Michael Friedman on Kant and Newton

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RÉSUMÉ : La section 1 introduit l'idée de Kant selon laquelle des principes purement empiriques ne seraient pas appropriés pour la certitude apodictique que les physiciens mathématiciens, comme Newton, souhaitaient atteindre avec leurs lois de la nature. Elle introduit aussi un corollaire aux lois du mouvement de Newton qui est central à la discussion par Michael Friedman de l'entreprise kantienne d'examiner les principes postulés par Newton du point de vue de leurs sources a priori. Dans la section 2, on porte attention à d'autres corollaires des lois du mouvement de Newton qui limitent singulièrement l'intérêt de ce que Friedman voit comme une tentative de la part de Kant d'utiliser l'idée du centre d'une masse là où Newton recourait — à tort, selon Kant — à l'espace absolu. La section 3 entend montrer que l'argument transcendantal pour l'universalité de la constante gravitationnelle, qui est attribué à Kant par Friedman, n'ajoute rien à la défense qu'en a fournie Newton. On suggère ensuite, à la section 4, que ce furent les succès empiriques des applications qui rendirent acceptable la distinction entre le mouvement absolu et le mouvement purement relatif que Friedman tient pour l'élément-clé de l'argument transcendantal de Kant. Dans la section 5, un examen attentif des postulats de la pensée empirique selon Kant révèle que le recours newtonien aux phénomènes dans la défense de la gravitation universelle est compatible avec l'attribution à cette loi de la nature d'une certitude apodictique convenant à la nécessité matérielle telle qu'elle est définie dans le troisième postulat de Kant. La section 6, enfin, en portant attention au détail de l'argument de Newton pour l'universalité de la constante gravitationnelle, montre que ce que Newton voit comme une inférence inductive à partir des phénomènes permet de légitimer beaucoup plus que l'universalité toute relative en vertu de laquelle nous ne pourrions rien dire d'autre que ceci : nos observations jusqu'ici n'ont repéré aucune exception à telle ou telle règle.

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In my review of Michael Friedman's book, *Kant and the Exact Sciences,*[1] I suggested that, though the position he attributes to Kant is informed and ingenious, it is not clear that it offers any epistemic advantage over Newton's treatment of space and scientific method. The issues raised by confronting Friedman's and Kant's remarks with some details of Newton's practice are quite illuminating, not just for understanding Kant and Newton, but also for understanding important aspects of scientific practice more generally.

1.

In books one and two of *Principia,*[2] Newton develops propositions from his Laws of Motion which make possible measurement of forces by phenomena of motion. In book three these are applied to argue from phenomena to Universal Gravitation in propositions 1-7. This initial argument is then backed up by applications in which universal gravitation resolves the two chief world systems problem and explains phenomena such as the tides, precession of the equinoxes, lunar motions, and motions of comets. In these applications Newton's aim is not just prediction but prediction backed up by accurate measurement from phenomena of theoretical parameters, such as the masses of the sun and planets.

It was the extraordinary realization of this sort of empirical success in the hands of such successors as Clairout, Euler, Lagrange, and Laplace that led first to the defeat of the rival mechanical philosophy and, then, to the installation of Newton's new way of enquiry as the paradigm that transformed natural philosophy into natural science. For Newton and his successors in the development of this science, it would seem that it was enough that the accepted laws of motion generated measurements which allowed Universal Gravitation to be gathered from phenomena by induction and that this theory of gravitation clearly surpassed any rivals at realizing empirical successes of prediction backed up by accurate measurement of theoretical parameters.

As Friedman suggests, this may not have been enough for Kant. Consider the following passage from Kant's *Metaphysical Foundations of Natural Science* (*MFNS*), which Friedman quotes (1992, p. 137):

> Thus these mathematical physicists could certainly not avoid metaphysical principles and among those certainly not such as to make the concept of their proper object, namely matter, a priori suitable for application to outer experience: as the concepts of motion, the filling of space, inertia, etc. However, they rightly held that to let merely empirical principles govern these concepts would be absolutely inappropriate to the apodeictic certainty they wished their laws of nature to possess; they therefore preferred to postulate such principles, without investigating them in accordance with their a priori sources.

Here, it would seem that Newton's laws of motion and the corresponding definitions of centripetal force, absolute space, time, and motion are paradigmatic of the principles referred to, and that Universal Gravitation is paradigmatic as a law of nature. If this is correct, then Kant is suggesting that merely empirical principles would be inappropriate to ground the argument to Universal Gravitation. According to this suggestion, Newton should have investigated his laws of motion "in accordance with their *a priori* sources" in order to do justice to the "apodeictic certainty" he wished Universal Gravitation to possess.

Kant proposes arguments to demonstrate *a priori* his version of the Laws of Mechanics. The relevant chapter of *MFNS* follows a chapter on phoronomy in which Newton's idea of absolute space is rejected and a chapter on dynamics in which the mechanical philosophers' objections to forces acting at a distance are also rejected. Friedman concentrates on Kant's last chapter, "Metaphysical Foundations of Phenomenology." He sees it as an extension of Newton's appeal to corollary 4 of his Laws of Motion to resolve the two chief worlds system problem by showing that the centre of mass of the solar system is "never very far from the sun's center." According to Newton's corollary 4,

> The common center of gravity of two or more bodies does not change its state whether of motion or of rest as a result of the actions of the bodies upon one another; and therefore the common center of gravity of all bodies acting upon one another (excluding external actions and impediments) either is at rest or moves uniformly straight forward. (Cohen and Whitman 1999, p. 421; Cajori 1934, p. 19)

This corollary of Newton's is central to two of the themes I want to comment on. One is Friedman's discussion of Kant's use of the idea of the centre of mass of all bodies in the universe as a substitute for—what Kant takes to be—Newton's objectionable appeal to absolute space. The other is his discussion of Kant's chapter on phenomenology as a sort of transcendental deduction of the immediacy and universality of gravitation between bodies.

2.

The following quotation articulates central features of Friedman's account of Kant's appeal to the centre of mass as an alternative to Newton's absolute space.

> For Kant, the center of mass of the solar system is not strictly privileged: the solar system itself experiences a slow rotation around the center of mass of the Milky Way galaxy, and the latter experiences a slow rotation around the center of mass of the entire cosmic system of the galaxies. In the end, only the forever

> unreachable "common center of gravity of all matter" can furnish us with a truly privileged frame of reference, and our procedure for "reducing all motion and rest to absolute space" never terminates: absolute space is an idea of reason. (Friedman 1992, p. 149)

Just what is the advantage over Newton's absolute space supposed to be? Neither it nor the centre of mass of the universe counts as a possible object of experience. Friedman emphasizes the constructive procedure that lets one tell objectively when one approximation is better than another. His quotation suggests that, on Kant's view, the centre of mass of a more inclusive system of bodies overrides the centre of mass of any subsystem.

One advantage of Kant's concentration on the outcome actually reached at any stage might be that it would—up to tolerances of measurement—pick out a single frame. Newton's corollary 5 of his Laws of Motion points out that any Galilean transformation of an inertial frame is as good as any other.

> When bodies are enclosed in a given space, their motions in relation to one another are the same whether the space is at rest or whether it is moving uniformly straight forward without circular motion. (Cohen and Whitman 1999, p. 423; Cajori 1934, p. 20)

Kant's centre of mass idea shares with Newton's definition of absolute space the feature of going beyond the family of interchangeable inertial frames to pick a specific frame which counts as absolute rest. Should we count such an appeal to Kant's constructive procedure as an improvement that would turn Newton's idea of absolute rest from a mistaken step beyond what the laws of motion allow to something that counts as legitimately approximated by the outcome of any stage?[3]

To construe Kant's explicit limitation to the outcomes of centre of mass constructions as an improvement on Newton's treatment would seem to ignore Newton's corollary 6 of his Laws of Motion.

> If bodies are moving in any way whatsoever with respect to one another and are urged by equal accelerative forces along parallel lines, they will all continue to move with respect to one another in the same way as they would if they were not acted upon by those forces. (Cohen and Whitman 1999, p. 423; Cajori 1934, p. 21)

How well do the centre of mass of Jupiter or the centre of mass of the system of Jupiter together with its moons approximate reference frames that pick out true motions? According to Kant's suggestion the centre of mass of the Jupiter-moons system would be only a slight improvement on the centre of mass of Jupiter (because Jupiter is so much more massive than its moons), but both are wildly bad approximations because the Jupiter system is not even approximately at rest (nor in uniform motion) with respect to the centre of mass of the solar system. According to corollary 6 of Newton's laws, however, how well the Jupiter frames approximate ones adequate to fix what can count as true motions of Jupiter's moons depends only on the extent to which the accelerations toward other bodies (such as the sun) are approximately equal and parallel.[4] If these approximations are good enough then such local frames are good enough to specify motions that measure gravitational forces of interaction among bodies in the Jupiter system. As we shall see, such local frames will satisfy Friedman's specific constructive procedure for checking adequacy to specify true motions among bodies in a given system.

The very great distance to the nearest stars effectively isolates interactions among solar system bodies from even very great, gravitational accelerations of the solar system with respect to larger systems. One implication of corollary 6 is that, as far as interactions among solar system bodies are concerned, the centre of mass of our galaxy, or system of galaxies, would be no better approximation to an appropriate frame for distinguishing true from merely relative motions than the centre of mass of the solar system.

3.

According Kant, Newton's appeal to the Laws of Motion needs to be backed up by investigations of their *a priori* status. Newton tells his readers, "The principles I have set forth are accepted by mathematicians and confirmed by experiments of many kinds" (Cohen and Whitman 1999, p. 424; Cajori 1934, p. 21). Friedman sees Kant as arguing that the Laws of Motion are not empirical facts, but, instead, are conditions under which alone the notion of true motion has objective meaning.

> Thus, Newton presents the laws of motion as facts, as it were, about a notion of true motion that is antecedently well defined. Accordingly, he attempts to provide empirical evidence for their truth—especially in the case of the third law. For Kant, on the other hand, since there is no such antecedently well-defined notion of true motion, the laws of motion are not facts but rather conditions under which alone the notion of true motion first has objective meaning. And, as we have seen, the third law is particularly important in this regard, for the true motions are defined relative to the common center of mass of the system of interacting bodies in question: in other words, true motions are just those satisfying the third law.[5] (Friedman 1992, p. 171)

As we shall see, Friedman sees Kant's appeal to the centre of mass construction for distinguishing true motions as a transcendental argument not just for the laws of motion but for the universality of gravitation as well.

Let us now turn to the transcendental argument itself. A key step is Friedman's discussion of that most delicate part of Newton's argument for Universal Gravitation—his case for proposition 7, book 3.

> Gravity exists in all bodies universally and is proportional to the quantity of matter in each. (Cohen and Whitman 1999, p. 810; Cajori 1934, p. 414)

Here is the part of Newton's argument Friedman focuses on.

> We have already proved that all planets are heavy [gravitate] toward one another and also that the gravity toward any one planet, taken by itself, is inversely as the square of the distance of places from the center of the planet. And it follows (by Bk 1, Prop. 69 and its corollaries) that the gravity toward all the planets is proportional to the matter in them. (Cohen and Whitman 1999, p. 810; Cajori 1934, p. 414)

Here is proposition 69, book 1:

> If, in a system of several bodies A, B, C, D, . . . , some body A, attracts all the others, B, C, D, . . . , by accelerative forces that are inversely as the squares of the distances from the attracting body; and another body B also attracts the rest of the bodies A, C, D, . . . , by forces that are inversely as the squares of the distances from the attracting body; then the absolute forces of the attracting bodies A and B will be to each other in the same ratio as those very bodies [i.e., the masses] A and B themselves to which those forces belong. (Cajori 1934, p. 191)

The key step in Newton's proof of this proposition is as follows:

> But the accelerative attraction of body B toward A is to the accelerative attraction of body A toward B as the mass of body A is to the mass of body B, because the motive forces—which (by defs. 2, 7, and 8) are as the accelerative forces and the attracted bodies jointly—are in this case (by the third law of motion) equal to each other. (Cohen and Whitman 1999, p. 507; Cajori 1934, p. 191)

Friedman illustrates Kant's transcendental argument by having the system consist of two planets, Jupiter and Saturn, with their respective satellites.

> We know, by the first property of gravitational acceleration, that the acceleration-field on Saturn's moons is given by $a_1 = k_s/r_1 2$ and the acceleration-field on Jupiter's moons is given by $a_2 = k_j/r_2 2$. We want to show that when $r_1 = r_2$, $a_1/a_2 = k_s/k_j = m_s/m_j$, where m_s and m_j are the masses of Saturn and Jupiter

respectively. To do so we assume that the acceleration-fields of our two planets extend far beyond their respective satellites, so that we also have an acceleration $a_j = k_s/r^2$ of Jupiter and an acceleration $a_s = -k_j/r^2$ of Saturn, where r is now the distance between the two planets. But, according to the third law of motion, $m_j a_j = -m_s a_s$. Therefore, we have $m_s/m_j = -a_j/a_s = k_s/k_j$, as desired. We are now—*and only now*—in a position to compare the masses of Jupiter and Saturn by reference to the acceleration-fields on their respective satellites, that is by reference to k_j and k_s. (Friedman 1992, p. 155, emphasis added)

Friedman drives his point home in a footnote to this passage:

Using just the third law of motion and our first property of gravitational acceleration discussed above we can show the following: for any two gravitationally interacting masses m_A and m_B, there is a constant G_{AB} such that $F_{AB} = -F_{BA} = G_{AB}m_A m_B/r^2$. We need the *universality* of gravitational interaction, however, to conclude that G_{AB} is the same constant for every such pair; that is that G is a "universal constant." It now—*and only now*—follows that, universally, $k_A = Gm_A$. (1992, p. 155 note, emphasis added)

What are we to make of Friedman's "only now"s? Consider a sceptic who doubts that G_{AB} for gravitational interaction between A and B is the same constant as G_{CD} for gravitational interaction between C and D. Kant, surely, would not want to say that the sceptic's position is viable until we can actually measure the relevant accelerations. Presumably, he posits that the relevant accelerations all satisfy a single constant until such time as a sceptic would actually deliver on interactions that violated this assumption.

For Newton, using the same units of distance (Astronomical Units) and time (sidereal days) for harmonic law ratios from Jupiter's moons as for the harmonic law ratio of Venus's orbit of the sun allows comparison of the mass of Jupiter with the mass of the sun (corollary 1, proposition 8, book 3; Cajori 1934, p. 416). No explicit appeal to direct interactions between Jupiter and Venus, or even between Jupiter and the sun, is made. Newton's practice is to assume that the same constant governs both interactions until such time as this assumption would actually run into trouble. I see no epistemic advantage of Kant's treatment over Newton's.

There is, however, some advantage for Newton in not being saddled with what Friedman takes to be Kant's *a priori* commitment to measuring masses by the active gravitation they produce. For one thing Kant's commitment only makes sense for bodies massive enough to produce measurable gravitational effects on other bodies. How can this somewhat accidental feature fix universal gravitation as a necessary condition for distinguishing true motions? Suppose that, instead of looking at perturbations produced by them on other bodies, we most accurately measured masses of planets by pushing them, ever so carefully, with rockets equipped with Kantian repulsive force generators. Would this undercut Friedman's version of Kant's story?

4.

The distinction between true and merely relative motions Friedman sees as Kant's premise for a transcendental argument is just what Berkeley objected to. According to Berkeley, thought experiments and unbiased reflection are sufficient to reveal that only relative motions can be empirically established.[6] For Berkeley, the claim that only relative motions can be empirically established counts as *a priori*. For Newton, whether or not true motions can be empirically determined is itself an empirical question to be decided by the empirical successes supporting background assumptions like the laws of motion.[7] The transcendental argument attributed to Kant by Friedman would not help against a sceptic like Berkeley who rejected the fundamental premise.

Howard Stein (1991) has identified Newton's appeal to proposition 69 as an appeal to the hypothesis that the third law of motion can be applied to gravitation between bodies as though they were directly attracting each other. Huygens and Leibniz objected to this hypothesis.[8] As vortex theorists they regarded the appropriate application of the third law of motion to be between each body and the vortical particles pushing it. Perhaps the point of the transcendental argument Friedman attributes to Kant is to show that the application of the third law Stein identifies as a questionable hypothesis is unavoidable. In a passage that strongly suggests the transcendental argument attributed to him by Friedman, Kant accuses Newton of putting himself at variance with himself when he tries to allow that gravitation might not be essential to bodies.

> One can well note that the offense which his contemporaries and perhaps he himself took at the concept of an original attraction made him at variance with himself. For he *absolutely could not say* that the attractive forces of two planets, e.g. Jupiter and Saturn, which they manifest at equal distances of their satellites (whose mass is unknown), are proportional to the quantity of matter of these heavenly bodies, *unless he assumed* that they merely as matter, and hence according to a universal property of the same, attracted other matter. (*MFNS*, observation 2, proposition 7, chap. 2; Kant 1970, p. 66, emphasis added)

Kant's "absolutely could not say . . . unless he assumed," like Friedman's "only now"s, suggest that without direct attraction between planets Newton's centre of mass construction for distinguishing true from merely relative motions of solar system bodies could not be carried out. Our criticism of Friedman's "only now"s suggests that such transcendental arguments do not add to the warrant provided by Newton's empirical argument.

Indeed, even if such an argument could provide additional warrant, to have it do so the key premise that one can distinguish true from merely relative motions needs to be established. It seems clear that it was empirical successes, rather than additional transcendental arguments, that did the establishing. As Stein points out, Newton's argument for Universal Gravitation includes all the applications in the rest of book 3, in addition to the explicit argument from phenomena in propositions 1-7. Later researchers such as Clairaut, Euler, and others greatly extended and improved upon Newton's initial empirical successes.[9] Perhaps it was the indirect support afforded to Newton's laws of motion by these successful applications that, by Kant's time, made such laws plausible as candidates for *a priori* status.

The central theme of Euler's 1748 defence of Newton's distinction between true and merely relative motion against Leibnizian metaphysicians is the enormous warrant he takes the empirical successes of applications to provide for Newton's laws of motion. Euler argues:

> The principles of mechanics have already been established on such a sound basis that one would greatly err if he wished to encourage any doubt about their validity. Even if one were not in position to demonstrate them by the use of general principles of metaphysics, the excellent agreement of all the conclusions which one draws from them by means of the calculus, with all the movements of bodies both solid and liquids, on the earth, and likewise with the movements of the heavenly bodies, would be sufficient to place the truth of the principles of mechanics beyond doubt. (See Koslow, 1967, p. 116)

The policy advocated by Euler, according to which it is such empirical success rather than metaphysical principles that should guide research, was central to the transformation of natural philosophy into natural science.

5.

The certainty Newton claims for the universality of the constant of proportionality of gravitation to mass is that expressed in his fourth Rule of Reasoning:

> 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 (Cohen and Whitman 1999, p. 796; Cajori 1934, p. 400)

This is a certainty which Newton claims should be accorded to propositions gathered from phenomena by induction. To what extent is this something Kant might want to count as "apodeictic certainty?" The "notwithstanding any contrary hypotheses" suggests a fairly robust sense of "certainty," even if the addition of "very nearly true" (as an alternative to "exactly true") suggests that what counts as "certain" might be limited to the approximate truth of propositions gathered from phenomena by induction. It endorses the policy discussed above, according to which sceptical doubts about the universality of the constant of proportionality of gravitation to mass are dismissed until such time as a sceptic can deliver on actual phenomena that require correcting this proposition.[10] In the next section we shall attempt to clarify what Newton regards as sufficient to count as "gathering a proposition from phenomena by induction" by looking at his argument for proposition 6, book 3. This is his argument for the universality of the constant proportion of weight to mass.

For Kant the "apodeictic certainty" appropriate to true universality requires the necessity that goes with the *a priori*, while induction confers only assumed and comparative universality.

> First, then, if we have a proposition which in being thought is thought as *necessary*, it is an *a priori* judgment; and if, besides, it is not derived from any proposition except one which also has the validity of a necessary judgment, it is an absolutely *a priori* judgment. Secondly, experience never confers on its judgments true or strict but only, assumed and comparative *universality*, through induction. We can properly only say, therefore, that, so far as we have hitherto observed, there is no exception to this or that rule. If, then, a judgment is thought with strict universality, that is, in such manner that no exception is allowed as possible, it is not derived from experience, but is valid absolutely *a priori*. Empirical universality is only an arbitrary extension of a validity holding in most cases to one which holds in all, for instance, in the proposition, "all bodies are heavy." (Kant 1963, pp. 43-44)

The openness to correction by new phenomena built into Newton's fourth rule does seem to rule out the strict universality that goes with counting as valid absolutely *a priori*. Does this not suggest that, for Kant, the certainty Newton wants for the universality of the gravitational constant as "gathered from phenomena by induction" can only be "an arbitrary extension of a validity holding in most cases to one which holds in all?" Is such a suggestion not further supported by Kant's use of "all bodies are heavy" as his example of a proposition having this merely empirical universality conferred by experience through induction? More generally, how could Friedman's attribution to Kant of a transcendental deduction of the universality of the gravitational constant allow Kant to regard

Newton's appeal to phenomena as essential to the evidence for this proposition?

Careful attention to Kant's passage shows that it makes room for a proposition which is thought as necessary even though it may, in part, be derived from propositions which do not have the validity of necessary judgements. Thought of such a proposition counts as an *a priori* judgement, but not as an absolutely *a priori* judgement. This weaker notion of *a priori* is apparently required by Kant's official definition of necessity in the postulates of empirical thought (A 218, B 265-266; Kant 1963, p. 239):

1. That which agrees with the formal conditions of experience, that is, with the conditions of intuition and concepts, is *possible.*

2. That which is bound up with the material conditions of experience, that is, with sensation, is *actual.*

3. That which in its connection with the actual is determined in accordance with universal conditions of experience, is (that is, exists as) *necessary.*

According to these definitions, the necessity defined in 3 is not the one that corresponds, by what we now regard as the standard relation ($\Box A \leftrightarrow \sim\Diamond\sim A$), to the possibility defined in 1.[11] Kant is quite clear that judgements of actuality require appeal to experience; therefore, the material necessity he defines in 3 also requires appeal to experience. This differentiates it from the more formal necessity corresponding to the possibility defined in 1.

Suppose the sense of "*a priori*" involved in Friedman's attribution to Kant of a transcendental deduction of the universality of the gravitational constant is the weaker sort that corresponds to the material necessity defined in the third postulate.[12] This suggests that Kant need not regard Newton's appeal to phenomena as incompatible with establishing apodeictic certainty of the universality of the gravitational constant.

The phenomena from which Newton wants to gather propositions by induction are themselves generalizations. Consider Newton's inferences from Kepler's area law to centripetal forces. Newton derives systematic dependencies that make the constancy of the rate at which areas are swept out by radii to the centre measure the centripetal direction of the force deflecting a body into an orbit.[13] It can be argued that the general assumptions appealed to in these derivations are all what Kant would have regarded as appropriately universal conditions on objects of experience.[14] What we need to consider, in addition, is what would be required to have the area law count as actual. For Kant it may not be enough to have the merely assumed and comparative generality conferred by what he calls "induction." He wants more than an arbitrary extension to future astronomical data of what, strictly speaking, should be limited to the claim that the area law fits the data we have up to now.

Kant was well aware that perturbations due to gravitational interactions with other planets make the area law for any given orbit hold only approximately. The striking empirical successes of prediction based on accurate measurement of parameters by phenomena were applications of perturbation theory developed by Euler and other successors. Central to these developments was having corrections of Keplerian phenomena to account for perturbations that provided quite significant improvements of fit with the increasingly precise data that became available. These developments went hand in hand with increasingly accurate measurements of the masses, and therefore the inverse square centripetal acceleration fields, for the sun and planets.[15]

I want to suggest that the corrected orbit, taking into account all known perturbations, would have a sort of apodeictic certainty that Kant would regard as going beyond the merely assumed and comparative universality conferred by what he calls "induction." Such a corrected orbit, based on accurate measurements of masses and motions, would be more than an arbitrary extension to future astronomical data of what, strictly speaking, should be limited to the claim that it fits the data we have up to now.[16] Similarly, someone who understands universal gravitation will be able to realize an apodeictic certainty not available to the man who avoids undermining the foundation of his house simply on the basis that his experience so far fits the generalization all bodies are heavy. If these suggestions are correct then the apodeictic certainty corresponding to Kant's weaker sort of *a priori* knowledge may be compatible with the openness to correction by future phenomena that Newton builds into his fourth rule.

6.

According to Friedman what Kant did find problematic was Newton's appeal to induction.

> In the *Principia* itself the remarkable extrapolation to *universal* gravitation is of course supported by quasi-inductive arguments .... Corollary II to Proposition VI—which Kant singles out for special criticism, it will be recalled—is typical: "Universally, all bodies about the earth gravitate towards the earth; and the weights of all, at equal distances from the earth's center, are as the quantities of matter which they severally contain. This is the quality of all bodies within the reach of our experiments; and therefore (by Rule III) to be affirmed of all bodies whatsoever" ([82], p. 574; [83], p. 413). The moon and all sublunary bodies experience an inverse-square acceleration towards the earth; therefore, all bodies whatsoever—no matter how distant—must also experience an inverse-square acceleration towards the earth! Such an extrapolation can certainly be ques-

tioned by reasonable men, as it was by intelligent critics such as Huygens and Leibniz. The irony is that Kant, in acknowledging the force of the criticisms of Newton's quasi-inductive arguments for universal gravitation put forward by Huygens and Leibniz, responds by giving an a priori foundation for precisely what they feared most: immediate action at a distance (to infinity) across empty space. (1992, p. 158, note 33)

Here is Newton's Rule III, referred to in the passage Friedman quotes from corollary 2 of proposition 6.[17]

Rule 3. Those qualities of bodies that cannot be intended and remitted [that is, qualities that cannot be increased and diminished] and that belong to all bodies on which experiments can be made should be taken as qualities of all bodies universally. (Cohen and Whitman 1999, p. 795; Cajori 1934, p. 398)

The application of this rule in the corollary Friedman cites can be illuminated by Newton's main argument for proposition 6.

This argument for proposition 6 is the heart of what Newton counts as the gathering of the universality of the constant of gravitation from phenomena by induction.

Proposition 6: All bodies gravitate toward each of the planets, and at any given distance from the center of any one planet the weight of any body whatever toward that planet is proportional to the quantity of matter which the body contains.

Others have long since observed that the falling of all heavy bodies toward the earth (at least on making an adjustment for the inequality of the retardation that arises from the very slight resistance of the air) takes place in equal times, and it is possible to discern that equality of the times, to a very high degree of accuracy by using pendulums. I have tested this with gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I got two wooden boxes round and equal. I filled one of them with wood, and I suspended the same weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes, hanging by equal eleven-foot cords, made pendulums exactly like one another with respect to their weight, shape, and air-resistance. Then when placed close to each other [and set into vibration], they kept swinging back and forth together with equal vibrations for a very long time. Accordingly, the amount of matter in the gold (by Bk 2, Prop. 24, corol. 1 and 6) was to the amount of matter in the wood as the action of the motive force upon all the [added] wood—that is, as the weight of one to the weight of the other. And so it was for the rest of the materials. In these experiments, in bodies of the same weight, a difference of matter that would be even less than a thousandth part of the whole would have been clearly noticed.(Cohen and Whitman 1999, pp. 806-807; Cajori 1934, p. 411)

Newton begins with a pendulum experiment that measures the direct proportionality of weight to inertial mass for samples of all these varied materials. To say that such direct proportionality holds is to say that there is a single constant giving the ratio in all these cases. For rule 3, the quality that cannot be increased or diminished in this experiment is the proportionality of weight to mass.

The equality of the periods of such pairs of pendulums counts as a phenomenon—a generalization fitting an open-ended body of data—insofar as the experiment is regarded as repeatable. Newton appeals to theorems about pendulums from his laws of motion to make the tolerances to which this phenomenon is established measure to three decimal places the constancy of the ratio between weight and mass. This illustrates how Newton's provision for "very nearly true" in rule 4 makes room for the precision to which phenomena measure parameter values.[18]

Newton next appeals to the moon test, which measured the agreement between the acceleration of gravity at the surface of the earth and the result of increasing the inverse-square centripetal acceleration of the lunar orbit to obtain what the corresponding acceleration at the surface of the earth would be.

> Now there is no doubt that the nature of gravity toward the planets is the same as toward the earth. For imagine our terrestrial bodies to be raised as far as the orbit of the moon and, together with the moon, deprived of all motion, to be released so as to fall to the earth simultaneously; and by what has already been shown, it is certain that in equal times these falling terrestrial bodies will describe the same spaces as the moon, and therefore that they are to the quantity of matter in the moon as their own weight is to its weight. (Cohen and Whitman 1999, p. 807; Cajori 1934, p. 411)

That different bodies have equal accelerations at any given distance exhibits that the earth's gravitation is an inverse-square acceleration field, not just an inverse-square force field. To have a gravitation field be an acceleration field is to have the same proportionality between weight and mass for all attracted bodies at any given distance. The equality of these accelerations at any given distance is a phenomenon that measures the constancy of the proportionality of weight to mass at each distance.[19]

Newton goes on to point out that inverse-square acceleration fields are exhibited by orbits satisfying Kepler's harmonic law—that periods are as the 3/2 power of distances.

> Further, since the satellites of Jupiter revolve in times that are as the 3/2 power of their distances from the center of Jupiter, their accelerative gravities toward Jupiter will be inversely as the squares of the distances from the center of Jupiter, and, therefore, at equal distances from Jupiter their accelerative gravities

> would come out equal. Accordingly, in equal times in falling from equal heights [toward Jupiter] they would describe equal spaces, just as happens with heavy bodies on this earth of ours. And by the same argument the circumsolar [or primary] planets, let fall from equal distances from the sun, would describe equal spaces in equal times in their descent to the sun. Moreover, the forces by which unequal bodies are equally accelerated are as the bodies; that is, the weights [of the primary planets toward the sun] are as the quantities of matter in the planets. (Cohen and Whitman 1999, p. 807; Cajori 1934, pp. 411-12)

The harmonic law for Jupiter's moons is a phenomena measuring the equality of ratios of gravitation toward Jupiter to the inertial masses of those moons, while the harmonic law for the primary planets measures the equality of such ratios for the gravitation of the planets toward the sun.[20]

Newton next appeals to the absence of observable polarization of orbits of Jupiter's moons with respect to the sun to measure that the moons and Jupiter are equally accelerated toward the sun by solar gravity.

> Further, that the weights of Jupiter and its satellites toward the sun are proportional to the quantities of their matter is evident from the extremely regular motion of the satellites, according to Bk 1, Prop. 65, Corol. 3. For if some of these were more strongly attracted toward the sun in proportion to the quantity of their matter than the rest, the motions of the satellites (by Bk 1, Prop. 65, Corol. 2) would be perturbed by that inequality of attraction. If, at equal distances from the sun, some satellite were heavier [or gravitated more] toward the sun in proportion to the quantity of its matter than Jupiter in proportion to the quantity of its own matter, in any given ratio, say d to e, the distance between the center of the sun and the center of the orbit of the satellite would always be greater than the distance between the center of the sun and the center of Jupiter very nearly and these distances would be to each other as the square root of d to the square root of e, as I found out by making a certain calculation. And if the satellite were less heavy [or gravitated less] toward the sun in that ratio of d to e, the distance of the center of the orbit of the satellite from the sun would be less than the distance of Jupiter from the sun in that same ratio of the square root of d to the square root of e. And so if, at equal distances from the sun, the accelerative gravity of any satellite toward the sun were greater or smaller than the accelerative gravity of Jupiter toward the sun, by only a thousandth of the whole gravity, the distance of the center of the orbit of the satellite from the sun would be greater or smaller than the distance of Jupiter from the sun by 1/2000 of the total distance, that is by a fifth of the distance of the outermost satellite from the center of Jupiter; and this eccentricity of the orbit would be very sensible indeed. But the orbits of the satellites are concentric to Jupiter, and therefore the accelerative gravities of Jupiter and of the satellites toward the sun are equal to one another.

Here again theorems are appealed to which make the tolerances to which a phenomenon—the absence of polarization of the orbits—is established to measure the constancy of the ratio of weight toward the sun to inertial mass for Jupiter and all of its satellites.[21]

The same argument also applies to the orbits of Saturn's moons, and to the earth and its moon if they also gravitate toward the sun.

> And by the same argument the weights [or gravities] of Saturn and its companions toward the sun, at equal distances from the sun, are as the quantities of matter in them; and the weights of the moon and earth toward the sun are either nil or exactly proportional to their masses. But they do have some weight, according to Prop. 5, Corol. 1 and 3. (Cohen and Whitman 1999, p. 808; Cajori 1934, pp. 412-13)

The unpolarizations of all these orbits are phenomena which measure the constancy of the proportionality of weight toward the sun and inertial mass for each planet and all its moons.[22]

Finally, Newton extends his argument for equal ratios between weight and inertial mass to individual parts of planets.

> But further, the weights [or gravities] of the individual parts of each planet toward any other planet are to one another as the matter in the individual parts. For if some parts gravitated more, and others less, than in proportion to their quantity of matter, the whole planet, according to the kind of parts in which it most abounded, would gravitate more or gravitate less than in proportion to the quantity of matter of the whole. But it does not matter whether those parts are external or internal. For if, for example, it is imagined that bodies on our earth are raised to the orbit of the moon and compared with the body of the moon, then if their weights were to the weights of the external parts of the moon as the quantities of matter in them, but were to the weights of the internal parts in a greater or lesser ratio, they would be to the whole moon in a greater or lesser ratio, contrary to what has been shown above. (Cohen and Whitman 1999, p. 809; Cajori 1934, p. 413)

The phenomena measuring the constancy of the ratios of mass to weight for whole planets or moons also testify to the constancy of these ratios for parts of these bodies.

In his discussion of *Prolegomena*, sec. 38, Friedman adds further comments about Newton's appeal to induction:

> Because Newton explicitly includes induction in his method, and especially because he places the laws of motion and the law of gravitation on the same level, he himself was not in a position to give a strong interpretation to "deduction from the phenomena." On the contrary, I do not see how Newton's method can, in the end, be distinguished from the hypothetico-deductive method: in the

end, therefore, he does not and cannot avoid hypotheses. (Of course, in the end this Newtonian predicament remains our predicament as well, for we certainly cannot embrace Kantian apriorism in the context of contemporary physics [note 15, p. 175].

Our examination of the details of Newton's argument has revealed one important way in which it goes beyond the hypothetico-deductive method. On the hypothetico-deductive method all that would matter for empirical success is accurate prediction of all the phenomena. The universality of the constant of proportionality of gravitation to mass, however, is not just a hypotheses that accounts for all the cited phenomena. In addition to this, for each phenomenon Newton is able to appeal to theorems making it measure the same constant ratio of gravitation to mass. His inductive step is to regard all these phenomena as giving agreeing measurements to a single general parameter, which express the constancy of the ratio of gravitation to mass. This makes Newton's inductive inference to universality more compelling than the merely hypothetico-deductive success of predicting all the phenomena.[23]

The wide range of phenomena considered in Newton's main argument supports a more general application of rule 3 than the application limited to gravitation toward the earth in the corollary Friedman discusses.[24] Attention to the details we have pointed out in Newton's argument suggest that the "extension to all bodies of qualities found to hold without increase or diminution on all bodies within reach of our experiments" endorsed in rule 3 is not construed as a mere Humean induction of the sort Kant found problematic. Rather, it is backed up by Newton's ideal of empirical success. In the case in question we have a theoretical parameter, limiting (for any given gravitational source) differences between ratios of gravitation on bodies (at equal distances) to the inertial masses of those bodies.[25] This parameter is accurately bounded toward zero by measurements for gravitation of terrestrial bodies toward the earth, gravitation of moons of Jupiter and Saturn toward their respective planets, for gravitation of planets toward the sun, for gravitation of moons of Jupiter and of Jupiter toward the sun, for gravitation toward the sun of the earth and its moon as well as Saturn and its moons, and for parts of bodies toward planets and sun. The extension to all bodies of what has been found to hold for all bodies within reach of experiment is to regard all these phenomena as agreeing measurements of a constant value for a universal parameter, until such time as further phenomena require revision. So far, the very intensive testing of Newton's equivalence principle, and its generalization by Einstein, has resulted in increasingly precise measurements of the constancy of the ratios for which Newton argued.[26]

Notes

1 See Friedman (1992) and Harper (1995).

2 I am using a 1999 Cohen and Whitman's translation of Newton's *Principia.* Page numbers to the widely accessible Mott-Cajori (1934) translation are provided to supplement those to Cohen and Whitman.

3 Friedman does not stress this as an advantage. He (1992, p. 144, note 11) cites Robert DiSalle and Howard Stein for correcting an earlier exposition "which too hastily assimilated Kant's privileged frame of reference to the modern idea of an arbitrary inertial frame." DiSalle (1990) argues that, far from counting as an improvement, this feature of Kant's construction renders it a step backward from the appropriate way to construe the idea of absolute space built into Newton's laws. For Newton, the role of absolute space is simply to make available the distinction between true and merely relative motion built into the laws of motion. It follows from corollaries 5 and 6 that no measurement of force will pick out a frame at rest in absolute space. DiSalle argues that attention to the limited role absolute space plays for Newton disarms objections by Einstein and Reichenbach based on construing absolute space as an entity posited as a cause of inertial effects (1992, 1995).

4 Taking corollary 6 seriously motivates Einstein's equivalence principle and the version of Newtonian gravitation proposed by Cartan, according to which gravitation is represented directly by local space-time curvature (see Misner, Thorne, and Wheeler 1973, chap. 12). Our discussion of Friedman's account of Kant's centre of mass construction illustrates difficulties of specifying inertial frames that are avoided by the Cartan formulation. (See Malament [1995] for more on advantages of Cartan's formulation.)

5 Kant's "Third mechanical law: In all communication of motion action and reaction are always equal to one another" (Ellington 1970, p. 106) is a version of Newton's third law of motion "To any action there is always an opposite and equal reaction; in other words, the actions of two bodies upon each other are always equal and always opposite in direction" (Cohen and Whitman 1999, p. 417; Cajori 1934, p. 13). A central part of what Friedman takes to be Kant's constructive procedure for judging whether a given frame is adequate to specify true motions in a given system is whether this third law can be applied to accelerations between each pair of bodies.

6 See *Treatise Concerning the Principles of Human Knowledge,* sections 110-13, pp. 93-95 in Jessop (1969), and *DeMotu*, sections 57-60, pp. 207-69 in Armstrong (1969).

7 As Laymon (*History of Philosophy,* 1976) and DiSalle (cited in note 3) have argued, the point of Newton's bucket experiment is that it shows that, to the extent that his laws of motion are empirically adequate, his definitions distinguishing true from merely relative accelerations by dynamical effects can be successfully applied empirically.

8 See Koyré 1968, pp. 115-38, and Taton and Wilson 1995, pp. 3-21.

9 For a good general account of these developments see sections V and VI in Taton and Wilson (1995).

10 What count as mere hypotheses are conjectures not sufficiently backed up by empirical successes to count as alternatives to be taken as serious rivals. This was exactly what the vortex theory failed to deliver on. Once this became sufficiently clear, the mechanical philosophy's commitment to explanation by contact forces, which had supported the vortex theory and objections against Universal Gravitation's apparent commitment to action at a distance, became irrelevant to the practice of science. See Harper (1997).

11 It may be natural to explicate the material necessity of the third postulate so that a proposition B counts as necessary if for some proposition A that counts as actual the conditional (If A then B) follows from universal conditions on objects of experience. If the trivial conditional (If A then A) is allowed, this has the consequence that any proposition A that counts as actual also counts as necessary in the material sense specified in the third postulate. Even if such trivial conditionals are not allowed, Kant's causal principle would make any "happening" count as a material necessity.

The actualities involved in Newton's argument from phenomena are generalizations. As we shall see below, Kant requires more than what he takes to be "the merely assumed and comparative universality" conferred by experience through induction to have a generalization count as actual.

12 It should be clear that it is only propositions satisfying Kant's stricter notion of necessity that could be candidates for the explication of *a priori* knowledge provided by Philip Kitcher (1980; see also his 1996).

13 According to propositions 1 and 2 of book 1, given Newton's assumptions (which include absence of additional perturbing forces), Kepler's area law for an orbit (the rate at which areas are swept out by radii from the centre of the primary is constant) is equivalent to the centripetal direction of the force deflecting a body into that orbit. According to corollary 2 of proposition 2, an increasing areal rate corresponds to having the force off centre in the direction of motion while a decreasing rate corresponds to having the force off centre in the opposite direction. Thus, having the areal rate be constant measures the centripetal direction of the deflecting force. (See Harper 1991.)

14 This is argued in some detail in Harper (1986).

15 Indeed, it was Laplace's success at solving the great, over 800-year period, interaction between Jupiter and Saturn (see Taton and Wilson 1995, pp. 138-41, and especially Wilson 1985) that may have made his determinism seem plausible as an ideal limit corresponding to exact values of masses and motions that the increasingly precise astronomical data and corrected orbits could be regarded as approximating. Newton's method, which is directed to the approximations of actual measurement by phenomena (Harper 1998), neither requires nor endorses such determinism (Harper 1997).

16 One nice feature of classical perturbation theory in Newtonian gravity is that the basic centripetal force inferred from the area law is maintained when force

components toward other bodies are added to produce perturbations that introduce deviations from the area law. The corrections to account for perturbations do not undercut the propositions about forces inferred from the basic phenomena, even when those phenomena are found to be only approximations to the corrected orbit (see Harper 1993, pp. 156-59).

17 It may be worth pointing out that Kant's (*MFNS*, observation 2, proposition 7, chap. 2; Kant 1970, pp. 65-66) actual criticism of Newton's corollary 2 of proposition 6 is not specifically directed against the appeal to rule 3 cited by Friedman. Instead, it is directed toward arguing that Newton's appeal to a thought experiment, which was aimed at ruling out transformations of form that would allow the gravity of matter to fade away, implies that Newton was committed to treat gravitation as essential to bodies.

18 Clifford Will (1991, p. 27) argues that Newton's pendulum experiment establishes what we now call the "Weak Equivalence Principle"—the identification of passive gravitational mass with inertial mass for laboratory-sized bodies—to three decimal places.

19 Let us define $G_e = Qr^2$ where Q is the ratio of a body's weight toward the earth to its inertial mass and r is its distance from the centre of the earth. Given the inverse-square variation of gravitation toward the earth with distance from the centre of the earth argued for in propositions 3 and 4, G_e is a measure of gravitation toward the earth which counts as a quality of all bodies (at or above the surface of the earth) that does not exhibit intension or diminution of degree.

Newton's cited estimates of the lunar distance in his moon test support measurements that put an upper bound of .03 on Eötvös ratios representing differences between measurements of G_e from the centripetal acceleration of the lunar orbit and Huygens's measurement of G_e from the length of a seconds pendulum at the surface of the earth. See Harper and DiSalle (1996, p. S48).

20 The data from Newton's table on Jupiter's moons (Cajori 1934, p. 401) put an upper bound of less than .03 on Eötvös ratios representing differences between measurements of G_j by the orbits of those moons, where G_j is defined for gravitation toward Jupiter as G_e for gravitation toward the earth in note 19 (Harper and DiSalle 1996, p. S48). Similarly, the data Newton cites for the primary planets (Cajori 1934, p. 404) put bounds of less than .008 on Eötvös ratios representing differences between measurements of G_s for gravitation toward the sun (Harper and DiSalle 1996, p. S49).

21 Damour (1987) points out that the result Newton claims is wrong in both sign and magnitude. Where Q(x) is the ratio of the sun's gravitation on a body to its inertial mass, Nordtvedt (1968) gives the correct calculation for the direction and magnitude of the orbital polarization that would correspond to a given difference in Q-ratio between a planet one of its moons. Harper and Valluri (2000) have shown that using Nordtvedt's calculation the data from Pound, cited by Newton (Cajori 1934, p. 402), could have limited differences in Q ratios to about .004.

22 By 1825 Laplace was able to establish this phenomenon and relevant theorems sufficiently precisely for our moon to measure the constancy of these ratios to seven decimal places (Damour and Vokrouhlicki 1996).

23 A proposal which introduced separate constants for different phenomena, e.g., a separate constant for the force attracting each planet, could make exactly the same predictions and so equal the H-D success of Newton's unified account, but it would not seriously rival the success of agreeing measurements of the same parameter by diverse phenomena exhibited by Newton's inferences from these phenomena. One of the advantages of Newton's stronger ideal of empirical success is that it makes his rule 4 able to count such proposals as mere hypotheses that are not to be allowed to undercut his inferences to a single inverse-square centripetal acceleration field maintaining the planets in their orbits about the sun.

24 It also illustrates that this Newtonian methodology, even though it goes beyond the hypothetico-deductive method, is far less top-down than what Friedman takes to be the Kantian *a priorism* that we cannot embrace in contemporary physics.

25 Where a_x and a_y are the gravitational accelerations that would be produced on bodies x and y at similar locations in a given gravitational field, the Eötvös ratio

$$2\,|a_x - a_y| / |a_x + a_y|$$

measures violations of the equality of ratios of gravitation to inertial mass for these bodies (Harper and DiSalle 1996, p. S47). Phenomena bounding such Eötvös ratios toward zero can be construed as measurements bounding toward zero a single universal parameter. To have all these phenomena bound such a parameter toward zero is to have them all count as agreeing measurements of the equality of ratios of gravitation to inertial mass that any two bodies would have at similar locations in any gravitational field.

26 Will cites the Moscow torsion balance experiments which limit violations of the weak equivalence principle (WEP) to 10^{-12} (Will 1991, p. 26). He calls the extension of WEP to bodies large enough to have significant gravitational self-energy, such as planets and moons, the gravitational weak equivalence principle (GWEP) (Will 1991, p. 184). Lunar laser ranging has provided measurements which limit violations of GWEP to $(2 \pm 5) \times 10^{-13}$ (Dickey et al. 1994).

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