"From the Phenomena of Motions to the Forces of Nature": Hypothesis or Deduction?

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There is a passage in Hume's *Enquiry concerning Human Understanding* that I have always found striking and rather charming. It concerns a metaphysical theory that Hume regards as bizarre; and he offers two philosophical arguments in its confutation. It is the first of these that I have in mind:

> *First,* [he says,] It seems to me, that this theory ... is too bold ever to carry conviction with it to a man, sufficiently apprized of the weakness of human reason, and the narrow limits, to which it is confined in all its operations. Though the chain of arguments, which conduct to it, were ever so logical, there must arise a strong suspicion, if not an absolute assurance, that it has carried us quite beyond the reach of our faculties, when it leads to conclusions so extraordinary, and so remote from common life and experience. We are got into fairy land, long ere we have reached the last steps of our theory; and *there* we have no reason to trust our common methods of argument, or to think that our usual analogies and probabilities have any authority. Our line is too short to fathom such immense abysses. And however we may flatter ourselves, that we are guided, in every step which we take, by a kind of verisimilitude and experience; we may be assured, that this fancied experience has no authority, when we thus apply it to subjects, that lie entirely out of the sphere of experience. (Hume 1777, pp. 59–60)

Hume was, of course, a great admirer of Newton, and took Newton's theory of gravitation in particular as the very paradigm of science. Can Hume have reflected seriously upon the chain of arguments by which Newton claims to establish—on the basis of phenomena accessible to everyone—that each particle of matter in the universe attracts each other particle by a force whose value he precisely states? By what line did Newton fathom *that* abyss?— Nothing in Hume's philosophy suggests that he did seriously consider this question; but the passage I have just cited sets the mood in which we ourselves, I think, ought to consider it.

Let me suggest two further points of general perspective. First, in the preface to the *Principia* Newton tells us that his subject in the book is what he calls *potentiæ naturales,* or *vires naturæ*—natural powers, or forces of nature. From the point of view of the physics of our own time, the discovery of universal gravitation was the discovery of the first of what we ourselves call the fundamental natural forces. In this sense, quite apart from the extraordinary *scope* of the law Newton stated—the enormous extrapolation involved in it—we have to see its discovery as one of astonishing *depth*; indeed, it was a discovery of a sort that the reigning epistemology of the circle of philosophers with whom Newton is most closely associated (and of which Hume is often considered the culmination) considered demonstrably beyond the scope of human capacities (cf. Stein 1990). There is, moreover, a second statement by Newton of his aim in the *Principia*: namely, to show how to distinguish the true from the apparent motions of bodies. This remark—which occurs at the end of the celebrated scholium on space, time, place, and motion—has occasioned some rather pointed comment on the blindness of scientists to the significance of their work; but one now understands pretty clearly that what Newton is talking about is his success in obtaining a decisive resolution of the issue posed by the competing geocentric and heliocentric cosmologies.[1] Since

[1] Reichenbach (1957, p. 212) writes as follows: "Newton distinguishes ... between the real and the apparent motion of a body. The distinction is not always obvious, and Newton regards it as the task of mechanics to develop methods that allow us to carry through the distinction between real and apparent motion in all cases. '... how we are to obtain the

that resolution is essentially a corollary of the theory of gravitation, we see that whatever argument leads from the phenomena to this theory must in some way implicate the deeper philosophical problems of space and time.

The law of universal gravitation is stated by Newton in Proposition VII of Book III of the *Principia* and its second corollary. Although I do not believe, as I shall explain presently, that what Newton calls the "deduction from the phenomena" of the law of gravitation is properly said to have been completed at that point, it is clearly the case that a "chain of arguments that conduct to" that proposition has occurred by then. It is thus our first task—and a great part of our main task—to examine that catenation of reasoning.

Let me review briefly the state of affairs with respect to Propositions I–IV of Book III. It is almost (but not quite) true that the first three of these are *derived mathematically*—"mathematically demonstrated," Newton would say—from what are called *Phænomena* in the introductory material to Book III.[2] These latter are actually formulations of astronomical regularities, as regularities of the *motions* of the heavenly bodies (planets or their satellites), each referred to a suitable frame of reference: for each system of satellites of a central body, the motions are described from a perspective in which the fixed stars and the central body in question are taken to be at rest.[3] In particular, then, Newton's statements of the *Phænomena* carefully abstain from any commitment as to the "true motions." (In view of his statement of aim in the scholium to the Definitions, this is of course essential to his purpose: to "*collect* the true motions from their causes, effects, and apparent differences.")

Propositions I–III in effect *translate* the mathematical description of the astronomical regularities, from the form in which they were first propounded by Kepler, into a simpler but (nearly) equivalent form, relating the *acceleration* of the satellite to its position relative to the central body: the acceleration is directed always towards the central body, and its magnitude varies—both for a given satellite within its orbit, and from satellite to satellite within a single system—inversely as the square of the distance from the central body.[4]

Proposition IV is another matter. What it claims to tell us is something about the *cause* of the behavior of a certain astronomical body. The proposition reads: *That the Moon gravitates towards the Earth, and by the force of gravity is drawn continually away from rectilinear motion, and retained in its orbit.* It is established by a calculation, with the help of the inverse square law, of what the orbital acceleration of the moon *would become* if the moon were brought down to the surface of the earth. The result agrees with the acceleration of terrestrial falling bodies; whence Newton concludes: "And therefore the force by which the Moon is retained in its orbit is that very same force, which we commonly call gravity."

It is most important to be clear about the content of such an assertion: that a certain force is *the very same force as* something-or-other. What notion of force is operative here—and what notion of identity? A parenthetic comment added in the second edition of the *Principia,* referring at this point to Rules I

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true motions from their causes, effects, and apparent differences, and the converse, shall be explained more at large in the following treatise. For to this end it was that I composed it.' These concluding words of the introduction to Newton's main work illustrate the contrast that may exist between the *objective significance* of a discovery and the *subjective interpretation* given to it by its author. Whereas the physical system of Newton's dynamics has become a basic part of science, which, though transformed by later developments into a higher form of knowledge, will always be retained as an approximation, his philosophical interpretation did not survive." For a discussion of the point, cf. Stein (1967, pp. 176–182; 1970, pp. 260–266).

[2] The designation *Phænomena* for these statements of astronomical regularities was introduced in the second edition of the *Principia* (1713). In the first edition (1686), they are called "Hypotheses."

[3] A fully accurate account would require some qualification and elaboration, but none that would affect the point here at issue.

[4] To be somewhat more precise: for the satellites of Jupiter, Newton takes the orbits to "differ but insensibly from circles concentric to Jupiter"—hence there is no variation of distance within a single satellite's orbit; and the same is (tacitly) assumed for the satellites of Saturn (in the second and third editions: these were not treated in the first edition). Intraorbital variation of distance and of acceleration applies, therefore, only to the motions of the planets about the sun and to that of the moon about the earth. As to the "near" mathematical equivalence of Propositions I–III with the Keplerian regularities, it must first of all be noted that to obtain this one must assume all three of what are now usually called "Kepler's Laws"; whereas Newton does not in fact sate the law of ellipses in his *Phænomena*. (This makes for some trouble in his actual arguments for Propositions II and III; but to deal with that point is beyond the scope of the present discussion.) If we do presuppose the law of ellipses, then Kepler's regularities imply Newton's; and Newton's, conversely, imply Kepler's, *under the additional condition that the bodies move in closed paths* (or just that the distance of a given satellite from its central body remains bounded).

and II (of the *Rules of Philosophizing*), implies that what is asserted to be the same is "the cause" of the moon's behavior and that of falling bodies. By itself, however, this is not obviously helpful; for "same cause" is at least as problematic a concept as "same force." Within the bounds of the *Principia*, what seems the best guide to Newton's intention is contained in the section of Definitions prefatory to Book I. An action exerted upon a body [tending] to produce a change in its motion is called by Newton "a force impressed on the body." The acceleration of the moon, therefore, or that of a falling body, manifests such an "impressed force"; and so does the tendency of a heavy body (at any instant) to weigh down upon an obstacle that prevents it from accelerating downwards. But Newton says that such actions have in general diverse causes: "Impressed forces are of different origins; as from percussion, from pressure, from centripetal force." Since Propositions I–III of Book III in effect identify the forces on the planets and satellites as "centripetal forces," it is this last category that we are concerned with.

Now, it surely should seem odd to anyone who has had a course in what is called Newtonian mechanics to read in Newton that an "impressed force" may be *caused by* a "centripetal force." Newton proceeds to define the latter as *that by which bodies are drawn or impelled, or any way tend, towards a point as to a center.* Is this not—one may wish to ask—a *special case,* rather than a *cause,* of "impressed force"?

The answer to this is that Newton himself uses the term "force" in a different way from that customary in Newtonian mechanics; rather, indeed, in a way resembling the one in which physicists today use the word when they speak of the "four [or however many—more or fewer—] fundamental forces." Thus the first force he actually defines is *materiæ vis insita,* the "intrinsic force of matter," otherwise its *vis inertiæ,* or "force of inactivity": committing the notorious crime of the freshman physics student, to call inertia a force. *Vis impressa,* I think, is best understood as a phrase denoting, not a *kind* of force at all, but as the *functioning* of a certain kind of force (the kind Newton elsewhere calls "active force," in contrast with the passive "force of inactivity"): thus, I am suggesting, "impressed force" = "exerted force," and its "origin" or cause—e.g., a centripetal force—is "the force that is being exerted."

But this, if clarifying at all, is so only of (so to speak) the syntax of Newton's usage, not of its semantics. How are we to understand the notion of a centripetal force as the "cause" of an action on a body tending towards a center, and in particular to understand the notion of *the same* centripetal force, as *the same cause*?

Remaining within the text of the *Principia,* further instruction is to be found in the definitions Newton gives of *three distinct "measures"* of a centripetal force; and especially in his elucidating comments on these measures. Taking them in reverse order, the "motive measure" (which Newton also calls simply the "motive force")—Definition VIII—is the one we have all been introduced to in elementary physics. It is (in effect) the product of mass and acceleration;[5] and Newton characterizes it as measuring the action actually exerted upon a body: "I refer the motive force to the body, as an endeavor and propensity of the whole towards a center, composed out of the propensities of all the parts." (Thus the motive measure is properly the measure of an *impressed* force—of the force *as acting on the body.*) A second measure, the "accelerative measure" or "accelerative force," is just the acceleration engendered by the force;[6] a notion that is a little puzzling, and far from innocent, as I shall explain in a moment. Finally—in Newton's sequence, initially—there is what he calls the "absolute quantity of a centripetal force." He defines it (Definition VI) as "the measure of the same, greater or less according to the efficacy of the cause that propagates it from the center through the surrounding regions." The definition is of considerable interest, but it certainly does not succeed in *defining* a *quantity*: rather, I should say, it gives us to understand what the absolute quantity of a centripetal force is supposed to be a measure of—so that we may, in favorable circumstances, come to recognize a particular quantity as of the sort required.[7] And

[5] Newton's definitions of the various "measures" he introduces always leave the choice of a unit free; and, in accordance with classical usage, he never speaks of the "product" of two magnitudes, but only of *proportionality,* which may be proportionality of one magnitude to two others *conjointly.* Thus he defines the motive quantity of a (centripetal) force as its measure, "proportional to the motion it generates in a given time." But as to this latter notion—"motion" as a quantity—Definition II tells us: "The quantity of motion is the measure of the same arising from velocity and quantity of matter conjointly."

[6] Definition VII: "The accelerative quantity of a centripetal force is the measure thereof proportional to the velocity it generates in a given time."

[7] The difference in phraseology between Definition VI (just quoted), on the one hand, and Definitions VII and VIII (see nn. 5, 6 above), on the other, is worth noting. Motte's translation ignores the difference, using the words "measure ... proportional to" in each case; but Newton's looser wording of Definition VI—"measure ... greater or less according to"—seems to acknowledge that he has not really specified a measure, but has only indicated to us roughly what he intends

we do in fact succeed in this: Proposition VII of Book III allows us to say with precision what is the absolute measure of the centripetal force of gravity. Definition VI, therefore, rather identifies an aspect of the problem that confronts us, than contributes to its solution.

It is in a series of elucidatory paragraphs following Definition VIII that Newton goes to some length to try to convey to us what is on his mind in offering these several definitions of quantities of centripetal force. It is from that place that I have quoted his characterization of the motive force, as the action on—or the "endeavor or propensity of"—a particular body, composed of the propensities of all its parts. Of the absolute force he says that he refers it "to the center, as endowed with some cause, without which the motive forces would not be propagated through the surrounding regions; whether that cause be some central body (such as is the magnet in the center of the magnetic force or the Earth in the center of the gravitating force) or something else that does not appear. This concept is only a mathematical one. For I do not now consider the physical seats and causes of the forces." (In other words: the absolute quantity is in a certain sense referred "mathematically" to the center; and it is conceived as a measure of the "efficacy of the cause," *whatever the physical nature of that cause may be.* This, once again, we shall later find of some interest; but it is of dubious assistance in clarifying Proposition IV.

For that, what Newton has to say here about the accelerative force turns out to be of most direct use. And what he does say is quite strange: he refers the accelerative force "to the place of the body, as a certain efficacy, diffused from the center to the several places around it, for moving the bodies that are in them." How, we may ask, does the acceleration of a body acted upon by a force towards a center serve appropriately as a measure of something or other that has been "diffused" to the *place* of that body. If the "motive force" measures something in which the body itself is involved, does not the acceleration—which is, after all, the acceleration *of that body*—do so equally? Most crucially: how does it make sense to speak of an "accelerative quantity" that characterizes *all the places* around the center of force—whether or not there happen to be bodies in them?

The answer, of course, is that it does not make sense in general. The notion that Newton has characterized qualitatively in his discussion of accelerative force as "referred to" place is the concept of what we call a "field" (on this point, cf. Stein 1970, pp. 265ff., 304ff.); but his quantitative definition does not in most cases accord with this notion—it does not, for instance, in the case, adduced by him, of magnetic force. What Newton has actually done is (a) to make it quite clear that the basic notion of centripetal force that he is concerned with is the notion of a *central force field,* and (b) to define that measure of the intensity of the field that happens to be appropriate to the force he intends to deal with in Book III.

And this does suggest an answer to our problem about the sense of the identification of the force on the moon with its weight. Proposition IV asserts, in effect, that the acceleration of the moon towards the earth (or rather, its accelerations, at all the points it traverses in its orbit), and the accelerations of freely falling terrestrial bodies, are all manifestations of a single *field of acceleration*: directed towards the earth at all points around it, and varying in magnitude inversely with the square of the distance from the earth's center.

But now it may seem that this puts Proposition IV into the same class, after all, as Propositions I–III. Is it not simply a matter of calculation from the data—a calculation that Newton carries out, and presents to establish the proposition—that the moon's acceleration is related to that of terrestrial bodies as Proposition IV claims? It is so indeed; but this is not the content of Proposition IV. The latter has an extremely far-reaching physical implication that is neither contained in the data nor extrapolated in any direct way from the data: an implication that was hardly dreamt of before Newton published the *Principia,* and that was received with assent and astonishment by the scientific community; for instance, it was regarded as a great and wholly unanticipated discovery by such distinguished opponents of the more general theory of Book III as Huygens and Leibniz.

The discovery I mean is that gravity—weight—varies inversely with the square of the distance from the center of the body towards which it tends. For note that the inverse square law for the acceleration *of the moon* is asserted in Proposition III on the basis of an argument from the phenomena, and this result plays a crucial role in the argument for Proposition IV; but before Proposition IV has been established, *no grounds whatever* are apparent for asserting a like law of variation for the weight of a terrestrial

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it as a measure *of*.

body. That the two "natural effects," the acceleration of the moon and that of falling bodies, are to be regarded as "the same" in the sense of Rule II, and thus as having "the same cause," has this as at least an important part of its effective meaning: the law governing the "diffusion from the center [that is, the earth,] to the several places around it" of "an efficacy for moving the bodies" that are in those places is the same for terrestrial bodies and for the moon.

So much for "brief" review; the complications we have encountered involving the notion of cause and Newton's concept(s) of force will prove of central importance for what now follows.

Proposition V of Book III is quite straightforward. What has been asserted of the moon in relation to the earth is now repeated for the other satellite systems: the satellites of Jupiter "gravitate"—have weight—towards Jupiter (and analogously, in the second and third editions, the satellites of Saturn towards Saturn); the circumsolar planets gravitate towards the sun. The argument is a simple appeal to Rule II: "The revolutions of the circumjovial planets about Jupiter, of the circumsaturnal about Saturn,[8] and of Mercury and Venus and the other circumsolar planets about the sun are phenomena of the same kind as the revolution of the moon about the earth; and therefore (by Rule II) will depend upon causes of the same kind." Thus we are led to the conclusion that each of the bodies that has at least one satellite is a center of gravitational force. In the first corollary to Proposition V Newton extends this conclusion to the remaining planets: "Gravity therefore is to be conceded towards all the planets. For doubtless Venus, Mercury, and the rest, are bodies of the same sort with Jupiter and Saturn."[9] Corollary 2 then adds that the force of gravity towards any one planet varies varies inversely with the square of the distance from that planet's center. (Of course the inverse square law is taken to hold for gravity towards the sun as well; Newton has in fact made this clear—if not, strictly speaking, explicit—in a supplementary remark to his main argument for Proposition V.)

But although this argument presents no difficulties, there are two points about the conclusion so far reached that it is most important to understand—and that are easily overlooked. The first is this: As I have said earlier, Newton has formulated the astronomical regularities for each satellite system with respect to a frame of reference suitable for that system; and—in accordance with his program of *inferring* the true motions from their "causes, effects, and apparent differences"—with no commitment to a definitive statement about the "absolute" motions. (Indeed, the very word *Phænomena,* introduced by Newton in the second edition as the heading for these formulations, indicates that they are concerned with the *apparent*—that is, the "relative"—motions.) It follows that the "accelerative forces" involved in the arguments for Propositions I–V have the same "relative" character. Thus the conclusion we are entitled to at this point is the following: For each satellite system, let us consider a rigid geometric frame F in which the central body A and the fixed stars are all at rest. Each point fixed relative to F will have, according to Newton's theory of absolute space and time, a certain "true" or "absolute" acceleration (not, of course, assumed to be known to us). By Proposition V and its corollaries, there is a force of weight towards A, whose accelerative measure relative to F varies inversely with the square of the distance from the center of A. The total (true) accelerative force on any body B at any moment will, then, be the sum of the accelerative force of weight towards A, the resultant acceleration produced by any other forces that act on it relative to F, and the absolute acceleration of the point (fixed relative to F) at which B is located at that moment. We have thus, at this point of the argument, a distinct picture of several fields of gravitational force, not only directed to as many distinct central bodies, but also characterized with respect to as many distinct frames of kinematical reference; we have so far no account of how this whole ensemble is to be organized into a single account of the motions and forces.

One supplementary remark about this first point ought still to be made: namely, that the task of combining the several gravitational fields is essentially simplified by Newton's sixth corollary to the Laws of Motion.[10] For the distances of any planet and its satellites from the sun are so large in comparison to

[8] The reference to the circumsaturnal planets does not occur in the first edition.

[9] The words "and Saturn" do not occur in the first edition; instead, the quoted sentence is there followed by another, referring to the one satellite of Saturn known at the time—the "Huygenian Planet"—as undoubtedly having weight towards Saturn.

[10] Newton does not cite the sixth corollary directly in this connection. However, his discussion of the fact that gravity towards the sun does not greatly perturb the motions of the satellites about their ruling planets (in the argument for Proposition VI of Book III) is based upon Proposition LXV of Book I; and in the demonstration of Case 2 of this proposition (the relevant case), the substance of that corollary is used: namely, the proposition that "the equal accelerative forces with which the bodies are impelled in parallel directions do not change the situation of the bodies with respect to each other,

their distances from one another, that their "accelerative gravities" towards the sun may be regarded as very nearly parallel and equal; and under these circumstances, Corollary VI of the Laws assures us that the motions of the bodies among themselves will be the same as if that common additional acceleration were absent.

The second point to be made about the conclusion so far reached is less technical, but more startling: it has not, so far as my exposition has yet carried us, been claimed by Newton that the earth gravitates towards the sun (or towards anything at all); nor that the moon gravitates towards the sun. Proposition V mentions, as gravitating, the circumjovial planets towards Jupiter; the circumsaturnal towards Saturn; and the circumsolar planets towards the sun. But we are not here justified in taking the earth to be a circumsolar planet. Indeed, in his formulation of Phænomenon IV—which states Kepler's third law for the planets—Newton has very carefully referred to "the periodic times of the five primary planets, and of either the sun about the earth or the earth about the sun"; and he has said nothing so far to remove that uncertainty. Of course, we might remove it for him: Newton's evidence and arguments are sufficient to license the conclusion that in the frame defined by the sun and the fixed stars the earth is subject to the same accelerative gravitational field as the traditional primary planets (and the moon of course, as "satellite"—that is, in its strict sense, *follower*—of the earth, must undergo a corresponding acceleration, approximately equal to that of the earth). Indeed, to proceed thus is what the strategy of describing the apparent motions with respect to the chosen reference frames would dictate: there is no reason in principle to treat the earth and moon here in any different fashion from (e.g.) Jupiter and its satellites. That Newton does treat it differently shows how strong his concern was not to *seem* to prejudice the cosmological question.

But curiously enough it is a little hard to see where in the course of Newton's argument this uncertainty is resolved. Resolved it must be somewhere in the remaining parts of the discussion of Proposition V (for I have not yet dealt with all the material presented as "corollary" to it) together with the discussion of Proposition VI. But (as I shall explain) the text is obscure on this point; and what makes the matter more tangled, there is a significant revision in the second edition that directly concerns this issue, but whose effect is just to alter, not to remove, the obscurity. The point is worth pursuing, not so much for its own sake, as because there is critically involved in it an aspect of Newton's argument, not yet mentioned, which has to be considered by far the most critical point in the reasoning that led him to the law of gravitation.

Let us first consider how things stand in Newton's first edition. Although there is a clause in Corollary 1 of Proposition V that remains to be discussed (and forms part of the critical aspect I have mentioned), let us for the moment pass over that and proceed to Proposition VI. This states: "That all bodies gravitate towards each planet, and that their weights towards any one planet, at equal distances from the center of the planet, are proportional to their respective quantities of matter ."

Now, that just means that the acceleration-field of gravitation towards any planet affects all bodies: for, of course, that the forces, at any given distance, are proportional to the masses of the bodies affected, is equivalent to the proposition that at such distance equal accelerations affect all the bodies concerned; and the proposition says that in fact all bodies are concerned. Although the discussion offered by Newton in support of this proposition is rather lengthy, its substance is very simple: For the weight of terrestrial bodies towards the earth, equal accelerations of all bodies in free fall is a principle that had been advanced by Galileo and widely confirmed: Newton says this equality "has been long since observed by others." He adds that he has himself conducted very careful experiments, using pendulums of different materials (taking pains to equalize air-resistance), to verify that the periods of pendulums of equal length and equal weight are independent of the material they are made of. ("I tried the thing," he tells us, "in gold, silver, lead, glass, sand, common salt, wood, water, and wheat.") Assuming the Laws of Motion (as of course Newton does), equal periods for pendulums of equal weight imply equal accelerations produced by the equal weights of those bodies, and therefore equal masses of the bodies; and he comments: "By these experiments, in bodies of the same weight, I could manifestly have discovered a difference of matter less than the thousandth part of the whole, had any such been." Thus we have strong experimental confirmation of the proportionality of terrestrial weight to mass for all bodies near the earth.

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but only oblige the whole system to change its place while the parts still retain their motions among themselves." Again here the corollary is not cited explicitly; but in the corresponding place in the demonstration of Proposition LXIV—which treats an analogous situation—that corollary is explicitly named.

One thing perhaps should be noted as odd here. The proposition that the acceleration of heavy bodies towards the earth has a definite value—independent of the nature of the body—has already played a crucial role for Newton: it was essential to the reasoning for Proposition IV. Is there something amiss, then, in the logic of his argument? In actual fact, one sees immediately that the pendulum experiments, and the inference from them to the proportionality of weight and mass, are entirely independent of the astronomical phenomena and the preceding propositions. So far at least, therefore, there is no *damaging* flaw in the reasoning, and we need not press the matter; but it is worth bearing in mind the question whether this undoubtedly infelicitous ordering of Newton's argument may be related to something about its structure as he conceives it.

As to the celestial bodies, we already know that the moon is affected towards the earth just as ordinary bodies on the earth are: its weight towards the earth is to that of any terrestrial body as are their respective masses. The satellites of Jupiter, since their motions satisfy the third law of Kepler, have accelerations that depend only upon the distance (as we have indeed already concluded explicitly in Proposition I), and therefore have weights towards Jupiter which, acting at equal distances, would produce equal accelerations—i.e., again, weights proportional to the masses of the satellites; and analogously for the primary planets and their weights towards the sun.[11] Newton then gives a special argument, based upon Proposition LXV of Book I, to infer that the weights towards the sun of a planet and its satellites must be very accurately proportional to their masses (i.e., once more, their accelerations equal at equal distances); for the corollaries of that proposition established that if this were not so, the action of the sun would noticeably disturb the motions of the satellites relative to the planet. In particular, Newton says, he has made a calculation—he does not give details—which shows that if, at equal distances from the sun, the acceleration towards the sun of a satellite of Jupiter were greater or less by a thousandth part than the acceleration of Jupiter towards the sun, then the center of the satellite's orbit would be further from or nearer to the sun than the center of Jupiter by an appreciable fraction of the diameter of the satellite's orbit around Jupiter. This is "an eccentricity of the orbit that would be very sensible. But the orbits of the satellites are concentric to Jupiter, and therefore the accelerative gravities of Jupiter and of all its satellites towards the sun are equal among themselves."

Let us pause for a moment to consider the nature of the argument that has been so far given for Proposition VI. The proposition states, as we have seen, simply that each planet is the center of a field of gravitational acceleration to which all bodies are equally subject. We already knew from Galileo and his successors that this holds, *near* the earth, for the weight towards the earth; and from Newton's Proposition IV that the moon is subject to the same field. We also knew that the sun, Jupiter, and Saturn are centers of acceleration affecting their respective satellites; and Proposition V had identified these acceleration-fields too as gravitational, and its first and second corollaries had extended to all the planets the conclusion that they are centers of gravitational force varying in each case inversely as the square of the distance. This latter statement has to be understood as referring to the "accelerative" force, for the argument is just by simple induction from the evidence already cited—evidence that directly concerns the fields of acceleration. The pendulum argument and the argument concerning the satellites of Jupiter, therefore, alike serve merely to *strengthen the evidence* for conclusions that had in effect already been drawn. And in the case of the argument concerning the satellites, in particular, what this additional evidence essentially bears upon is the assertion that the accelerative field of gravitation towards the sun affects the satellites equally with the planets they belong to; so that it lends support to the conclusion that each field of gravitational acceleration affects *all bodies without exception.*

The argument for Proposition VI ends with a paragraph devoted to another extension of the evidence for this universality of gravitational susceptibility. In this case, the extension of evidence is from the weights of *entire planets* to the weights of all their parts: Newton asks us to consider the weights towards a planet A of several others—B, C, D, etc.; and he argues in effect that unless all the parts of

[11] It will be noted that although the proposition explicitly refers to the forces of weight towards the several *planets,* its discussion establishes the corresponding statement also for weights towards the sun. I am inclined to think this indicates that Newton's use of the word 'planet' is effectively the ancient one: the planets are the "wandering stars": i.e., those objects visible in the sky whose positions relative to the constellations vary with time. On this usage, it is indeed not gravity towards the sun, but towards the earth, that the wording of the proposition overlooks. (This reading seems to be confirmed by a passage in the argument for the proposition. Having drawn his inference from the results of the pendulum experiments, Newton says: "But it is beyond doubt that the nature of gravity towards the planets is the same as towards the earth"—"*towards the planets*"; not "towards the *other* planets.")

(for instance) B were subject to equal accelerative gravity with one another and with all the parts of C, the net gravitational acceleration would be greater or less according as one or another kind of part happened to be more abundant in B or in C—so that the weights of the wholes would not, in general, be proportional to their masses.—Now, there is something a little obscure in this: it makes good sense only in so far as we have prior evidence for weights of whole planets, and not for their parts (or better prior evidence for the former than for the latter). But just this situation does hold in certain cases: e.g., the circumjovial planets all gravitate towards Jupiter, with weights proportional to their masses; the primary planets *and* their satellites (by the preceding argument) all gravitate towards the sun, with weights proportional to their masses; terrestrial bodies *and the moon*—that is, the moon *as a whole body*—gravitate correspondingly towards the earth. The argument then supports the conclusion, in each of these cases, that the weight towards the central body of its large-scale satellites is the resultant of the weights of all the parts of the latter, each part having weight proportional to its mass; and this in turn supports the induction: that *all* bodies have weight, proportional to their mass, towards *each* center of gravitational force.

What, then, of the question of the weight of the earth and moon towards the sun? Proposition VI itself certainly implies that they have such weight: the sun is a center of gravitational force, and all bodies have weight, proportional to their mass, towards each such center. Of course, one might perfectly well be content here to appeal to the induction already made from the evidence cited—this is parallel to the point already made, in the same connection, with regard to Proposition V. But once again, Newton does not rest his case on such an appeal. Or rather, he does not do so in the second and third editions; in the first, there is an ambiguity.

The crucial passage occurs just at the end of the discussion of the evidence concerning the accelerative gravities towards the sun of a planet and its satellites. At this point, in the first edition, Newton continues: "And the weights of the moon and the earth towards the sun are either none, or accurately proportional to their masses." The argument to be understood is simply that whatever accelerations towards the sun may affect the earth and the moon must be equal at equal distances (by the same argument as for Jupiter or Saturn and its satellites). If the accelerations are not both zero, they must be proportional to the masses. The matter is then left hanging: we are not told exactly how the remaining doubt is to be removed.

The ambiguity is removed—but puzzlingly—in the second edition. Here Newton adds a single short sentence, following the one I have just quoted; the two together read: "And the weights of the moon and the earth towards the sun are either none, or accurately proportional to their masses. But some they are, by corol. 1. and 3. prop. v."—How, then, do these corollaries settle the issue? I have already quoted a part, but not all, of Corollary 1 of Proposition V; I have not mentioned Corollary 3, which did not appear in the first edition. What I have quoted of the former states that there is a power of gravity tending to all the planets—that each planet is a center of gravitational force; the grounds being that these are doubtless all bodies of the same kind, and we have already concluded that there are such powers tending towards Jupiter and Saturn. What I have not yet quoted is this:

> And since all attraction (by the third law of motion) is mutual, Saturn reciprocally will gravitate towards the Huygenian Planet [that is, the satellite discovered by Huygens]. By the same argument Jupiter will gravitate towards all his satellites, the Earth towards the Moon, and the Sun towards all the primary planets."[12]

It will be recalled that Corollary 2 just affirmed the inverse square law for the forces of gravity towards the planets. As to the new third corollary, it takes up the theme of the last sentence of Corollary 1:

> All the planets do mutually gravitate toward one another, by cor. 1. and 2. And hence Jupiter and Saturn when near conjunction sensibly disturb each other's motions, by their mutual attractions; the sun disturbs the motions of the moon; and both sun and moon disturb our sea; as we shall hereafter explain.

[12] I translate from the first edition; the later ones differ somewhat in wording here (but not in substance—except for the fact that the single "Huygenian planet" now gives way to the plural: five satellites of Saturn are known to the second and third editions).

The statements about disturbances of the motions are important, and I shall return to them; but at this point, they can only be regarded as parenthetical *anticipations* of matters to be discussed later (as Newton's concluding phrase indicates explicitly).[13] The whole force of Corollary 3 is to allude to these later discussions, and to adduce them as evidence supporting the mutuality of the gravitation of the planets—which had itself been inferred already in Corollary 1.[14] Thus we are invited by Newton to conclude, somehow, from the mutuality of gravitation between the planets, to his assertion that the weights of the earth and moon towards the sun are non-null. What in the world can be the basis for such an inference?

There is no straightforward answer to this question; or rather: there is no basis for a straightforward inference to that conclusion. What we do have is this: First, Corollary 1 explicitly asserts, from the mutuality of gravitation, that the earth has weight *towards the moon* (since the latter has weight towards it); thus the earth is, at any rate, subject to *some* force of gravitation. So, of course, is the moon. We may then rest upon the proposition that bodies subject to weight towards *some* center will be subject to weight towards *every* center of gravitational force (an assumption a little weaker than the conclusion of Proposition VI itself). Second, we may after all appeal to what I have characterized as parenthetical anticipations in Corollary 3. Indeed, that corollary mentions *disturbances of the moon by the attraction of the sun* as observable (furthermore, a similar promise of discussion to come of the action of the sun on the moon has been given in the discussion of Proposition III). But of course, once this is granted, we have granted the gravitational acceleration of the moon towards the sun to be non-null; and the argument already given made this a sufficient condition for the full conclusion about the earth as well as the moon.

This is altogether a tangled argument; and Newton's words bearing on it—and the revision I have described from the first to the second edition—are surely among his less lucid, more cryptic utterances. One would like, I think, to fall back on the very much simpler and clearer line of inductive inference to the conclusion that any gravitational field affects all bodies alike. For us—who of course have believed that conclusion since rather early in our schooling—this inference is easy to accept. Obviously it was less so for Newton's contemporaries—and for Newton himself.

It happens that we actually know an argument that Newton gave on this point before he wrote Book III of the *Principia* in its present highly mathematical form at all. He tells us, in the introduction to Book III, that he had originally composed that book "in a popular method, that it might be read by many;" and we possess this original draft, which was published not long after Newton's death. The passage in question occurs in §27 of that work, under the heading *All the planets revolve around the sun*:

> From comparing the forces of the planets one with another, we have above seen that the circumsolar [force] does more than a thousand times exceed all the rest; but by the action of a force so great it is unavoidable that all bodies within, nay, and far beyond, the bounds of the planetary system must descend directly to the sun, unless by other motions they are impelled towards other parts: *nor is our earth to be excluded from the number of such bodies*; for certainly the moon is a body of the same nature with the planets, and subject to the same attractions with the other planets, seeing it is by the circumterrestrial force that it is retained in its orbit. But that the earth and moon are equally attracted towards the sun, we have above proved. (Newton 1728, pp. 570–571; emphasis added.)

Here, therefore, no appeal to mutuality and the third law of motion is implied; rather, we simply take the moon to be of the same kind as the planets, and therefore subject to the inductive inference based upon the planets. The earth is then, as it were, carried along, through the argument that the earth and moon must have equal accelerations towards the sun.

Looking back once more over such complexities as we have found in Propositions V and VI, I want to emphasize how—in one sense—relatively marginal these are. One part of Newton's audience, and cer-

[13] Here Motte is misleading: he breaks Newton's single sentence (with clauses separated by mere commas where I have used semicolons) into two sentences; and so punctuates as to attach the closing phrase—"as we shall hereafter explain"—solely to the clause about the tides.

[14] It may be remarked, therefore, that Corollary 3 is not in fact a corollary of Proposition V in any ordinary sense of the word. In some other cases, too, the corollaries are not inferences drawn from the proposition to which they are attached, but rather supplementations of its content. Corollary 3, in contrast, states no new conclusion whatever, but supplements the *evidence* for its proposition (and does so, at that, only with a hand-wave forwards).

tainly the chief part of those who were prepared to take his work seriously at all, was already Copernican by conviction. For all such, the conclusion that the earth has weight towards the sun would pose no difficulty: after Proposition IV, Propositions V and VI would be unproblematic inductive generalizations. And as for Newton's own deeper and more systematic concern with the issue of inferring the "true" from the "apparent" motions, the fundamental position I have already described is surely the most cogent: if I could rewrite this portion of the *Principia,* I would relegate the arguments we have been considering to a scholium, and rest the main discussion on the inference, genuinely from the phenomena, that *relative to the sun and fixed stars,* there is a field of force affecting the primary planets, their satellites, the earth and moon and all bodies *on* the earth, and indeed presumably (by induction) all bodies whatever, by virtue of which they all have weights towards the sun, proportional to their masses and inversely proportional to the squares of their distances from it. This conclusion—and the analogous ones for each planet: namely, that relative to the planet and the fixed stars there is a field of similar character about the planet, likewise affecting all bodies—was, as I have already remarked about Proposition IV, accepted by such judges as Huygens and Leibniz and applauded by them as a great discovery.

But now we are on the threshold of the chief step. Proposition VII states the law of universal gravitation: *That there is gravity towards all bodies*—gravitatem in corpora universa—*proportional to the quantity of matter in each.* The argument for this proposition is based entirely upon the conclusions already drawn; and the argument is very short. Newton's exposition refers back to Proposition LXIX of Book I; let me first state and prove a somewhat more general proposition (greater generality here makes both the content and the proof simpler),[15] and then quote Newton's argument for Proposition VII in full.

The preliminary proposition is this: Suppose we have a system of bodies—A, B, C, etc.—in which A attracts all the others with accelerative forces depending only upon the distance, and B does likewise (in particular, then, each of the bodies A and B is subject to the accelerative force that tends towards the other); then (and I here quote Newton's words) *the absolute forces of the attracting bodies A, B will be to each other, as are those bodies themselves A, B, whose forces they are.*

In confronting this proposition, we are faced with the problem I mentioned earlier: we have been given no quantitative definition of "absolute [measure of a] force." But let d be any distance, and consider the effects of A and B respectively on any third body C placed at that distance from each of them in turn. The accelerative force depends only upon the distance. Therefore, the acceleration of C towards A at distance d will be the same as the acceleration that B would have towards A at that distance. By the same token, the acceleration of C towards B at distance d will be the same as the acceleration that A would have towards B at that distance. Now conceive A and B to be placed at the distance d from one another. Then, Newton argues—by the third law of motion—the motive forces on the two bodies, A and B, each towards the other, must be equal; and it follows that the acceleration of B towards A is to that of A towards B as the mass of A is to the mass of B. Therefore the ratio of the mass of A to that of B is the same as the ratio of the accelerations towards A and B respectively of *any* body C, at *any* equal distances from the two. This is therefore clearly the right measure of the strength of A and of B as centers of force—i.e., of their "absolute forces."

Newton adds as a corollary that if (not only the first two listed, but) each of the bodies of the system attracts all the rest, with the corresponding condition (in effect: *well-defined fields of accelerative force*), then the absolute forces of all the bodies will be proportional to their masses.

With this prefaced, here is Newton's proof of Proposition VII of Book III:

> That all the planets mutually gravitate towards one another I have proved before, as also that gravity towards each one of them considered separately is inversely as the square of the distance of places from the center of the planet. And thence it follows (by prop. LXIX bk. I.) that gravity towards them all is proportional to the quantity of matter in them.
>
> Further, since all the parts of any planet A gravitate towards any planet B, and the gravity of any part is to the gravity of the whole as the matter of the part is to the matter of the whole, and to every action there is (by the third law of motion) an equal reaction; the planet B will in turn gravitate towards every part of the planet A, and its gravity towards

[15]The proposition I give here is in fact stated by Newton as Corollary 2 of his theorem: he remarks that this more general theorem follows "by a like reasoning."

any one part will be to its gravity towards the whole, as the matter of the part to the matter of the whole. *Q. E. D.*

The proof, then, is very simple indeed; and yet it takes us directly to universal gravitation. What is the engine of this enormous step?

The answer is that this proof has two crucial premises. The first is that gravity—weight—is a force whose nature is to act, independently, upon all the parts of a body. This is a point that has already played a role in the argument for Proposition VI; and it had been memorably invoked by Galileo (1638, pp. 67–68) to dissolve the apparent paradox that increasing the weight of a falling object— e.g., by putting one brick on top of another—does not increase its rate of fall. The other crucial premise is the third law of motion. Or, rather: it is an *application* of the third law of motion *in a very special form.*

Is there, then, some leeway in the application of the third law—so that the latter allows of being applied in alternative ways? The answer, in the present case, is that there indeed are alternatives. Back, for instance, in Corollary 3 to Proposition V, Newton, having concluded in the proposition that Jupiter's satellites gravitate towards Jupiter, argues thus: "And since all attraction (by the third law of motion) is mutual ... Jupiter will gravitate towards all his satellites." But *does* this follow? What has been established about the satellites of Jupiter is that they are subject to forces directed towards Jupiter. But the third law of motion does not tell us that whenever one body is urged by a force directed towards a second, that second body experiences an equal force towards the first—it tells us, rather, that whenever one body is acted upon *by* a second, the second body is subject to a force equal in magnitude and opposite in direction. Therefore—putting the point in proper generality—what we may legitimately conclude, from the proposition that each planet is a center of gravitational force acting upon all bodies, is that for each body B there must be some body (or system of bodies) B' which, exerting this force on B, is subject to the required equal and opposite reaction. Furthermore, it must not be thought that the leeway implied by this formulation is one merely of far-fetched possibilities—that the only plausible subject of the reaction to gravitational force towards a planet is the planet itself. On the contrary, the very widespread view of Newton's time that one body can act upon another only by contact—a view that is well known to have had a powerful influence on Newton himself—makes for precisely the opposite assessment: that it is far-fetched to apply the third law in the fashion Newton does.[16]

Here, then, in short, is the shape of the whole main argument, from Proposition IV through Proposition VII: We have identified the force on the moon with its weight to the earth. Of weight, we have reason to believe that it acts on all bodies, and independently on all the parts of a body: the weight of the whole is simply the sum or resultant of the weights of the parts. Of the force on the moon, we have reason to believe that it varies inversely with the square of the distance from the earth. The latter holds as well for the various acceleration-fields about the astronomical bodies that have satellites; these too, then, ought to be considered to be fields of weight—gravity—acting, like weight to the earth, on all bodies. Thus we have a handful of centers of gravitational force, in each case producing accelerations determined by the inverse square law of variation with distance; in particular, accelerations the same for all bodies at any place; producing, therefore, *weights—motive* forces—proportional to the masses of the bodies acted upon. *We now make a very far-reaching hypothesis*: noting that in each case, there is a central *body* towards which the force of gravity tends, we ask, in effect, *What will be the consequence of assuming that the reaction called for by the third law is exercised upon that central body?*

What in fact follows directly is that each central body experiences, towards every particle of matter whatsoever, a force proportional in each case to the mass of the particle and inversely proportional to the square of the distance from the particle. But Newton infers more: he concludes that each of these

[16] The point was raised in acute form by Cotes in the course of his editorial work on the second edition of the *Principia.* Writing to Newton on March 18, 1712/13, when printing of the work was almost complete, he suggests that a problem exists that might call for an Addendum to be printed with the Errata Table; and adds: "For 'till this Objection be cleared I would not undertake to answer any one who should assert You do *Hypothesim fingere* I think You seem tacitly to make this Supposition that the Attractive force resides in the Central Body." (Newton 1975, p. 392.) Unfortunately, Newton's reply was not responsive to the real force of the objection (ibid., pp. 396–397); and Cotes did not pursue the matter.

The editors of Newton's correspondence, in a note to Cotes's letter, endorse Newton's position: they say—ibid., p. 393, n. 5—"Cotes seems naively to have misunderstood the implications of the third law here"; in fact, quite the opposite is the case.

For some further discussion of this matter, with particular reference to Huygens's objections to Newton's theory Newton's reply to those objections, see (Stein 1967, pp. 179–180; 1970, pp. 263–264).

forces on the central body really acts *independently upon each part of the central body,* and *proportionally to the mass of each part.* This consequence is not stated explicitly in Newton's argument. It is implicit, however, in his designation of the reaction-force as itself *a force of gravity*: of *weight*; and it is explicit in what he mentions as an objection that could be made to Proposition VII: namely, "that according to this law, all bodies about us must mutually gravitate one towards another." We must therefore still consider what justifies this step—the classing of the reaction-force as a kind of weight, with, therefore, the characteristic properties already assigned to weight.

On this point, there is a most illuminating passage—illuminating not for this point alone, but for Newton's conception of the forces of nature in general—in the first version of what is now Book III of the *Principia,* composed "in a popular method." In §20 of that book we have the remark: "[A]ll action is mutual, and (. . . by the third Law of Motion) makes the bodies approach one to the other, *and therefore must be the same in both bodies.*" (Newton 1728, p. 568; emphasis added.) He continues:

> It is true that we may consider one body as attracting, another as attracted; but this distinction is more mathematical than natural. The attraction really resides in each body towards the other, and is therefore of the same kind in both.

The ensuing §21 is then devoted entirely to the elaboration of this theme: the *unity* of the process involved in "action and reaction" (of *any* sort). The discussion is a full page long; and this is of interest, as showing how important it was to Newton to make his point clearly; but considerations of time and space prevent me from quoting it here in full, so I give only the central passage:

> It is not one action by which the sun attracts Jupiter, and another by which Jupiter attracts the sun; but it is one action by which the sun and Jupiter mutually endeavor to approach each the other. By the action with which the sun attracts Jupiter, Jupiter and the sun endeavor to come nearer together . . . ; and by the action with which Jupiter attracts the sun, likewise,[17] Jupiter and the sun endeavor to come nearer together. But the sun is not attracted towards Jupiter by a twofold action, nor Jupiter by a twofold action towards the sun; but it is one single intermediate action, by which both approach nearer together. [Earlier in the passage, Newton has analogized this "one single intermediate action" to the contraction of a cord that is stretched between two bodies.]

We can now see clearly the answer to our question: what the engine is that drives the enormous step to the law of universal gravitation. It is a certain conception of the character of a force of nature, or natural power, such as gravitation: namely, that a force of nature is *a force of interaction*; and that such a force is characterized by a *law of interaction*: a law in which the interacting bodies enter altogether symmetrically. It is worth remarking that the conception of a force as characterized by a law is just what we really needed as far back as our discussion of Proposition IV: we can see the assertion that the moon's acceleration and that of falling bodies are *due to the same cause,* precisely as the assertion that these two classes of phenomena are *governed by the same law of nature.*[18] With this interpretation of what the third law of motion requires—combined with what I have called the far-reaching hypothesis that gravity *is* such an interaction *between* the heavy body and the central body towards which it has weight—Newton's short and simple argument for Proposition VII leads directly to universal gravitation.

That, then, is how we have managed to get to fairy land. What are we to say—what could Newton have to say—in response to the strictures of Hume, applied to this chain of reasoning, and the reliability of our arguments in this realm?

[17] Cajori's text is here defective, and I have taken the liberty of emending it despite the fact that I have not had the opportunity of checking the original. Cajori has a full stop and a new sentence after the word 'sun', where I have substituted a comma (and accordingly changed the initial letter of 'likewise' to lower case, and added a balancing comma after that word).

[18] The conception of "forces of nature" as essentially characterized by *laws* of nature is made explicit by Newton in the last Query of the *Opticks* (this query first appeared in the Latin edition of 1706): he there describes the *Vis inertiæ* as "a passive Principle" characterized by a law—namely, by the conjunction of the three Laws of Motion (Newton 1952, p. 397); and declares, concerning the moving "active Principles" (gravity, for example; in general, we may say, the principles by virtue of which forces are "impressed" on bodies), that he regards such principles "not as occult Qualities, . . . but as general Laws of Nature" (ibid., p. 401).

I said at the outset that, in my opinion, what Newton calls the "deduction from the phenomena" of the law of universal gravitation is not properly said to be complete when Proposition VII has been formulated and the argument for it has been given. To discuss the issue with adequate reference to Newton's own statements about his view of proper method in natural philosophy would exceed the bounds placed on the present paper; I must therefore rather baldly state my own interpretation of Newton's view, with only one or two citations in support.[19]

Baldly, then: In Newton's terminology, three terms describing kinds of argument are used in sharply distinguished fashion: namely, *demonstration, deduction,* and *proof.* The first of these is Newton's characteristic term for *purely mathematical reasoning.* The second—"deduction"—is used by him in a quite wide sense, for reasoning *competent to establish a conclusion as warranted* (in general, on the basis of *available evidence.* As for "proof," Newton typically means by it *the subjection of a proposition to test by experiment or observation* (with a successful outcome). Besides these, Newton uses the words "gather" or "collect"—themselves semantically equivalent, corresponding to the Latin *colligere*—for any sort of inference, whether conclusive or merely tentative. In these terms, when a proposition has been "gathered" from evidence, it may or may not have thereby received adequate warrant—and so qualify as "deduced." If not, then the desideratum is to subject the proposition to further "proof," in the hope of achieving either a "deduction" or a refutation. Of course, *when* such proof is sufficient to provide adequate warrant, and so constitute the grounds for a proper deduction, remains a very difficult question indeed; but it is clear that Newton is not Popperian: he believes that this difficult question can be answered in practice, even in the absence of a general principle for deciding it. On the other hand, although a proposition may qualify as deduced from the phenomena, the issue may always be reopened by the discovery of new evidence from phenomena—this is explicit in Rule IV of the Rules of Philosophizing; and in this sense, the process of "proof" is in principle unending.

To know, therefore, whether the law of gravitation has been, properly, deduced from the phenomena when the argument for Proposition VII has been stated, we must ask whether that proposition has at that point been given adequate warrant. My own account suggests a negative answer. But what of Newton's procedure—what does it suggest he thought about this? Although I do not think one can feel entirely confident about Newton's actual state of mind, especially in view of the fact that this would be for him a charged issue, I believe it quite possible to arrive at a *definitive* judgment about the position that his procedure *objectively* entails. There are three main relevant points.

In the first place, it is essential to recognize that Proposition VII implies a vast range of consequences not implied by the propositions antecedent to it—and, in part, *contradictory* of the statements of "Phænomena" on which the initial reasoning of Book III was based. That Newton understood this cannot possibly be called in question: the entire remainder of Book III of the *Principia* is devoted to the derivation of such consequences, *and to their confrontation,* so far as it was possible at the time, *with actual phenomena.* In short, in the formal terms I have suggested above as characteristic of Newton's usage, the remainder of Book III can be seen as devoted to the "proof by phenomena" of the law of gravitation;[20] and furthermore, the proofs so obtained, in so far as they involve in part new (and confirmed) astronomical discoveries, and a great increase in both the scope and the precision of astronomical prediction, provide a kind of warrant for that law which can quite reasonably be seen as drawing the

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[19] One cautionary remark is in order concerning the reading of Newton in translation, on these points. In the interpretation I offer, certain logical/methodological terms are of particular importance, as carrying, for Newton, rather precise meanings. Among these are "demonstrate", "deduce," and "prove" (with the nouns derived from them). But in translations from the Latin, the corresponding words—*demonstrare, deducere, probare*—are by no means uniformly rendered by their precise English equivalents; and, conversely, the latter are sometimes used to translate other terms than their cognates. For instance, in the introduction to Book III of the *Principia,* Motte represents Newton as saying: "It remains ... that I now demonstrate the frame of the System of the World." Believing that Newton (with just one exception known to me—and that an interesting and explicable one) regularly uses the verb "demonstrate" for arguments that carry *full mathematical certainty,* and that reasoning in natural philosophy is, by him, carefully distinguished from this, I was taken aback by this sentence. But the Latin text, as it turns out, does not speak of "demonstration" at all: Newton says, "Superest ut ... *doceamus* consititutionem Systematis Mundani"—"It remains ... that we *teach* [or "expound"] the System of the World" (emphasis added; orthography as in the first edition). Again, in the scholium to the Laws of Motion Motte has Newton say, speaking of the third law: "In attractions, I briefly demonstrate the thing after this manner"; but Newton's Latin reads, not *demonstro,* but *ostendo.*

[20] To avoid a possible misunderstanding: Newton does not characterize the rest of Book III in this way (if he did, I should be far less uncertain of his own subjective assessment of the state of the case); I say only that this characterization is in full accord with Newton's verbal usage and scientific practice.

sting from the charge of "wild hypothesis" that could otherwise be levelled at Newton's way of applying the third law of motion.

In the second place (less important in principle, but perhaps more nearly decisive for the question of how Newton himself saw the structure of his own "deduction from the phenomena"), we should recall that the crucial argument for the moon—the argument that the force on it towards the earth varies inversely with the square of its distance from the earth, a conclusion without whose help the identification of the astronomical force with that of gravity could never have been reached—had a slightly shaky relation to the data; and that Newton, in his own formulation of that argument, said of the discrepancy: "This in fact arises from the action of the sun (as will be shown later), and is therefore to be neglected." But *that* makes the argument leading from the "Phænomena" through Proposition IV to Proposition VII itself *formally and explicitly dependent* upon consequences to be drawn from Proposition VII for its own proper completion.

In the third place, we do have at least two statements made later by Newton that bear directly upon the question of where he saw the main weight of evidence for the law of gravitation to lie. These agree with one another in resting the case for that law, not on the chain of arguments leading to Proposition VII, but on the fact that by means of it he has accounted for the behavior of the planets, the comets, the moon, and the sea.[21] Moreover, one of these statements is now to be found in the text of the *Principia* itself, in the third Rule of Philosophizing (which was added in the second edition—that prepared by Cotes). The passage reads as follows:

> Lastly, if it universally holds by experiments and astronomical observations that all bodies about the earth gravitate towards the earth, and that in proportion to the quantity of matter in each, and the moon gravitates towards the earth in proportion to her quantity of matter, and our sea in turn gravitates towards the moon, and all the planets gravitate mutually towards one another, and the comets in like manner gravitate towards the sun: it is to be asserted, by this rule [i.e., Rule III itself], that all bodies gravitate mutually towards one another. For the argument from the phenomena will be even stronger for universal gravitation, than for the impenetrability of bodies: for which among the heavenly bodies in particular we have no experiment, no observation whatever.

Since this rule was added expressly to clarify, and strengthen the force of, the argument for universal gravitation, it surely carries great authority in respect of Newton's late, considered view. And I think it clear that what in the text I have just quoted is called "the argument from the phenomena"—*argumentum ex phænomenis*—for universal gravitation is indeed what I have claimed it to be: the entire third book of the *Principia*.

Something remains to be said about this whole "deduction from the phenomena"—or perhaps two closely related things. It may be asked, first: What has become of the question of the "true motions"—the program, to "collect" these from their causes, effects, and apparent differences? The answer is that this, too, is carried out in Book III. The procedure is simple. From the separate pieces of the puzzle—the conclusions about the forces acting in the several satellite systems, *relative* in each case to *the appropriate frame of reference*—we have been led (on the basis also, as I have described it, of a daring speculative move) to a general law respecting a force of nature. We then consider a system of bodies let loose, in Newton's terminology, in "absolute space," and we ask how these bodies will behave *under the sole influence of this one natural power.* The answer is that if these bodies have the particular characteristics of the earth, moon, sun, planets, and comets, their behavior will be such as to produce just the phenomena we observe—that is the "argument from the phenomena" just characterized; and it leads to the conclusion, not only that the law of universal gravitation is to be affirmed, but that the force of gravity is the only one of significance in governing the observed behavior of those bodies. And this in turn settles the question of the "true motions," so far as that is at all possible in Newtonian physics: it determines those motions up to a common uniform translation of the entire system. The appropriate conclusion is drawn by Newton in his argument for Proposition XI of Book III; its further, and rather spectacular, "proof" can be seen in the later discussion—in Propositions XXI and XXXIX—of the precession of the equinoxes (cf. Stein 1967, p 182; 1970, p. 266).

[21] I quote one of the statements immediately below; for the other, see (Stein 1967, p. 180; 1970, p. 263)—cf. n. 16 above.