

## *Newton's Demonstration of Universal Gravitation and Philosophical Theories of Confirmation*

Newton consistently asserted that his method was not hypothetico-deductive.

I cannot think it effectull for determining truth to examine the severall ways by wch Phaenomena may be explained, unless there can be a perfect enumeration of all those ways. You know the proper Method for inquiring after the properties of things is to deduce them from Experiments. And I told you that the Theory wch I propounded was evinced by me, not by inferring tis this because not otherwise, but by deriving it from Experiments concluding positively & directly.

. . . what I shall tell . . . is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus . . . because it satisfies all phaenomena (the Philosophers universall Topick), but evinced by the mediation of experiments concluding directly and without any suspicion of doubt.

In this [my experimental] philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that [among other things] the laws of . . . gravitation were discovered.<sup>1</sup>

Philosophers—and critical historians—have not tended to give Newton a sympathetic ear. Probably influenced by the hypothetico-deductive account (henceforth HD) and by its first cousin holism, they have tended to dismiss Newton's remarks as the necessarily ineffectual rationalizations of the paranoid who is unable to accept human limitations and honest criticism. Newton's seclusion after his early optical battles, the publication of the *Opticks* after Hooke's death, and the acrimonious debate over the calculus are all well-known and documented incidents, which lend easy credence to a dismissal of Newton's claims of demonstration. There is also

the not inconsiderable point that Newtonian mechanics is, strictly speaking, false!

Given Glymour's confirmation theory, however, Newton's assertions begin to ring true, for on this theory confirmation is the coherent *deduction* of instances of hypotheses *from* observational data and other laws and theories. Glymour's application of his bootstrapping confirmation theory to Newton's argument for universal gravitation is particularly impressive given Newton's hitherto bad philosophical press.<sup>2</sup>

One important difference between Newton and Glymour on confirmation is Newton's insistence that hypotheses are deduced, and Glymour's insistence that only instances are deduced. But this difference depends primarily on Newton's idiosyncratic use of his Rules of Reasoning as premises in a purportedly deductive argument. Glymour correctly notes that interpreted as a bootstrapper, Newton does *not* compute instances of universal gravitation, but instead computes instances of special-case corollaries of universal gravitation. So, for example, the data about the planets and the moons of Jupiter and Saturn are used to calculate, assuming the second law, instances of the corollary that "the sun and each of the planets exert inverse square attractions on whatever satellites they may have." (TE, p. 217) These corollaries, which are confirmed by bootstrap instantiation, are then fed into Newton's *Regulae Philosophandi*, aptly described by Glymour as rules of detachment, and generalized *in stages* to universal gravitation. Glymour makes no pretense that Newton's argument is a pure case of bootstrapping. Obviously the Rules do much of the work.

My aim in this paper is not to analyze how good a mix can be made of Newton's Rules and Glymour's bootstrapping procedures. It is Newton's use of *idealized evidence* that will be of concern here: what it is, what controls its use, and what, if anything, bootstrapping has to do with the use of simplified and computationally tractable data. Before I consider these questions, allow me first to review the first four propositions of *Principia*, Book III, as seen through the eyes of a bootstrapper. These propositions will be the focus of my analysis.

The first three propositions can be seen as Glymourian computations of instances of this corollary of universal gravitation: all satellites of the sun and of the planets are attracted to their respective centers by an inverse square force. For the purpose of constructing a bootstrapping computation diagram, I shall represent this corollary as:

$$((Sx \vee Px) \ \& \ Txy) \rightarrow Ixy$$

where  $S_x$  =  $x$  is the sun

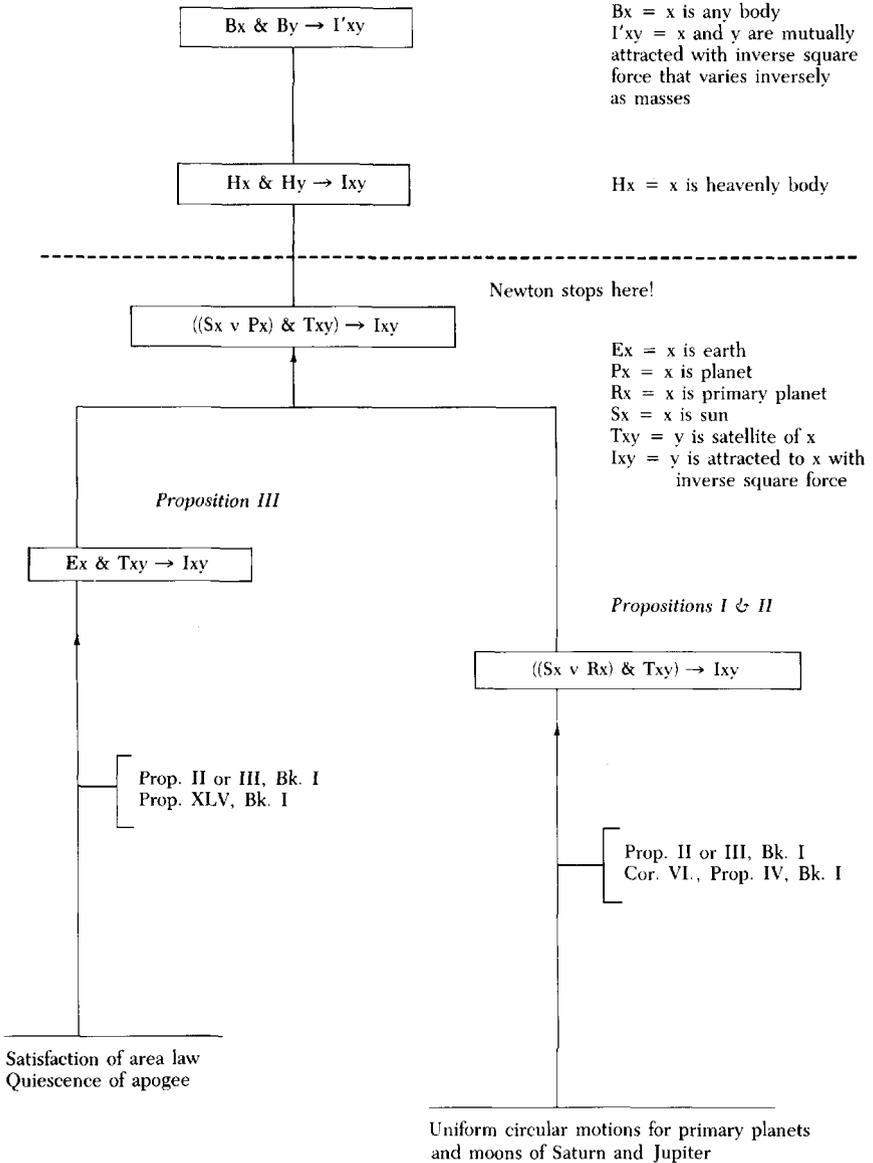
$P_x$  =  $x$  is a planet

$T_{xy}$  =  $y$  is a satellite of  $x$

$I_{xy}$  =  $y$  is attracted to  $x$  with an inverse square force

In order to generate an instance of this corollary, Newton needs to compute—i.e., generate an instance of—the “quantity”  $I_{xy}$  on the basis of the observational quantities of the antecedent and other laws. In the case of the moons of Jupiter (Prop. I), Newton computes his instances by using Proposition II “or” III, and Corollary VI of Proposition IV, all of Book I. In the case of the “primary” planets (Prop. II), Newton makes up his mind and uses only Proposition II as well as Corollary VI. Propositions II and III state in modern terms that if the area swept by the radius vector of a curved motion varies directly as time, then any object in such motion is acted on by a centripetal force directed toward the origin of the radius vector. In short, satisfaction of the area law requires a centripetal force. The difference between the two propositions is that Proposition II is with respect to a center in inertial motion, whereas Proposition III is with respect to a center “howsoever moved.” In the latter case, the total force on the body in motion is the vector sum of the centripetal force and those forces acting on the center. I shall discuss the importance of this difference later. Corollary VI of Proposition IV is the well-known result that if in the case of concentric circular motions the ratios of the square of the periods and the cube of the radii are constant, then the centripetal forces are inverse square. Implicit in this corollary, and explicit in its proof, is the satisfaction of the area law, which is the antecedent condition of Propositions II and III.

The instantiation of the desired corollary of universal gravitation, i.e., the confirmationally relevant content of Propositions I and II of Book III, can now be seen as a straightforward bootstrapping deduction from the “observational” data and two theorems. This deduction is represented by the right-hand fork of the following computational diagram.



*Figure I.* Newton's bootstrapping computations in Propositions I, II, and III.

Proposition III, that the moon is attracted to the earth with an inverse-square force, receives a similar bootstrapping interpretation. Again, Proposition II “or” III is used to show that the moon is acted on by a centripetal force. That this force is inverse-square is shown by using the near absence of the motion of the moon’s apogee and Proposition XXV, Corollary 1 of Book I. These computations are represented by the left-hand fork of the diagram.

Proposition IV is the famous moon test. The centripetal force acting on the moon is calculated and compared with that calculated to be acting on bodies on the earth’s surface. There is equality of magnitude. Therefore, “by Rule 1 and 2,” there is identity of force. Although no bootstrapping *per se* occurs here, this proposition will be important for the purposes of this paper given Newton’s use, as we shall see, of idealized evidence.<sup>3</sup>

As already noted, because of Newton’s use of his Rules of Reasoning, he cannot be interpreted as having computed instances of universal gravitation. So bootstrapping for Newton stops at the location indicated in the diagram.<sup>4</sup> It must be admitted that the propositions leading to this stage of the argument appear to be no more than extremely straightforward, indeed trivial, bootstrapping computations. They do not in fact satisfy Glymour’s conditions for successful bootstrapping, however. Specifically, the computations violate his fourth condition, which states, in essence, that it must be possible for observational data to lead to a negative-instance of the hypothesis in question. (TE, pp. 130-132) But this is *not* possible if we use as resources only those theorems and corollaries that Newton explicitly mentions. The reason is that on this basis alone an input of nonsatisfaction of the area law or failure of the constancy of  $\text{period}^2/\text{radius}^3$  will not allow for a computation of *not-Ixy*. If we interpret Newton as having intended the corollaries of Propositions II and III, and Proposition IV itself, then our problem is solved, since these additions allow for the deduction of negative instances of inverse-square attraction. Newton’s normal practice, though, was to distinguish explicitly between propositions and corollaries used. So some creative reconstruction is required to make good Glymour’s claim of having captured Newton’s practice.<sup>5</sup> But since the possibility of a negative instance is, as Glymour stresses, the very heart of the bootstrapping requirement (TE, pp. 114-117), Newton’s apparent carelessness is a cause for worry if we view bootstrapping as an historical account. And Glymour’s claim is that bootstrapping receives some of its normative support from the fact that it is historically instantiated. (TE, pp. 64, 176)

More serious problems for a bootstrapping interpretation arise when we consider Newton's use of Propositions II and III of Book I. As already noted, they state that satisfaction of the area law entails a centrally directed force with respect to a center in inertial motion (Prop. II) or to a center "howsoever moved" (Prop. III). With respect to Jupiter's moons and that of the earth, Newton uses Proposition II "or" III. The reason for the apparent indecision is clear. Proposition II is inappropriate, since Jupiter and the earth are presumably not in inertial motion. But, on the other hand, Proposition III is also inappropriate, since Newton needs *real forces* for his argument and not indeterminate sums or components.<sup>6</sup> This dilemma is not explicitly resolved by Newton in the *Principia*. With respect to the planets, Newton makes a decision and opts for Proposition II. This choice raises the obvious question of how Newton can know at this stage of the argument that the sun is in inertial motion. Duhem claimed, as the reader may recall, that Newton could not know this, since such knowledge entailed already knowing what were real forces.<sup>7</sup> Therefore, a sun in inertial motion was for Duhem a *convention*. Glymour, however, makes the case that Newton can be read as having given arguments for the sun being in inertial motion that are "both cogent and powerful." (TE, p. 213)<sup>8</sup>

The components of these arguments occur in the context of Proposition XIV of Book III and its corollaries. The proposition states that the aphelions and nodes of the planetary orbits are fixed. The propositions given as justification—respectively, XI and I of Book I—require that there be no forces other than an inverse-square force acting at one focus and that the orbits be elliptical with respect to absolute space. Stationary apsides, therefore, are a deduction from an inverse-square force originating at a stationary sun plus the additional assumption that there are no disturbing forces. Corollary I, states that the stars are at absolute rest, since they are stationary with respect to orbits in absolute rest. Now none of this is relevant per se to showing the sun to be at absolute rest (or in inertial motion), since that is assumed from the beginning. The proof goes from one kind of absolute motion to another, from that of the sun to that of the stars.

Corollary II, though, circles back and shows the noninfluence of the stationary stellar system on our solar system:

And since these stars are liable to no sensible parallax from the annual motion of the earth, they can have no force, because of their immense distance, to produce any sensible effect in our system. Not to mention that the fixed stars, everywhere promiscuously disposed in the

heavens, by the contrary attractions destroy their mutual actions, by Prop. LXX, Book I.<sup>9</sup>

In HD terms, this can be read as stating that no untoward consequences follow from stationary stars, since, being far away and randomly dispersed, they will have no influence on our system. In other words, a consequence of universal gravitation and a stationary sun—namely, stationary stars—is not disconfirmed by observation since this consequence when conjoined with distant and random placement yields a null influence on the planets. On this reading of Proposition XIV and its *corollaries*, we do not have an independent proof for a sun in inertial motion, or for that matter, for a solar system in inertial motion. Glymour, however, sees Corollary II as not really being a corollary at all, but instead as an “independent” argument for the sun’s inertial motion. (TE, p. 212) Evidence will be given below that suggests strongly that this is incorrect.

An argument similar to that given in Corollary II is given by Newton in the *System of the World* (MS Add. 3990), which is an early version of Book III of *Principia*. But here the argument is clearly intended to be an independent proof for the inertial motion of the solar system. In this work, composed sometime before 1685 but not published until 1728, Newton argues directly from stars in relative rest to the conclusion that the center of gravity of the solar system (calculated to be near the surface of the sun) “will either be quiescent, or move uniformly forwards in the right line.”<sup>10</sup> It is this move that Glymour claims can be seen, with elaboration, as “both cogent and powerful.” (TE, p. 213) Since my interest here is with Newton’s use of idealized evidence, I shall not dispute this claim, but shall note only that understood this way Newton is in some trouble because the planetary aphelions *do* rotate. This was known by Newton and is in fact mentioned in Proposition XIV and its scholium! There is also the complication that showing the center of gravity of the solar system to be in inertial motion does no good, since our original problem was to justify Newton’s assumption that the sun was in inertial motion. This assumption was needed in order to utilize Proposition II, Book I, in the bootstrapping computation of Proposition II (and really I and III as well) of Book III. So again Newton is somewhat wide of his mark.

Pemberton, apparently thinking in terms similar to our HD account, communicated very much this sort of observation to Newton when involved in the preparation of the third edition of *Principia*:

Do not the words in which prop. 14 are [sic] expressed seem almost to be contradicted in the demonstration of it?<sup>9</sup> For as in the proposition it is said, that the Aphelia and Nodes remain fixed; in the demonstration it is only shewn, that they would remain so, if they were not moved by certain causes, which, both here and more particularly in the following scholium, are allowed to take effect.<sup>11</sup>

That Newton was concerned by the observable rotation of the aphelions is clear upon examination of the additions made to Proposition XIV in the second edition of *Principia*. In the first edition, Newton simply dismissed these rotations with the comment that “it is true that some inequalities may arise from the mutual actions of the planets and comets in their revolutions; but these will be so small, that they may be here passed by.”

In the second edition, however, Newton added a scholium to justify the claim that these inequalities could be successfully explained away:

Since the planets near the sun (viz., Mercury, Venus, the earth, and Mars) are so small that they can act with but little force upon one another, therefore their aphelions and nodes must be fixed, except so far as they are disturbed by the actions of Jupiter and Saturn, and other higher bodies. And hence we may find, by the theory of gravity, that their aphelions move forwards a little, in respect of the fixed stars, and that as the  $\frac{3}{2}$ th power of their several distances from the sun. So that if the aphelion of Mars, in a space of a hundred years, is carried forwards 33'20'', in respect of the fixed stars, the aphelions of the earth, of Venus, and of Mercury, will in a hundred years be carried forwards 17'40'', 10'53'', and 4'16'', respectively.<sup>12</sup>

What we have in Proposition XIV, then, is this. A stationary sun and inverse-square force entail stationary orbits and (given the observational data) stationary stars. And the great distance and “promiscuous” placement of these stationary stars entail the lack of a *disturbing* force on the planetary orbits. But since the orbits *do* rotate, this prediction of stationary apsides is violated. This violation, though, can be explained away by the hypothesis that Jupiter and Saturn are sources of disturbing forces. Furthermore, a successful conditional prediction can be obtained from data on Mars’s precession. So on my view the essential features of Newton’s argumentation are these. First, the initial data used are idealized so that clean simple calculations can be obtained. Second, *supplementary* arguments are added to show that *better* observational fit is obtainable if certain complications are attended to. There is *not* a unified monolithic HD or bootstrapping account that accommodates the known data in a single coherent calcula-

tion. And it is this patchwork nature that was the source of Pemberton's complaint to Newton. As a last observation, the role of Corollary II is now seen to be not that of providing an independent proof for the sun's inertial motion, but of providing some justification for the identification of Jupiter and Saturn as the sole sources of significant disturbing forces.

Duhem was on a similar interpretational trail when he noted that, whereas Newton accepted as a datum the description of the planetary orbits as elliptical, it is a formal consequence of his theory that because of perturbational forces, these orbits cannot be elliptical.<sup>13</sup> (Actually, as we have seen, Newton starts with circular orbits!) That is, the theory is used to correct its originally supporting data. Newton's use of idealized stationary apsides and elliptical orbits raises the question, then, of what *controls* the form that data take when used as inputs to theories and as confirmational tests. Glymour, after recording Duhem's observation, suggests this analysis: scientists may describe data in a simplified way so as to readily attach them to theory *only if* that description is compatible with the data within the range of calculated observable error: "one fraction of Newton's genius was to see that empirical laws, inconsistent with a theory, could still be used to argue for that theory, provided the inconsistencies were within observational uncertainties." (TE, pp. 222-224)

So on Glymour's view, what Newton did was to pick a mathematically tractable description of data that was later corrected by theory, but that was all the while compatible with then accepted observation. There are significant problems here with the concept of "observational uncertainties," since the calculation of these uncertainties is theory-dependent. And in some important cases, these uncertainties have been differing functions of the theories to be tested. An experiment may be feasible from one theoretical point of view and yet be unfeasible from the point of view of a competing theory.<sup>14</sup> Leaving aside this sort of problem, however, there is this *crushing* difficulty for Glymour: *Newton's descriptions of the phenomena were typically incompatible with the then accepted observational data.*

The most striking example of using idealized data not consistent with known observational uncertainties is Newton's use in Proposition II, Book III, of circular and not elliptical orbits for the planets. Glymour accounts for this counter example to his above thesis with the plausible though ad hoc (and nonbootstrapping) suggestion that "Newton wanted elliptical orbits to be demonstrated in his system rather than to be assumed at the outset . . . ." (TE, p. 208) That is, Newton wanted to show that his system was

self-correcting: idealized data would lead to laws that could then be applied to correct the data. An inverse-square force entails elliptical orbits as a more general case. What I want to show is that this sort of self-correction is not an isolated feature restricted to the move from circular to elliptical orbits. Following Duhem, we have already noted that elliptical orbits also get corrected to accommodate perturbations due to other planets. But here, unlike the circular orbit case, we have a theory suggesting to the experimenter likely corrections to be made to currently accepted observational description. Newton told Flamsteed where and what to look for.<sup>15</sup> In the circular orbit case, on the other hand, theory had to catch up to accepted data. The “logic” of both sorts of cases is similar in that theory is articulated in order to demonstrate how experimental fit can be improved with respect to data less idealized as compared with known or suspected data.

The correction for apsidal rotation in Proposition XIV is like the circular orbit case (Prop. II) in that the originally supporting data were known *beforehand* to be idealized and false. Again, theory is articulated to show how better observational fit is possible. There is also significant use made of idealized data in Propositions III and IV dealing with the moon. As noted above in our initial review of Glymour’s analysis of the beginning of Book III, Newton’s bootstrap computation for the moon depends on the assumptions that the area law is satisfied and that the moon’s apogee does not rotate. As Newton admits, however, the moon’s apogee rotates at the eminently observational rate of 3°3’ per revolution. Furthermore, it is a consequence of Proposition XLV, Book I, that because of this rotation the (net) force acting on the moon cannot be inverse square, and in fact must vary “inversely as  $D^2 \text{ }^{1/243}$ .” Therefore Newton’s attempt to *deduce* universal gravitation directly from phenomena has gone awry. And so as well has Glymour’s interpretation of Newton as having bootstrapped his way from phenomena to instances of universal gravitation. Newton’s response to this failure was at first to dismiss it, since the fractional increase “is due to the action of the sun.” But, as was the case with the unexplained motion of the aphelions in Proposition XIV, Newton felt some obligation to explain away better the factor 4/243 in the second edition of *Principia*. After several false starts the following finally appeared:

The action of the sun, attracting the moon from the earth, is nearly as the moon’s distance from the earth; and therefore (by what we have shown in Cor. II, Prop. XLV, Book I) is to the centripetal force of the

moon as 2 to 357.45 or nearly so; that is, as 1 to  $178^{29/40}$ . And if we neglect so inconsiderable a force of the sun, the remaining force, by which the moon is retained in its orb, will be inversely as  $D^2$ .<sup>16</sup>

Seen as an *isolated* piece of argumentation, the passage can be reconstructed, as Glymour notes, as being HD in form: if only the sun and the earth are exerting inverse square forces on the moon, then the moon's apogee should rotate. But, such a reconstruction by itself does not explain what this apparently isolated piece of HD argumentation is doing in the *midst* of a Glymour-like instance derivation.

Glymour proposes this two-part explanatory account. First, Prop. III is *not* to be interpreted as a piece of bootstrapping: the moon data are not being used as a premise in the deduction of an instance of some corollary of universal gravitation. Second, that "the main point of the *argument* for Theorem III . . . is to demonstrate that the motion of the moon is *consistent* with the assumption that the Earth exerts an inverse square attractive force upon it." (TE, pp. 217-218) These proposals taken together are mildly surprising, since we would expect Glymour to have made some attempt to show that this is truly a case of *bootstrapping* in the sense of assuming the very hypothesis that one wishes to confirm.<sup>17</sup> Furthermore, it is not specified in what sense this is a consistency proof other than the obvious HD sense discussed earlier. But if this is so then Glymour's account seems to collapse into the admittedly unsatisfactory HD account. Finally, there is no real explanation as to why a consistency proof would be needed here. Glymour says merely that "without such a demonstration it might appear that the phenomena contradict the claim." (TE, p. 218)

These problems with Glymour's account worsen when we observe (and Glymour seems to have overlooked this) that a similarly HD-looking argument is given in Prop. II! Newton argues that the *accuracy* of the claim that the planets are attracted to the sun with an inverse square force is demonstrated by "the quiescence of the aphelion points"—i.e., on the assumption of universal gravitation, what we are out to prove, and Proposition XLV of Book I. And this is the same proposition used in Proposition III of Book III.

Looking on to Proposition IV of Book III, we find more instances of Newton's use of data not within the range of experimental error. First, the discrepancy is ignored between the predicted value of the rate of fall at the earth's surface ("15 [*Paris*] feet, 1 inch, and 1 line 4/9") and the observed rate of fall ("15 *Paris* feet, 1 inch, 1 line 7/9"). Second, an admittedly

inaccurate earth-to-moon distance of 60 earth radii is used instead of the more accurate value of  $60\frac{1}{2}$  radii.

Newton planned, for the second edition, an explanation for the discrepancy in Proposition IV between observed and predicted rates of fall. This explanation, which was supposed to appear in the scholium to Proposition IV, was based on improved values for the size of the earth, the complication that the experimental values for the rate of fall were obtained not at the equator but at “the latitude of Paris,” and once again the assumption of universal inverse-square gravitation. But, as Cotes objected, these improved values and corresponding calculations did not mesh with the rest of Book III, especially Propositions XIX (dealing with variations in the moon’s orbit) and XXXVII (dealing with the influence of the moon on the tides). There was a lengthy and involved correspondence between Cotes and Newton as to “how to make the numbers appear to best advantage.”<sup>18</sup> The net result was that Newton was unable to meet his printer’s deadlines, so the proposed explanation was moved to the scholia of Propositions XIX and XXXVII.<sup>19</sup> (Does this sound familiar?) The relevance of all of this, then, is that in Proposition IV, as in the previous two propositions, there is a discrepancy between prediction and observed result that is not explained away as being due to observational error, as required by Glymour’s account. Instead there was an attempt to explain away, in HD fashion, the discrepancy as being due to a hitherto ignored complication.

The other idealized datum used in Proposition IV is that the distance between earth and moon is 60 earth radii. The consensus was that the actual value was in excess of this figure by about one-half a radius, and Newton knew and accepted this.<sup>20</sup> Even so, the value 60 was used in his comparison of the force at the surface of the earth with that acting on the moon. What the argument of Proposition IV in fact establishes is this *counterfactual*:<sup>21</sup>

- If
- (a) the earth is stationary (it is not);
  - (b) the distance between earth and moon is 60 earth radii (it is not);
  - (c) the moon’s period is  $27^d7^h43^m$  (an accurate though mean value);
  - (d) bodies fall at the surface of the earth at  $15\frac{1}{12}$  feet/second (not observed at Paris latitudes);
- then via Rules of Reasoning I and II, the same inverse square force acts on the moon and bodies on the surface of the earth.

Although, as noted, Newton gave the justification for (d) only in Propositions XIX and XXXVII, he gave the justification for (a) and (b) within his discussion of Proposition IV. Propositions LVII and LX of Book I show how to convert systems of rotation about a stationary body to systems of rotation about the center of mass of such systems. In particular, Proposition LX shows what the separation must be in the latter sort of case, given that the period of rotation is to be preserved and given that the force of attraction is inverse square and varies directly as the product of the masses. Newton asserts that use of this proposition converts the distance of the counterfactual situation into the more accurate value of 60 and 1/2 earth radii. (The details, though, are only given in *De mundi systemate*.)<sup>22</sup> All of this, however, is on the *assumption* of universal gravitation, the deductive goal of the first seven propositions of Book III.

Focusing attention on just the first four propositions of Book III shows that if Newton is a bootstrapper, it is only with highly idealized data not compatible with known observational error. Furthermore, bootstrapping gives an incomplete account of the argumentation, since Newton uses supplementary arguments to justify, on the basis of universal gravitation, these idealizations. A summary of these idealizations and their justifications will perhaps be useful. In Proposition II the quiescence of the aphelion points is used to prove "the great accuracy" of the existence of an inverse-square force and not *per se* to correct or justify anything. But showing great accuracy here is just to say that the original description of the orbits as circular is not crucial to the result, given the immobility of the aphelions. That the planetary orbits are not really stationary is admitted and explained away later in Proposition XIV. In Proposition III, the motion of the moon's apogee is at first assumed zero for bootstrapping purposes, but later in the proposition a more accurate nonzero value is given for this motion and accounted for. A conversion algorithm is referred to in Proposition IV that will transform the idealized counterfactual into something more realistic. In addition, if it were not for a printer's deadline, Proposition IV would have contained an account explaining away the discrepancy between observed and predicted rates of fall for terrestrial bodies.

Given, then, that Newton's deductive bootstrapping is filled with inaccurate and simplified data descriptions, our problem is to give a unified explanation of *the confirmatory value* of this sort of (strictly speaking) unsound argument. Since philosophers have almost universally ignored

questions of accuracy and precision, as well as complications of the sort here illustrated, I shall return to Duhem as a useful source of insight. With respect to descriptions involving physical magnitudes, Duhem distinguished between what he called *theoretical* and *practical* facts.<sup>23</sup> The basic idea is this: true, precise, quantitative descriptions of phenomena, practical facts, are because of the complexity of nature either unavailable or unusable. So science must do with theoretical facts, i.e., with idealizations that are usually *not* compatible with experimental error, but that are justified in part by their *logical attachability* to scientific theories. Truth in descriptions can be achieved only at the cost of vagueness, but science demands precision. Typically, scientists have large amounts of quantitative data that are individual measurements of key parameters and properties. But these numbers do not immediately attach to theory. Newton gives only a carefully selected distillation of locational data in *Principia*, but the problem is clear enough. How are these individual pieces of data to be incorporated into theory? The problem is a species of curve fitting. What *prima facie* justifies our curves, however, is not simplicity per se, but theoretical attachability and practically possible computation.

In the case at hand, the idealized descriptions (or summaries) used by Newton were *prima facie* justified because of their ready attachment to the theorems of Book I of the *Principia*. Glymour is right: partial instances of universal gravitation were deducible from data and the laws of motion—but only from false and simplified data. However, logical attachability and practically computable consequences are obviously not enough to insure the rationality of the procedure. What is needed, I contend, is argumentation showing that *if* more accurate descriptions were fed into the theoretical hopper, correspondingly more accurate output would be obtained. That is, one must show that there are real possibilities for accommodating more accurate descriptions of data. To show that such possibilities exist need not require actually constructing the appropriate HD (or statistical) account. All that needs be shown is that a better (in terms of experimental fit) account is possible. And since, as I shall show, not every theory can demonstrate such possibilities, this is not a trivial requirement to place on theories.

A few examples will indicate what it is I am after. In his studies on optics, when confronted with the plain observational fact that there is color separation and diffusion after the second prism of the famous *experimentum crucis*, Newton responded by noting that in his treatment of the

experiment, the aperture descriptions were excessively idealized as being infinitely small. Finite aperture size would, Newton argued, lead to the observed color separation and diffusion. But Newton did not actually construct an HD account that did this; he merely argued that it *could* be done. "But why the image is in one case circular, and in others a little oblong, and how the diffusion of light lengthwise may in any case be diminished at pleasure, I leave to be determined by geometricians, and compared with experiment."<sup>24</sup>

When Shankland argued that Miller's carefully obtained results from numerous repetitions of the Michelson-Morely experiment did not refute the Special Theory of Relativity, he tried to show that there was no reason to think that more accurate thermal convection theories *if available* could not explain away Miller's results. Again, Shankland did not actually construct the better HD mousetrap. "It is practically impossible to carry through calculations which would predict the over-all behavior of the interferometer due to temperature anomalies, since hardly any of the necessary data for such calculations exist."<sup>25</sup>

A rough estimate of the temperature fluctuations needed to account for Miller's positive results required these fluctuations to be ten times greater than those recorded. Nevertheless, Shankland went on to assert and conclude that:

There is no doubt, however, that this factor ten would be very considerably reduced if convection of the air inside the casing were taken into account and if the contribution of the cover of this casing, facing the hut, could be evaluated. . . . We conclude from the foregoing estimate that an interpretation of the systematic effects in terms of the radiation field established by the non-uniform temperatures of the roof, the walls, and the floor of the observation hut is not in quantitative contradiction with the physical conditions of the experiment.<sup>26</sup>

Of course, the most decisive way to show that something is possible is to actually go ahead and do it. Just as good is to give an algorithm that completely specifies a constructive procedure. And these sorts of possibility proofs certainly have occurred in science. An example of a constructive procedure is Newton's reference to Propositions LVII and LX of Book I as recipes for converting a stationary earth-moon system of 60 radii separation into a more realistic moving system with a 60 1/2 radii separation. My point is that actually constructing better accounts and providing constructive

procedures do not exhaust the types of arguments used in science to show that better experimental fit is possible. Newton's elaboration of the *experimentum crucis* shows this. Another, more complex, instance is his response in Proposition XIV to the fact that the aphelions do rotate. Newton did not construct an HD account that predicted these rotations solely on the basis of masses, locations, and initial velocities. The computational problems were too severe. What he did do was to provide a conditional account that predicted aphelion rotations when that of Mars was given. This showed that the theory was on the right track and could, with better data and computational methods, provide better experimental fit.

The other side of the possibility coin is that sometimes it can be demonstrated that better input and computational methods will not lead to better results. Newton's projection of a beam of sunlight through a prism provides an example of such a case. According to the then received view, the image should have been circular, but the experiment showed it to be oblong by a factor of about five. Despite this large discrepancy, Newton did not announce at this stage of his narrative the refutation of the received view.

But because this computation was founded on the Hypothesis of proportionality of the *sines* of Incidence, and Refraction, which through by my own & others Experience I could not imagine to be so erroneous, as to make that Angle by 31', which in reality was 2 deg. 49'; yet by curiosity caused me again to take my Prisme. And having placed it at my window, as before, I observed that by turning it a little about its *axis* to and fro, so as to vary its obliquity to the light, more than by an angle of 4 or 5 degrees, the Colours were not thereby sensibly translated from their place on the wall, and consequently by that variation of Incidence, the quantity of Refraction was not sensibly varied.<sup>27</sup>

It is only after this variation on the experiment that Newton claims to have achieved refutation. Given the perspective of this paper, the point of Newton's prism rotation is easy to see. An exactly circular image would on the received view occur only in the case of a prism placed symmetrically with respect to incoming and outgoing rays. All other orientations would result in oblong images. So it appeared open to supporters of the received view to claim that Newton was in error with respect to his experimental assessment of symmetrical orientation. What the rotation of the prism shows, however, is that even if Newton were grossly in error on this point,

by as much as five degrees, the experimental outcome, the oblong spectrum, would not be sensibly varied. In others words, one type of saving argument is not a real possibility for the received view. Even if more accurate data were available, they would not help.<sup>28</sup>

Pardies objected that Newton's theoretical prediction was based on a mathematical treatment that assumed the beam aperture to be infinitely small. Perhaps a more realistic account would save the received view. But Newton was able to show that the benefits in terms of image elongation did not come close to accommodating the observed discrepancy.<sup>29</sup> Lorentz made a similar response to some detractors of the Michelson-Morley experiment. They contended that more realistic and detailed treatments of the experiment, as opposed to simple single ray accounts, would show that a null result was to be anticipated on the basis of aether theory. And several such demonstrations were in fact published. But Lorentz put an end to such demonstrations by constructing a generalized proof showing that the null result would remain *despite* more realistic treatments. Hence there were mistakes in the published and very complicated detailed treatments of the experiment.<sup>30</sup>

What these examples suggest is that scientific reasoning is a *two-part* affair. First there is the idealization of data until they can be attached in some coherent way to theory. A theory is confirmed with respect to idealized but (strictly speaking) false data if it satisfies Glymour's bootstrapping, or the HD, or perhaps other accounts of confirmation. This, however, is just the initial step. A more severe test of theory occurs at the next stage. Here there are arguments, quite various in form, showing that *if* the idealizations were to be replaced with more realistic descriptions, then the relations between theory and data would *continue* to be confirmatory. A theory then is confirmed with respect to more accurate and less idealized data, if this latter class of arguments, which I have elsewhere called modal auxiliaries, exists.<sup>31</sup> A theory is *disconfirmed* if it can be shown that the introduction of realism does not lead to convergence with more accurate descriptions of the data.

I shall now apply this sort of gestalt to the case of gravitation. Newton can be reconstructed as showing how universal gravitation is deducible from the Rules and from bootstrapped instances. In particular, the first three propositions of Book III can be read as examples of simple bootstrapping. But this tidy connection is achieved only at the cost of using idealized evidence, i.e., evidence simplified for theoretical convenience and not

consistent with observational error. What adds confirmatory value to this deductive but unsound story are Newton's various demonstrations that experimental fit can be improved, or, in the case of Proposition IV, that the same result is obtainable if more accurate data are used. The precession of the moon's apogee can be accounted for in terms of the sun's interference. The use of the earth-to-moon distance of 60 earth radii converts to the observationally more accurate 60 and 1/2 radii if the earth's motion around the sun is taken into account. The variation in predicted and observed rates or terrestrial acceleration can be explained away in terms of the interfering centrifugal forces at higher latitudes. The purpose of the various HD arguments sprinkled among the deductive derivation of theory from data is to provide the necessary justification for the initial use of idealized evidence. Newton is showing that things can be made better.

Finally, it should be noted that Newton's demonstration of universal gravitation is immediately followed by Proposition VIII, which shows that no harm is done in treating spherical gravitational sources as having their mass concentrated at their centers. At distances greater than their radii, gravitational attraction will continue to be inverse square. Therefore, *a more realistic treatment* of objects as spatially extended will *not* disturb Newton's basic results.<sup>32</sup>

Glymour admits in *Theory and Evidence* the greater historical accuracy of my account. It provides a better surface grammar than his account. He balks, however, at my "radical treatment of observation." (TE, p. 217) I have tried to show here that my approach also makes good *normative* sense, since it provides a way of living with the Duhemian dilemma of truth or theoretical tractability. To quote one of Glymour's requirements for a confirmation theory, my account does, I contend, "*explain* both methodological truism and particular judgments that have occurred within the history of science." (TE, p. 64) It is a necessary adjunct to Glymour's bootstrapping account.

## Notes

1. The first two quotations are from Newton's correspondence with Oldenburg (6 July 1672, 6 February 1671/2) and are reprinted in H. W. Turnbull, ed., *The Correspondence of Isaac Newton*, I (Cambridge: Cambridge University Press, 1959), 209, 96-97. The last quotation is from the General Scholium of Newton's *Principia* and originally appeared as a late addition in Newton's correspondence with Cotes (28 March 1713), A. Rupert Hall and Laura Tilling, eds., *Correspondence*, V (1975), 397. See also Alexandre Koyré and I. Bernard Cohen, ed.,

*Isaac Newton's Philosophiæ Naturalis Principia Mathematica*, in two volumes (Cambridge, Mass.: Harvard University Press, 1972), II, pp. 763-764. I shall use Cajori's revision of Motte's translation of *Principia*: *Sir Isaac Newton: Principia* (Berkeley and Los Angeles: University of California Press, 1966), p. 547.

2. Clark Glymour, *Theory and Evidence* (Princeton: Princeton University Press, 1980), pp. 203-226. For the sake of brevity I shall make all references to this work *within* the text of the paper and I shall use the notation TE.

3. Glymour's account of Proposition IV (TE 218) is somewhat abbreviated, but I do not believe he intends this to be interpreted as bootstrapping. Given some stretching of the text, however, a bootstrapping interpretation is not impossible.

4. Because of Newton's use of his Rules of Reasoning, it is impossible to determine exactly where bootstrapping stops and where use of the Rules begins. For example, Proposition II deals only with the primary planets, i.e., excluding the earth, whereas in Proposition V Newton assumes that all of the planets are attracted to the sun. Newton can be interpreted therefore as having instantiated a more general proposition or as having applied his Rules. See also *The System of the World*, translated in Cajori, *Principia*, pp. 554-559.

5. Even liberally reconstructed, Newton's computations are still only "partial" since no instance or counter-instance is computable on the basis of noncurved orbits. But Glymour, with laudable foresight, allows for such partial functions in his statement of the bootstrapping conditions. See TE, pp. 158-159.

6. Cf. Cotes to Newton, 18 March 1712/13, *Correspondence*, V, p. 392.

7. Pierre Duhem, *The Aim and Structure of Physical Theory*, trans. Philip P. Wiener (New York: Atheneum, 1962), p. 192.

8. Another option, surprisingly not noted by Glymour, would be to simply accept universal gravitation as a bootstrapping assumption of the computation and to then demonstrate that the production of negative instances is nevertheless possible. But this need not be unwelcome by a Duhemian, since it could be interpreted as specifying the consistency requirements that need be satisfied by our conventions. Cf. Hans Reichenbach, *The Philosophy of Space and Time* (New York: Dover, 1959), p. 17.

9. Koyré and Cohen, *Principia*, II, p. 590. Cajori, *Principia*, p. 422.

10. Cajori, *Principia*, pp. 574-575. For the history of this early version of Book III of *Principia*, see I. Bernard Cohen, *Introduction to Newton's 'Principia'* (Cambridge: University Press, 1971), pp. 327-335.

11. Queries on *Principia*, (February 1725), reprinted in A. Rupert Hall and Laura Tilling, eds., *Correspondence* VII, p. 306.

12. For documentation of Newton's revisions to Proposition XIV, see Koyré and Cohen, *Principia*, II, pp. 590-591. Translation in Cajori, *Principia*, p. 422. Incidentally, no changes were made in response to Pemberton's query.

13. Duhem, *Aim and Structure*, p. 193.

14. For some examples see my Independent Testability: The Michelson-Morley and Kennedy-Thorndike Experiments. *Philosophy of Science* 47 (1980): pp. 1-35.

15. To Flamsteed (30 December 1684), reprinted in H. W. Turnbull, *Correspondence*, II, pp. 406-407.

16. Koyré and Cohen, *Principia*, II, p. 566; Cajori, *Principia*, p. 407. For the correspondence leading up to this addition start with Cotes to Newton (23 June 1711), *Correspondence*, V, p. 170, where Cotes complains (rightly) to Newton concerning a proposed addition: "I should be glad to understand this place, if it will not be too much trouble to make it out to me. I do not at present so much as understand what it is yt You assert."

17. Glymour, in his closing appraisal of Newton's demonstration of universal gravitation (TE, p. 225), mentions as a possibility that Proposition III be given a bootstrapping interpretation.

18. Cotes to Newton (16 February 1711/12), *Correspondence*, V, p. 226. See also Cotes to Newton (23 February 1711/12), *Ibid.*, p. 233 where Cotes writes: "I am satisfied that these exactnesses, as well here as in other places, are inconsiderable to those who can judge rightly

of Your book: but ye generality of Your Readers must be gratified with such trifles, upon which they commonly lay ye greatest stress. . . . You have very easily dispatch'd the 32 Miles in Prop. XXXIXth, I think You have put the matter in the best method which the nature of the thing will bear."

19. See Cotes to Newton (13 March 1711/12) and Newton to Cotes (18 March 1711/12), *Correspondence*, V, pp. 246-248.

20. This, I believe, is clearly the sense of Newton's discussion of the data in Proposition IV. However, if there is any doubt about this reading, for additional evidence see Newton's draft scholium to Proposition IV, reprinted in *Correspondence*, V, pp. 216-218. See also Corollary VII of Proposition XXXVII, Book III, where some of this draft finally appeared.

21. For additional support that this is the correct reading, see Pemberton's Queries on *Principia* (? February 1725), *Correspondence*, V, p. 306, where he writes: "the whole paragraph seems to me not to express, what is intended by it, in the fullest manner: your design being to give a reason why you assumed the distance of the moon from the earth a little less than what you shew astronomical observations to make it. Would not this intent be a little more fully expressed after the following manner? 'As the computation here is based on the hypothesis that the Earth is at rest, the distance of the Moon from the Earth taken a little less than astronomers have found it. If account is taken of the Earth's motion about the common centre of gravity of the Earth and the Moon, the distance here postulated must be increased (by Prop. 60, Book I) so that the law of gravity may remain the same; and afterwards (corol. 7, Prop. 37 of this book) if may be found to be about  $60\frac{1}{2}$  terrestrial radii.'"

22. I.e., MS Add. 3990; Cajori, *Principia*, pp. 560-561.

23. Duhem, *Aim and Structure*, pp. 132-138.

24. To Oldenburg for Pardies (10 June 1672), *Correspondence*, I, p. 167. For more details about the *experimentum crucis* see my Newton's *Experimentum Crucis* and the Logic of Idealization and Theory Refutation. *Studies in History and Philosophy of Science* 9 (1978): 51-77.

25. R. S. Shankland, New Analysis of the Interferometer Observations of Dayton C. Miller. *Reviews of Modern Physics* 27 (1955): 175.

26. Ibid. For more details about this case and a comparison with Newton's *experimentum crucis* see my Newton's *Experimentum Crucis*, pp. 71-75.

27. To Oldenburg (6 February 1671/2) [from *Philosophical Transactions* 6 (1671/2), 3075-3087], *Correspondence*, I, pp. 93-94.

28. What Newton showed experimentally is that at orientations near symmetrical, the size of the spectrum is insensitive to changes in orientation. So large errors in initial conditions have only small effect on predicted effect. Duhem gives a sort of converse example in *Aim and Structure*, p. 139, where small errors in initial conditions yield large errors in predicted effect. Surprisingly, no commentator that I am aware of has correctly reported the logic of Newton's argument. They all give a traditional *modus tollens* analysis whereby it is held that Newton rejected the received view simply because of the lack of experimental fit. And these commentators include the usually perceptive Thomas Kuhn in, for example, his Newton's Optical Papers, *Isaac Newton's Papers & Letters On Natural Philosophy*, ed. I. B. Cohen (Cambridge, Mass.: Harvard University Press, 1958), p. 32. For more details about the spectrum experiment see my Newton's Advertised Precision and His Refutation of the Received Laws of Refraction. *Studies in Perception: Interrelations in the History and Philosophy of Science*, ed. P. K. Machamer and R. G. Turnbull (Columbus: Ohio State University Press, 1977), pp. 231-258.

29. Pardies to Oldenburg (30 March 1672), Newton to Oldenburg (13 April 1672), *Correspondence*, I, pp. 130-133, 140-142. For an analysis of this interchange see my Newton's Advertised Precision, pp. 252-254.

30. Lorentz's proof, several references, as well as discussion, appear in Conference on the Michelson-Morley Experiment, *Astrophysical Journal* 68 (1928): 341-373.

31. In my Idealization, Explanation and Confirmation, *PSA 1980: Proceedings of the 1980 Biennial Meeting of the Philosophy of Science Association*, I, ed. Peter D. Asquith and Ronald N. Giere (East Lansing: Philosophy of Science Association, 1980), pp. 336-350. What

distinguishes my view from that of Lakatos is his use of series of (predictionally) complete theories as compared with my emphasis on possibility proofs. What distinguishes my view from that of Kuhn is my insistence on the rationality of possibility proofs. See my "Idealization," 347, fn. 6 and 8.

32. See the well-known Newton to Halley (26 June 1686), *Correspondence*, II, p. 435.