"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 *potentiae naturales*, or *vires naturae*—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

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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. 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 to the Definitions—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. Since 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 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 *Phaenomena* in the introductory material to Book III. 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. In particular, then, Newton's statements of the *Phaenomena* 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 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. To be sure, when one looks closely at the details, there are certain liberties in what I have called the "translation." I shall have occasion to refer later to the fact that the moon departs appreciably from Keplerian motion—a point that Newton not only acknowledges, but even accords a certain emphasis; he promises that the discrepancy will be shown to be due to the sun's action on the moon.

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 calcula-

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tion, 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 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 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 now customary in what we call Newtonian mechanics; a way resembling, rather, that in which physicists today use the word when they speak of the "four fundamental forces." The first force Newton actually defines is *materiae vis insita*, the "intrinsic force of matter," otherwise its vis *inertiae*, 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; 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 *qua* acting on the body.) A second measure, the "accelerative measure" or "accelerative force," is just the acceleration engendered by the force; 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. And 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"; 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, as measure of the field intensity, that quantity which happens to be appropriate to the force he intends to deal with in Book III.

But this does suggest an answer to our problem about the sense of the identification of the force on the moon with its weight: Newton draws the conclusion 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.

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 actually contains an extremely far-reaching physical implication that is not contained in the data, and not extrapolated in any straightforward way from the data; an implication that was hardly dreamt of before Newton published the Principia, and that was received with astonishment—and with assent—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 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 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.

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, 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."

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 *Phaenomena*, 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, 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*; and the total (true) accelerative force on any body *B* at any moment will be the sum of the accelerative force of weight towards *A*, the resultant acceleration produced by any other forces that act on *B* relative to **F**, and the absolute acceleration of the point (fixed relative to **F**) at which *B* is located at that moment. Thus we have, at this point of the argument, a picture of several distinct 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 representation 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. For the distances of any planet and its satellites from the sun are so large, in comparison to 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); or 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 Phaenomenon 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.

This issue—not only, that is, of the earth's motion, but of its standing subject to gravitation towards the sun, or subject to gravitation at all—turns out on close reading to present a minor crux in the interpretation of Newton's text. He seems to delay committing himself on the issue as long as he can; and the precise point at which the commitment is made, and the argument on which it there rests, is strangely hard to determine. There is a difference, small and puzzling, between the first and second editions of the *Principia* in their treatment of the matter; and a further quite interesting difference between both of them and the still earlier version of that book (Newton 1728), composed, as Newton tells us, "in a popular [rather than mathematical] method, that it might be read by many" (Newton 1687, introduction to Book III). But a discussion of these intricacies would be too long for the present occasion; and substantively they are of secondary importance. I shall confine myself to one or two points related to Newton's special arguments, but chiefly shall suggest how the knot can be cut.

First, however, one step that turns out to be relevant for his own approach to this question is taken by Newton in a portion of Corollary 1 of Proposition V that I have not yet quoted; and in this he introduces a theme that we shall find of pivotal importance for the reasoning that leads to the law of gravitation. Corollary 1, it will be recalled, has stated that there is a power of gravity tending to all the planets—on the grounds that these are doubtless all bodies of the same kind, and that we have already inferred the existence of 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.

Here, therefore, the assertion is made (on the basis of the third law) that the earth is subject to gravitation *at least towards the moon*; and a part of Newton's later argumentative strategy is to exploit this as a basis for the claim that the earth is the *kind* of body that is subject to gravity—and therefore should be supposed to gravitate towards the sun.

In the second edition of the Principia, there is a new corollary to Proposition V, which takes up the theme of mutual gravitation introduced by Corollary 1. This reads:

> All the planets do mutually gravitate towards 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.

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). 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.

Proposition VI 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 quantities of matter.* Of course this just says that gravity towards any planet is characterized by a field of "accelerative force," everywhere well-defined as a function of place alone, that affects all bodies without exception: that the weights are proportional to the masses precisely *means* that the "accelerative measures" are the same for all. The discussion offered by Newton in support of this proposition is somewhat lengthy, but its substance is very simple. It is tempting merely to say that we already know this proposition: for it is precisely the earth's acceleration-field, extending to all terrestrial bodies and to the moon, that has served to identify the force on the moon with its weight; and it is the character of their acceleration-fields, agreeing with that of the earth, that has led us to identify the sun, Jupiter, and Saturn—and then by induction the other primary planets—as gravitational centers. What Newton actually does in his proof of Proposition VI is, first, to cite additional and more precise evidence for the proposition that gravitational acceleration is the same for all terrestrial bodies; and then to argue, from the astronomical phenomena, that the *satellites* must have the same accelerations towards the sun, at equal distances, as the planets to which they belong. Finally, he offers a special argument that each *part* of a planet must tend to gravitate with the same acceleration at the same distance, on the grounds that otherwise the whole planet would gravitate more or less according as one or another kind of part predominates in its composition. All this serves, then, not as initial grounds for accepting Proposition VI—as I have said, its content had already been used in the earlier argument—but to provide strengthened support for it.

As to the question of the earth's gravitation towards the sun, Newton suggests the following argument: First, as for any other satellite and its central body, the moon and the earth must either both gravitate, or neither. But the moon is undoubtedly a body of the same kind as the planets; so it should be presumed to gravitate as they all do; and the earth is therefore carried along (not, of course, carried along by the moon, but by the argument).

We are now 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 it 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, 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 consider the bodies *A* and *B*, at any given distance *d* apart. According to the third law of motion, Newton argues, the motive forces on *A* and *B* will be equal; therefore the acceleration of *B* towards *A* will be to that of *A* towards *B* as the mass of *A* to the mass of *B*. But acceleration towards *A* at distance *d* is the same for all bodies; and likewise that towards *B*. So we have, quite simply:

> The accelerative force towards *A* at any distance is to that towards *B* at the same distance, as the mass of *A* to the mass of *B*.

Clearly, then, mass serves as the appropriate measure of the efficacy of whatever cause makes the body a center of force.

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

The answer is that the proof employs two crucial premises. The first is that weight is a force which acts independently upon all the parts of a body. This is a point that has already played a role in the argument for Proposition VI; it had been invoked by Galileo (1638, pp. 67-68) to resolve 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, after concluding that Jupiter's satellites gravitate towards Jupiter, argued 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. The third law of motion does not tell us that whenever one body is urged by a force directed towards a second, the 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 of equal magnitude and opposite 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. 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 way Newton does.

Here, then, 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 given 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,

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What will be the consequence of *assuming* that the reactions called for by the third law are *exercised upon those central bodies*?

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 forces on each central body really acts *independently upon each part of the central body, proportionally to the mass of the part;* and then, further—on the grounds that any particle of matter whatever can be a part of a planet (e.g. just by falling on it)—that the force acts between all pairs of particles *simpliciter*. 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 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 work 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, 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.] (Newton 1728, p. 569.)

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. 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?

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.

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"—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 no 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, then, 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 state of mind, 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 "Phaenomena" 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

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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; 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 that can quite reasonably be seen as drawing the 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 had a slightly shaky relation to the data. 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 Phaenomena 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. I shall here cite only one of them; it occurs in the *Principia* itself, in the third Rule of Philosophizing (which was added in the second edition). 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 as an indication of Newton's late, considered view. I think it clear that what in the text I have just quoted is called "the argument from the phenomena"—*argumentum ex phaenomenis*—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

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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 significantly affecting the observed behavior of those bodies. But 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 motion 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.

This means that the deduction from the phenomena in Book III can be regarded as not *only* a deduction of the law of universal gravitation, but *also* a deduction—or at any rate a contribution of evidence; a "proof" in Newton's sense—of a major metaphysical element of Newton's science: his theory of space and time. But one can say more than this. For clearly, in so far as the "deduction" validates what I have called Newton's speculative application of the third law of motion, it also contributes evidence for the cogency of the general conception of the natural powers that lies behind that application: that is, as I would put it, it "proves," besides the metaphysics of space and time, the *general metaphysics of nature* expressed in the introductory sections of the Principia and in the preface to the first edition. I believe that this whole conception of the constitutional frame of nature was actually developed by Newton *at the same time* that he was discovering the law of gravitation. In other words, as I see the situation, not only the "proof," but the discovery itself, of the background theory that made possible Newton's reasoning from the phenomena to the force of gravitation, occurred simultaneously and marched hand-in-hand with the latter.

Note

[1]Because of limitations of space, it has been necessary to abbreviate this paper and to omit all discursive notes and all but the most essential references; a fuller version will appear elsewhere.

References to Newton (1687) occur in the text *passim*. They are always obvious (since embedded in discussion of the work itself); and the source loci are always indicated by such structural references as Book and Proposition number. It has therefore been thought superfluous to flag them by author, date, and page.

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References

Galileo (1638), *Discorsi e Dimostrazioni Matematiche intorno à due nuove scienze*. Cited from Galileo Galilei, *Two New Sciences*, trans. S. Drake. Madison, Wis.: University of Wisconsin Press, 1974.

Hume, D. (1777), *An Enquiry Concerning Human Understanding*. Cited from David Hume, *The Philosophical Works* (ed.), T.H. Green and T.H. Grose, vol. IV. London, 1882 (reprinted Darmstadt: Scientia Verlag Aalen, 1967).

Newton, I. (1687), *Philosophiae naturalis Principia Mathematica*. Cited from A. Koyré and I.B. Cohen (eds.), *Isaac Newton's Philosophiae Naturalis Principia Mathematica, the Third Edition (1726) with Variant Readings*. Cambridge, Mass.: Harvard University Press, 1972. (This edition records all significant differences among the first, second, and third editions—of 1687, 1713, and 1726, respectively.) In translating passages from this work, I have been guided by the English translation of Andrew Motte (1729; reprinted London: Dawsons of Pall Mall, 1968).

______. (1728), *De Mundi Systemate*. Cited from the English translation (presumably by Motte, revised by Cajori) in F. Cajori (ed.), *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and his System of the World*. Berkeley, California: University of California Press, 1946.

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