Mathematical Models in Newton's Principia: A New View of the "Newtonian Style"

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Abstract

In this essay I argue against Bernard I. Cohen's influential account of Newton's methodology in the Principia: the "Newtonian Style". The crux of Cohen's account is the successive adaptation of "mental constructs" through comparisons with nature. In Cohen's view there is a direct dynamics between the mental constructs and physical systems. I argue that his account is essentially hypothetical-deductive which is at odds with Newton's rejection of the hypothetical-deductive method. An adequate account of Newton's methodology needs to show how Newton's method proceeds different from the hypothetical-deductive method. In the constructive part I argue for my own account which is model-based: it focuses on how Newton constructed his models in Book I of the Principia. I will show that Newton understood Book I as an exercise in determining the mathematical consequences of certain force functions. The growing complexity of Newton's models is a result of exploring increasingly complex force functions (intra-theoretical dynamics) rather than a successive comparison with nature (extra-theoretical dynamics). Nature did not enter the scene here. This intra-theoretical dynamics is related to the "autonomy of the models".

1. Introduction

Many of Newton's contemporaries were baffled by the technicality of the Principia (first edition: 1687). Even today Newton scholars vigorously discuss the nature of the methods employed in it. One of the most well-known interpretations of Newton's methodology is I. Bernard Cohen's "Newtonian Style" (1980). The crux of the "Newtonian Style" is the piecemeal adaptation of "mental constructs" through successive comparisons with nature. In Cohen's view Newton compares the simplest models with nature and adapts them in view of these observations; this continues until a sufficient level of approximation is reached.

There is a serious problem for Cohen's account. After presenting Cohen's account in 2.1, I will discuss this problem in 2.2. Let me briefly mention it. Ultimately the "Newtonian Style" is hypothetical-deductive. It does not preclude the overt introduction of arbitrary hypotheses (which is characteristic for the hypothetical-deductive method (henceforth: H-D method)). In Cohen's view Newton ab initio assumed that centripetal forces are the true causes of the celestial motions, since he decided to model the force(s) involved in planetary motion. Newton would be modelling something to which he is already committed. This is simply at odds with Newton's rejection of the H-D method. Some authors have indeed stressed the fact that Newton's method is not H-D (e.g. Harper, 1998: 274; Harper, 2002; Smith, 2002a, p. 149; Smith, 2002b, p. 44). I will argue that Newton only later started inferring centripetal forces from these models in Book III. Cohen's account cannot explain these deviations of Newton's method from the H-D method. One remark is in order here. Cohen's "Newtonian Style" in the Principia is about Newton's methodology as a "mode of presentation", a "manner of composing" (Newton, 1726, p. 60). Therefore, my claims – as Cohen's – about Newton's methodology are restricted to the presentational sequence of Newton's theory (the method of justification) and do not pertain to the chronological sequence of the theory (the method of discovery). When I use "method" in this paper, I only refer to Newton's method of justification. There is surely no guarantee that the sequence presented in the Principia represents Newton's original train of thought which led to the theory. Chances are very low that this is the case. Original discoveries are very often "rationally reconstructed". The published version often is a rhetoric re-structuring of the original discovery process. The rigid deductive scheme Newton lays out is very unlikely a correct historical account of his discovery of universal gravitation. As Nickles pointed out, we need to distinguish between the historical mode of generating an idea and the method of justification:

> The initial introduction of the salient ideas may be as hypothetical as you please. (…) But the justificatory ideal remains to show that, given what we know at the end, the problem solution is logically derivable – and preferably derivable in a routine manner. (Nickles, 1984: 19-20; see also Nickles, 1988: 35).

The rejection of hypotheses à la Descartes is a well known feature of Newton's natural philosophy (Newton, 1726, p. 943). This rejection has its repercussions for Newton's logic of argumentation. The Cartesians started by boldly positing their causal explanations and tried to infer the required observational data from these (from theory to phenomena). Newton, on the contrary, preferred to begin with the phenomena and to infer the types of forces which produce these phenomena given the laws of motion (from phenomena to theory). An adequate analysis of Newton's methodology should incorporate Newton's justificatory ideal or "argumentative logic". This is lacking in Cohen's account. It was precisely this argumentative component that allowed Newton to be convinced that his methodology was different from a H-D approach.

The problem is then to formulate an account that does not encounter these problems. In section 3 I will propose my account of Newton's methodology. Precisely the shortcomings of the "Newtonian Style" highlight what an adequate account of Newton's methodology in the Principia should include. Cohen does not perceive that the models in the Principia "live a life of their own". The topic of the autonomy of the models has recently gained much interest in the literature on models in science (see especially Morgan & Morrison, 1999). Much of what Newton is doing with his models can be described with the conceptual tools offered by this programme. My claim is that Newton essentially proceeded by focussing on his mechanical models and their internal dynamics rather than constructing them successively in accordance with empirical data as Cohen claims. In order to gain some insight in the Newtonian models it is necessary to look at what they presuppose. This will be done in my examination of some of the propositions of Book I of the Principia in 3.1. I will point at the constituents of Newton's models. In 3.2 I will discuss some recent work on Newton's "deductions from phenomena". It is my claim that these accounts – although each of them reveals relevant features of Newton's method – are unable to capture Newton's way of modelling the natural phenomena. In 3.3 I will explain and clarify why Newton indeed could have thought that his method is different from and superior to the H-D method. I will argue that it is precisely the autonomy of the models that helps us explain this. The Newtonian models themselves predicted that perturbations must occur. Ultimately, for Newton methodological validity was equivalent to the a priori deduction of (idealised and abstracted) phenomena from the laws and definitions of motion.

2. I. Bernard Cohen's Account of Newton's Way of Modelling

2. 1. The "Newtonian Style"

In his seminal study The Newtonian Revolution Cohen describes Newton's method in the Principia. In his view Newton's method is essentially reductive:

> As we shall see in the following chapter, Newton's success in analyzing the physics of motion depended to a large degree on his ability to reduce complex physical situations to a mathematical simplicity, in effect by the mathematical properties of an analogue of the reality that he eventually wished to understand. (Cohen, 1980, p. 55)

This typical mathematization of nature has been described by I. Bernard Cohen as the "Newtonian Style" (Cohen, 1980). The Newtonian Style consists of three successive phases:

(1) We start with set of assumed physical entities and physical conditions that are simpler than those in nature (Cohen, 1980, p. 62). For instance: the problem of planetary motion is reduced to a one-body-system (Ibid.).[1] This mental construct is imaginatively conceived as "the parallel or analogue of the natural system" (Ibid., p. 63). Note that it is necessary here to assume that Newton a priori considers this mechanism as a true cause since he models it – see 2.2. Newton starts with a set of simplified physical entities and conditions – e.g. point masses, one-body-systems, two-body-systems – which can be translated in terms of mathematics. These mathematical terms do not belong to the domain of pure mathematics: they are derived from physical conditions (Ibid., p. 55). To the degree that the physical conditions of the system become mathematical rules or propositions, their consequences may be deduced by the application of mathematical techniques (Ibid., p. 63).

(2) The second phase is the transfer of the results Newton has obtained in mathematics to physical nature (Ibid.). Because the mathematical system duplicates the idealised physical system, the rules or proportions arrived mathematically in one may be transferred back to the other and then be compared and contrasted with the data of experiment and observation. The models are rendered more complex by direct and successive comparison with astronomical data. This is the strong claim of Cohen's account (the weaker claim will be described below). For instance: an initial component of inertial movement in a central force field is shown to be a necessary and sufficient condition for the law of areas which had been found to be "a phenomenologically verifiable relation in the external world" (Ibid.). This comparison usually results in the modification of the system of the first stage. Newton adds further entities, concepts or conditions to the imaginatively constructed system. To continue the example: Newton further adds Kepler's third law. This law combined with Newton's notion for centripetal force yields the inverse square law. Next an elliptical orbit with the force's centre at one focus is then shown to require an inverse-square force, as does a parabolic or hyperbolic orbit. In a further stage of complexity Newton adds to the system a second mass point. This leads to new deductions and a new phase two. In this way there is an alternation between these stages will lead to an increasing complexity and hence an equal increasing "vraisemblance". Newton does not carry out phase two in full. Once it has been shown that close approximations occur, then the investigation could move on to phase three (Cohen, 1980, p. 102). We also could distinguish a weaker version of Cohen's account: in the process of adapting the models Newton clearly has an agenda in mind of constructing a gravitational theory of celestial motion. The models in Book I are a myriad of increasingly complex models. I have no particular problems with this claim. Whereas Cohen assumes the dynamics in that process of rendering the models more complex is intra-theoretical, i.e. between theory and data, I will argue that it is inter-theoretical.

(3) In the third phase these principles will no longer be purely mathematical but will be applied to the real world of physical nature revealed by experiment and observation (Cohen, 1980, p. 64; Cohen, 1982, p. 51). The mathematical conditions and entities will no longer be simplified or idealised or an imagined mathematical construct but seem to conform to (or at least duplicate) the realities of the external world (Cohen, 1980, p. 64). However they are not identical equivalents of the conditions of the external world; but only approximations (Ibid., p. 65).

So it is mathematics and not a series of experiments that lead knowledge of the world (Ibid., p. 64). Nevertheless, the data are, of course, used in determining the initial conditions and they are important to test the system of the world (Ibid.).

This interpretation converges, according to Cohen, with the scholium to section 11 of Book I (Cohen, 1980, p. 85):

> (1) Mathematics requires an investigation of those quantities of forces and their proportions that follow from any conditions that may be supposed.
>
> (2) Then, coming down to physics, these proportions must be compared with the phenomena, so that it may be found out which conditions (or laws)[2] of forces apply to each kind of attracting bodies.
>
> (3) And then, finally, it will be possible to argue more securely concerning the physical species, physical causes and physical proportions of these forces. Let us see, therefore, what the forces are

by which spherical bodies, consisting of particles that attract in the way already set forth, must act upon one another, and what sorts of motions results from such forces.

(Newton, 1726, pp. 588-589; my numbering)

Cohen interprets the second clause as: the comparison usually leads to an alteration of the initial conditions (Cohen, 1980, p. 99). This procedure continues until the system of the world is reached. I will offer a different reading of this scholium in 3.2. Did Newton think that this final stage is still an imagined construction? Shortly after finishing the Principia he did not, as Cohen admits. But after the first flush of victory faded, he realised that he "is dealing with the mathematics of limited or arbitrary conditions; or, he is exploring the mathematical properties of artificial situations or imagined constructs and not studying nature in all her complexities" (Cohen, 1980, p. 72, also see Ibid., p. 91). These constructs suppose a certain set of conditions *"quale tamen vix estat in rerum natura"*. Mathematics is exact and nature is not.

Visually the "Newtonian Style" can be represented as follows (figure 1):

Figure 1.

2.2. The Predicament of the "Newtonian Style"

It is a well know fact that Newton was very reluctant to accept hypotheses and that he often criticised the H-D method.[3] Newton claimed that his method was different from and superior to the H-D method. It was different and superior for the same reason: it did not feign hypotheses. Hence his famous declaration in the Scholium Generale (second edition: 1713):

> I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypothesis. For whatever is not deduced from the phenomena must be called hypothesis; and hypotheses, whether metaphysical of physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. (Newton, 1726, p. 943)

A hypothesis is a proposition that is not a phenomenon, nor deduced from any phenomena but assumed or supposed without any experimental proof (Edleston, 1969, p. 155). In The Opticks Newton wrote that the main business of natural philosophy is *"to argue from Phaenomena without feigning Hypotheses, and to deduce Causes from Effects"* (Newton, 1730, p. 369). Arguing from phenomena is, as Roger Cotes wrote in the preface to the second edition, that "incomparably best way of philosophizing" of Newton (Newton, 1726, p. 386).

According to Bechler, Cohen's interpretation faces some difficulties (Bechler, 1982). Cohen's interpretation has affinities with the H-D method. Although I am not satisfied with Bechler's analysis, he did make a point though: it is not reality, but the theory which dictates (Ibid., p. 10). This criticism needs to be adapted, elaborated and clarified. Further it is necessary to show how Newton could think that his method was different from and superior to the H-D method (see section 3). Newton starts with reducing planetary motion to a one-body-system, according to Cohen. Such a system contains the cause of this motion: a centripetal force. So *ab initio*, i.e. at the beginning of the Principia, a huge assumption is made: centripetal forces are de facto the real causes of the celestial motions. This entails that Newton already was committed to the existence of centripetal forces, since he tries to model nature (and hence these forces). As we shall see Newton only infers centripetal forces in Book III. In the beginning of Book I Newton is already taking into account the real world, according to Cohen. How could Newton at this stage, i.e. using only a one-body-system, justify that e.g. the earth and the moon behave as such a model? Moreover, Newton at this stage simply assumes that the motions of these models occur in vacuo (Newton, 1726, p. 403). In Book I Newton did not say anything on vacua in the real world. The proof that vacua exist is given further in proposition 6 of Book III. How could he be "comparing" or "reducing" if there is no proof yet of the fact that the spaces between the celestial bodies are non-resisting? The world only enters the scene in Book III. Book I only concerns discovering the mathematical implications of certain force functions in given systems. Book I is mainly a mathematical exercise, not a physical one. It is crucial that the process of constructing increasingly complex models is the result of an inter-theoretical dynamics (this requires an autonomous exploration of the models), and not of an intra-theoretical dynamics (this would make Newton's method overtly H-D, since we model forces of which we assume that they are de facto responsible for the observed celestial motions). Book I will later be used to infer which forces are active in nature from the mathematical regularities we encounter. During the construction of the models Newton is not yet committed to the actual existence of centripetal forces.

If Cohen is right, there would be no difference between Newton's method and the H-D method. Many hypotheses are possibly compatible with the phenomena. But this is not how Newton wanted it: by means of his physico-mathemathical propositions he was able to infer that such centripetal forces nota bene exist in the real world (see 3.2).

3. Models in the Principia

In this section I will try to develop my model-based account which stresses the autonomy of the Newtonian models. In 3.1 I will treat some features of some of the propositions of Book I of the Principia. Obviously, it is not my aim here to treat the rich content of these propositions exhaustively. For my present purposes it will be sufficient to gain some basic insight in the suppositions and functioning of these propositions. This will result in a description of the constituents of the Newtonian models. In 3.2 I will review some work on Newton's deductions from phenomena. This will be a stage-setting for what happens in 3.3. In the second subsection I will present and defend my account of Newton's methodology.

3.1. The Propositions of Book I

I start with propositions 1 and 2, where Newton deals with the dynamical implications of Kepler's second law:

> Proposition 1, Book I:
>
> The areas which bodies made to move in orbits describe by radii drawn to an unmoving center of forces lie in unmoving planes and are proportional to the times. (Newton, 1726, p. 444)

The proof proceeds as follows (see figure 2).

Figure 2.

Let a body by its inherent force, i.e. by its vis inertiae, describe the straight line AB. After B the body would normally (by law 1) go straight to c. Let cC be parallel to BS and meet BC at C. A second force acts on B and makes it deviate from Bc and follow BC (by corollary 1 of the laws of motion). When the second part of time has been completed – the time is divided in equal intervals and the force acts instantaneously after each equal interval – the body will be found at C in the same plane as triangle ASB. Since SB and Cc are parallel triangle SBC will be equal to SBc and thus to SAB. SBC and SBc are equal since both their height and base is equal. That their base SB is equal is evident. The height is the same because both triangles are between the parallels SB and Cc. Hence they describe an equal area in an equal amount of time. By similar argument this can be extended to all triangles. If the number of triangles is increased infinitely one can apply the same reasoning: they will describe equal areas in equal times. Proposition 1 is essentially based on: the first law of motion, Corollary 1 of the laws, and a limiting procedure[4] (Brackenbridge, 1995, p. 26). Several interesting suppositions are made: the inertial component is visualised as Bc, Cd, De, Ef, …; the centripetal force as cC, dD, eE, fF,…; the resulting force of these forces as BC, CD, DE, EF, …; and a one-body-system, i.e. a system with one point mass drawn towards a empty point. On the mathematization of centripetal forces, see De Gandt, 1995, especially Chapter I. Let us now turn to Proposition 2.

> Proposition 2, Book I:
>
> Every body that moves in some curved line described in a plane and, by a radius drawn to a point, either unmoving or moving uniformly forward with a rectilinear motion, describes areas around that point proportional to the times, is urged by a centripetal force tending toward that same point. (Ibid., p. 446)

Proposition 2 amounts to saying that Kepler's second law (which supposes a constant areal velocity and equality of the plane of motion) presupposes a centripetal force (thus: it demonstrates the converse of Proposition 1). Newton derives that a centripetal force is a necessary (Proposition 1) and sufficient (Proposition 2) cause for Kepler's area law: Kepler's area law is valid if and only if there is a centripetal force.[5] The proof proceeds as follows (again see figure 2). By Newton's first law we know that a body that moves in a curved line is deflected from a rectilinear course by some force acting on it. The force by which the body is deflected and in equal times is made to describe around an immovable point S the equal minimally small triangles SAB, SBC, SCD,… acts along a line parallel to cC, i.e. along the line BS; at place C, parallel along the line dD, i.e. along CS, …etc. Therefore it always acts along lines tending towards S. Similar suppositions are made as in the previous proposition.

Now we proceed to proposition 4.

> Corollary 6, Proposition 4, Book I:
> If the periodic times are as 3/2 powers of the radii, and therefore the velocities are inversely as the square roots of the radii, the centripetal forces will be inversely as the squares of the radii; and conversely. (Ibid., p. 451)

The reasoning proceeds as follows. In Proposition 4 Newton had already proven that the magnitude of this force is proportional to v^2/r. Corollary 6 shows that this fact combined with Kepler's harmonic law delivers an inverse square law and vice versa. In modern terminology this can be demonstrated as follows (Newton does not give any further comment to this corollary). Huygens published the result that a body travelling in a circle with constant angular speed needs a force proportional to v^2/r to keep it in orbit: $F= k.v^2/r$. Since v equals $2.\pi.r/t$: $F=k.4.\pi^2.r^2/t^2.r$. Multiplied by r/r: $F= k.4.\pi^2.r^3/t^2.r^2$. Since r^3/t^2 is a constant according to Kepler's third law, we can write: $F= (\text{constant})/r^2$. Thus, Kepler's third law implies an inverse square law, and conversely.[6]

So far I have presented the propositions concerning one-body-systems. The last propositions I will discuss belong to section 11, i.e. "the motion of bodies drawn to one another by centripetal forces" (Newton, 1726, pp. 561-589). These sections include two-body-systems, three-body-systems and many-body-systems. In this section Newton presents his theory of perturbation. Interacting bodies will perturb each other motions. I will only look at the many-body-systems.

> Proposition 65, Book I:
>
> More than two bodies whose forces decrease as the squares of the distances from their centers are able to move with respect to one another in ellipses, by radii drawn to the foci, are able to describe areas proportional to the times very nearly. (Ibid., p. 568)

Newton demonstrates that perturbations will occur from Kepler's first and second law. Next he proves this for two simple cases these perturbations will be negligible: (1) for several lesser bodies revolving around a greater one at various distances (this can later be used for the primary planets), and (2) for several smaller bodies revolving around a greater body which is at the same time urged sideways by the force of another very much greater body situated at a great distance (this can later be used for the secondary planets).[7] This, as Newton adds, can be extended for more complex cases indefinitely. In the first case these bodies will describe areas proportional to the times insofar as errors introduced either by the departure from the greater body from that common centre of gravity or by the mutual interactions between the lesser bodies are neglected (Ibid.). In the second case, the several smaller bodies revolving around the greater body can be conceived as one body "because of the slight distance of those parts from one another". This will only give rise the small errors produced by the distances between the parts (Ibid., p. 569). This proposition is accompanied by three corollaries:

> Corollary 1, 2 and 3, Proposition, 65:
>
> In case 2, the closer the greater body approaches to the system of two or more bodies, the more motions of the parts of the system with respect to one another will be perturbed, because the

> inclinations to one another of the lines drawn from this great body of those parts are now greater, and the inequality of the proportion is likewise greater.
>
> But these perturbations will be the greatest if the accelerative attractions of the parts of the system toward the greater body are not to one another inversely as the squares of the distances, especially if the inequality of this proportion is greater than the inequality of the proportion of the distances from the greater body. (…)
>
> Hence, if the parts of this system – without any significant perturbation – move in ellipses or circles, it is manifest that either these parts are not urged at all (except to a very slight degree indeed) by accelerative forces tending toward other bodies, or are all urged equally and very nearly along parallel lines. (Ibid., pp. 569-570)

The following proposition also deserves our attention:

> Proposition 69:
> If in a system of several bodies A, B, C, D, …, some body A attracts all the others, B, C, D, …, by accelerative forces that are inversely as the squares of the distances from the attracting body; and another body B also attracts the rest of the bodies A, C, D, …, by forces that are inversely as the squares of the distances from the attracting body; then the absolute forces of the attracting bodies A and B will be to each other in the same ratio as the bodies [i.e. the masses] A and B themselves to which those forces belong. (Ibid., p. 587)

The crucial thing is that the motive forces are as the accelerative forces and quantity of matter, i.e. mass, of the attracted bodies jointly. This can easily be derived from definitions 2, 7, 8 and the third law of motion.[8] In this case, the motive forces are equal to each other (by the third law). Obviously, in this case Newton presupposes a many-body-system. Note that in a n-body-system there are n.(n-1) forces directed to n centres.

I am fully aware of the fact that I did not present all these propositions at length. My aim was, however, to give an idea of the flavour of these propositions and to clarify the conditions under which these propositions hold. I think these examples have given us an insight in the constitutive elements of these models. Let us – to conclude this subsection – look at the constitutive parts or elements of these systems. I think that at least the following constituents are relevant:

(1) an "ontological" set: a set of fundamental "entities", i.e. point masses, "empty" points, and forces, which constitute one-body-systems, two-body-systems, three-body-systems or many-body-systems in vacuo,

(2) a nomological and theoretical-conceptual set: a set of laws and definitions (3 laws and 8 definitions), and finally,

(3) a mathematical set: a mathematical description of these entities and operations upon them

As can be gathered from the propositions above, **(2)** and **(3)** constitute the descriptive and inferential toolbox that allows Newton to generate information from **(1)**. All these propositions follow by deduction from the laws and definitions of motion, and some mathematical operations (e.g. Euclidean geometry, Newton's method of "ultimate ratios").

3.2. Newton and "Deductions" from Phenomena

Let us take a look at some work related to Newton's optical work. I will discuss Athanasios Raftopoulos' and John Worrall's account. Raftopoulos analyses Newton's first optical paper (1672) as an eliminative induction procedure and inference to the best explanation (1999). Newton first begins to eliminate the other possible causes (e.g. an irregularity in the prism) for the observed oblong image produced by a prism. He shows that these causes could not entail the phenomenon. Then he shows that the theory of heterogeneity of light entails this phenomenon. This is the vera causa of the oblong form. The procedure, described by Raftopoulos, does obviously not shed much light on Newton's method in the Principia. Worrall argues that Newton's argument for the heterogeneity of white light is (at least at first sight) different from a H-D approach. Newton starts with a prism experiment in the position of minimal deviation that produces an oblong image. If we re-refract the refracted beams by a second prism we notice that they are refracted in the same order. Newton takes this to mean that the beams each have their "inherent degree of refrangibility". Then, by a continuity argument, Newton argues that light *consists* of inherent degree of refrangibility. Whereas a H-D approach starts with the theory and proceeds by investigating the observational consequences, in Newton's method the theory (heterogeneity of light) is the conclusion of an argument that begins with observational premises (prism experiments) (Worrall, 2000, pp. 64-65). This agrees with the Principia: Newton infers centripetal forces from Keplerian motion. But again: an answer to the question "How did Newton infer centripetal forces from Keplerian motion?", is only provided by carefully looking at the Newtonian models themselves.

Let us also look at some work related more directly to the Principia. William Harper and George Smith have done an excellent job in trying to clarify the difference between Newton's method and the H-D method. According to William Harper Newton infers values of parameters (e.g., a centripetal force) that are measured by the phenomenon (e.g. Kepler's area law) it is inferred from (Harper, 1998, p. 277). Let look at Proposition 60, Book I. This proposition involves a two-body-system which allows one to calculate the corrected the harmonic law distance given the masses of the two bodies (Newton, 1726, p. 564). If *r'* is the corrected distance, *r* is the harmonic distance, *s* is the mass of the sun, and finally *p* is the mass of the planet, then: $r'/r=(s+p)/((s+p)^2\times s)^{1/3}$. Newton's theory allowed him to predict and explain these perturbations. Smith stresses that the approximative propositions are deduced from the laws of motion. The "quam proxime" inferences are backed-up by the deductions from the laws and definitions of motion (Smith, 2002a, pp. 152-167). Newton's propositions enabled him to predict and explain systematic discrepancies between the phenomena and the most simple (idealized) cases. Although both these accounts certainly have their merits, they do not discuss the constituents of the Newtonian models nor the way these models function and relate to the actual world. An account which sheds light on these features is highly desirable. It will be argued in the following section that the "Models as Mediators"-programme can establish such an account.

3.3. The Newtonian Models as Autonomous Agents

As noticed above: the models are decisive and they dictate. An essential feature of Newton's methodology is that the models of Book I independently show that in certain idealized and abstracted situations, where the same laws of nature hold as in our world and where the same theoretical concepts are apt to describe phenomena, perturbations will occur. Newton must have thought that he succeeded in *a priori* deducing real world phenomena from the laws and definitions of motion. This is the autonomy necessary for his more secure method. In Book I it is investigated what the (mathematical) effects are of certain physical systems – given the laws of motion. At a further moment of investigation we assume that the models are sufficiently similar to our actual world. When similar effects are observed (e.g. a many-body-system where the area law holds very nearly) we can infer that these effects are caused by the mechanisms described in the models. These inferences are backed-up by the laws of motion. If one accepts this identification of these models and the actual world, one can more safely infer that the perturbations in rerum natura are caused by the mechanisms postulated in the models. To refute these models is almost equivalent with rejecting the laws and definitions of motion together with the mathematical procedures employed in the Principia. The effects are rigidly deduced from these general premises. This is exactly what Smith stresses:

> The "if –then" propositions used in deducing the law, as well as their approximative counterparts ("if-quam-proxime-then-quam-proxime"), are rigorously derived from the laws of motion. The phenomena – that is, the propositions expressing Newton's phenomena – are inductive generalizations from specific observations, and hence they hold at least quam proxime of specific observations. But then, unless the laws of motion are fundamentally mistaken, the force law too is guaranteed to hold at least quam proxime of these observations. By way of contrast, the fact that a consequence deduced from a hypothesized force law holds quam proxime of specific observations need not to provide any such guarantee. A conjectural hypothesis can reach far beyond the observations providing evidence for not merely its generality, but in its content. (Smith, 2001, p. 160)

This is really crucial. Some of the more complex models (e.g. Proposition 65) predict that under certain constellations small deviations from Kepler's area law will occur. This conclusion is reached by deductions from the ontological set by the nomological/theoretical-conceptual set and the mathematical set. Now, unless the deductions are false or the laws and definitions of motions are wrong, this model explains the deviations in the real world. Let us consider Proposition 1 and 2, Book III. From the observation that Kepler's second and third law hold for the secondary planets Newton infers a centripetal force (by Proposition 2 (or 3), Book I) varying inversely as the square as the distance (by Corollary 6 to Proposition 4).[9] From the observation that the area law and the harmonic law hold for the primary planets we are likewise able to infer a centripetal force (by Proposition 2, Book I) varying inversely as the square as the distance (by (Corollary 6 to) Proposition 4, which presupposes a circular approximation). Moreover, in the case of the primary planets the inverse square law is proved "with the greatest exactness from the fact that the aphelia are at rest", since the slightest departure from an inverse square law would entail motion in the aphelia (by Book I, Proposition 45, which concerns ellipses) (Newton, 1726, p. 802). Newton considered this proposition as the best proof of the inverse-square Sun-centred force. Now we can rely on Proposition 65 which allows us to fully explain the deviations from the area law. The inference to the corresponding centripetal forces is now fully backed-up. This was, I think, the more secure method Newton had in mind. The backing-up is different from the H-D method. Since these models and the actual world are assumed to be similar, the "quam proxime" inferences are backed-up by the deductions from the laws (which are considered to be fundamental laws and of nature), definitions (which are conceived to be the true, universal and adequate concepts of motion) and some mathematics. I take this as the main lesson from Smith's account (Smith, 2002a, pp. 152-167). Provided that the laws and definitions of motion are correct, these mathematical effects are caused by the stipulated forces. The laws of motion were indeed considered by Newton as true principles of nature, and certainly not as hypotheses (Smith, 2001, pp. 333-334).[10] Of course these laws are hypothetical but this is surely not how Newton thought about them. They are deduced from phenomena (Edleston, 1969, pp. 154-5). Since the models themselves predicted perturbations no ad-hoc hypothesis explaining these deviations is necessary.

The actual world is not the subject of Book I; Book I lacks physical content (De Gandt, 1995, p. 267). The propositions of Book I are part of an autonomous enterprise.[11] As such Book I is written with the purpose to demonstrate what will be the case if certain physical conditions hold (neglecting at that moment the real conditions in the actual world). Put differently: it is an investigation of what follows from the laws of motion given some force functions.[12] It is an independent enterprise from what happens in Book III, the Systema Mundi. Book I is an investigation of the mathematical consequences that follow from certain physical conditions, i.e. an "investigation of those quantities of forces and their proportions that follow from any conditions that may be supposed". Then "coming down the physics" one starts by looking at which mathematical properties are present in nature and infers from this which mechanisms are active (cf. "these proportions must be compared with the phenomena, so that it may be found out which conditions (or laws) of forces apply to each kind of attracting bodies"). Thus in other words: from the mathematical properties present in nature one infers the physical agents (for converging assertions see Newton, 1730, p. 369; Newton, 1726, pp. 382-383, p. 415). Newton is arguing from effects (Kepler's laws, or Kepler's rules as they were called at the time) to causes (centripetal forces). Newton starts with a set of physical conditions, $\underline{\Phi_x}$, and next he deduces the mathematical properties, $\underline{M_x}$. He then observes $\underline{M_x}$ en finally concludes to $\underline{\Phi_x}$. Harper's account has a slightly different stress: Newton infers values of parameters (e.g., a centripetal force) that are measured by the phenomenon (e.g. Kepler's area law) it is inferred from. This amounts to abduction:

(1) $\Phi_x \rightarrow M_x$

(2) M_x

(3) Φ_x

I admit that there is some ambiguity in labelling Newton's strategy "abductive". Proposition 2, Book I allows one to conclude that Kepler's second law entails a centripetal force. So we could say that one can "deduce" a centripetal force from Kepler's second law. However, in the natural order it is the centripetal force which produces or causes motion that conforms to Kepler's second law. That is why it amounts to abduction. This agrees with Newton parlance on "arguing from Effects to Causes". The essential thing is the backing-up by the laws of motion by the conditional sentences in (1). As can be gathered from Proposition 65 Newton sometimes uses if-then sentences or "inference-tickets" that are robust with respect to deviations. The term is due to Arthur Prior – see Smith, 2002a. In general "inference-tickets" link motions to forces, forces to motions, and macrophysical to microphysical forces composing them (Ibid., p. 143). We establish that if $\underline{\Phi_x}$ (e.g. a many-body-system) holds then $\underline{M_x}$ (e.g. the area-law) holds very nearly. Then we observe that $\underline{M_x}$ holds very nearly. From this we can abductively infer that $\underline{\Phi_x}$ is the cause of the observed mathematical regularities. This is a striking feature of Newton's methodology. This interpretation also enables to alleviate the apparent tension between the fact that these models do not exhaustively describe the actual world and Newton's realistic-causal stance. The awareness of this incongruence can be found in various statements of Book III (e.g. Newton, 1726, p. 818, p. 819, p. 832, p. 840, p. 841, p. 845, p. 847, p. 864, p. 867). Although these models do not exactly coincide with nature[13], they can teach us something about the causal agents in the world. This was of utter importance to Newton.

Newton did not start from the perturbations and next decided to model them, as Cohen claims. He rather constructed certain models and concluded from them that perturbations will occur. This can schematically be visualised as follows:

Figure 3.

A constant dynamics in Book I between the phenomena and the models would imply the problems described in 2.2. The models are constructed independently, i.e. as an investigation which motions will be produced by certain force functions. Newton does not from the beginning focus on the kinds of forces that are present in nature. Therefore these models can be considered to have a stronger explanatory power, since they are directly deduced from the principles of motion. We obviously assume that the principles of motion apply to our world. Since the laws and definitions of motion show that the certain motions are produced by a stipulated set of forces we are allowed to regressively infer the corresponding set of forces as the relevant cause for these motions. We see that for Newton a priori deductions of phenomena were intertwined with methodological validity. This is a very specific and stringent form of autonomy.

In their volume on the use of models in science, Morrison and Morgan stress that in scientific praxis models function as autonomous agents (in the sense that they are partially independent of both theories and the world) (Morrison & Morgan, 1999, p. 10). Because models are made up from a mixture of elements (elements that originate from outside of the original domain of investigation), they maintain this partially independent status (Ibid., p. 14). The ways in which models can function autonomously are various. The essential thing is that models are not wholly theory-driven nor data-driven. They live a life of their own. Scientific investigation centres on the model rather than on nature itself. As Morrison puts it:

> Not only do models function in their own right by providing solutions to and explanations of particular problems and processes, but in some cases they even supplant the physical system they were designed to represent and become the primary object of inquiry. In other words, investigation proceeds on the basis of the model and its structural constraints rather than the model being developed piecemeal in response to empirical data of phenomena. (Quoted from Suárez, 1999, p. 169)

This seems to fit hand in glove with Newton's scientific praxis. Instead of piecemeal comparing and constructing models in constant comparison with nature, Newton rather independently investigated the properties of the different models. The three sets minus the laws of motion are the extra-theoretical and extra-phenomenal ingredients. They are the "structural constraints". These elements and their internal dynamics give the models their independent status.

4. Summary and Conclusion

That the deviations are backed-up by the laws of motion is an essential ingredient for Newton to differentiate his methodology from the H-D method. It was Newton's aim to infer (without hypothesizing) the true causes active in rerum natura. Let me sum up how this is done:

(1) The first step consists of the construction of models of planetary motion:

(1a) In Book I Newton does not take into account any medium: the forces, i.e. the causes, are presumed to act in non-resisting media. It is Newton's purpose to demonstrate which mathematical conditions (e.g. area law) will follow from some presupposed active forces (e.g. centripetal forces). His aim is to construct if-then propositions of the form $\underline{\Phi_x \rightarrow M_x}$.[14] These propositions of models are later used to infer the forces producing the observed motions. They are abductive inference-tickets. Note that the secondary mechanisms for these forces are left unspecified.[15]

(1b) The second presupposition Newton makes is that these models obey the laws of motions – "axiomata sive leges motus" – and that these motions can be described with the concepts described in the "definitiones".

(1c) Point masses are assumed to approximate real celestial bodies.

(1d) The more complex models (cf. proposition 60 or 65) demonstrate that perturbations will occur and stipulate the conditions under which these perturbations will be minimal ("quam proxime"). Since these propositions are conceived to be deduced from the laws of motion, they can in a later stadium justify small deviations in cases where the stipulated conditions are met.

(2) The second important phase is the interpretation of the phenomena in view of these models:

(2a) The first step of this phase consists in arguing for the propinquity of the models and the actual world. Newton demonstrates that there is no plenum (the condition in **(1a)** is met). He simply assumes that these models and the actual world are isonomological and isoconceptual (the condition in **(1b)** is met) and that point masses approximate real celestial bodies (**(1c)** is met).

(2b) The second step concerns the deviations. It can be shown that the actual world fits the two cases described in proposition 65. One may infer from this that the deviations from the area law can be ascribed to the forces or causes postulated in these models. This step is backed-up by the deductions from the laws of motion.

Of course, this schematic summary does not capture the full richness of Newton's methodological considerations. However, I think it is an alternative for Cohen's problematic interpretation. Although Smith also defends a sequence of successive approximation he seems to stress that every further derivations and refinements are deduced from the theory (Smith, 2002b, 47). My critique is not directed at this type of approximation approach. However, Smith does not mention the problems with the "Newtonian Style" (Smith, 2002a, p. 154; Smith, 2002b, p. 62, n10). My model-based account gives some insight in how Newton thought that his method differed from the H-D method. In their volume Morgan and Morrison focus on modern science (19th and especially 20th century), particularly in the fields of physics and economics. If my analysis is correct then this promising programme is important not only for an understanding of modern science but also for an understanding of classical mechanics. The models occupy centre stage, next come the phenomena.

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Notes

[1] Cohen notes that 'the Newtonian "one-body-system" is a "system" to the extent that it is composed of two entities, even though these are not homologous, as in the case of a system of two bodies: these are a single body (or mass point) and a center of force'. (Cohen, 1980, p. 302n) Newton remarks: 'Up to this point I have been setting forth the motions of bodies attracted toward an immovable center, such as, however, hardly exist in the natural world.' (Newton, 1726, p. 561)

[2] This is an insertion made by Cohen himself.

[3] Newton banned as much as possible all propositions which were explicitly called "hypotheses" in the first edition and renamed them in the second edition under the names of "rule" or "phenomenon". On this matter see: Crombie, 1994, pp. 1060-1061; Gjertsen, 1986, p. 466; Cohen, 1999, p. 24.

[4] A discontinuous motion along the sides of a polygon is reduced to a continuous motion along a smooth orbital path, by letting the triangles tend to infinity and hence each of the surfaces to zero. Can a continuous force be approximated by as a limit of discontinuous impulsive force as the time interval shrinks to zero? I will not enter the discussion on the validity of Newton's procedure of taking a limit. For a defence of Newton's argument see Nauenberg, 2003; for a critique see Pourciau, 2003.

[5] Hence a centripetal force is a necessary and sufficient condition for the area law. As Newton writes somewhat further: 'Since the uniform description of areas indicates the center towards which that force is directed by which a body is most affected and by which it is drawn away from rectilinear motion and kept in orbit, why should we not in what follows use uniform description of areas as a criterion for a center about which all orbital motion takes place in free spaces?' (Newton, 1726, p. 449)

[6] In the following scholium Newton notes that 'the case of corol. 6 holds for our heavenly bodies (as our compatriots Wren, Hooke, and Halley have also found out independently)' (Newton, 1726, p. 452). While Newton only accepted the area law as applied to the primary planets only as a very approximate empirical truth, he accepted the third Keplerian rule as empirically accurate (Wilson, 1989, p. 91, p. 141, p. 143).

[7] He further writes: 'The more the law of force departs from the law there supposed, the more the bodies will perturb their mutual motions; nor can it happen that bodies will move exactly in ellipses while attracting one another according to the law here supposed, except by maintaining a fixed proportion of distances one from another. In the following cases, however, the orbits will not be very different from ellipses.' (Newton, 1726, p. 568).

[8] Since the motive forces are as the accelerative forces and the attracted bodies jointly (F_m=F_a.m), the absolute attractive force of body A is to the absolute attractive force of body B as the mass of body B. This can be shown as

follows. By the definition of motive force, we can derive: $F_{a1}/F_{a2}=F_{m1}.m_2/F_{m2}.m_1$ (since: $F_a=F_m/m$). By the third law of motion both motive forces are equal. We establish: $F_{a1}/F_{a2}=m_2/m_1$ or that $F_{a2}/F_{a1}=m_1/m_2$.

[9] For the secondary planets Newton's application of Corollary 6 is no surprise, since he assumes that the orbits of the circumjovial planets, e.g., do 'not differ sensibly from circles concentric with Jupiter' (Newton, 1726, p. 797). In the first proposition of Book III Newton is very silent about the perturbations between the sun and Jupiter. If we take a closer look at the Principia however we see that Proposition 60, Book I explains these perturbations (Harper, 1990, p. 192). This proposition involves a two-body-system which allows one to calculate the corrected the harmonic law distance given the masses of the two bodies. If r' is the corrected distance, r is the harmonic distance, s is the mass of the sun, and finally p is the mass of the planet, then: $r'/r=(s+p)/((s+p)^2\times s)^{1/3}$. In this case, we can similarly say that the perturbations are backed-up and explained by the model in Proposition 60.

[10] Smith considers the laws as working hypotheses: 'they are not testable in and of themselves, yet they are indispensable to a train of evidential reasoning. (…) Mediating elements of some kind are always needed to turn data into evidence.' (Smith, 2001, p. 335) Ernan McMullin argues that the laws of motion are not open to being made 'either more exact or liable to exceptions'. McMullin calls them constitutive principles: 'they enable an otherwise purely formal mathematical framework confront experience' (McMullin, 2001, p. 345). He further notes: 'There is, still, of course the question of how well the language works in the long run, so the element of hypothesis may re-enter.' (Ibid.)

[11] Newton seemed to think that Book I was an autonomous enterprise. He commented on Books I and II, as follows: 'In the preceding books I have presented principles of philosophy that are not, however, philosophical but strictly mathematical – that is, those on which the study of nature can be based. These principles are the laws and conditions of motions and of forces, which especially relate to philosophy. (…) It still remains for us to exhibit the system of the world from these same principles.' (Newton, 1726, p. 783; see also Ibid., p. 561)

[12] Recall that the quote in 2.1 continues as follows: 'Let us see, therefore, what the forces are by which spherical bodies, consisting of particles that attract in the way already set forth, must act upon one another, and what sorts of motions results from such forces.' (emphasis added).

[13] In fact Newton when writing De Motu already realised that the true motions of celestial bodies are immensely complicated and far from being perfectly Keplerian (Smith, 2002a, pp. 152-154). Taking in account all causes 'exceeds the force of any human mind' (Whiteside, 1974, VI, p. 78). This makes McMullin's picture of Newton as a defender of successive approximation hard to believe (McMullin, 2001, p. 344). Smith notes: 'any systematic discrepancy from the idealized theoretical motions has to be identical with a specific force – if not a gravitational force

then some other generic force law. This restriction precludes inventing ad hoc forces to save the law of gravity. It hereby makes success in carrying out a program of successive approximations far from guaranteed.' (Smith, 2002a, p. 158)

[14] It is only in Book III that Newton is concerned with the actual celestial motions in nature. We proceed from effects to causes (cf. the main business of natural philosophy is 'to argue from Phaenomena without feigning Hypotheses, and to deduce Causes from Effects'). As Newton wrote at the beginning of the Principia: 'For the basic problem of philosophy seems to be to discover the forces of nature from the phenomena of motions and then to demonstrate the other phenomena from these forces.' (Newton 1726, 382; emphasis added).

[15] Newton's rejection of the H-D method can therefore be read in two ways. One could read it as a stance on the argumentative logic of science (see introduction). One could also read Newton's rejection of hypotheses as a very specific rejection of Cartesian causal explanatory models based on non-observable particles.