G. E. Smith, “The Methodology of the *Principia*,” in I. B. Cohen and G. E. Smith, eds., *The Cambridge Companion to Newton* (Cambridge: Cambridge University Press, 2002), 138-73.

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4 The methodology of the *Principia*

In the Preface to the first edition (1687) Newton informs the reader straight off that he intends the *Principia* to illustrate a new way of doing what we now call empirical science:

> And therefore our present work sets forth mathematical principles of natural philosophy. For the whole difficulty of philosophy seems to be to find the forces of nature from the phenomena of motions and then to demonstrate the other phenomena from these forces. It is to these ends that the general propositions in Books 1 and 2 are directed, while in Book 3 our explanation of the system of the universe illustrates these propositions ... If only we could derive the other phenomena of nature from mechanical principles by the same kind of reasoning! For many things lead me to have a suspicion that all phenomena may depend on certain forces by which the particles of bodies, by causes yet unknown, either are impelled toward one another and cohere in regular figures, or are repelled from one another and recede. Since these forces are unknown, philosophers have hitherto made trial of nature in vain. But I hope that the principles set down here will shed some light on either this mode of philosophizing or some truer one.[1]

Surprisingly, however, the main body of the first edition contains only two further comments about methodology: (1) a cryptic remark at the end of the opening discussion of space and time, announcing that the purpose of the work is to explain "how to determine the true motions from their causes, effects, and apparent differences, and, conversely, how to determine from motions, whether true or apparent, their causes and effects";[2] and (2) a scholium buried at the end of Book 1, Section 11 in which Newton proposes that his distinctive approach will make it possible to *argue more securely* in natural philosophy.

In the second edition (1713), clearly in response to complaints about his methodology, Newton introduces separate sections for the Phenomena and Rules for Natural Philosophy[3] involved in his derivation of universal gravity (adding a fourth rule in the third edition, 1726), and he adds at the end the General Scholium containing his most famous – and troubling – methodological pronouncement:

> I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypotheses. For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction.[4]

In a later (anonymous) work, Newton softened his renunciation of hypotheses by adding, "unless as conjectures or questions proposed to be examined by experiments."[5]

With or without this qualification, the thrust of the pronouncement remains mostly negative: Newton's new *experimental philosophy* does not proceed hypothetico-deductively, even under the supposedly safe constraint imposed by the then-prevailing *mechanical philosophy* that all hypothesized action arises strictly through contact of matter with matter. How, then, does theory construction proceed on Newton's approach? Vague talk of "deductions from phenomena" provided no more adequate an answer to this question then than it does now.

Newton leaves the task of extracting the answer from the *Principia* largely to the reader. Three centuries of disagreement give reason to think that the answer is far more complex than the hypothetico-deductive alternative, which Christiaan Huygens, the foremost figure in science at the time, managed to lay out in a single paragraph in his January 1690 Preface to his *Treatise on Light*, published thirty months after the *Principia*:

> One finds in this subject a kind of demonstration which does not carry with it so high a degree of certainty as that employed in geometry; and which differs distinctly from the method employed by geometers in that they prove their propositions by well-established and incontrovertible principles, while here principles are tested by the inferences which are derivable from them. The nature of the subject permits of no other treatment. It is possible, however, in

this way to establish a probability which is little short of certainty. This is the case when the consequences of the assumed principles are in perfect accord with the observed phenomena, and especially when these verifications are numerous; but above all when one employs the hypothesis to predict new phenomena and finds his expectations realized.[6]

Huygens's *Discourse on the Cause of Gravity*, which contains his critical evaluation of the *Principia*, was published in combination with his *Treatise on Light*, making this paragraph prefatory to both.

The nearest Newton ever comes to such a capsule summary of his approach is the one methodological pronouncement from the first edition from which I have yet to quote, the Scholium at the end of Book 1, Section 11:

By these propositions we are directed to the analogy between centripetal forces and the central bodies toward which those forces tend. For it is reasonable that forces directed toward bodies depend on the nature and the quantity of matter of such bodies, as happens in the case of magnetic bodies. And whenever cases of this sort occur, the attractions of the bodies must be reckoned by assigning proper forces to their individual particles and then taking the sums of these forces.

I use the word "attraction" here in a general sense for any endeavor whatever of bodies to approach one another, whether that endeavor occurs as a result of the action of the bodies either drawn toward one another or acting on one another by means of spirits emitted or whether it arises from the action of ether or of air or of any medium whatsoever – whether corporeal or incorporeal – in any way impelling toward one another the bodies floating therein. I use the word "impulse" in the same general sense, considering in this treatise not the species of forces and their physical qualities but their quantities and mathematical proportions, as I have explained in the definitions.

Mathematics requires an investigation of those quantities of forces and their proportions that follow from any conditions that may be supposed. Then, coming down to physics, these proportions must be compared with the phenomena, so that it may be found out which conditions of forces apply to each kind of attracting bodies. And then, finally, it will be possible to argue more securely concerning the physical species, physical causes, and physical proportions of these forces. Let us see, therefore, what the forces are by which spherical bodies, consisting of particles that attract in the way already set forth, must act upon one another, and what sorts of motions result from such forces.[7]

The goal in what follows is to describe the methodology of the *Principia* in the light of this too often neglected Scholium.[8]

First, however, the Scholium (which remained word-for-word the same in all three editions) should be put into context. Section 11 treats bodies moving under centripetal forces directed not toward a point in space, as in the preceding sections, but toward other moving bodies – so-called "two-body" and "three-body" problems. By far the largest portion of Section 11 presents Newton's limited, qualitative results for three-body effects on the motions of the planets and the Moon, results that he called "imperfect" in the Preface. The Scholium thus occurs just after it should have become clear to readers that the true orbital motions are so intractably complex as to preclude hope of exact agreement between theory and observation. To concede that theory can at best only approximate the real world, however, appears to concede that multiple conflicting theories can claim equal support from the available evidence at any time. Seventeenth-century readers would have been quick to note this, for equipollence of astronomical theories had been a celebrated concern for over a century,[9] and such leading figures as Descartes and Marin Mersenne had frequently called pointed attention to the limitations of experimental evidence.[10] Newton would have accordingly expected his readers to see his remark about *arguing more securely* as making a startling claim in the face of a concession that the real world is intractably complex.

Proposition 69, to which the Scholium is attached, lays the groundwork for Newton's law of gravity by asserting that in the relevant inverse-square case the forces directed toward the various bodies must be proportional to the masses of those bodies. Sections 12 and 13 examine the characteristics of forces directed toward bodies when these forces are composed out of forces directed toward the individual particles of matter making up the bodies. In other words, they lay the groundwork for Newton's claim that his law of gravity holds *universally* between individual particles of matter. Now, the mechanical philosophy did not bar "attractive" forces among macroscopic bodies, for intervening unseen matter could be hypothesized to effect these forces in the manner Descartes had proposed in the case of magnets, and also gravity.[11] As Newton well realized, however, no hypothetical contact mechanism seems even imaginable to effect "attractive" forces among particles of matter generally. The Scholium thus occurs at the point where adherents to the mechanical philosophy would start viewing Newton's reasoning as "absurd" (to use the word Huygens chose privately).[12] The Scholium attempts to carry the reader past this worry, but not by facing the demand for a contact mechanism head-on. Instead, Newton warns that he is employing mathematically formulated theory in physics in a new way, with forces treated abstractly, independently of mechanism. What we need to do first, then, is to understand how Newton is using mathematical theory and talk of forces in the *Principia*, and how he is departing from his predecessors. Then we can turn, in the last two sections of the chapter, to the questions of how Newton prefers to argue for theoretical claims and whether this way of arguing is more secure.

MATHEMATICAL THEORY IN NEWTON'S *PRINCIPIA*

The two most prominent books presenting mathematical theories of motion before the *Principia* were Galileo's *Two New Sciences* (1638)[13] and Huygens's *Horologium Oscillatorium* (1673).[14] Newton almost certainly never saw the former, but he knew the latter well, and it together with Galileo's *Dialogues on the Two Chief World Systems* (1632)[15] and various secondary sources[16] made him familiar with Galileo's results. Outwardly, the *Principia* appears to take the same mathematical approach as these two earlier books, proceeding from axioms to a series of rigorously demonstrated propositions. In fact, however, the approach to mathematical theory in Books 1 and 2 of the *Principia* differs from that taken by Galileo and Huygens in two important respects.

The first difference is subtle. Almost without exception, the demonstrated propositions of Books 1 and 2 of the *Principia* are of an "if-then" logical form, as illustrated by Propositions 1 and 2, restated in modern form: *if the forces acting on a moving body are all directed toward a single point in space, then a radius from that point to the body sweeps out equal areas in equal times, and conversely*.[17] So far as strict logic is concerned, the same can be said of the demonstrated propositions of Galileo and Huygens, as illustrated by the latter's celebrated isochronism theorem: *if a body descends along a path described by a cycloid, then the time of descent is the same regardless of the point along the path from which its descent begins*.[18] From the point of view of empirical science, however, this and the other demonstrated propositions of Galileo and Huygens are better described as having a "when-then" form, in which the antecedent describes an experimental situation and the consequent, a prediction of what will occur whenever that situation is realized. A primary aim of Galileo's and Huygens's mathematical theories is to derive observable consequences from their axioms that can provide evidence supporting these axioms, taken as hypotheses, or that can facilitate practical applications, such as the design of pendulum clocks.[19]

What lies behind this "when-then" form is the kind of quantities employed in the theories laid out by Galileo and Huygens. With the notable exception of the latter's theorems on centrifugal force, appended without proofs at the end of *Horologium Oscillatorium*, their axioms and demonstrated propositions make no reference to forces. Surprising as it may be, even the rate of acceleration in vertical fall – for us, *g*, and for them the distance of fall in the first second – enters nowhere into Galileo's propositions. This quantity does enter into the very last propositions of *Horologium Oscillatorium*, enabling Huygens to carry out a theory-mediated measurement of it to very high accuracy by means of pendulums; nonetheless, it plays no role in the development of his theory. The quantities central to the mathematical theories of motion under uniform gravity laid out by Galileo and Huygens were all open to measurement without having to presuppose any propositions of the theories themselves.

Unlike Galileo and Huygens, Newton takes his "axioms or laws of motion" to hold true from the outset of Books 1 and 2 of the *Principia*. His demonstrated "if-then" propositions amount to *inference-tickets*[20] linking motions to forces, forces to motions, and macrophysical forces to microphysical forces composing them. As Newton indicates in the quotation given earlier from the Preface to the first edition, the aim of the mathematical theories of Books 1 and 2 is first to establish means for inferring conclusions about forces from phenomena of motion and then to demonstrate further phenomena from these conclusions about forces. In Newton's hands *force* is a flagrantly theoretical quantity. The principal problem Newton's mathematical theories address is to find ways to characterize forces.

The second critical difference between Newton's mathematical theories and those of Galileo and Huygens concerns their respective scopes. Galileo offered a mathematical theory of uniformly accelerated motion, and Huygens extended this theory to curvilinear trajectories and uniform circular motion. Newton, by contrast, does not offer a theory of motion under inverse-square centripetal forces, much less under gravity, alone. Rather, Book 1 offers a *generic* theory of centripetal forces and motion under them. Inverse-square forces receive extra attention, but the theory also covers centripetal forces that vary linearly with distance to the force-center, that vary as the inverse-cube, and finally that vary as any function whatever of distance to the center. Similarly, while Book 2 emphasizes resistance forces that vary as the square of the velocity, it ultimately derives "if-then" propositions that allow resistance forces to vary as the sum of any powers of velocity whatever, including non-integer powers.[21] Book 2 thus strives to offer a generic theory of resistance forces, where these are characterized as arising from the velocity of a moving body in a fluid medium. The generic scope of these two theories is not simply a case of Newton displaying his mathematical prowess, as is sometimes suggested. The theories need to be generic in order to allow him to establish strong conclusions about forces from phenomena of motions, conclusions that exclude potential competing claims.

The propositions from Books 1 and 2 that become most important to the overall *Principia* are of two types. The first type consists of propositions that link parameters in rules characterizing forces to parameters of motion. The historically most significant example of this type is Newton's "precession theorem" for nearly circular orbits under centripetal forces.[22] It establishes a strict relationship between the apsidal angle θ – the angle at the force-center between, for example, the aphelion and the perihelion – to the square root of the index n, namely $n = (\pi/\theta)^2$, where the centripetal force varies as $r^{(n-3)}$. This relationship not only confirms that the exponent of r is exactly -2 when the apsidal angle is 180 degrees and exactly $+1$ when the angle is 90 degrees, but also yields a value of n and hence of the exponent for any other apsidal angle, or in other words for any rate at which the overall orbit precesses. This proposition and others of its type thus enable *theory-mediated measurements* of parameters characterizing forces to be made from parameters characterizing motions.[23] The propositions laid out earlier relating centripetal forces to Kepler's area rule, and their corollaries, provide another example of this type in which areal velocity yields a theory-mediated measure of the direction of the forces acting on a body.

As alluded to above, in his theory of motion under uniform gravity Huygens had derived propositions expressing the laws of the cycloidal and small-arc circular pendulums; and these results had enabled him to obtain from the periods and lengths of such pendulums a theory-mediated measure of the strength of surface gravity to four significant figures. This was a spectacular advance over prior attempts to measure the distance of vertical fall in the first second directly. Also, Huygens's theory of centrifugal force in uniform circular motion had allowed him to characterize the strength of these forces in terms of such motions, and from this to derive the law of the conical pendulum; and this result had enabled him to obtain a still further theory-mediated measure of the strength of surface gravity, in precise agreement with his other measures.[24] So, regardless of whether Newton first learned about propositions enabling theory-mediated measurements from Huygens, he at the very least had seen the utility of such propositions in *Horologium Oscillatorium*. Huygens, however, seems never to have seen any special evidential significance in his precise, stable measures of gravity. In Newton's hands, by contrast, theory-mediated measures became central to a new approach to marshaling evidence.

It is difficult to exaggerate the importance of measurement to the methodology of the *Principia*[25] or, for that matter, the sophistication with which Newton thought through philosophical issues concerning measurement. The importance is clear even in the definitions of key quantities with which the *Principia* opens, which are at least as much about measures of these quantities as they are about terminology. As the discussion of astronomical measures of *time* in the Scholium immediately following these definitions makes clear, Newton recognized that measures invariably involve theoretical assumptions, and hence remain provisional, even if not theory-mediated in the more restricted sense invoked above. He also seems to have appreciated that, because measurements in physics involve physical procedures and assumptions, a distinctive feature of this science is that it cannot help but include within itself its own empirically revisable theory of measurement. This insight may explain why Newton was so quick to view success in measurement as a form of evidence in its own right; here success includes (1) stability of values as a measure is repeated in varying circumstances – as illustrated by the stability of Huygens's measure of surface gravity by cycloidal pendulums of different lengths – and (2) convergence of values when the same quantity is determined through different measures involving different assumptions – as illustrated by the convergence of Huygens's cycloidal and conical pendulum measures. (Being open to increasingly greater precision appears to be a still further dimension of success in measurement for Newton.) Achieving success of this sort in determining values for forces is almost certainly what Newton had in mind with the cryptic remark at the end of the Scholium on space and time about the book explaining "how to determine the true motions from their causes, effects, and apparent differences."

The second type of proposition important to the *Principia* consists of combinations that draw clear contrasts between different conditions of force in terms of different conditions of motion. An historically significant example is the contrast between the simple form of Kepler's 3/2 power rule and the form requiring a specific small correction for each individual orbiting body; the latter holds if the orbiting and central bodies are interacting with one another in accord with the third law of motion, while the former holds if the orbiting body does not exert a force causing motion of the central body. Another historically significant example is the contrast between inverse-square celestial gravity acting to hold bodies in their orbits – a form of gravity that Huygens thought Newton had established – and inverse-square *universal* gravity between all the particles of matter in the universe: only under the latter does gravity vary linearly with distance from the center beneath the surface of a (uniformly dense) spherical Earth; and only under the latter does a particular relationship hold between the non-sphericity of a (uniformly dense) Earth and the variation of surface gravity with latitude. Combinations of propositions of this type thus provide contrasts that open the way to crossroads experiments – *experimenta crucis* – enabling phenomena of motion to pick out which among alternative kinds of conditions hold true of forces.

As these examples and the examples for the first type suggest, Newton prefers "if-and-only-if" results with both types. When he is unable to establish a strict converse, he typically looks for a result that falls as little short of it as he can find, as illustrated by the qualitative theorems on the "three-body" problem in Section 11.

Once these two types are identified, an examination of the overall development of the mathematical theories of Books 1 and 2 makes clear that the propositions Newton was most pursuing in these books are of these two types. His preoccupation with these explains why he included the propositions he did and not others that he could easily have added. Propositions that do not fall into these types generally serve to enable ones that do. By contrast, an examination of the overall development of the mathematical theories of Galileo and Huygens indicates that the propositions they were most pursuing are ones that make a highly distinctive empirical prediction, that provide an answer to some practical question, or that explain some known phenomenon. In other words, the mathematical theories of motion of Galileo and Huygens are primarily aimed at predicting and explaining phenomena. The mathematical theories of motion developed in Books 1 and 2 of the *Principia* do not have this aim. Rather, their aim is to provide a basis for specifying experiments and observations by means of which the empirical world can provide answers to questions – this in contrast to conjecturing answers and then testing the implications of these conjectures. Newton is using mathematical theory in an effort to turn otherwise recalcitrant questions into empirically tractable questions. This is what he is describing when he says:

> Mathematics requires an investigation of those quantities of forces and their proportions that follow from any conditions that may be supposed. Then, coming down to physics, these proportions must be compared with the phenomena, so that it may be found out which conditions of forces apply to each kind of attracting bodies.

This initial picture of Newton's approach is too simple in one crucial respect: if only because of imprecision of measurement, the empirical world rarely yields straightforward univocal answers to questions. That Newton was acutely aware of this is clear from his supplementing key "if-then" propositions with corollaries noting that the consequent still holds *quam proxime* (i.e., very nearly) when the antecedent holds only *quam proxime*. Nothing adds to the complexity of Newton's methodology more than his approach to inexactitude. We will return to this subject after considering the way in which he talks of force.

NEWTONIAN FORCES: MATHEMATICAL AND PHYSICAL

The theories developed in the *Principia*, unlike the theory of uniformly accelerated motion developed by Galileo and extended by

Huygens, are first and foremost about forces. Book 1 develops a general theory of centripetal forces and motions under them, and the first two-thirds of Book 2, a general theory of resistance forces and motions under them; the last third of Book 2 then develops a theory of the contribution the inertia of fluid media makes to resistance forces, and Book 3, a theory of gravitational forces and their effects. Newton was not the first to employ talk of forces in theories of motion. As the warning in the Scholium at the end of Section 11 about how he uses "attraction" and "impulse" indicates, he saw his way of employing such terms as novel, threatening confusion he needed to obviate. Definition 8 at the beginning of the *Principia* includes essentially the same warning about these terms, and "force" as well, adding, "this concept is purely mathematical, for I am not now considering the physical causes and sites of forces."[26] The warnings themselves are clear enough: Newton wants to be taken as talking of forces in the abstract, as quantities unto themselves, totally without regard to the physical mechanisms producing them. Not so clear are the ramifications of talking in this way.

The prior work that comes closest to treating forces in the manner of Newton is Huygens's theory of centrifugal force arising from uniform circular motion.[27] Like Descartes, Huygens uses the contrapositive of the principle of inertia to infer that something must be impeding any body that is not moving uniformly in a straight line. He further concludes that the magnitude of the force acting on the impediment is proportional to the extent of departure from what we now call inertial motion, obtaining for uniform circular motion the familiar v^2/r result. What Huygens means by "centrifugal force," however, is the force exerted on the impediment – for example, the tension in the string retaining the object in a circle. Huygens's centrifugal force is thus a form of static force, expressly analogous to the force a heavy object exerts on a string from which it is dangling. Talk of static forces was widespread in accounts of mechanical devices during the seventeenth century. Huygens was reaching beyond such talk only in inferring the magnitude of the force from the motion.

As Newton's discussion of his laws of motion makes clear, he too intended his treatment of forces to be continuous with the traditional treatment of static forces. Unlike Huygens, however, he singles out the unbalanced force that acts on the moving body, making it depart from inertial motion. Where Descartes and Huygens used the contrapositive of the principle of inertia to infer the existence of an impediment in contact with the non-inertially moving body, Newton uses it to infer the existence of an unbalanced force, *independently of all consideration of what is effecting that force*. His second law of motion then enables the magnitude and direction of any such force to be inferred from the extent and direction of the departure from inertial motion. Unbalanced force as a quantity can thus be fully characterized in abstraction from whatever might be producing it. This is what Newton means when he speaks in Definition 8 of considering "forces not from a physical but only from a mathematical point of view."

Newton had reason to expect that this way of talking of forces would confuse many of his readers. In his writing on light and colors in the early 1670s he had adopted essentially the same strategy in talking of rays of light as purely mathematically characterizable, independently of the underlying physics of light and the process or mechanism of its transmission. His warnings notwithstanding, many readers had insisted on equating his rays of light with paths defined by hypothetical particles comprising light; they had then argued, to his consternation, that his claims about refraction had not been established because he had not established that light consists of such particles.[28] His warnings about considering forces "from a mathematical point of view" were scarcely any better heeded.

From the mathematical point of view any unbalanced force acting on a body is a quantity with magnitude and direction. The general theory of centripetal forces developed in Book 1 considers forces from this point of view, with the direction specified toward a center and the magnitude taken to vary as a function of distance from that center. The same is true of the general theory of resistance forces developed in the first two-thirds of Book 2, but with the direction specified opposite to the direction of motion and the magnitude varying as a function of velocity. An unbalanced force that is thus fully characterized by its direction and magnitude can be resolved into correspondingly fully characterized components in any way one wishes, without regard to the particular physical components that happen to be giving rise to it. This absence of constraint in resolving forces into components is important in several places in Books 1 and 2, perhaps most strikingly in Proposition 3 of the former:

> Every body that, by a radius drawn to the center of a second body moving in any way whatever, describes about that center areas that are proportional to the times is urged by a force compounded of the centripetal force tending toward that second body and of the whole accelerative force by which that second body is urged.[29]

In principle – indeed, in practice – this situation can occur without there being any form of physical interaction, or physical forces, between the two bodies.

Still, as Newton's remark about "arguing more securely concerning the physical species, physical causes, and physical proportions of these forces" indicates, it does make sense according to his way of talking about forces to ask what *physical* forces a net unbalanced force results from. The theory of gravitational forces of Book 3 and the theory of the constituent of resistance forces arising from the inertia of the fluid at the end of Book 2 both treat forces from a physical point of view. Judging from the development of these two theories, Newton requires five conditions to be met for a component of a mathematically characterized force to be considered a physical force: (1) its direction must be determined by some material body other than the one it is acting on;[30] (2) all respects in which its magnitude can vary must be given by a general law that is independent of the first two laws of motion, such as the law of gravity, $F \propto Mm/r^2$; (3) some of the physical quantities entering into this law must pertain to the other body that determines the direction of the force; (4) this law must hold for some forces that are indisputably real, such as terrestrial gravity in the case of the law of gravity; and (5) if the force acts on a macroscopic body, then it must be composed of forces acting on microphysical parts of that body – this primarily to safeguard against inexactitude in the force law introduced by inferring it from macroscopic phenomena.

Notably absent from this list is anything about the mechanism or process effecting the force. Adherents to the "mechanical philosophy," such as Descartes and Huygens, and undoubtedly Galileo as well, would have required not just a mechanism effecting the force, but specifically a contact mechanism. Otherwise the putative force might be beyond explanation and hence occult. This is where Newton's new "experimental philosophy" departed most radically from the prevailing "mechanical philosophy."

The law characterizing a force from a physical point of view gives its "physical proportions" and assigns it to a "physical species." Two forces are of the same physical species only if they are characterized by the same law. Thus the inverse-square forces retaining the planets and their statellites in their orbits are the same in kind as terrestrial gravity, while (for Newton) the constituent of resistance forces arising from the inertia of the fluid is different in kind from that arising from its viscosity in so far as the former varies as velocity squared, and the latter does not. A theory of any physical species of force is required to give (1) necessary and sufficient conditions for a force to be present, (2) a law or laws dictating the relative magnitude and direction of this force in terms of determinable physical quantities, and (3) where relevant, an account of how it is composed out of microstructural forces.

Microstructural forces have a more fundamental status in the overall taxonomy of forces. In the *Principia* Newton identifies three species of microstructural force, gravity, pressure, and, percussion, where the theory of the latter had already been put forward by Huygens, Christopher Wren, and John Wallis.[31] The remark in the Preface to the first edition – "all phenomena may depend on certain forces by which the particles of bodies, by causes yet unknown, either are impelled toward one another and cohere in regular figures, or are repelled from one another and recede" – points to a program of pursuing theories of further species of microstructural force. This program is described in more detail in the unpublished portion of this Preface and an unpublished Conclusion, as illustrated by this passage from the former:

> I therefore propose the inquiry whether or not there be many forces of this kind, never yet perceived, by which the particles of bodies agitate one another and coalesce into various structures. For if Nature be simple and pretty conformable to herself, causes will operate in the same kind of way in all phenomena, so that the motions of smaller bodies depend upon certain smaller forces just as the motions of larger bodies are ruled by the greater force of gravity. It remains therefore that we inquire by means of fitting experiments whether there are forces of this kind in nature, then what are their properties, quantities, and effects. For if all natural motions of great or small bodies can be explained through such forces, nothing more will remain than to inquire the causes of gravity, magnetic attraction, and the other forces.[32]

To his contemporaries, what seemed most confusing about Newton's way of talking about forces was his willingness to put forward a theory of gravitational "attraction" without regard to the causal mechanism effecting it. They generally concluded that he had to be committed to action at a distance as a causal mechanism in its own right. The outspoken opposition to the *Principia* in many quarters stemmed primarily from the inexplicability of action at a distance. Present-day readers, viewing the *Principia* in the light of 300 years of success in physics, are not likely to find the way Newton talks of forces from a physical point of view confusing. What most tends to confuse them is the distinction between considering forces from a physical point of view and considering them purely from a mathematical point of view. A symptom of this confusion is the tendency to read Book 1 as if its subject is gravitational forces, wondering why Newton bothered to include in it so many seemingly irrelevant propositions.

ARGUING FROM PHENOMENA OF MOTION TO LAWS OF FORCE

In the Scholium at the end of Section 11 Newton says, rather vaguely, that the transition from mathematically to physically characterized forces is to be carried out by *comparing* the mathematically characterized proportions with phenomena. As other methodological remarks in the *Principia* make clear, the specific approach he prefers is to use the "if-then" propositions of his mathematical theory to "deduce" the physical laws characterizing forces from phenomena[33] – most notably, to deduce the law of gravity from the phenomena of orbital motion specified by two of Kepler's rules,[34] along with Thomas Streete's conclusion that the planetary aphelia are stationary.[35] Serious difficulties stand in the way of any such deduction, however. Much of the complexity of Newton's methodology comes from his approach to these difficulties.

One difficulty, noted earlier, is that limits of precision in observation entail that statements of phenomena hold at most *quam proxime*. This limitation was evident at the time in the case of Kepler's rules. Ishmaël Boulliau had replaced Kepler's area rule with a geometric construction, yet had achieved the same level of accuracy relative to Tycho Brahe's data as Kepler – roughly the level of accuracy that Tycho had claimed for observations at Uraniborg; and Vincent Wing had done almost as well using an oscillating equant instead of the area rule.[36] Jeremiah Horrocks and Streete were the only orbital astronomers to claim that the lengths of the semi-major axes of the planetary orbits could be inferred more accurately from the periods using Kepler's 3/2 power rule than by classical methods that were known to be sensitive to observational imprecision.[37] Even in the case of the ellipse, which virtually all orbital astronomers were using, the question whether it is merely a good approximation or the true exact trajectory remained open.[38] In short, Kepler's rules were at best established only *quam proxime*, and any "deduction" from them would have to concede that other ways of stating the phenomena could not be eliminated on grounds of accuracy alone.

From Newton's point of view, however, imprecision was not the worst difficulty. In the brief "De motu" tracts that preceded the *Principia* he had concluded that there are inverse-square centripetal acceleration fields (to use the modern term) around the Sun, Jupiter, Saturn, and the Earth, with the strength of each given by the invariant value $[a^3/P^2]$ for bodies orbiting them, where a is the mean distance for any orbit and P is the period.[39] Presumably, the acceleration fields around Jupiter, Saturn, and the Earth extend to the Sun, putting it into motion. By a generalization of the principle of inertia to a system of interacting bodies – a generalization that is equivalent to the third law of motion of the *Principia* – the interactions among the bodies cannot alter the motion of the center of gravity of the system. From this Newton reached a momentous conclusion:

> By reason of the deviation of the Sun from the center of gravity, the centripetal force does not always tend to that immobile center, and hence the planets neither move exactly in ellipses nor revolve twice in the same orbit. There are as many orbits of a planet as it has revolutions, as in the motion of the Moon . . . But to consider simultaneously all these causes of motion and to define these motions by exact laws admitting of easy calculation exceeds, if I am not mistaken, the force of any human mind.[40]

In other words, before he began writing the *Principia* itself (and, if I am right, before he had even discovered the law of gravity[41]), Newton had concluded that Kepler's rules can at best be true only *quam proxime* of the planets and their satellites, not because of imprecision of observation, but because the true motions are immensely more complicated than Kepler's or any other such rules could hope to capture.

Newton was not the first to conclude that real motions are exceedingly complex. Galileo had concluded that the multiplicity of factors affecting motion in resisting media preclude "fixed laws and exact description";[42] and, in a letter to Mersenne, Descartes too had denied the possibility of a science of air resistance.[43] Newton was most likely unaware of these remarks of Galileo and Descartes on resistance, but he definitely did know that Descartes, in his *Principia* (1644), had denied that the planetary orbits are mathematically exact, remarking that as "in all other natural things, they are only approximately so, and also they are continuously changed by the passing of the ages."[44] The response of Galileo, Huygens, and Descartes to the complexities of real-world motions and limits in precision of measurement was to employ the hypothetico-deductive approach to marshaling evidence, deducing testable conclusions from conjectured hypotheses and then exposing these conclusions to falsification. From the beginning of his work in optics in the 1660s, Newton had always distrusted the hypothetico-deductive approach, arguing that too many disparate hypotheses can be compatible with the same observations.[45] Inexactitude, whether from imprecision in observation or from the complexity of the real world, exacerbates this shortcoming. In saying that the approach illustrated by the *Principia* puts one in position to argue more securely about features of underlying physics, Newton was claiming to have a response to inexactitude that surmounts limitations of the hypothetico-deductive approach of his predecessors.

Because Newton never describes his approach in detail, we have to infer what it involves from the evidential reasoning in the *Principia*. A key clue is provided by what I. Bernard Cohen has called the "Newtonian style"[46] – proceeding from idealized simple cases to progressively more complicated ones, though still idealized. Thus, in the case of inverse-square centripetal forces, Book 1 first considers so-called "one-body" problems, for which Kepler's three rules hold exactly. Next are one-body problems in which inverse-cube centripetal forces are superposed on the inverse-square; Kepler's rules still hold exactly, but for orbits that rotate, that is, whose lines of apsides precess. Next are "two-body" problems subject to the third law of motion. The results for these show that two of Kepler's rules continue to hold, but the 3/2 power rule requires a correction. Last are problems involving three or more interacting bodies. For these Newton succeeds in obtaining only limited, qualitative results, yet still sufficient to show that none of Kepler's three rules holds. A distinctive feature of this sequence is the extent to which it focuses on systematic deviations from Kepler's simple rules that can serve as evidence for two-body and three-body interaction. Newton is putting himself in a position to address the complexity of real orbital motion in a sequence of successive approximations, with each approximation an idealized motion and systematic deviations from it providing evidence for the next stage in the sequence.

Here too Huygens had foreshadowed the Newtonian style, though again only up to a point. The initial theory of pendulum motion in *Horologium Oscillatorium* is for pendulums with idealized "point-mass" bobs.[47] Huygens then turns to the question of physical bobs with a distinctive shape and real bulk, solving the celebrated problem of the center of oscillation that Mersenne had put forward as a challenge decades earlier. The small-arc circular pendulum measurement of gravity presented near the end of the book incorporates a small correction to the length of the pendulum, corresponding to the distance between the center of gravity of the bob and its center of oscillation. This correction, however, holds only for the circular pendulum, not for the cycloidal pendulum that was the crowning achievement of Huygens's initial theory. For the correction depends not only on the shape of the bob, but also on the length of the string, and this length varies along the cycloidal path. (Indeed, it is this variation that makes the cycloid the isochronous path for a point-mass bob.) Huygens had tried to find the corrected path required for strict isochronism with a physically real bob, only to despair when the problem proved intractably complex. In the manner typical of pre-Newtonian science, the small residual discrepancies between idealized theory and the real world were dismissed as being of no practical importance. This is one more example of the way in which the complexity of the real world ended up being viewed as an impediment, limiting the quality of empirical evidence, and not as a resource for progressively higher-quality evidence that it became with Newtonian successive approximations.

Newton's "deductions" of the various parts of the law of gravity from phenomena of orbital motion reveal two restrictions, beyond mathematical tractability, that he at least prefers to impose on the successive approximations.[48] First, in every case in which he deduces some feature of celestial gravitational forces, he has taken the trouble in Book 1 to prove that the consequent of the "if-then" proposition licensing the deduction still holds *quam proxime* so long as the antecedent holds *quam proxime*. For instance, two corollaries of Proposition 3 show that the force on the orbital body is at least very nearly centripetal so long as the areas swept out in equal times remain very nearly equal. This, by the way, explains why Newton himself never deduced the inverse-square variation from the Keplerian ellipse even though he had proved in Book 1 that an exact Keplerian ellipse entails an exact inverse-square variation: an orbital motion can approximate a Keplerian ellipse without the exponent of r in the rule governing the centripetal force variation being even approximately minus 2.[49] Restricting the deductions to ones that hold *quam proxime* so long as the phenomenon describes the true motions *quam proxime* provides a guarantee: under the assumption that the laws of motion hold, the deduced feature of the physical forces holds at least *quam proxime* of the specific motions that license the statement of the phenomenon. In other words, thanks to this restriction, unless his laws of motion are seriously wrong, Newton's law of gravity is definitely true at least *quam proxime* of celestial motions over the century of observations from Tycho to the *Principia*.

Second, in every case in which Newton deduces some feature of celestial gravitational forces, mathematical results established in Book 1 allow him to identify specific conditions under which the phenomenon from which the deduction is made would hold not merely *quam proxime*, but exactly. For instance, the orbiting body would sweep out equal areas in equal times exactly if the only forces acting on it were centripetal, and its line of apsides would be stationary if the only forces acting on it were inverse-square centripetal forces. The choice of the subjunctive here is not mine, but Newton's: in Proposition 13 of Book 3, for example, he remarks, "if the Sun were at rest and the remaining planets did not act upon one another, their orbits would be elliptical, having the Sun at their common focus, and they would describe areas proportional to the times."[50] By imposing this restriction on the phenomena from which force laws are deduced, Newton is assuring that these phenomena are not just arbitrary approximations to the true motions; at least according to the theory of the "deduced" physical force, the true motions would be in exact accord with the phenomena were it not for specific complicating factors.

Let me here restrict the term "idealization" to approximations that would hold exactly in certain specifiable circumstances. If, as I have proposed, Newton is addressing the complexity of real orbital motion in a sequence of successive approximations, then he had profound reasons for preferring that each successive approximation be an idealization in this sense. For any deviation of the actual motions from a given approximation will then be physically meaningful, and not just a reflection of the particular mathematical scheme employed in achieving the approximation, as in curve fitting. Of course, omniscience is required to know whether any approximation really is an idealization in the requisite sense, and (as Book 2 attests) Newton was far from omniscient. The most he could demand is that the theory being "deduced" from the approximations entails that they be idealizations of this sort. At least from the point of view of the theory, then, any observed systematic pattern in the deviations from a given approximation would have the promise of being physically informative, and hence a promise of becoming telling evidence.

In sum, judging from details of Newton's "deductions" from phenomena, his approach to the complexities of real-world motions is to try to address them in a sequence of progressively more complex idealizations, with systematic deviations from the idealizations at any stage providing the "phenomena" serving as evidence for the refinement achieved in the next. Such systematic deviations are appropriately called "second-order phenomena" in so far as they are not observable in their own right, but presuppose the theory. Thus, for example, no one can observe the famous 43 arc-seconds per century discrepancy in the motion of the perihelion of Mercury that emerged in the second half of the nineteenth century and then became evidence for Einstein's theory of general relativity: they are the residual left over after subtracting the 531 arc-seconds per century produced by the other planets according to *Newtonian* theory from the 574 arc-seconds derived from observation once allowance is made for the 5600 arc-seconds associated with the precession of the equinoxes.

Attempting to proceed in *successive* approximations in this way involves restrictions on how second-order phenomena are to be marshaled as evidence. In the case of orbital motions, any systematic discrepancy from the idealized theoretical motions has to be identified with a specific physical force – if not a gravitational force, then one governed by some other generic force law. This restriction precludes inventing *ad hoc* forces to save the law of gravity. It thereby makes success in carrying out a program of successive approximations far from guaranteed.

A second, less familiar example shows this in a different way. In Propositions 19 and 20 of Book 3 Newton first calculates a 17 mile difference between the radii to the poles and to the equator of the Earth, and then a specific variation of surface gravity with latitude. These calculations presuppose *universal* gravity. Indeed, as Huygens was quick to notice (and Maupertuis and Clairaut forty years later), this is the sole result in the *Principia* amenable at the time to empirical assessment that differentiates *universal* gravity from macroscopic inverse-square celestial gravity. Newton's calculations also presuppose that the density of the Earth is perfectly uniform. Hence, his results are not straightforwardly testable predictions, for they apply only to an idealized Earth. In all three editions Newton pointed out that any deviation from the calculated results is a sign that the Earth's density increases from the surface to the core. In the first edition he went so far as to propose that a linear increase in density be assumed for the next idealized approximation.[51] This was not an *ad hoc* way of protecting the law of universal gravity from refutation because, as Huygens's efforts in his *Discourse on the Cause of Gravity* showed, different assumptions about gravity yield very different relationships between the Earth's oblateness on the one hand, and the variation of surface gravity with latitude on the other.[52] Therefore, a variation in density inferred from, say, an observed oblateness differing from Newton's 17 miles was not guaranteed to yield a corresponding improvement between the observed variation in surface gravity and Newton's calculated variation. (From Clairaut forward the field of physical geodesy has been inferring the internal density distribution of the Earth from features of its shape and gravitational field, always presupposing the law of universal gravity; the discrepancies between observation and current theory have grown continually smaller.[53])

Needless to say, Newton's theory of gravity provides an explanation of Kepler's rules and of each of the subsequent idealized orbital motions in the sequence of successive approximations. That is, the theory explains why these idealizations hold at least *quam proxime* and why they have claim to being preferred descriptions of the actual motions even though they are not exact and observation is not precise. Providing such explanations, however, is not the distinctive feature of the theory. As Leibniz showed in print within months after the *Principia* first appeared, a theory of a very different sort, one that meets the demands of the mechanical philosophy, can explain Kepler's rules too.[54] The distinctive feature of Newtonian theory is the spotlight it shines on discrepancies between theory and observation. In his "System of the World" in Book 3 Newton no sooner spells out the conditions under which, for example, Keplerian motion would hold exactly than he turns to the principal real-world respects in which it does not, such as the gravitational effect of Jupiter on the motion of Saturn and on the precession of the aphelia of the inner planets. In adopting his approach of successive approximations, with its focus on theory-dependent second-order phenomena, Newton was turning theory into an indispensable instrument for ongoing research. Exact science as illustrated by the *Principia* is thus not exact science in the sense of Newton's predecessors, an account of how the world would be if it were more rational. It is exact science in the sense that every systematic deviation from current theory automatically has the status of a pressing unsolved problem.

Even with the above restrictions, the "deduction" of the law of gravity, or any other force law, from phenomena of motion that hold only *quam proxime* shows at most that it holds *quam proxime*. When the restrictions are met, however, as they by and large are in the case of the law of gravity,[55] Newton views the derivation as authorizing the force law to be *taken*, provisionally, as exact. Specifically, his fourth Rule for Natural Philosophy says:

> *In experimental philosophy, propositions gathered from phenomena by induction should be considered either exactly or very nearly true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact or liable to exceptions.*
>
> This rule should be followed so that arguments based on induction may not be nullified by hypotheses.[56]

Taking the force law to be exact when the evidence for it shows at most that it holds *quam proxime* amounts to an evidential strategy for purposes of ongoing research. This strategy is transparently appropriate when the goal is to use systematic deviations from current theory as evidence in a process of successive approximations.

ARGUING *MORE SECURELY*

The preceding section has offered a detailed description of how Newton prefers to *argue* from phenomena to physically characterized forces. Nothing has yet been said, however, about why this way of arguing might have claim to yielding conclusions that are *more secure*.

One respect in which it offers more security is easy to see. The "if-then" propositions used in deducing the law, as well as their approximative counterparts ("if-*quam-proxime*-then-*quam-proxime*"), are rigorously derived from the laws of motion. The phenomena – that is, the propositions expressing Newton's phenomena – are inductive generalizations from specific observations, and hence they hold at least *quam proxime* of these observations. But then, unless the laws of motion are fundamentally mistaken, the force law too is guaranteed to hold at least *quam proxime* of these observations. By way of contrast, the fact that a consequence deduced from a hypothesized force law holds *quam proxime* of specific observations need not provide any such guarantee. A conjectural hypothesis can reach far beyond the observations providing evidence for it not merely in its generality, but in its content. In practice Newton's first Rule for Natural Philosophy – *no more causes . . . should be admitted than are both true and sufficient to explain their phenomena* – has the effect of confining the content of theory to no more than the data clearly demand. Calling for the force law to be deduced from phenomena is a way of meeting this Rule.

Put another way, Newton's demand for a deduction from phenomena is an attempt to confine risk in theorizing as much as possible to "inductive generalization." What Newton means by "made general by induction" and "propositions gathered from phenomena by induction" amounts to more than merely projecting an open-ended generalization from some of its instances. The Phenomena he lists at the beginning of Book 3 involve first projection from discrete observations to orbital rules that fill in the gaps among these observations, and then projection of these rules into the indefinite past and future. His second Rule for Natural Philosophy – *same effect, same* *cause* – authorizes inferences that Charles Saunders Peirce would have labeled *abductive* in contrast to inductive. Even his third Rule, which at first glance seems most akin to induction, authorizes inferences of much greater sweep than is customary in simple induction: it specifies conditions under which conclusions based on observations and experiments within our reach may be extended to the far reaches of the universe and to microphysical reaches far beyond our capacity to observe. The care Newton put into this third Rule,[57] which he formulated in the early 1690s when he was in close contact with John Locke, indicates that he was acutely aware of the risk in "propositions gathered from phenomena by induction." So too does his insistence on the provisional status of these propositions in the subsequently added fourth Rule.

Newton's further demand that the theory entail specific conditions under which the phenomena in question hold exactly provides some support for projecting these phenomena inductively beyond the available observations. Specifically, as noted earlier, such a "re-deduction" gives reason to take the phenomena as lawlike, and not just one among many possible curve-fits. The deduced force law itself, however, can hold *quam proxime* of these observations and still turn out not to be suitable for inductive generalization; the most that can be said is that its deduction and the subsequent re-deduction of the phenomena make it an exceptionally promising candidate for inductive generalization.

Over the long term, pursuit of refinements in a sequence of successive approximations can provide a further source of security. Any current approximation to, for example, orbital motions is an idealization predicated on the force law. Hence observed deviations from it continually, so to speak, put the law to test. Recalcitrant deviations point to deficiencies in the law. If, however, second-order phenomena emerge and the presence of further forces complicating the motions is successfully established from them, then new evidence accrues to the law. Such new evidence does more than just support the original inductive generalization. The process of successive approximations leads to increasingly small residual deviations from current theory, which in turn tighten the range over which the force law holds *quam proxime*. More important, because the process of successive approximations presupposes the force law, continuing success in it leads to progressively deeper *entrenchment* of the law, to use Nelson Goodman's term.[58] This, of course, is precisely what happened in the case of Newton's law of gravity, with continuing improvement over the last three centuries in the agreement between theory and observation not only for orbital motion within celestial mechanics, but also for the Earth's shape and gravity field within physical geodesy. Indeed, the process of successive approximations issuing from Newton's *Principia* in these fields has yielded evidence of a quality beyond anything his predecessors ever dreamed of.

Evidence from long-term success in pursuit of successive approximations, however, can in principle be achieved by a hypothetico-deductive approach as well. The most that can be said for Newton's approach in this regard is that its confining the risk to the extent it does to inductive generalization may enhance its prospects for achieving such success.

What form does the risk take with Newton's approach? His inductively generalized law of *universal* gravity is presupposed as holding exactly in evidential reasoning at each stage after the first in the process of successive approximations. The main risk is a discovery that would falsify this law in a way that nullifies all or part of the evidential reasoning that has been predicated on it. Suppose, for example, that a discovery entails that various second-order phenomena that had been crucial as evidence were not phenomena at all, but mere artifacts of a supposed law that just so happens to hold *quam proxime* under parochial circumstances. Then, to the extent the evidence for this discovery is predicated on advances based on these second-order phenomena, the discovery itself would, in a sense, be self-nullifying. The conclusion would have to be that the pursuit of successive approximations had been proceeding down a garden path, and the area of science in question would have to be restarted from some earlier point.

Newton's attempt to initiate successive approximations in the case of resistance forces was shown to be going down just such a garden path by Jean d'Alembert twenty-five years after the third edition of the *Principia* appeared.[59] Surprising as it may seem to many readers, however, this has yet to happen in the case of his theory of gravity. The large conceptual gap between Newtonian and Einsteinian gravitation notwithstanding, the theory of gravity in general relativity has not nullified the evidential reasoning predicated on Newton's theory. In particular, it has not nullified the evidential reasoning from which the phenomenon of the residual 43 arc-seconds per century precession of the perihelion of Mercury emerged; if it had, this phenomenon could not be used directly as evidence supporting it. The reason why evidential reasoning predicated on Newtonian gravity was not nullified is because general relativity entails that Newton's law holds in the weak-field limit, and virtually none of this reasoning, viewed in retrospect, required anything more of Newton's law than that it hold to very high approximation in weak gravitational fields.[60]

The risk of a garden path with Newton's approach, therefore, does not as such derive from the possibility that the force law deduced from phenomena at the outset is not exact. This law itself can be open to refinement as part of the process of successive approximations without undercutting the process and having to restart from some earlier point. The relativistic refinements to Newton's first two laws of motion show that the same can be said about the axioms presupposed in the deduction of the force law. Rather, the risk comes from the huge inductive leap, from a celestial force law that holds at least *quam proxime* over a narrow body of data to the law of *universal* gravity – a leap authorized by Newton's first three Rules governing inductive reasoning. More specifically, the risk comes from two "taxonomic" presuppositions entering into this leap. Newton's vision of a fundamental taxonomy based on physical forces – or, more accurately, interactions[61] – is largely beside the point so far as gravity alone is concerned. Nevertheless, his inductive generalization does presuppose (1) that there is a distinct species – or natural kind, to use our current term – of elementary motion and a distinct species of static force which are characterized at least to a first approximation by his deduced law of gravity. The risk lies in the possibility that subsequent research will conclude either that there are no such distinct species or that they are species of limited range, even artifacts of the data from which he was working. Further, his inductive generalization presupposes (2) that certain specific motions – primarily planetary motions – are pure enough examples of motions of a specific elementary species to typify this species as a whole.

The risks from both of these presuppositions are evident in the garden path formed by Newton's efforts on resistance forces. In the first edition of the *Principia* he thought that phenomena of pendulum decay would allow him to demarcate the different species of resistance force and their respective variation with velocity. Recognizing the failure of this,[62] in the second and third editions he assumed that vertical fall of ordinary-size objects is dominated by resistance forces arising purely from the inertia of the fluid – at least to a sufficient extent to allow a law to be established for this kind of resistance force. His announced plan was for the other kinds to be addressed using discrepancies between observations and this law.[63] The garden path arose because both of these taxonomic presuppositions were wrong. First, there are no distinct species of resistance force, but only one species governed by interaction between inertial and viscous effects in the fluid, interaction that is so complicated that we still have no law for resistance of the sort Newton was pursuing, but only empirically determined relationships for bodies of various shapes.[64] Second, as d'Alembert showed, resistance in an idealized inviscid fluid of the sort Newton had assumed in deriving his law for purely inertial resistance is exactly zero, regardless of shape and velocity. Newton's supposed "law" for the purely inertial effects of the fluid turns out to amount to nothing more than a very rough approximation to the total resistance on spheres for a limited combination of diameters, velocities, and fluid densities and viscosities – a mere curve-fit over a restricted domain.[65]

Newton's taxonomic presuppositions are best regarded as working hypotheses underpinning his inductive generalizations. As with all such working hypotheses, some immediate protection is afforded by demanding that the evidence developed out of the data be of high quality, without lots of loose ends. Newton's "deduction" of the law of gravity met this demand to a much greater extent than did his evidential reasoning on resistance.[66] Still, the "deduction" was based primarily on the motion of only five planets over an astronomically brief period of time. The danger of being misled by such limited data is always high.

I know of nowhere that Newton acknowledges the risk that such taxonomic working hypotheses introduce into inductive generalization. He does acknowledge the risk of inductive generalization in the most famous methodological passage in the *Opticks*, in the discussion of the methods of "analysis and synthesis" in the next to last paragraph of the final Query, which was added in 1706:

This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no

Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of the Thing admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phenomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur. By this way of Analysis we may proceed from Compounds to Ingredients and from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particular Causes to more general ones, till the Argument end in the most general.[67]

Perhaps Newton saw success in achieving unrestricted generality as the ultimate safeguard against the risk introduced by the unavoidable taxonomic hypotheses entering into induction.

This brings us to the last distinctive aspect of the approach to theory construction illustrated by the *Principia* – that is, illustrated in the case of gravity, though not in the case of resistance. After establishing the law of universal gravity and the conditions for Keplerian motion, Book 3 goes on to "applications" of the law in unresolved problems at some remove from the phenomena from which it was "deduced": (1) the non-spherical shape of the Earth and the variation of surface gravity with latitude; (2) the area-rule violation in the orbit of the Moon, the motion of its nodes, and its fluctuating inclination; (3) the tides; (4) the precession of the equinoxes; and (5) the trajectories of comets. The idea seems to be to protect against risks arising in the inductive leap by immediately pushing the theory for all it is worth, employing it as a tool of research on problems that *prima facie* have nothing to do with the original evidence for it. It goes without saying that, regardless of how far afield such "applications" may be, they still provide no *guarantee* against a garden path. Nevertheless, they do represent a concerted effort to expose limitations in the taxonomic presuppositions set out above. As already noted, the shape of the Earth and the variation of surface gravity directly involve the generalization from celestial to universal gravity, as does the precession of the equinoxes indirectly. The vagaries in the lunar orbit address the most glaring known counterexample to Keplerian motion and hence worries about generalizing beyond planetary motion. Both the tides and the precession of the equinoxes involve the generalization from simple centripetal forces to interactive gravity, as does a gravitational treatment of vagaries in the motions of Jupiter and Saturn. And finally the comets involve the extension of the law of gravity to bodies that appear to consist of matter very different from that of the planets and their satellites and that pass through the intermediate distances from the Sun between the orbits of the planets.[68] The fact that all of these address evidential worries in the original inductive generalization indicates that the process of comparison with phenomena, and hence the argument for securing universal gravity, extends across all of Book 3.[69]

The efforts occupying the rest of Book 3 were extraordinarily innovative. In this respect they are akin to predictions of novel phenomena of the sort Huygens singled out as the strongest form of evidence for empirical theories. None of them, however, is a truly straightforward prediction of the sort classically called for in hypothetico-deductive evidence. In every case some further, contestable assumptions were needed beyond Newton's theory, if only the assumption that no other forces are at work besides gravity. Still, Newton's inductive generalization to *universal* gravity clearly introduced a large conjectural element in his theory; and the applications of it beyond Keplerian motion put this element to the test, ultimately supplying the most compelling evidence for it. The key prediction put to the test in these applications was not so much that every two particles of matter interact gravitationally, but rather one that is more abstract: *every discrepancy between Newtonian theory and observation will prove to be physically significant and hence can be taken to be telling us something further about the physical world*. Contrast this with deviations from a curve-fit, which usually reflect nothing more than the particular mathematical framework that happened to have been used. Lacking omniscience, the only way we have of deciding whether a discrepancy is physically significant is from the point of view of ongoing theory. The issue of physical significance from this point of view turns most crucially on whether the taxonomic working hypotheses underlying Newton's inductive step to universal gravity remain intact as theory advances. Does the discrepancy give reason to conclude that a taxonomy of interactions is not fundamental or that gravitational interactions do not comprise a distinct kind within that taxonomy?

In part because of the further contestable assumptions, every one of the efforts occupying the rest of Book 3, as well as Newton's brief suggestions about the motions of Jupiter and Saturn, initiated its own historical sequence of successive approximations subsequent to the *Principia*. Moreover, even at the time the third edition appeared, almost forty years after the first, serious loose ends remained in the treatment of every one of these topics in the *Principia*. These loose ends may help to explain why so many capable scientists who came of age after the *Principia* were initially so cautious in accepting Newton's theory. A decade or so after Newton died, Clairaut, Euler, and d'Alembert began their efforts to tie up these loose ends, followed by Lagrange and Laplace over the last forty years of the eighteenth century.[70] In a very real sense, then, Newton's argument for universal gravity was not completed until a century after the publication of the first edition of the *Principia*. With its completion, the new approach to theory construction that the book was intended to illustrate – that is, the new type of generic mathematical theory, the contrast between mathematical and physical points of view, the roles of "deduced" theory and idealizations in ongoing research, and the insistence on pushing theory far beyond its original basis – became a permanent part of the science of physics.

NOTES

I thank Kenneth G. Wilson, Eric Schliesser, and I. Bernard Cohen for several useful comments on an earlier draft of this chapter.

1 Isaac Newton, *The Principia, Mathematical Principles of Natural Philosophy: A New Translation*, trans. I. Bernard Cohen and Anne Whitman (Berkeley: University of California Press, 1999), pp. 382f.

2 *Ibid.*, p. 415; see Robert DiSalle's chapter in this volume for a discussion of Newton's views on relative versus absolute motion.

3 In Latin, *Regulae Philosophandi*; see William Harper's chapter in this volume for a discussion of Newton's use of these Rules in his "deduction" of universal gravitation.

4 Newton, *Principia*, p. 943.

5 Isaac Newton, "An Account of the Book Entituled *Commercium Epistolicum*," reprinted in A. Rupert Hall, *Philosophers at War: The Quarrel between Newton and Leibniz* (Cambridge: Cambridge University Press, 1980), p. 312. Newton made much the same concession to hypotheses in 1672 in one of his exchanges with Pardies on his light and

colors experiments; see I. Bernard Cohen and Robert E. Schefield (eds.), *Isaac Newton's Papers and Letters on Natural Philosophy*, revised edition (Cambridge, MA: Harvard University Press, 1978), p. 106; see note 45 below.

6 Christiaan Huygens, *Traité de la Lumière*, in *Œuvres complètes de Christiaan Huygens*, vol. 19 (The Hague: Martinus Nijhoff, 1937), p. 454; the English translation is from Michael R. Matthews, *Scientific Background to Modern Philosophy* (Indianapolis: Hackett, 1989), p. 126. The hypothesis which Huygens had most in mind was the longitudinal wave theory of light.

7 Newton, *Principia*, pp. 588f.

8 A few Newton scholars have emphasized this Scholium, most notably I. Bernard Cohen in his *The Newtonian Revolution* (Cambridge: Cambridge University Press, 1980), Clifford Truesdell in "Reactions of Late Baroque Mechanics to Success, Conjecture, Error, and Failure in Newton's *Principia*," reprinted in his *Essays in the History of Mechanics* (New York: Springer-Verlag, 1968), and E. W. Strong in "Newton's 'Mathematical Way'," *Journal of the History of Ideas* 12 (1951), 90–110.

9 See N. Jardine, *The Birth of History and Philosophy of Science: Kepler's* A Defence of Tycho against Ursus (Cambridge: Cambridge University Press, 1984).

10 See Alexandre Koyré, "An Experiment in Measurement," in his *Metaphysics and Measurement* (Cambridge, MA: Harvard University Press, 1968).

11 René Descartes, *Principles of Philosophy*, trans. Valentine Rodger Miller and Reese P. Miller (Dordrecht: D. Reidel, 1983); gravity and magnetism are discussed in Part 4, the former in Propositions 20 through 27 and the latter in Propositions 133 through 183.

12 In a letter of 1690 from Huygens to Leibniz; see *Œuvres complètes de Christiaan Huygens*, vol. 9 (1901), p. 538.

13 Galileo Galilei, *Dialogues concerning Two New Sciences*, trans. Henry Crew and Alfonso de Salvio (Buffalo: Prometheus Books, 1991).

14 Christiaan Huygens, *The Pendulum Clock; or, Geometrical Demonstration concerning the Motion of Pendula as Applied to Clocks*, trans. Richard J. Blackwell (Ames: Iowa State University Press, 1986).

15 Galileo Galilei, *Dialogue concerning the Two Chief World Systems*, 2nd edn, trans. Stillman Drake (Berkeley: University of California Press, 1967). Newton read the English translation by Thomas Salusbury, published in 1661.

16 For example, Robert Anderson's *The Genuine Use and Effects of the Gun*; Kenelm Digby's "The Nature of Bodies" in his *Two Treatises*; and Walter Charleton's *Physiologia: Epicuro-Gassendo-Carltoniai, or*

A Fabrick of Science Natural, Upon the Hypothesis of Atoms. Newton either owned copies or copied out portions of each of these. I thank I. B. Cohen for this point.

17 See Newton, *Principia*, pp. 444 and 446.

18 See Huygens, *The Pendulum Clock*, Proposition 25, p. 69.

19 In his *Horologium Oscillatorium* Huygens expressly calls the three opening principles (the first of which is the principle of inertia) "hypotheses" (p. 33). Apparently following Huygens, Newton too called the forerunners of his laws of motion "hypotheses" in his tract, "De motu corporum in gyrum," the seed from which the *Principia* grew; the change to "laws" appears first as a correction to "hypotheses" in the revised version of this tract. See D. T. Whiteside (ed.), *The Preliminary Manuscripts for Isaac Newton's 1687* Principia: *1684–1686* (Cambridge: Cambridge University Press, 1989), pp. 3 and 13.

20 The term is Arthur Prior's.

21 See Newton, *Principia*, Book 2, Proposition 30 and 31, pp. 708–12.

22 Newton, *Principia*, Book 1, Proposition 45, pp. 539–45. This proposition is discussed in See Ram Valluri, Curtis Wilson, and William Harper, "Newton's Apsidal Precession Theorem and Eccentric Orbits," *Journal for the History of Astronomy* 28 (1997), 13–27.

23 Newton's use of such measurements has been discussed in several places by William Harper; see his chapter in this volume.

24 Huygens presents his simple pendulum measurement in Part 4 of his *Horologium Oscillatorium*, Proposition 26 (*The Pendulum Clock*, pp. 170–2), and he describes a conical pendulum measurement in Part v (pp. 173–5). See chapters 2–4 of Joella Yoder's *Unrolling Time: Christiaan Huygens and the Mathematization of Nature* (Cambridge: Cambridge University Press, 1988) for a discussion of the original measurements Huygens carried out in 1659.

25 E. W. Strong makes clear the indispensability of measurement to Newton's "mathematical way" in his "Newton's 'Mathematical Way'," cited in note 8 above. Unfortunately, the passage from the English translation of Newton's *System of the World* from which Strong develops his essay appears to be spurious, added by the translator; Strong's argument, however, requires no recourse to this passage.

26 Newton, *Principia*, p. 407.

27 Huygens lists 13 propositions on centrifugal force, a term he coined, at the end of his *Horologium Oscillatorium* (*The Pendulum Clock*, pp. 176–8). A full manuscript including proofs was published in 1703, in the edition of his posthumous papers prepared by de Volder and Fullenius. See *Œuvres complètes de Christiaan Huygens*, vol. 16 (1929), pp. 255–301.

28 This complaint was voiced most outspokenly by Robert Hooke; see p. 111 of *Isaac Newton's Papers and Letters on Natural Philosophy*, cited in note 5 above. Newton's mathematical treatment of rays of light is discussed in Alan Shapiro's chapter in this volume.

29 Newton, *Principia*, p. 448.

30 This requirement is met in the case of resistance forces because the velocity which determines their direction is the velocity of the resisted body *relative* to the fluid medium.

31 Papers summarizing the "laws of motion" by Wallis and Wren appeared in *Philosophical Transactions of the Royal Society* in the spring of 1669 (pp. 864–8), followed shortly after (pp. 925–8) by a summary of the theorems of Huygens, who had in effect refereed the papers by Wallis and Wren. Huygens's beautiful proofs of his account of impact did not appear in print until his posthumous papers were published in 1703; see *Œuvres complètes de Christiaan Huygens*, vol. 16, pp. 29–91.

32 A. Rupert Hall and Marie Boas Hall (eds.), *Unpublished Scientific Papers of Isaac Newton* (Cambridge: Cambridge University Press, 1962), p. 307.

33 The word "phenomena" for Newton does not refer to individual observations, but to inductively generalized summaries of observations, such as Kepler's area rule.

34 The word "rules" best describes Kepler's famous orbital claims at the time Newton was writing the *Principia*. They came to be called "laws" only after the *Principia* was published – first apparently in Leibniz's *Illustrio Tentaminis de Motuum Coelestium Causis* of 1689 (a translation of which can be found in Domenico Bertolini Meli's *Equivalence and Priority: Newton versus Leibniz* [Oxford: Oxford University Press, 1993], pp. 126–42).

35 Streete's *Astronomia Carolina*, from which Newton first learned his orbital astronomy, was published in 1661. Streete's claim that the orbits are stationary was challenged in Vincent Wing's *Examen Astronomiae Carolinae* of 1665, and then defended anew in Streete's *Examen Examinatum* of 1667.

36 See Curtis Wilson, "Predictive Astronomy in the Century after Kepler," in René Taton and Curtis Wilson (eds.), *Planetary Astronomy from the Renaissance to the Rise of Astrophysics, Part A: Tycho Brahe to Newton* (Cambridge: Cambridge University Press, 1989), pp. 172–85.

37 *Ibid.*, pp. 168 and 179.

38 Thus we find Robert Hooke, in the correspondence of 1679–80 with Newton that initiated his key discoveries on orbital motion, asking Newton to calculate the curve described by a body under inverse-square forces, and remarking, "this curve truly calculated will show the error of those many lame shifts made use of by astronomers to approach the true

motions of the planets with their tables." (*The Correspondence of Isaac Newton*, vol. 2, ed. H. W. Turnbull [Cambridge: Cambridge University Press, 1960], p. 309.)

39 Newton, "De motu corporum in gyrum," in D. T. Whiteside (ed.), *The Mathematical Papers of Isaac Newton*, vol. 6 (Cambridge: Cambridge University Press, 1974), pp. 30–74.

40 *Ibid.*, pp. 74–80. An English translation of the augmented version of "De motu" can be found in *Unpublished Scientific Papers of Isaac Newton*, cited in note 32 above, pp. 239–92. The English translation given here is from Curtis Wilson, "The Newtonian Achievement in Astronomy," in Taton and Wilson (eds.), *Planetary Astronomy*, p. 253.

41 See William Harper and George E. Smith, "Newton's New Way of Inquiry," in Jarrett Leplin (ed.), *The Creation of Ideas in Physics: Studies for a Methodology of Theory Construction* (Norwell: Kluwar, 1995), pp. 133–9.

42 Galileo, *Two New Sciences*, cited in note 13 above, p. 252.

43 René Descartes, *The Philosophical Writings of Descartes*, vol. 3, trans. John Cottingham, Robert Stoothoff, Dugald Murdoch, and Anthony Kenny (Cambridge: Cambridge University Press, 1991), pp. 9ff.

44 Descartes, *Principles*, cited in note 11 above, p. 98.

45 Thus, Newton remarked in a response to objections to his early publications in optics,

> For the best and safest method of philosophizing seems to be, first to inquire diligently into the properties of things, and establishing those properties by experiments and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments. For if the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties. (Cohen, *Isaac Newton's Papers and Letters on Natural Philosophy*, cited in note 5 above, p. 106)

Newton's attitude toward hypotheses in his work is optics in discussed in detail in Alan Shapiro's chapter in this volume.

46 Cohen, *The Newtonian Revolution*, cited in note 8 above, ch. 3; see his chapter in this volume as well.

47 The term "point-mass" is Euler's, not Newton's or Huygens's.

48 Newton's "deduction" of universal gravity from phenomena is examined in detail in William Harper's chapter in this volume.

49 For details, see my "From the Phenomenon of the Ellipse to an Inverse-Square Force: Why Not?," in David Malament (ed.), *Reading Natural Philosophy: Essays in the History of Science and Mathematics to Honor Howard Stein on his 70th Birthday* (La Salle: Open Court, 2002).

50 Newton, *Principia*, pp. 817ff.
51 Newton, *Principia*, textual note bb, p. 827.
52 See Huygens, *Discours de la Cause de la Pesanteur*, in *Œuvres complètes de Christiaan Huygens*, vol. 21 (1944), pp. 462–71, and pp. 476ff.
53 For a discussion of the current state of these discrepancies, see Kurt Lambeck, *Geophysical Geodesy*: The *Slow Deformations of the Earth* (Oxford: Oxford University Press, 1988).
54 See Leibniz, *Tentamen*, cited in note 34 above.
55 The one notable exception is the tacit assumption that the third law of motion holds between the Sun and the individual planets. This assumption has been pointed out by Howard Stein in his "'From the Phenomena of Motions to the Forces of Nature': Hypothesis or Deduction?" (*PSA* 2 [1990], 209–22); Dana Densmore in her *Newton's* Principia: *The Central Argument* (Santa Fe: Green Lion Press, 1995), p. 353; and before them by Roger Cotes, the editor of the second edition of the *Principia*, in correspondence with Newton (see *The Correspondence of Isaac Newton*, vol. 5, ed. A. Rupert Hall and Laura Tilling [Cambridge: Cambridge University Press, 1975], pp. 391ff). William Harper's chapter in this volume discusses this and the other details of Newton's "deduction" of universal gravity from phenomena.
56 Newton, *Principia*, p. 796.
57 The history of Newton's third Rule for Natural Philosophy is discussed in I. Bernard Cohen's *Introduction to Newton's "Principia"* (Cambridge, MA: Harvard University Press, 1978), pp. 23–6.
58 Nelson Goodman, *Fact, Fiction, and Forecast*, 3rd edn (Indianapolis: Bobbs-Merrill, 1973).
59 Jean d' Alembert, *Essai d'une Nouvelle Théorie de la Résistance des Fluides* (Paris: David, 1752).
60 Newton, by the way, took the trouble in Book 1, Section 10 to show that Galileo's and Huygens's results similarly hold in the limit in the case of *universal* gravity, namely the limit of the linear variation of gravity up to the surface of a uniformly dense Earth as the radius of this surface approaches infinity. This result authenticates Newton's use of Huygens's precise theory-mediated measurement of surface gravity in his crucial argument in Book 3, Proposition 4 that the Moon is held in orbit by terrestrial gravity.
61 See the chapter by Howard Stein in this volume for a discussion of the centrality of interactions in Newton's metaphysics.
62 See George E. Smith, "Fluid Resistance: Why Did Newton Change His Mind?," in Richard Dalitz and Michael Nauenberg (eds.), *Foundations of Newtonian Scholarship* (Singapore: World Scientific, 2000), pp. 105–36.
63 Newton, *Principia*, p. 749.

64 See L. D. Landau and E. M. Lifshitz, *Fluid Mechanics*, vol. 6 in *Course in Theoretical Physics* (Oxford: Pergamon, 1959), pp. 31–6, 168–79.

65 See George E. Smith, "The Newtonian Style in Book 2 of the *Principia*," in J. Z. Buchwald and I. B. Cohen (eds.), *Isaac Newton's Natural Philosophy* (Cambridge, MA: MIT Press, 2001), pp. 249–98, esp. p. 278, Fig. 9.7.

66 *Ibid.*, pp. 276–87.

67 Isaac Newton, *Opticks:* or, *A Treatise of the Reflections, Refractions, Inflections and Colours of Light* (New York: Dover, 1952), p. 404. The quotation continues: "This is the Method of Analysis: And the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phenomena proceeding from them, and proving the Explanations." This passage was undoubtedly a direct response to Huygens's description of the hypothetico-deductive method quoted at the beginning of this chapter.

68 Extending gravity to comets was more important than first meets the eye. Hooke had expressed a general principle of celestial attraction in his *Attempt to Prove the Motion of the Earth* of 1674, but had denied that it extends to comets in his *Cometa* of 1678. See Curtis Wilson, "The Newtonian Achievement in Astronomy," p. 239.

69 Newton indicates as much in a letter to Leibniz in 1693 when he defends the *Principia* by remarking, "all phenomena of the heavens and the sea follow precisely, so far as I am aware, from nothing but gravity acting in accordance with the laws described by me." (*The Correspondence of Isaac Newton*, vol. 3, ed. H. W. Turnbull [Cambridge: Cambridge University Press, 1961], pp. 284 ff.)

70 See Curtis Wilson's chapter in this volume for a discussion of the development of celestial mechanics during the eighteenth century. This development culminates in the five volumes of Laplace's *Mécanique Céleste*, the first four of which appeared from 1798 to 1805, and the fifth in 1825. (All but the fifth volume are available in English in the translation of 1829–39 by Nathaniel Bowditch [Bronx, NY: Chelsea Publishing Company, 1966].)