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Dossier Pierre Duhem

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

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

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

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Introduction

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

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

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

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

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

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

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

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

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

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

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

> This first law of Kepler's, "The radial vector from the sun to a planet sweeps out an area proportional to the time during which the planet's motion is observed," did, in fact, teach Newton that each planet is constantly subjected to a force directed toward the sun.
>
> The second law of Kepler's, "The orbit of each planet is an ellipse having the sun at one focus," taught him that the force attracting a given planet varies with the distance of this planet from the sun, and that it is in an inverse ratio to the square of this distance.
>
> The third law of Kepler's, "The squares of the periods of revolution of the various planets are proportional to the cubes of the major axes of their orbits," showed him that different planets would, if they were brought to the same distance from the sun, undergo in relation to it attractions proportional to their respective masses. (Duhem 1991, 191)

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

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

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

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

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

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

Objections to Duhem's Criticisms to the Newtonian Method

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

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

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

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

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

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

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

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

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

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

Harper's Critique of the Thesis of Underdetermination

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Even so Newton was not satisfied. What about small deviations from the law of the square inverse? What about fractional powers between 2 and 3? Are these alternatives also excluded? From another series of studies (Section 9 of Book I), Newton concluded that the smallest deviation in the inverse square law would lead to a precession motion in the planetary orbits, which could be clearly distinguished in the astronomical observations. As no similar motion is observed for the planets around the Sun, all alternatives to the inverse square law were excluded, and the study of systematic dependencies converged onto a single hypothesis. For Harper, it was this set of systematic dependencies studies that allowed Newton to effectively confirm the inverse square law. Therefore, Newton's method is not merely HD. Likewise, the same can be said of current scientific method. The expression "deduction from phenomena" would be only a generic reference to this inference procedure that makes use of the analysis of systematic dependencies from independent phenomena. Newton made no mistake in describing his procedure in the *Principia*; Duhem is the one who misunderstood him.

The Autonomy of Physics from Metaphysics and the Newtonian Physical System

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

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

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

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

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

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

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

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

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

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

Therefore, Newton's *Principia* would have established one of the most fundamental characteristics of the physical method. He would strictly follow an epistemological order of physics toward metaphysics, and never the opposite. For Duhem, scientists such as Euler, Lagrange, Laplace, Gauss, Fresnel, Poisson, Ampère, and Cauchy, among others, can see the fecundity of this understanding in the significant progress made by this approach in the research of diverse phenomena, such as electricity, capillary, elasticity and heat theories. After Newton's abstract notion of "attraction" disconnected from metaphysical meanings, molecular attraction has become a potent tool for constructing synthetic representations of physical phenomena. As a result, there has been a long period of continuous progress in these "attractionist" theories, which have remained loyal to the limits established by the Newtonian system. This trend, according to Duhem, was prevalent until the middle of the nineteenth century, when the rapid development of thermodynamics, which corroborated Descartes' assumptions about the nature of heat, triggered a new trend of explanatory theories. However, in his own time Duhem diagnosed that the contradictions and failures of this new trend were gradually led the physicists "back to the sound doctrines Newton had expressed so forcefully" (Duhem 1991, 53).

Hypotheses non Fingo and the Logical Priority of Physics over Metaphysics

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

But secondly, it must also be said that in the light of even more recent studies on the *Principia* it is possible to conclude that not only was Newton himself aware of this methodological prioritization, this would in fact be the most fundamental meaning of his General Scholium[3]

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

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

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

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

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

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

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

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

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