant repeatedly mentioned in various works in strict conjunction with his "Copernican revolution": namely, Galileo Galilei's demonstration of the law of free fall. Indeed, in the Preface to the second edition of the *Critique of Pure Reason* (1787), as a paradigmatic example of his Copernican revolution Kant chose precisely Galileo (as well as Torricelli and Stahl), and his famous experiment with the inclined plane:

> When Galileo rolled balls of a weight chosen by himself down an inclined plane, . . . a light dawned on all those who study nature. They comprehended that reason has insight only into what it itself produces according to its own design; that it must take the lead

> with principles for its judgements according to constant laws and compel nature to answer its questions, rather than letting nature guide its movements by keeping reason, as it were, in leading-strings; for otherwise accidental observations . . . can never connect up into a necessary law, which is yet what reason seeks and requires. Reason, in order to be taught by nature, must approach nature with its **principles** in one hand, . . . and, in the other hand, the **experiments** thought out in accordance with these principles—yet in order to be instructed by nature not like a pupil, who has recited to him whatever the teacher wants to say, but like an appointed judge who compels witnesses to answer the questions he puts to them. . . . This is how natural science was first brought to the secure course of a science after groping about for so many centuries. (Bxiii–xiv, emphases added)

Galileo is here portrayed as the scientist who paradigmatically accomplished the revolutionary shift that Kant was urging for: namely, the shift from the empiricist view that our scientific knowledge of nature proceeds from nature itself to the opposite Kantian view, according to which "we can cognize of things a priori only what we ourselves have put into them" (Bvxiii). The certainty and secure foundation achieved by natural science from the time of Galileo onwards was—to Kant's eyes—the paradigmatic expression of this shift. Reason must approach nature with its *principles* on the one hand, and with *experiments* thought out in accordance with these principles, on the other hand. We should therefore take a look at Kant's new conception of phenomena in close connection with his position on Galileo's mathematization of nature.

3. Kant on Galileo's Mathematization of Nature

In what follows, I want to clarify the particular stance Kant took on Galileo against the empiricist tradition exemplified by Duhem's aforementioned remarks. Kant too, like Duhem, saw in Galileo a turning-point in the history of science, but for very different reasons. By asking how pure natural science is possible, Kant was trying to justify why we *do* in fact have a new science of nature from the time of Galileo onwards, against the empiricist tradition that takes nature as a bunch of phenomena to be saved by introducing hypotheses that cannot be proved to be true. In particular, I concentrate my analysis on Kant's last, and incomplete work "Transition from the Metaphysical Foundations of Natural Science to Physics" published in the *Opus postumum.* Indeed, it is in this last and never completed work, which in Kant's intentions was meant to fill a gap that he felt was still open in his critical philosophy after the *Critique of Judgment,* that

Kant's new conception of phenomena is discussed in strict relation to Galileo's mathematization of nature.[9]

Kant wrote the "Transition to physics" in the last years of his life (fascicles Xth and XIth are dated around 1799–1800, just four years before Kant's death in 1804), when under the influence of Lavoisier's chemical revolution and the recent discoveries in pneumatic chemistry and ether theories, he became increasingly concerned with the problem of grounding a *system of empirically given forces* in nature.[10] The problem is that in nature we may observe objects moving, changing physical state (from solid to liquid to gaseous) or displaying some properties (for example, being elastic). But these are only appearances [*Erscheinungen*], for Kant. Only when we introduce moving forces as the underlying *causes* that make the objects move in a certain way, or change physical state or displaying some physical or chemical properties, do we have a conceptually determined appearance or *phenomenon* as the proper object of scientific knowledge.

I think this is the crucial, distinctively new feature that Kant introduced in the conception of phenomena: a physical phenomenon—intended as a conceptually determined appearance—has built in it from the very outset the concept of a moving force as the *cause* of the observed appearance. It is the causal concept of a moving force that distinguishes phenomena from appearances, or better, that transforms appearances into phenomena, that is, into *objects of experience.* If this analysis is correct, we can begin to catch a glimpse of the radically new conception of phenomena that Kant was introducing. Physical phenomena have built into them the concept of, say, a *dynamic cause* responsible for the observed appearances and their kinematical properties. Appearances are related to phenomena as kinematics is related to dynamics. Physical phenomena are *constituted* by applying dynamical concepts to kinematical appearances, where by phenomena being *constituted* I mean—in the original Kantian sense—that phenomena require and presuppose the principle of causality as a *constitutive a priori* principle that finds its counterpart in the dynamical concept of a moving force. No wonder, the search for a system of moving forces became central to Kant's "Transition to physics."

We can envisage here an interesting link with Duhem. While Duhem listed Newtonian mechanics with its attractive and repulsive forces acting at a distance among the 'metaphysical' theories postulating occult causes, Kant gave a very different story about Newtonian mechanics. According

---

[9] I have discussed in more detail the new conception of phenomena outlined in Kant's "Transition to physics" in Massimi (2008), on which I draw here.

[10] For an illuminating analysis of how Lavoisier's chemical revolution somehow finds echoes in this last incomplete work of Kant, see Friedman (1992a) chapter 5; for an alternative analysis of the "Transition," see Eckart Förster (2000).

to Kant, those forces, far from being occult causes, were the physical expression of the fundamental role the a priori principle of causality plays in the *constitution* of phenomena (as a *constitutive a priori* principle).[11] The fundamental role that in the "Transition to physics" Kant assigned to moving forces, in particular to Newton's gravitational attraction, as the *efficient cause* of *appearances,* e.g. of relative motions of planets kinematically described by Kepler as well as of relative motions of free-falling objects described by Galileo, sits squarely with the interpretive line I am suggesting about Kant's new conception of phenomena. Most importantly, to Kant's eyes, Galileo paved the way to Newton by offering a kinematical analysis of free fall that Newton completed with his dynamical concept of gravitational attraction:

> The laws of motion were sufficiently established by Kepler's three analogies. They were entirely mechanical. Huygens knew also of composite yet derivative motion. . . . But no matter how close they both [came to postulating universal gravitation]—for Galileo had long before that given the law of the gravity of falling bodies at heights which led to an approximately equal moment in their fall—all that which had been achieved *remained empiricism in the doctrine of motion,* and there was as yet no universal principle properly so-called, that is, a *concept of reason,* from which it would be possible to infer a priori to a law for the determination of forces, as *from a cause to its effect.* This solution was given by Newton, inasmuch as he gave the moving force the name attraction, by which he made apparent that this cause was effected by the body itself immediately, not by communication of the motion to other bodies—thus, not

---

[11] "*Motion* can be treated entirely mathematically, for it is nothing but concepts of space and time, which can be presented *a priori* in pure intuition; the understanding *makes* them. Moving forces, however, as efficient *causes* of these motions, such as are required by physics and its laws, need philosophical principles. All mathematics, then brings one not the least bit nearer to philosophical knowledge unless a *causal combination,* such as that of attraction and repulsion of matter *by its moving forces,* is first brought onto the scene and postulated for the sake of appearances. As soon as the latter occurs, the transition to physics has taken place, and there can be *philosophiae naturalis principia mathematica.* This step was taken by Newton in the role of a philosopher who brings new forces onto the scene. . . . Once Kepler's three analogies had grounded all the mathematically determined laws of motion of the planets by sufficient observation, there yet remained the question for physics regarding the *efficient cause of this appearance;* Newton, in order to find a way out of this difficulty, built a bridge from mathematics to physics, namely the principle of an attractive force. . . . according to the law of the inverse square of the distance. He did not, thus, rest content with appearances, but brought into play a primordially moving force" (Kant "Transition," Ak 22:516 in Kant [1936, 1938] 1993; emphasis added).

> mechanically, but purely dynamically. (Kant 'Transition', Ak 22: 528 in Kant [1936, 1938] 1993; emphases added)

The passage from what Kant calls *empiricism in the doctrine of motion* to a proper science of nature, namely to Newton's *philosophiae naturalis principia mathematica,* runs parallel to the passage from kinematics to dynamics: from spatio-temporal *appearances* that can be described kinematically, to Newtonian physical *phenomena,* whose kinematical properties can be traced back to dynamic causes (that is, gravitational attraction).[12] Galileo occupies a central role in this passage. No wonder Kant mentioned Galileo not only in the Preface to the second edition of the *Critique of Pure Reason* but also in the *Opus postumum.* In what follows, I want to clarify how Kant's revolutionary new conception of phenomena may have been patterned upon Galileo's investigation of nature; or better, how some aspects of the latter may have inspired the former.

In the next section I take a historical look at Galileo's demonstration *ex suppositione* of the law of free fall. The purpose of this historical section is to clarify the crucial role Galileo played in the passage from Aristotelian to Newtonian mechanics, and more precisely, to highlight some aspects of Galileo's mathematization of nature that can be interpreted in a Kantian way, that is, as a step towards the *constitution* of the new phenomenon of uniformly accelerated free-falling objects, in between Aristotle's theory of natural motion and Newton's later theory of gravitational attraction.

4. Galileo and the Law of Free Fall: Science between Reason and Experiment

In the past four decades, Galileo's scientific methodology has been at the centre of a voluminous literature that in various ways has combined detailed historical investigations of Galileo's manuscripts and folios with a re-assessment of some traditional philosophical views of Galileo as a ratio-

---

[12] It must be noted here that what Newton in the *Principia* called the phenomena, from which he deduced the law of gravitational attraction, were simply relative motions of satellites around their main planets or relative motions of planets around the Sun as described by Kepler's laws (I thank Torretti and Achinstein for stressing the different uses of the expression 'phenomena' in Newton and in Kant). Thus, in a way, what Newton called 'phenomena' is closer to what Kant would probably call 'appearances'. On the other hand, the very same fact that Newton deduced the law of gravitational attraction from them may suggest that these appearances (in Kant's terminology) hinted already at the dynamic concept of an attractive force as their *cause,* which is precisely the way Kant interpreted Newton's deduction of the law of gravitational attraction starting from 'appearances' (namely, relative motions of planets around the Sun) *and* Newton's second law (instantiating the a priori principle of *causality* in the form of an impressed force F responsible for changes in velocity, following Friedman's analysis).

nalist and a Platonist, mainly due to Alexander Koyré's (1939) influential historiography. Thanks to the acute analyses of Settle (1961), Drake (1973a; 1973b; 1974), Naylor (1974; 1977), McMullin (1967), among others, from the 1960s–70s onward increasing attention has been paid to Galileo the experimenter, and important studies have been conducted on Galileo's use of machines (lever, pendulum, balance, inclined plane; see for instance Machamer 1978; 1998b). Going beyond the stereotypical dichotomy of Galileo the experimenter *versus* Galileo the mathematician, Galileo's scientific methodology has been rediscovered in all its complexity and nuances.

As mentioned in the Introduction, it is not my goal here to enter into scholarly interpretations of Galileo nor to take one side rather than another. Instead, my goal is to highlight some salient aspects of Galileo's mathematization of nature that have been recognised by Galilean scholars, and show how these aspects can be interpreted along Kantian lines. In other words, I am not going to claim that Galileo was a rationalist as opposed to an empiricist, nor that Kant's analysis of Galileo is historically the correct one. I am simply not making a historiographic point about Galileo here. Instead, what I am going to claim is that there are aspects of Galileo's mathematization of nature that find a natural counterpart in Kant's conception of phenomena and can hence explain why Kant chose Galileo to illustrate his Copernican revolution in philosophy.

In particular, I refer to W. L. Wisan's historical analysis of Galileo and to the methodological difference she noted between Galileo's approach to astronomy and his approach to mechanics. *Contra* Duhem, Wisan claims that Galileo seemed to have followed the Platonic tradition behind the *method of the astronomer* in believing that astronomy resorted to hypotheses and consequences derived from the hypotheses, although in his defence of Copernicanism he strived to show that there is only one correct hypothesis and it is possible to prove wrong any alternative one.[13] However, this was not the method Galileo used in the science of mechanics and the new science of motion, where Wisan would agree with Duhem that Galileo introduced a brand new *method of the physicist:*

> his chief obstacle in completing the new science of motion arose precisely from the fact that it required new principles not immediately evident in the sense Galileo believed necessary. Far from using the modern method of hypothesis, deduction and experimental

---

13. "His method in astronomy was not that of the mathematician who derives true conclusions from true principles but that of deriving conclusions from hypotheses, confirming the conclusions by observations and showing that all alternative hypotheses which are proposed must fail" (Wisan 1978, p. 14).

> verification, Galileo never quite saw that the principles of mechanics, *once made evident by reason and immediate experience,* could still be falsified through remote consequences. (Wisan 1978, p. 4; emphasis added)

In the new science of motion, by contrast with astronomy, Galileo strived to provide a secure foundation in terms of self-evident and indubitable principles. While astronomy relied heavily on observations and experience, Galileo thought that in the science of motion, there are a few fundamental principles from which it should be possible to derive the whole body of knowledge. If it is possible to identify these self-evident and indubitable principles *through reason and immediate experience,* then since true conclusions follow from true premises, the whole issue of testing empirical consequences so as to ascertain empirically the validity of the new science of motion becomes superfluous. Finding self-evident and indubitable principles for the new science of motion became Galileo's central issue, right from the beginning in his Pisan treatise *De motu* (1590–2) up to his final masterpiece *Discourses and Mathematical Demonstrations on Two New Sciences* (1638). In this section, I take a brief look at Galileo's life-long attempt to provide a solid foundation to the new science of motion. It is not my aim to reconstruct the history of how he discovered the law of free fall, or what sort of experiments with the inclined planes he performed, or what type of data he may have had available for drawing his conclusions. Excellent and authoritative historical studies on this topic have already been conducted, to which I could hardly add anything.[14] Instead, more modestly, my goal here is to take a philosopher's look at Galileo's scientific methodology in the science of motion. I want to show that Kant's repeated emphasis on Galileo's approaching nature with principles of reason *and* experiments finds a natural counterpart in Galileo's search for self-evident and indubitable principles to ground his new science of motion, namely principles that—to use Kant's terminology—may be known to be true a priori, i.e. known to be true *independently of experience,* or better, despite the lack of sufficiently strong evidence.[15]

[14] In addition to the pioneering historical studies mentioned above, for a more recent historical analysis see Palmieri (2006).

[15] This is Kant's original definition of 'a priori'. As Caygill (1995), p. 36, notes: a priori judgments arise "'independently of experience' as opposed to those a posteriori modes of knowledge which are 'borrowed solely from experience' (A 2). They are independent of experience in that they do not contain any 'admixture' of sensibility and in that they may not be derived from it. Kant argues further that they . . . are even the condition of experience". This original definition of the Kantian a priori precedes and grounds of course the later Reichenbachian–Friedmanian re-interpretation of some a priori principles, such as causality, in terms of "constitutive of the object of experience". As we shall see below, there

4.1. Aristotelian A Priori *versus* Kantian A Priori: Galileo's Search for Self-Evident and Indubitable Principles in the Science of Motion

A ground-clearing clarification is necessary at this point. It may be objected that no one was more against any aprioristic reasoning than Galileo, as his long polemic with the Aristotelians easily testifies to. One of the most eloquent examples, for instance, can be found in his letters on sunspots (1613) against the Aristotelian Jesuit Christopher Scheiner, who had written a treatise claiming that the spots recently revealed by the new telescope could not have been sunspots because the sun as any other celestial body is incorruptible. Similar aprioristic lines of reasoning can be found in Simplicio's defence of Aristotle in the *Dialogue concerning the two chief world system* (1632), where as Wisan points out "Simplicio has claimed that Aristotle always began a priori, showing the necessity of his conclusions 'by means of natural, evident, and clear principles' and only afterward supported these a posteriori by means of the senses" (Wisan, 1978, p. 31; quotation from Galileo [1632] 1962; second edition 1967, p. 50). Hence we should clearly distinguish and not confuse the Aristotelian notion of 'a priori' that Galileo attributed to Simplicio and that he was at pains to avoid,[16] and the chronologically much later Kantian notion of 'a priori', which—if my interpretive analysis is correct—Kant saw at work in Galileo's scientific methodology. The Aristotelian notion criticised by Galileo starts from allegedly 'natural, evident, and clear principles' (such as the incorruptibility of celestial bodies) to draw physical conclusions that may well go against any available experimental evidence, as in the case of the sunspots revealed by the new telescope.

The Kantian notion of a priori is much more complex than Simplicio's: as clarified in section 2, a priori means *constitutive of the object of experience* and this is the relevant Kantian notion of a priori that still applies today. Having clarified this distinction between the Aristotelian a priori and the Kantian a priori, what remains to be shown is to what extent this Kantian notion of *constitutive a priori* can arguably be seen at work in Galileo's scientific methodology.

I want to suggest that a plausible candidate for the Kantian notion of

---

is an important step in Galileo's demonstration of the law of free fall where he introduces a postulate which he presents to be true *independently of experience,* or better—as I said above—true despite the lack of sufficiently strong evidence. This postulate embeds a causal / dynamical concept that—I will suggest—can be regarded as "constitutive of the object of experience" in the later Reichenbachian–Friedmanian sense clarified in Section 2.

[16] The Aristotelian version of the 'a priori' that Galileo ascribed to Simplicio is not necessarily Aristotle's, for whom a priori principles are themselves based on experience in rather complex ways. I thank an anonymous referee for drawing my attention to this point.

constitutive a priori can be found in Galileo's search for self-evident and indubitable principles to ground his new science of motion. In the science of motion, Galileo endorsed the mathematicians' and geometers' axiomatic method, exemplified by Euclid's *Elements,* whereby true and indubitable principles are established *through reason and immediate experience alone* as axioms, from which true theorems follow. Those true, self-evident, and indubitable principles cannot be established a posteriori via observation and experiment at the cost of losing their axiomatic, apodeictic character. Galileo repeatedly mentioned his search for such principles, most famously in his 1604 letter to fra' Paolo Sarpi where he originally announced the (mistaken) law of free fall (*v:s*) and spoke of "totally indubitable principles" from which to derive his propositions. A propos of this, Wisan notes:

> One result of Galileo's conversion by the mathematicians was an excessive rationalism as shown . . . in his treatment of acceleration. . . . Similarly, his first study of motion along inclined planes [*De motu*] had little bearing on the behaviour of physically real bodies on physically real planes and it resulted in a number of curious, unverifiable propositions. . . . [In *Two New Sciences*] we find him still assuming the mathematical model: true conclusions must be derived from true and evident principles. . . . But Galileo's science of motion . . . required principles which he could not establish according to this ideal. (Wisan 1978, p. 10 and 37)

Indeed, that Galileo's experiments with the inclined plane had little bearing on the actual behaviour of physically real bodies on physically real planes is also confirmed by another Galilean scholar R. H. Naylor (1974, p. 113, 115, 116), who in a detailed study of Galileo's experimental evidence for the law of free fall concluded

> What does seem certain is that Galileo noticed the persistent discrepancy between his theory and observations. . . . The view that Galileo would roll spheres down an incline, compile a list of observations, and then realise his total inability to interpret the information certainly seems a little out of character. Was Galileo such an ardent empiricist? I doubt it. . . . Although we know Galileo had discovered his law of free fall by 1604, it is difficult to establish for certain how and when he first obtained a conclusive experimental confirmation. . . . The law, according to which distances travelled from rest during an accelerated motion were always proportional to the time squared, had little observational evidence to support it.

In the following sub-sections, I want to try to make sense of what Wisan calls Galileo's "excessive rationalism", and his life-long search for indubitable principles for his new science of motion. In section 4.2, I take a brief look at Galileo's original treatise on accelerated motion, *De motu antiquiora,* written in his early Pisan years between 1589 and 1592, and containing some seminal ideas for his later analysis of free fall. In section 4.3, I turn to Galileo's main work on the subject, *Discourses and Mathematical Demonstrations concerning Two New Sciences,* written during the last years of his life in 1638. In particular, I am going to focus on the principle that Galileo employed to ground his times-squared law of free fall: it consists of an important supposition ('supposizione'), which despite Galileo's best efforts could not be proved to be self-evident and indubitable, and whose truth is ultimately established *independently of experience* and by reason alone.

4.2 Galileo's *De motu antiquiora*

The experiment Kant refers to in the Preface to the *Critique of Pure Reason* is Galileo's famous experiment with the inclined plane, which he began to perform at a very early stage of his career, when he was in Pisa around 1590. Motion through an inclined plane is easier to study than vertical motion because the moving objects can be slowed down (depending on the angle of inclination of the plane): this was indeed an experimental situation that could be measured with the instruments available at Galileo's time. Through these experiments, Galileo famously found that spaces were to one another as the squares of the times (and this held for all possible inclinations of the plane). As we shall see in the following section 4.3, Galileo offered a mathematical demonstration of the times-squared law in *Two New Sciences* (1638), where he adopts the literary fiction of making Salviati read a treatise in Latin entitled *De motu locali* written by what Salviati refers to as his Academic friend, namely Galileo, who wrote indeed a treatise called *De motu antiquiora* during his early Pisa period between 1589–1592 (probably around 1590, although the book was never published).[17]

Already in this early Pisan treatise, Galileo's aim was to prove that

---

[17] The original manuscript is part of *Manoscritti Galileani* conserved at the Biblioteca Nazionale Centrale in Florence. Antonio Favaro published it as part of his twenty-volume *Opere di Galileo Galilei,* but following a different order from the one of the original manuscript. Many of the observations announced in *Two New Sciences* had already been discussed by Galileo in this early treatise. For an introduction to the history of this manuscript, including its relation to other similar treatises on motion by some of Galileo's colleagues in Pisa (Francesco Buonamici and Girolamo Borro), see Wallace (1998). For an excellent, detailed analysis of *De motu locali* see Wisan (1974). For a historical reconstruction of the role of Galileo's kinematical studies for the following history of Newtonian dynamics, see Westfall (1971).

Aristotelians were wrong in claiming that free-falling bodies were moving towards a natural place. Against Aristotle's theory of motion towards a natural place (up or down), Galileo argued that there is in fact only one type of motion (down) that he explained in analogy with Archimedes' hydrostatics by considering the ratio between weight per volume of the body and weight per volume of the surrounding medium. Thus, if bodies seem sometimes to move upwards, this is not because they move towards a natural place (as Aristotle claimed) but because they must have a specific weight that makes them, so to speak, 'float' in their surrounding medium.[18] Of course, there was an immediate problem with this analysis: if the velocity of a free-falling body is directly proportional to its specific weight, the body can only move with uniform velocity. How is it possible to account for accelerated motion? In order to account for the possibility of accelerated motion, Galileo had to take the distance from the naïve view that related speeds to weights, and articulate an explanation of accelerated motion as due to the gradual decay of the force that originally pushed the body up to the starting point of the free fall. It is worth quoting in some detail Wisan's authoritative view on Galileo's scientific method here:

> Galileo gives what he considers a brilliant demonstration that acceleration in free fall is 'accidental' since it is caused by the gradual decay of the impressed force which raises the body to the point from which it begins to fall. Acceleration is thus shown to be an unnatural (or violent) motion. Here we find a 'resolutive method' being explicitly employed by Galileo as he announces that this is the method by which the cause of the effect was investigated: *huius effectus causam indagemus, haec resolutiva methodo utemur.* . . . From this principle [i.e. gradually decay of the projecting force], it follows that at some point the projecting force will no longer be sufficient to raise the projected object, at which point the object begins to accelerate downward.. . . The principle found is neither discovered nor established by the resolutive method but has already been rendered evident by numerous arguments and examples (in Chapter 17). *'Discovering the true cause' thus means discovering which known principle provide the ground for the given proposition.* (Wisan 1978, p. 9; emphasis added)

Let me stress two main points with respect to Wisan's analysis. First, Galileo's use of the so-called 'resolutive method' as a method for finding 'true causes' for a given effect (accelerated motion) testifies to Galileo's commit-

---

[18] On the Archimedean origins of Galileo's theory of motion, see Machamer (1998b) and Westfall (1971, ch. 1).

ment to causal explanations in his kinematical studies. The search for 'true causes' so understood should not be confused or conflated with Galileo's reluctance to engage with the Aristotelian hypothetico-deductive procedure of speculating about the possible *causal hypotheses* of a motion so accelerated, which—as will be clarified in section 4.3—he dismissed as "fantasies".[19] As Wisan nicely put it, 'discovering the true cause' for Galileo does **not** mean to speculate about possible causal hypotheses, because his method in the science of motion is **not** the hypothetico-deductive one. Rather, for Galileo, 'discovering the true cause' means discovering what known principle can provide the ground for a given proposition (e.g., accelerated motion of free-falling bodies). This leads me to the second point I want to make. Galileo identified the 'true cause' with the principle of a projecting force gradually decaying, which was not discovered empirically but—as Wisan says—was "rendered evident by numerous arguments and examples (in Chapter 17)". This principle is indeed the ancestor of the very similar principle that Galileo employed about forty years later in the mathematical demonstration of the times-squared law of free fall in *Two New Sciences* (1638), as we shall see in section 4.3.

It took Galileo some time to correct the basic mistake of searching for a sort of Archimedean-like relation between velocity change and specific density, rather than searching for velocity change with respect to *time* (which seems to date back to 1609). However, already in *De motu antiquiora,* Galileo opposed to Aristotle's cause of motion (i.e., motion towards a natural place) his own search for true cause *(vera causa).* What was crucially still missing, and always missed in Galileo's story is the later Newtonian notion of gravitational attraction. Although Galileo spoke of the heaviness *(gravitas)* of bodies characteristic of all matter, he did not have and never had a positive *dynamic* account of the nature of *gravitas,* i.e. of the *dynamic force* responsible for uniformly accelerated motion. In the *Dialogue concerning the two Chief World Systems* (1632), for instance, he wrote:

> We do not really understand what principle or what force it is that moves stones downward, any more than we understand what moves them upward after they leave the thrower's hand, or what moves the moon around. We have merely . . . assigned to the first the more specific and definite name "gravity". (Galileo [1632] 1962, p. 234)

Galileo's notion of 'gravity' is at quite a distance from Newton's later notion of an external accelerating force acting at a distance, and because of its

---

[19] I thank Ernan McMullin for drawing my attention to Galileo's rejection of the Aristotelian "fantasies".

Archimedean origins, it is closer to the idea of an *internal static* force of a body. At the same time, it is also at quite a distance from the Medieval impetus theory of Buridan and Oresme.[20] It is in this specific sense, equidistant from the impetus theory and Newton's later dynamics, that Galileo sometimes spoke of *momentum gravitatis* as a (weight-related) force concept **causally** responsible for the accelerated motion of free-falling bodies, and hence for their degrees of velocity acquired *(celeritatis momenta).* We can see here at work Duhem's continuity thesis: Galileo's concept of *momentum gravitatis*—which sometimes he simply referred to as *momento* (interchangeably with *impeto*)—bears continuity with the Medieval impetus theory. Indeed, Galileo shared with it the idea of an internal static force of a body that he still thought of as related to the concept of weight/heaviness. At the same time, as anticipated in the Introduction, despite this continuity, there is a revolutionary new element in Galileo's analysis of free-fall that was not anticipated by Buridan or Oresme's impetus theory. In the next sub-section, I try to clarify this revolutionary new element: namely, Galileo's use of this (weight-related) force concept in his demonstration *ex suppositione* of the law of free fall.

4.3. Galileo's De motu locali in Two New Sciences

There is a surprising continuity between the two aforementioned points we have just discussed about *De motu antiquiora* (1590), and some of the themes in Galileo's later mathematical demonstration of the law of free fall in *Two New Sciences* (1638). In the Third Day of *Two New Sciences,* in the Section on 'Naturally accelerated motion', Galileo begins with a declaration that sounds like a manifesto against the empiricist tradition of 'saving the phenomena':

> And first of all it seems desirable to find and explain a definition best fitting natural phenomena. For anyone may invent an arbitrary type of motion and discuss its properties; thus, for instance, some have imagined helices and conchoids as described by certain motions which are not met with in nature . . . but we have decided to consider the phenomena of bodies falling with an acceleration such as actually occurs in nature and to make this definition of accelerated motion exhibit the essential features of observed accelerated motions. (Galileo [1638] 1914, p. 160)

Galileo defines uniformly accelerated motion as the motion that "starting from rest, it acquires, during equal time-intervals, equal increments of

---

[20] On the relationship between Galileo and Newton on forces and inertial problems, see Hooper (1998).

speed [temporibus aequalibus aequalia celeritatis momenta sibi superaddit]" ([1638] 1914, p. 169). In other words, a uniformly accelerated motion is such that the ratio between Δv (i.e. the equal increments of speed or *celeritatis momenta*) and Δt (i.e. equal time-intervals) is constant. There follows a series of objections by Sagredo and Simplicio, including Sagredo's speculations about the possible cause of uniformly accelerated motion, to which Salviati replies

> The present does not seem to be the proper time to investigate the cause of the acceleration of natural motion concerning which various opinions have been expressed by various philosophers, some explaining it by attraction to the center, others to repulsion between the very small parts of the body while still others attribute it to a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its position to another. Now, all these fantasies, and others too, ought to be examined; but it is not really worth while. At the present it is the purpose of our Author merely to investigate and to demonstrate some of the properties ['passioni'] of accelerated motion *whatever the cause of this acceleration may be.* ([1638] 1914, pp. 166–7; emphasis added)

The expression 'fantasies' in this quotation echoes the similar expression Galileo used in reference to the Aristotelians in a letter of 1616 (Galileo *Opere* IV, p. 521) that Wisan quotes to support her thesis that Galileo's scientific method was very different from the traditional hypothetico-deductive one: "The Aristotelians are criticised because instead of proceeding deductively step by step, they form from their *fantasia* a proposition from which they go immediately to the conclusion they want to prove" (Wisan 1978, p. 30).

The refusal to investigate the causes of uniformly accelerated motion in the quotation above should be understood—I want to suggest, latching onto Wisan's analysis—as a stance against the tradition that takes phenomena as ready-made, and reduces science to introducing a series of hypotheses that can save them (that is, the same tradition that Duhem saw exemplified in what he called the *method of the astronomer*). Galileo seems to be taking distance from this tradition in declaring himself not interested in speculating about the causal hypotheses that can save the phenomenon of uniformly accelerated motion. Instead, he is interested in demonstrating "some attributes of a motion so accelerated." As we shall see below, this important methodological declaration can help us understand why Galileo's method has nothing to do with hypothetico-deductivism, that is, with the procedure of introducing hypotheses, testing their empirical consequences and hence verifying one hypothesis over others available. In this respect, Galileo anticipates Newton's methodological *hypotheses non fingo.* On the other hand, as anticipated in section 4.2, I also want to suggest that the aforementioned quote should not be read as an expression of Galileo's disinterest in searching for the causes of uniformly accelerated motion *tout court.* True, Galileo's main concern was kinematics, not dynamics: he wanted to demonstrate the kinematical properties of uniformly accelerated motion. But at the same time, both in *De motu antiquiora* and—as we shall see shortly—in *Two New Sciences,* he resorted to a (weight-related) force concept causally responsible for the kinematical properties of uniformly accelerated motion. And this fact, in conjunction with his life-long search for a link between the Archimedean science of weight and the new science of motion, in my opinion, testifies to the actual level of interest Galileo had in discovering the 'true causes' of such motions, in continuity with the method already employed in *De motu antiquiora.*[21] Let us then take a closer look at Galileo's mathematical demonstration of the kinematical properties of free-falling objects.

It is at this point of the Third Day of *Two New Sciences* that Salviati introduces a key assumption or, as he calls it, a *supposition:* "This definition established, the Author makes a single assumption, namely: the speeds acquired by one and the same body moving down planes of different inclinations are equal when the heights of these planes are equal" (Galileo [1638] 1914 , p. 169). This is the key assumption that is supposed to be true, and from which Galileo's demonstration of the law of free fall follows. Despite the fact that Galileo knew of the law of free fall as early as 1604, following a long period of experimenting with inclined planes in Padua, at the time he still did not have what he called a natural principle from which to deduce the law. And the fact that thirty-four years later in *Two New Sciences,* when almost blind and under house-arrest in Arcetri, he felt the need to spell out the key assumption or supposition behind the mathematical demonstration of the law of free fall testifies to the central role that this supposition plays in Galileo's mathematization of nature, and, more in

---

[21] Apropos of searching for causes, Machamer (1978, p. 162 and 173), has pointed out that "Galileo is unconcerned about extrinsic, efficient causes (. . .) and concerned very much with formal and final causes, and sometimes material causes. (. . .) Such efficient causes [as those Galileo referred to as 'fantasies' in Third Day] leave us in the realm of opinion about the nature of the effects and the nature of the causes which brought them about. For proper demonstrations we need formal, final, and material (necessitating) causes, as found in the mathematical tradition". I agree with Machamer's analysis that Galileo does not endorse the standard Aristotelian notion of efficient cause, which lands us in the realm of opinion and fantasy. Instead the later Kantian notion of causality as a constitutive a priori principle seems to me to capture Galileo's scenario much better, as I shall explain here below.

general, the central role he ascribed to the axiomatization of the new science of motion.[22]

The supposition says that the speeds acquired by the same body descending along say the inclined planes CA and CD, respectively, are equal since the heights of theses planes are equal, namely CB [see Figure 1]. More in general, this is the same speed that would also be acquired by the body falling vertically from C to B. In order "to increase the probability [of this assumption] to an extent which shall be little short of a rigid demonstration", Salviati presents the following thought experiment ('esperienza').[23]

Imagine a vertical wall with a nail driven into it, and from the nail let us suspend a fine vertical thread with a lead bullet from A to B [see Fig. 2]. Then consider the horizontal line DC, at right angles to the vertical thread AB. If we now bring the thread with the bullet into the position AC and we set it free, we can observe it to descend along the arc CBD, until it almost reaches the horizontal DC. From this we infer that the bullet in its descent through the arc CB acquired a momentum ['impeto'] on reaching B which was sufficient to carry it through a similar arc BD to the same height. If we now drive another nail along the perpendicular AB at the point E closer to the horizontal DC, when we set the thread free from AC, it strikes upon the nail E and it traverses the arc BG with E as a center. This is done by the same momentum ['impeto'] which previously starting at the same point B carried the same body through the arc BD. If we now drive another nail F into the wall at an even lower point on the vertical AB, and again we set the thread with the bullet free from AC, the thread will traverse the arc BI terminating exactly on the horizontal line CD. Salviati then concludes:

> this experiment leaves no room for doubt as to the truth of our **supposition;** for since the two arcs CB and DB are equal and similarly placed, the momentum ['momento'] acquired by the fall through the arc CB is the same as that gained by fall through the arc DB; but the momentum acquired at B, owing through the fall through CB, is able to lift the same body through the arc BD . . .

---

[22] As Domenico Bertoloni Meli (2008) has pointed out, in the second and third day of *Two New Sciences* Galileo's main concern was with establishing an axiomatic science of motion on the example of Archimedes. Despite a voluminous historical literature in recent times on Galileo's experiments and machines, "his foundational efforts have attracted less attention, yet they constitute a major episode in the history of science."

[23] The Italian 'esperienza' is translated in Crew and de Salvio as 'experiment'. I translate it as 'thought experiment' instead because there is an element of idealisation as indicated by the verb 'imagine' in the following discussion about arcs and chords (we are assuming that there is no air resistance or friction etc.).

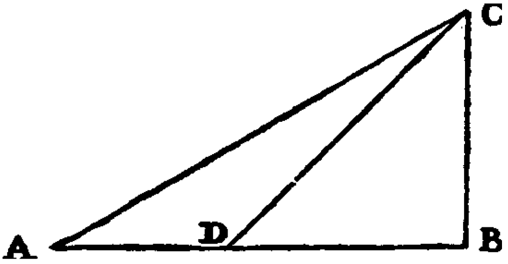

Figure 1

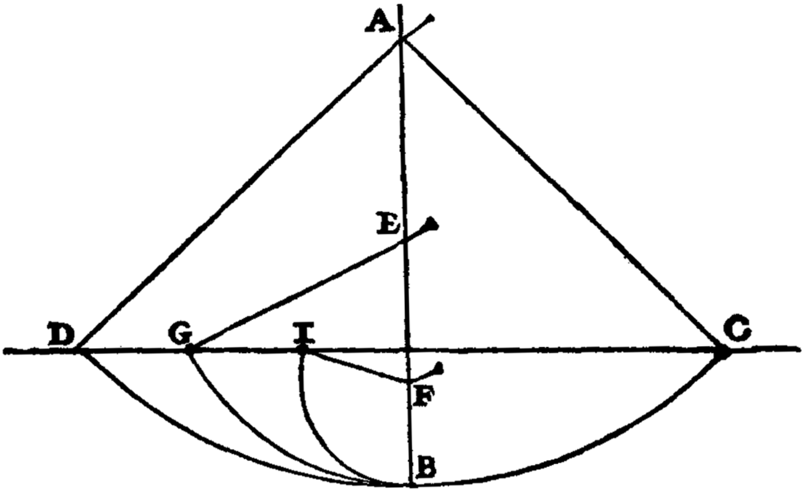

Figure 2

> in general, every momentum acquired by fall through an arc is equal to that which can lift the same body through the same arc. But all these momenta which cause a rise through the arcs BD, BG, and BI are equal, since they are produced by the same momentum, gained by fall through CB, as experiment shows. Therefore all the momenta gained by fall through the arcs DB, GB, IB are equal. ([1638] 1914, pp. 171–2)

This is further generalized and taken to be valid not just for arcs but also for the chords subtended to these arcs. There is however an inferential leap in this procedure and Salviati concedes that "we are not able, by similar means, to show that the event would be identical in the case of a perfectly round ball descending along planes whose inclinations are respectively the same as the chords of these arcs". In other words, if instead of arcs we consider the chords subtended to the arcs DB, GB and IB, a ball that has descended along the chord CB would lose part of the momentum [impeto] it acquired in the fall CB and would not be able to rise to the height of CD along the inclined planes DB, GB and IB. This difficulty notwithstanding, Salviati concludes "but this obstacle, which interferes with the experiment, once removed, it is clear that the momentum [impeto] (which gains in strength with descent) will be able to carry the body to the same height. Let us then, for the present, take this as a **postulate,** the absolute truth of which will be established when we find that the inferences from it correspond to and agree perfectly with experiment" ([1638] 1914, p. 172).

Let me make two comments. First, I think we should note here an important difference Galileo introduces between what he calls a supposition and a postulate. The experiment with the arcs is presented as a quasi-demonstration of the supposition that the speeds acquired by a body moving down planes of different inclinations are equal whenever the heights are equal. Whereas the supposition is presented as a principle whose truth is almost indubitable and self-evident as the thought experiment with the arcs is meant to show, the postulate is a counterintuitive assumption that must be accepted to back up the quasi-demonstration generalized from arcs to chords and hence to inclined planes.[24] Galileo's quasi-demonstration of the supposition depends on accepting the postulate; but the postulate is not itself self-evident and it goes in fact against intuitive experience.[25] Second, we can see how the (weight-related) force concept of

---

[24] I thank Katherine Dunlop for helpful advice on the notion of postulate at Galileo's time and the distinction between axioms/definitions vs. postulates, with a particular emphasis on the counterintuitive nature of postulates.

[25] Wisan (1978) does not distinguish, as I do, the supposition from the postulate necessary to back-up the supposition (following Galileo's terminology) and refers to the supposition itself as a 'postulate', and sometimes as a 'principle'. However, what she writes about it is in agreement with my analysis: "To confirm the postulate of equal speeds he had a demonstration with a pendulum from which the motion of bodies along inclined planes could be inferred by analogy. But neither of these provided a direct and exact visual demonstration of the principle to be established, and in fact Galileo could only confirm theorems derived from these principles" (p. 40). And again, p. 42: "the postulate is to be established by the method of hypothesis, deduction and experimental verification as it is understood today. Yet not a single experiment is offered to justify the postulate in this way! Nor does he appear to have looked for such an experiment. . . . Instead he continues to search for a more immediate demonstration or a deeper principle from which the postulate might be derived. . . . Later, in a letter to Baliani, 1 August 1639, replying to criticism of the way in which he tried to establish his postulate, Galileo admits that the principle he has supposed does not seem to have that *evidenza* for which one looks in principles which are to be assumed as known".

impeto, whose ancestor was the gradually decay of the projecting force in *De motu antiquiora,* is already at a distance from the Medieval impetus theory (that is, an internal force keeping the projectile in motion) because of the revolutionary new way in which Galileo used it in the thought experiment with the arcs and the chords. In Galileo, 'impeto' is synonymous with 'momento', and it is the product of a body's weight and speed. Already in the Pisan work *Le meccaniche,* working on balances, Galileo had defined the 'momento' as the propensity of a body to move downwards because of its weight and its position on the balance. In Koyré's words, "the *impetus* of the moving body is nothing other than the dynamic impulse given to it by its gravity" (Koyré [1939] 1978, p. 185). As Hooper (1998, pp. 159–60) has illuminatingly pointed out "In motion on inclined planes, the *momenta gravitatis,* which are due to the angle of descent, are shown to be congruent to the *momenta velocitatis* given by the rules of speed, and are taken as the explanation and cause of the latter." Thus the main function of this (weight-related) force concept is to *causally explain* why bodies moving down different inclined planes (with the same height) acquire the same degrees of speed. Galileo is using this force concept as the *cause* of the kinematical properties captured by the supposition (that is, equal degrees of speed over different inclined planes).

We can see in this specific aspect of Galileo's mathematization of nature a counterpart of Kant's view on phenomena as 'conceptualised appearances'. For Kant, phenomena are constituted by subsuming appearances, that is, spatio-temporal objects given in empirical intuition, under the categories of understanding (for example, quality), among whose a priori principles causality plays a key role. Most importantly, Kant saw in moving forces in nature an instantiation of the a priori principle of causality: in particular, he saw in Newton's gravitational attraction the principle that causally explained a great variety of appearances (from free fall to planetary motions) and could hence unify physics into a system. From a Kantian point of view, this process of 'constituting' phenomena by subsuming appearances under an a priori principle such as causality can be regarded, in a way, as beginning with Galileo. Kant's aforementioned remarks about Galileo's kinematical studies as a way of approaching nature via "principles of reason" and via "experiments thought out in accordance with those principles" seems to find a counterpart in Galileo's procedure. Galileo constituted the spatio-temporal properties of appearances such as balls rolling down inclined planes (that is, equal degrees of speed over different inclined planes) and then subsumed them under a causal concept (i.e. a weight-related force concept such as 'impeto' that entered into the quasi-demonstration of the supposition). The new phenomenon of uniformly accelerated free-falling bodies can then be regarded—from a Kantian point of view—as the end product of this two-stage process: 1) first, we have to 'constitute' the spatio-temporal properties of appearances, i.e. of balls rolling down inclined planes; and this is what Galileo's supposition does by ascribing equal degrees of speed over different inclined planes; and 2) we then have to subsume these spatio-temporal properties under a causal concept (such as Galileo's 'impeto' as the cause of the equal degrees of speed over different inclined planes). But how did Galileo demonstrate uniformly accelerated motion? How could he differentiate his procedure from the 'fantasies' of the Aristotelians?

From the supposition, Galileo derived two theorems, in particular the famous one is "[Theorem II] If a moveable descend from rest in uniformly accelerated motion, the spaces run through in any times whatever are to each other as the duplicate ratio of their times; that is, are as the squares of those times." The mathematical demonstration of this theorem, which contains Galileo's times squared law of free fall, is very ingenious indeed. Galileo could not in fact avail himself of calculus to calculate instantaneous velocities. Nevertheless, he was able to prove that the ratio between space intervals was equal to the ratio between the *squares of the time intervals* required to traverse those spaces.

He imagined the flow of time between any initial and final instant A and B as a vertical line AB, in which he identified some time intervals AD and AE [see Figure 3]. He then represented the space with another vertical line going from H to I, with space intervals HL and HM. How could he prove that $HM:HL = AE^2 : AD^2$? He imagined another time line AC drawn from A at any angle whatever with AB. Suppose we now draw parallel lines that from points D and E intersect the new time line AC in O and P, respectively. The parallel line DO represents the maximum degree of speed acquired at instant D of time interval AD, and EP the maximum degree of speed acquired at instant E of time interval AE.[26] In the previous Theorem I, the so-called mean speed theorem, Galileo had proved that the

---

[26] The identification of the parallel lines DO and EP with the maximum degree of speed acquired respectively at instant D of time AD, and instant E of time AE is typical of Galileo and had already been employed in *Dialogue* (1632, 28): "We may likewise suppose that the degree of velocity acquired at a given point of the inclined plane is *equal* to the velocity of the body falling along the perpendicular to its point of intersection with a parallel to the horizon through the given point of the inclined plane". In the absence of calculus, to define instantaneous velocity Galileo resorted to the expedient of thinking velocity as the sum total of 'degrees of speeds' geometrically represented by parallel lines composing the surface of the triangle subtended to the inclined plane, with the triangle base representing the maximum degree of speed acquired in uniformly accelerated motion at the bottom of the plane, and the lines getting shorter and shorter until they reach 0 corresponding to the top end of the inclined plane where the body is at rest. For an analysis of this procedure as exposed in Galileo's *Dialogue,* see Feldhay (1998, pp. 114–5).

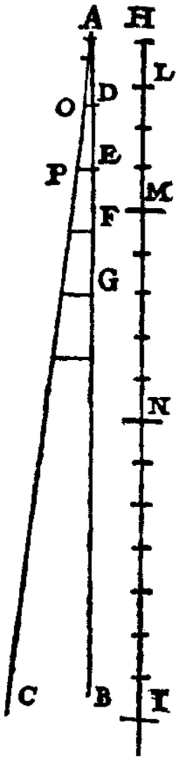

Figure 3

time in which a certain space is traversed by a moveable in uniformly accelerated motion from rest is equal to the time in which the same space would be traversed by the same moveable carried in uniform motion whose degree of speed is one-half the maximum and final degree of speed of the uniformly accelerated motion ([1638] 1914, 208).[27] Hence Galileo

---

[27] This is because if we imagine a body descending through an inclined plane at time $t_0 . . . t_2$ and acquiring a further degree of velocity at any further instant, and if we now imagine that at t_2 the plane from inclined becomes flat and perfectly horizontal, then we would have that the final and maximum degree of speed acquired in the uniformly accelerated motion at t_2 becomes the degree of speed that the same moveable would now have in its uniform horizontal motion. This means that in the following two seconds $t_2 . . . t_4$, the moveable would travel with uniform velocity on the horizontal plane a distance, which is exactly twice the distance it travelled in $t_0 . . . t_2$ with uniformly accelerated motion. So in the same time-intervals Δt, namely $t_0 . . . t_2$ and $t_2 . . . t_4$, the moveable covers twice the distance with uniform velocity. Hence with *half* uniform velocity (which recalls is equal to the

can now conclude that the spaces HM and HL are the same spaces that would be traversed in times AE and AD by a moveable in uniform motion whose degree of speed is one-half EP and DO (which, recall, represent the maximum degree of speed at instant E and D respectively). Therefore, the spaces HM and HL are in duplicate ratio of the times AE and AD. QED.

What is striking about this mathematical demonstration is the fact that without calculus, Galileo was nonetheless able to arrive at the law of free fall by identifying instantaneous velocities at points E and D through what Machamer illuminatingly describes as a "comparative, relativized geometry of ratios. . . . It measures one thing by showing its relation to another, which then may be quantitatively compared by supplying some arbitrarily or conveniently intelligible standards. . . . In this sort of geometry, there are no absolute values, numbers that describe the true properties of things. . . . Using this geometry one does not look for physical constants or solutions to problems in terms of absolute numerical values" (1998b, p. 65). Indeed, Galileo did not give any numerical value for uniform acceleration; nevertheless the result he found about $s = at^2$ (when the initial velocity is 0) smoothed the path to Newton, who identified the moving force causally responsible for those accelerated motions with gravitational acceleration (a was replaced by the gravitational constant g).

I want to draw attention to Galileo's deductive procedure of starting with a supposition and a postulate and deriving a series of theorems from them. As the analysis so far aimed to show, this procedure has nothing to do with and should not be confused with hypothetico-deductivism. The mathematical method that Galileo followed for the science of motion is demonstrative and is meant to start from true and indubitable principles, rather than from hypotheses. The problem, however, is that Galileo was in fact unable to find such self-evident and totally indubitable principles, as we have seen. This has far-reaching implication for the philosophical debate about saving the phenomena, with which I opened this paper.

Galileo marks indeed a turning-point in the tradition of saving the phenomena, as Duhem rightly saw. If the aim of science were really that of introducing hypotheses to save the phenomena, then Galileo would be no more right than Aristotle. What appears to us as a free-falling object could be accounted for either by the hypothesis of motion towards a natural place, with Aristotle, or by the hypothesis of uniformly accelerated motion, with Galileo.[28] And the role of Galileo's experiment would simply

---

maximum and final degree of speed of uniformly accelerated motion), it would cover exactly the *same* distance it covered with uniformly accelerated motion on the inclined plane. QED.

[28] This remark of course is not meant to suggest that Galileo's kinematics is on a par

reduce to testing these alternative hypotheses, according to some sort of hypothetico-deductive procedure.

No wonder many philosophers of science—including Feyerabend's (1975) famous analysis of Galileo's tower argument—have concluded that there was an element of propaganda in Galileo. In the end, Galileo was inventing new auxiliary dynamic hypotheses (be it circular inertia for the tower argument, or the aforementioned one about uniformly accelerated motion), and there was no intrinsic reason for the scientific community to shift to Galileo's new science, apart from the propaganda that finally gathered scientists' consensus around Galileo. I think that Feyerabend captured very nicely the theory-ladeness of observation in Galileo's strategy, but went astray in concluding that Galileo 'invented' a new conceptual system and used propaganda to defend it. This conclusion follows—I believe—from a widespread skepticism among philosophers of science about the possibility of choosing between alternative hypotheses that can both accommodate the available evidence. And this skepticism is, of course, nothing but a consequence of the empiricist tradition about 'saving the phenomena'.

By contrast with this tradition, I want to suggest that the particular use Galileo made of the postulate in backing up the quasi-demonstration of the supposition, from which the law of free fall follows, can be interpreted along Kant's lines as a *constitutive a priori* element in the phenomenon of free fall. Namely, for the sake of experiencing uniformly accelerated motion, we must *constitute* the kinematical properties of free-falling bodies according to the aforementioned supposition (no matter how counterintuitive the postulate necessary to back it up). Moreover, we need to subsume these kinematical properties under the causal concept of a force such as for instance Galileo's 'impeto', despite the fact that Galileo's notion of 'impeto' (as weight times speed) was still reminiscent of the Archimedean science of weight and was not the exact causal story about free-falling bodies. From a Kantian perspective, what matters is that for the very first time with Galileo, physics was not regarded as introducing hypotheses to save the phenomena, but instead as a science whose secure foundations depended on the specific mathematical–physical way in which phenomena were *constituted.*[29] This is the central contribution that to Kant's eyes Gali-

---

with Aristotelian physics. It is instead a remark about how philosophers of science have sometimes looked at the transition from Aristotelian physics to Galilean kinematics, because of their (more or less tacit) endorsement of the view of ready-made phenomena (which is the target of this paper).

[29] Let me clarify this important point in response to a referee's comment that one thing is to claim that Galileo's *demonstration* of the law of free fall required the use of some

leo's mathematization of nature made to the scientific revolution from Aristotelian to Newtonian physics.

5. Conclusion

To sum up and conclude, Kant suggested a radically new conception of phenomena, according to which a phenomenon, say the phenomenon of uniformly accelerated free-falling objects, is something that from the very outset we have mathematically–geometrically *constituted* as having certain *spatio-temporal properties* (for instance, the property of acquiring the same speeds over different inclined planes with the same height), and, most importantly, subsumed under a *causal concept* by tracing those spatio-temporal properties back to the concept of a moving force, such as Galileo's 'impeto'. In this specific sense, Galileo exemplified Kant's Copernican turn by showing how the phenomena that scientists investigate are not ready-made for us to either save them or give a literally true story of them, but instead they have built in them some a priori elements that we have then to extract and prove through experiment.

We can now understand why, according to Kant, Galileo marks the beginning of modern physics by displaying a unique and distinctive scientific methodology: we can gain scientific knowledge of nature only through principles of reason, on the one hand, and through experiments thought out in accordance with these principles, on the other hand. In other words, we can gain scientific knowledge of nature only by making appearances conform to *our way of representing,* rather than trying hard to make our hypotheses conform to nature. And this is why, as I hope to have clarified, Galileo's method finds natural echoes in Kant's Copernican turn, and as such it is all the more relevant to address epistemological problems that still trouble philosophers of science today.

---

causal / dynamical concept (e.g. impeto); another thing is to claim that without that causal / dynamical concept, the phenomenon in question would be unintelligible, which seems *prima facie* questionable since Galileo could successfully describe the experimental apparatus of the inclined plane without resorting to such a concept. As the discussion above was meant to show, Galileo could not in fact describe the experimental apparatus of the inclined plane as instantiating uniformly accelerated motion without making the series of assumptions just described. I think this objection buys into and originates from deeply rooted realist intuitions about phenomena as being ready-made, with science giving a true description of them (via demonstrations, for instance). As I clarified in the Introduction, the main motivation behind the Kantian view is precisely the rejection of such deeply rooted metaphysical intuitions. From a Kantian point of view, phenomena are constituted by ascribing spatio-temporal properties and subsuming them under causal / dynamical concepts, as I have clarified in this paper.

References

Achinstein, P. 2007. "Atom's empirical eve. Methodological disputes and how to evaluate them." *Perspectives on science* 15: 359–390.

Allison, H. E. 1994. "Causality and causal laws in Kant: a critique of Michael Friedman." Pp. 291–307 in *Kant and Contemporary Epistemology.* Edited by P. Parrini. Dordrecht: Kluwer Academic Publishers.

Ariew, R. and Barker, P. (eds.) 1990. *Pierre Duhem: Historian and Philosopher of Science. Synthese* 83: issues (2) and (3).

Barker, P. and Goldstein, B. R. 1998. "Realism and Instrumentalism in Sixteenth Century Astronomy: A Reappraisal." *Perspectives on Sciences* 6: 232–258.

Bertoloni Meli, D. 2008. "The axiomatic tradition in 17th century mechanics." In *Discourse on a new method: reinvigorating the marriage of history and philosophy of science.* Edited by M. Domski and M. Dickson. La Salle, IL: Open Court.

Buchdahl, G. 1969. "The Kantian 'Dynamic of Reason', with special reference to the place of causality in Kant's system." Pp. 341–74 in *Kant Studies Today.* Edited by L. W. Beck. La Salle, Ill.: Open Court.

Buchdahl, G. 1974. "The conception of lawlikeness in Kant's philosophy of science." Pp. 128–50 in *Kant's theory of knowledge.* Edited by L. W. Beck. Dordrecht: Reidel.

Cassirer, E. 1906. *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit. Erster Band.* Berlin: Bruno Cassirer.

Cassirer, E. [1927] 1972. *The Individual and the Cosmos in Renaissance Philosophy.* Translated by Mario Domandi. Philadelphia: University of Pennsylvania Press.

Caygill, H. 1995. *A Kant Dictionary.* Oxford: Blackwell Publishing.

Drake, S. 1973a. "Galileo's discovery of the law of free fall." *Scientific American* 228: 84–92.

Drake, S. 1973b. "Galileo's experimental confirmation of horizontal inertia: unpublished manuscripts." *Isis* 64: 291–305.

Drake, S. 1974. "Galileo's work on free fall in 1604." *Physis* 16: 309–322.

Duhem, P. [1908] 1969. "ΣΩΖΕΙΝ ΤΑ ΦΑΙΝΟΜΕΝΑ: Essay sur la notion de théorie physique de Platon à Galilée." *Annales de philosophie chrétienne* 79/156: 113–39. English translation by E. Dolan and C. Maschler. *To Save the Phenomena. An essay on the idea of physical theory from Plato to Galileo.* Chicago: The University of Chicago Press.

Feldhay, R. 1998. "The use and abuse of mathematical entities: Galileo and the Jesuits revisited." Pp. 80–145 in *The Cambridge Companion to Galileo.* Edited by P. Machamer. Cambridge: Cambridge University Press.

Feyerabend, P. 1975. *Against Method: Outline of an Anarchist Theory of Knowledge.* London: New Left Books.

Förster, E. 2000. *Kant's Final Synthesis. An essay on the Opus postumum.* Cambridge, Mass.: Harvard University Press.

Friedman, M. 1992a. *Kant and the Exact Sciences.* Cambridge, Mass.: Harvard University Press.

Friedman, M. 1992b. "Causal laws and the foundations of natural science." Pp. 161–99 in *The Cambridge Companion to Kant.* Edited by Paul Guyer. Cambridge: Cambridge University Press.

Friedman, M. 2000. "Transcendental Philosophy and A Priori Knowledge: a Neo-Kantian Perspective." Pp. 367–383 in *New Essays on the A Priori.* Edited by P. Boghossian and C. Peacocke. Oxford: Clarendon Press.

Friedman, M. 2001. *The dynamics of reason. Stanford Kant Lectures.* Stanford: CSLI Publications.

Galileo, G. [1632] 1962. *Dialogo Sopra i Due Massimi Sistemi del Mondo,* in (1890–1909) *Opere di Galileo,* vol. VII. Edited by Antonio Favaro. Firenze: Barbera Editrice. English translation by S. Drake. *Dialogue concerning the two Chief World Systems.* Berkeley: University of California Press.

Galilei, G. [1638] 1914. *Discorsi e Dimostrazioni Matematiche intorno a due nuove scienze,* in (1890–1909) *Opere di Galileo Galilei,* vol. VIII. Edited by Antonio Favaro. Firenze: Barbera Editrice. English translation by Henry Crew and Alfonso de Salvio. *Discourses and Mathematical Demonstrations concerning Two New Sciences.* New York: Dover Publications.

Goddu, A. "The realism that Duhem rejected in Copernicus." Pp. 301–315 in *Pierre Duhem: Historian and Philosopher of Science.* Edited by R. Ariew and P. Barker. *Synthese* 83: issues (2) and (3).

Hooper, W. 1998. "Inertial problems in Galileo's preinertial framework." Pp. 146–174 in *The Cambridge Companion to Galileo.* Edited by P. Machamer. Cambridge: Cambridge University Press.

Jaki, S. L. 1984. *Uneasy Genius: the Life and Work of Pierre Duhem.* The Hague: Martinus Nijhoff.

Kant, I [1781 / 1787] 1997. *Kritik der reinen Vernunft.* Riga: Johann Hartknoch. In *Kants gesammelte Schriften.* Herausgegeben von der Preussischen bzw. Deutschen Akademie der Wissenschaften. Berlin: de Gruyter. Ak 4: 1–252; 2nd edition Ak 3: 1–552. English translation by P. Guyer and A. W. Wood. *Critique of Pure Reason.* The Cambridge Edition of the Works of Immanuel Kant. Cambridge: Cambridge University Press.

Kant, I. [1786] 2004. *Metaphysische Anfangsgründe der Naturwissenschaft.* Riga: Johann Hartknoch. In *Kants gesammelte Schriften.* Herausgegeben von der Preussischen bzw. Deutschen Akademie der Wissenschaften. Berlin: de Gruyter. Ak 4: 465–565. English translation by M. Friedman. *Metaphysical Foundations of Natural Science.* Cambridge: Cambridge University Press.

Kant, I. [1936, 1938] 1993. *Opus postumum.* In *Kants gesammelte Schriften.* Herausgegeben von der Preussischen bzw. Deutschen Akademie der Wissenschaften. Berlin: de Gruyter. Ak 21, 22. English translation by Eckart Förster and Michael Rosen. *Opus postumum.* The Cambridge Edition of the Works of Immanuel Kant. Cambridge: Cambridge University Press.

Koyré, A. [1939] 1978. *Etudes Galiléennes.* Paris: Hermann. *Galileo Studies.* Translated by J. Mepham. NJ: Humanities Press.

Machamer, P. 1978. "Galileo and the causes." Pp. 161–80 in *New Perspectives on Galileo.* Edited by R. E. Butts and J. C. Pitt. Dordrecht: Reidel.

Machamer, P. (ed.) 1998a. *The Cambridge Companion to Galileo.* Cambridge: Cambridge University Press.

Machamer, P. 1998b. "Galileo's machines, his mathematics, and his experiments." In *The Cambridge Companion to Galileo.* Edited by P. Machamer. Cambridge: Cambridge University Press.

Martin, R. N. D. 1982. "Saving Duhem and Galileo." *History of science* 25: 302–319.

Martin, R. N. D. 1991. *Pierre Duhem: Philosophy and History in the work of a believing physicist.* La Salle: Open Court.

Massimi, M. 2007. "Saving unobservable phenomena." *British Journal for the Philosophy of Science* 58: 235–262.

Massimi, M. 2008. "Why there are no ready-made phenomena: what philosophers of science should learn from Kant." Pp. 1–35 in *Kant and Philosophy of Science Today.* Royal Institute of Philosophy Supplement 63. Edited by M. Massimi. Cambridge: Cambridge University Press.

McMullin, E. (ed.) 1967. *Galileo. Man of Science.* Basic Books.

Naylor, R. H. 1974. "Galileo and the Problem of Free Fall." *British Journal for the History of Science* 7: 105–34.

Naylor, R. H. 1977. "Galileo's theory of motion: processes of conceptual change in the period 1604." *Annals of Science* 34: 365–92.

Palmieri, P. 2006. "A new look at Galileo's search for mathematical proofs." *Archive for the history of exact science* 60: 285–317.

Reichenbach, H. [1920] 1965. *Relativitätstheorie und Erkenntnis Apriori.* Berlin: Springer. English translation. *The theory of relativity and a priori knowledge.* Los Angeles: University of California Press.

Ryckman, T. 2005. *The reign of relativity.* Oxford: Oxford University Press.

Settle, T. 1961. "An experiment in the history of science." *Science* cxxxiii: 19–23.

Van Fraassen, Bas. 1980. *The Scientific Image.* Oxford: Clarendon Press.

Van Fraassen, Bas. 1985. "Empiricism in the Philosophy of Science." Pp. 245–368 in *Images of science.* Edited by P. M. Churchland and C. A. Hooker. Chicago: The University of Chicago Press.

Van Fraassen, Bas. 2006. "Structure: its Shadow and Substance." *British Journal for the Philosophy of Science* 57: 275–307.

Wallace, W. A. 1998. "Galileo's Pisan studies in science and philosophy." Pp. 27–52 in *The Cambridge Companion to Galileo.* Edited by P. Machamer. Cambridge: Cambridge University Press.

Westfall, R. 1971. *Force in Newton's Physics.* New York: American Elsevier.

Wisan, W. L. 1974. "The new science of motion: a study of Galileo's *De motu locali*". *Archive for the history of exact sciences* 13: 103–306.

Wisan, W. L. 1978. "Galileo's scientific method: a re-examination." Pp. 1–58 in *New Perspectives on Galileo.* Edited by R. E. Butts and J. C. Pitt. Dordrecht: Reidel.