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Newton and the Fudge Factor

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The Mathematical Way

"In the preceding Books," Newton wrote as he introduced Book III of the *Principia* (*1*, p. 397), "I have laid down the principles of philosophy; principles not philosophical but mathematical. . . . It remains that, from the same principles, I now demonstrate the frame of the System of the World. Upon this subject I had, indeed, composed the third Book in a popular method, that it might be read by many; but afterwards, considering that such as had not sufficiently entered into the principles could not easily discern the strength of the consequences. . . . I chose to reduce the substance of this Book into the form of Propositions (in the mathematical way), which should be read by those only who had first made themselves masters of the principles established in the preceding Books." The very title he had chosen for his work, *Philosophiae naturalis principia mathematica*, had underlined the same theme, in evident contrast to Descartes' mere, unmodified *Principia philosophiae*. In the happy phrase of Alexandre Koyré, the dean of historians of the scientific revolution, the universe of precision had replaced the world of more or less. Newton's mathematical way turned out to be the road that modern science has followed with steadily waxing success ever since.

The quantitative thrust that distinguishes modern science certainly did not originate with Newton. The search for mathematical simplicity and harmony had lain behind Copernicus' suggested reordering of the heavens, and the influential examples of Kepler's laws of planetary motion and Galileo's kinematics ensured that mathematical precision in the description of natural phenomena would be a fundamental theme of the scientific revolution. Nevertheless, it is not too much to say that Newton consolidated and confirmed the quantitative character of modern science, if he did not originate it. Throughout the 17th century the Pythagorean tradition, represented by Kepler and Galileo, stood in tension with another equally basic theme of the scientific revolution, the mechanical tradition, represented by Descartes and innumerable others. Where the Pythagorean tradition pursued the geometric description of phenomena, the mechanical tradition strove to purge natural philosophy of occult superstition by seeking mechanical causes. The two traditions seemed to refuse every effort to bring them into harmony. Vortical theories of the universe did not yield Kepler's laws. Mechanistic explications of the descent of heavy bodies did not yield Galileo's kinematics. It was Newton who brought the two into harmony by expanding the mechanical philosophy to include the concept of force at a distance. Although mechanical philosophers of strict persuasion resisted this eruption of the occult for a generation, the power of Newtonian science in deriving quantitative results quickly swept them into oblivion and crystallized the enduring pattern of modern science.

The role of the *Principia* in establishing the quantitative paradigm of physical science extended well beyond its dynamic explication of accepted conclusions. Far more impressive was its success in raising quantitative science to a wholly new level of precision. The two classic examples of mathematical science had been those mentioned above, Kepler's laws of planetary motion and Galileo's kinematics. Kepler's laws applied to celestial phenomena, the accepted locus of geometric perfection untroubled by the flux of terrestrial events. If Galileo's kinematics brought geometry down to the mundane realm, it proposed ideal relations that were not completely realized in their material embodiment. When someone objected to his derivation of the parabolic trajectory by pointing to factors, such as the resistance of the air, that disturb the path, Galileo replied (*2*) that such perturbations do not "submit to fixed laws and exact description. . . . [H]ence, in order to handle this matter in a scientific way, it is necessary to cut loose from these difficulties; and having discovered and demonstrated the theorems, in the case of no resistance, to use them and apply them with such limitations as experience will teach." In contrast, Newton enlarged the definition of science to include those very perturbations by which material phenomena diverge from the ideal patterns that had represented the object of science to an earlier age. The *Principia* submitted the perturbations themselves to quantitative analysis, and it proposed the exact correlation of theory with material event as the ultimate criterion of scientific truth.

And having proposed exact correlation as the criterion of truth, it took care to see that exact correlation was presented, whether or not it was properly achieved. Not the least part of the *Principia*'s persuasiveness was its deliberate pretense to a degree of precision quite beyond its legitimate

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claim. If the *Principia* established the quantitative pattern of modern science, it equally suggested a less sublime truth—that no one can manipulate the fudge factor quite so effectively as the master mathematician himself.

Acceleration of Gravity

Consider three specific examples that rank among the *Principia*'s more impressive achievements. The law of universal gravitation rested squarely on the correlation of the measured acceleration of gravity at the surface of the earth with the centripetal acceleration of the moon. Starting from the principle of inertia, Newton had shown that a system of planets orbiting the sun in accordance with Kepler's three laws entails a centripetal attraction toward the sun that varies inversely with the square of the distance from the sun. Equally, a system of satellites orbiting Jupiter in accordance with Kepler's laws entails an inverse square force toward Jupiter. Before he could invoke the uniformity of nature as stated in his second "Rule of Reasoning in Philosophy" and apply the ancient word *gravitas* to this force, however, he had to show that the attraction holding the moon in its orbit is quantitatively identical to the cause of heaviness at the surface of the earth. Presented initially in Proposition IV, Book III, the correlation of the acceleration of gravity, g, with the moon's centripetal acceleration was further refined in Proposition XXXVII. Measurements with pendulums had set $\frac{1}{2}g$, the distance a body falls from rest in 1 second near the surface of the earth in the latitude of Paris, at 15 Parisian feet ($= 15 \times 1.068$ English feet), 1 inch, $1\frac{7}{9}$ lines (1 line $= \frac{1}{12}$ inch). Starting with the measured orbit of the moon, Newton calculated the distance that the attraction of gravity causes it to deviate from a rectilinear inertial path during 1 minute. This figure required correction by the amount of the sun's disturbance of the moon's orbit, measured by the motion of the moon's line of apsides, yielding a calculated distance of 14.8538067 feet. A body removed one-sixtieth as far from the earth would fall the same distance in 1 second. Since he had set the distance of the moon at $60\frac{2}{5}$ times the maximum radius of the earth, Newton corrected the calculated fall in 1 second accordingly. He took the oblate shape of the earth into account, computing the distance of fall at the mean radius (45° latitude), and adding two-thirds parts of a line to correct to the smaller radius at the latitude of Paris. Hence, at Paris, bodies would fall 15 Parisian feet, 1 inch, $4\frac{25}{33}$ lines in 1 second. The rotation of the earth gives rise to centrifugal effects which diminish the fall by 3.267 lines at Paris. Hence, by calculation from the centripetal acceleration of the moon, $\frac{1}{2}g$ should equal 15 feet, 1 inch, $1\frac{1}{2}$ lines (*3*). Beneath its unsettling combination of decimals carried to seven places and fractions of varying complexity, the calculation purports to find a correlation accurate to a fraction of a line, a degree of precision perhaps somewhat better than 1 part in 3000.

Velocity of Sound

Newton's calculation of the velocity of sound falls into a different category from the correlation of g with the moon's centripetal acceleration. In the latter case, he demonstrated the correlation between two independently measured values. In the former, he started from principles of dynamics and arrived at the measured velocity of sound. Because he advocated the corpuscular conception of light, we tend to forget that Newton carried out the first successful analysis of what we now call simple harmonic motion. In extending that analysis successfully to embrace the propagation of sound, a demonstration wholly without precedent which inaugurated a new branch of theoretical physics, he achieved another triumph of quantitative science.

The demonstration rested, of course, on his understanding of the dynamics of the simple pendulum—that the component of gravity parallel to the tangent at any point on a cycloid is proportional to the displacement of that point, measured along the arc, from the position of equilibrium. This is the dynamic condition of simple harmonic motion. By extending the analysis to waves on a surface of water, Newton calculated the period of a single wave, which yielded the time for a wave to advance one wavelength, and hence the velocity of propagation. Boyle's Law allowed him to translate the results to compressive waves propagated through air. He demonstrated that the velocity of pulses varies as the square root of the elastic force divided by the density and, comparing the density of water to that of air, arrived at a preliminary velocity of sound of 979 (English) feet per second. The figure required two corrections. In the calculation he had assumed a medium of pointlike particles, but since the solid particles, through which the pulses pass instantaneously, are of finite dimension in comparison with the spaces between them (a property which he called the "crassitude" of the particles), the velocity must be increased accordingly. From the measured comparative densities of air and water (1 : 870), Newton calculated that the dimensions of the particles of air occupy between one-ninth and one-tenth of linear distances in air (that is, $9^3 < 870 < 10^3$). Hence, the diameter of a particle is to the interval between particles as one to eight or nine (which he then took simply as nine), and to the 979 feet per second we must add 979/9, or 109, feet per second. A further correction must be made for vapor, which does not vibrate with the air and thus causes an increase in the velocity proportional to the square root of the amount of air that the vapor displaces. That is, if the atmosphere consists of ten parts of air to one part of vapor, the velocity will be increased by the factor $(11/10)^{1/2}$, which is nearly equal to 21/20. Sound moves more swiftly by this ratio through air containing vapor, that is, through the atmosphere as it is, than it would through "true air." The two corrections brought the calculated velocity of sound to 1142 feet per second (*1*, pp. 382–383). The most recent elaborate measurement of the velocity of sound, by William Derham, confirming the result which Derham reported that Halley and Flamsteed had obtained in other measurements at the Royal Observatory, was 1142 feet per second (*4, 5*). Although he dispensed with fractions and decimals in this calculation, Newton effectively claimed a precision of about 1 part in 1000.

Precession of the Equinoxes

With the precession of the equinoxes, Newton undertook to calculate the exact quantity of what he treated as a perturbation of the ideal uniform rotation of the earth about an axis fixed in direction. In Proposition LXVI, Book I, he developed a general analysis of the three body problem, which he then applied to several phenomena by adjusting the parameters. Primarily, it formed the basis of his lunar theory; but by shrinking the orbit of the satellite to coincide with the circumference of the planet it circled, and by replacing the satellite in its orbit with a ring of liquid or solid matter, he used the same analysis to derive the tides and the precession of the equinoxes. The mean motion of the lunar nodes, the result of the sun's action to change the plane of the moon's orbit, is $20°11'46''$ per year. The mean motion of the nodes of a moon at the equator of the earth revolving with the period of the earth's rotation would be less in the proportion of the sidereal day to the lunar month, and a ring of matter equal in diameter to the earth and inclined to the ecliptic would experience such a precession of its nodes as a result of the sun's attraction. Because the ring must carry not only its own mass but the mass of the entire earth, the precession is reduced in the proportion of 4,590 to 489,813. Because the matter that occasions precession is not concentrated in a ring, the mean precession must be reduced further to two-fifths of the value it would have for a ring. Moreover, the motion must be reduced yet again in the proportion of the cosine of $23\frac{1}{2}°$, the inclination of the plane of the equator to the ecliptic. Hence, the precession of the equinoxes arising from the sun's attraction is equal to $9''7'''20^{\text{iv}}$ annually. A greater precession results from the attraction of the moon. From the measured differences of spring and neap tides, Newton had calculated that the attraction of the moon on the earth is 4.4815 times greater than the attraction of the sun. The combined effect of both yields a precession of $50''0'''12^{\text{iv}}$, "the amount of which agrees with the phenomena; for the precession of the equinoxes, by astronomical observations, is about 50″ yearly" (*1*, pp. 489–491). As with the correlation of *g* with the moon's centripetal acceleration, he purported to have reached a precision of about 1 part in 3000.

Paradigm of Modern Science

So completely has modern physical science modeled itself on the *Principia* that we can scarcely realize how unprecedented such calculations were. Before Newton, only astronomy, the science that dealt with the eternal heavens, had realized accuracy of a similar degree. Some two decades before the *Principia* Robert Boyle had proposed Boyle's Law, one of the few precedents to Newton's physics, on the basis of measurements that frequently varied from theory by 1 part in 100, a lack of precision, he said, which was not to be avoided in such "nice" experiments (*6*). Newton insisted that it could be and must be avoided. He boldly transported the precision of the heavens into mundane physics, and beyond their intrinsic interest the calculations in the *Principia* were significant as advertisements of the new ideal of physical science thus proposed. The new ideal was intimately connected with the central conceptual innovation of Newtonian science, "force," or action at a distance, quantitatively defined as an element in rational mechanics. The correlation of the moon's centripetal acceleration with the acceleration of gravity, calculation of the velocity of sound, and the derivation of the precession of the equinoxes, together with other examples in the *Principia*, offered a compelling demonstration of the descriptive power of Newtonian natural philosophy.

Or was the compelling demonstration a cloud of exquisitely powdered fudge factor blown in the eyes of his scientific opponents? I have chosen the three examples above because all of them underwent significant modifications in the second edition of the *Principia*. I have cited them as they appear in the third and final edition, the basis of the standard English translation in general circulation today. With the exception of insignificant modifications of numbers in the correlation of *g*, as a result of the latest French determination of the radius of the earth, the final edition repeated the second edition. The second edition, however, introduced major changes in the treatment of all three problems, changes that signally increased the level of apparent precision by the mere numerical manipulation of the same basic body of data (*7–10*).

The second edition of the *Principia* was at once an amended version of the first edition and a justification of Newtonian science. The battle with the continental mechanical philosophers who refused to have truck with the occult notion of action at a distance still raged. The second edition made its appearance framed, as it were, by its two most important additions, Cotes' "Preface" at the beginning and Newton's "General Scholium" at the end, both of them devoted to the defense of Newtonian philosophy, of exact quantitative science as opposed to speculative hypotheses of causal mechanisms. By 1713, moreover, Newton's perpetual neurosis had reached its passionate climax in the crusade to destroy the arch-villain Leibniz. Only a year earlier the Royal Society had published its *Commercium epistolicum* (*11*), a condemnation of Leibniz for plagiary and a vindication of Newton, which Newton himself composed privately and thrust upon the society's committee of avowed impartial judges. In Newton's mind, the two battles merged into one, undoubtedly gaining emotional intensity in the process. Not only did Leibniz try to explain the planetary system by means of a vortex and inveigh against the concept of attraction, but he also encouraged others to attack Newton's philosophy. His arrogance in claiming the calculus was only a special instance of his arrogant presumption to trim nature to the mold of his philosophical hypotheses. In contrast, the true philosophy modestly and patiently followed nature instead of seeking to compel her (*12*). The increased show of precision in the second edition was the reverse side of the coin stamped *hypotheses non fingo*. It played a central role in the polemic supporting Newtonian science.

Fiddling with Sound

In examining the alterations, let us start with the velocity of sound since the deception in this case was patent enough that no one beyond Newton's most devoted followers was taken in. Any number of things were wrong with the demonstration. It calculated a velocity of sound in exact agreement with Derham's figure, whereas Derham himself had presented the conclusion merely as the average of a large number of measurements. Newton's assumptions that air contains vapor in the quantity of 10 parts to 1 and that vapor does not participate in the sound vibrations were wholly arbitrary, resting on no empirical foundation whatever. And his use of the "crassitude" of the air particles to raise the calculated velocity by more than 10 percent was nothing short of deliberate fraud. The adjustment involved the assumption that particles of water are completely solid. In fact, Newton believed that they contain the barest suggestion of solid matter strung out through a vast preponderance of void. One of the most radical aspects of Newtonian science was its conception of matter, which approached, without quite reaching, the notion that atoms are point sources of force, put forward by the Jesuit natural philosopher R. J. Boscovich in the middle of the 18th century. The calculation of the velocity of sound set this conception of matter aside in order to adjust the velocity upward by 109 feet per second. In all of this, Newton did himself a gross disservice. As I have suggested, his attempted derivation of the velocity of sound from fundamental dynamic principles was one of the *Principia*'s strokes of genius, an achievement wholly without precedent. He arrived at a figure, 979 feet per second, which is roughly 20 percent too low. Early in the 19th century, Laplace, building from the foundation Newton had laid, demonstrated that the correction derives from the heat generated in the compressions of sound waves; the correction is equal to the square root of the ratio of the specific heat of air at constant pressure to the specific heat at constant volume. When Newton wrote the *Principia*, the concept of quantity of heat had not been distinguished clearly from temperature, and the concept of specific heat did not exist, let alone the subtlety of specific heats at constant volume and constant pressure. Meanwhile, there he stood face to face with the hostile continental philosophers, his nakedness made manifest by an uncovered discrepancy of 20 percent. The very flagrancy of his adjustment in this case becomes evidence for the compulsion behind the pretense of precision in the other cases.

In the first edition he had approached the velocity of sound in quite a different mood. Impressed perhaps by the significance of his own achievement in deriving a velocity of sound that approached its measured value, and certainly not yet assaulted with the hostile taunt of occult, Newton had been willing to leave the demonstration as an approximation within broad limits. The basic features of the derivation were set in the first edition and did not subsequently change. The numbers were different, however. Using a ratio of 1 : 850 for the densities of air and water, he arrived at a calculated velocity of 968 feet per second. Over against this value were a wildly varying set of measured velocities. Newton reported two—one by Mersenne equivalent to 1474 English feet per second, and one by Roberval equivalent to 600 feet per second. In view of the discrepancy, he chose to make a measurement of his own in an arcade of Trinity College, adjusting a pendulum to swing in time with the return of an echo that traveled a total of 416 feet. He was satisfied to determine that the echo was slower in returning than the vibration of a pendulum $5\frac{1}{2}$ inches long and faster than that of an 8-inch pendulum, yielding a velocity between 1085 and 920 feet per second (*8*, p. 370). The calculated value fell squarely between them, and the first edition rested its case modestly on that loose approximation.

Perhaps there was more concealed in the first edition than meets the eye. A faint aura of deception clings to Newton's report of previous measurements and to his own measurement. The structure of his presentation implicitly justified both the necessity of a new measurement and its result, since its limiting values fell inside the broader limits of Roberval's and Mersenne's measurements. There were available a number of other measurements that Newton could have known about, however, all of them falling well above his calculation of 968 feet per second. In fact, Roberval's figure of 600 was utterly out of line with other 17th-century measurements of the velocity of sound. The majority arrived at a value higher than the one we accept (*13*). One cannot avoid wondering whether Newton performed his experiment before or after he made the calculation of 968 feet per second. Already in about 1694, at any rate, David Gregory found him planning to change the section on the propagation of sound in order to define its velocity "within narrower limits" (*14*, p. 384). A copy of the first edition in which Newton entered projected emendations apparently records a new measurement in the arcade at Trinity. He now stated that the echo was slower in returning than a pendulum of 5 inches and faster than one of 7 inches, corresponding to a velocity between 1109 and 984 feet per second (*15*, p. 531; *16*). This moved his measurement closer to the general range of measurements, but the lower limit was now above the calculated velocity of 968 feet per second.

What could he do? He could fiddle with the numbers. His calculation rested on the density of air, and the density of air was a difficult thing to measure in the 17th century. The ratio of the densities of air and water that he used in the first edition (1 : 850) was arbitrary. In a sheet of "errata" in the first edition, he tried both 1 : 900 and 1 : 950 and found that they yielded velocities of 996 and 1023 feet per second. He settled for the higher figure. While it placed his calculation within the range of his new measurement, he may have felt that it stood too close to the bottom limit. It is impossible to unravel every detail in the chronology of his manipulation of the calculation, but the evidence that he tried various devices cannot be mistaken. In addition to the density, he thought of the adjustment based on the "crassitude" of the particles; it appears in the paragraph that records his new measurement, raising the calculated velocity about 100 feet per second (*15*, p. 532).

It is reasonable to assume that the corrections above were made before he learned of the measurement by his friend William Derham and of the work of Joseph Sauveur in acoustics, since he crossed out the amended paragraph containing his own remeasurement and henceforth accepted Derham's value of 1142 feet per second. His mention of Sauveur's work, which correlated frequency with the length of organ pipes, suggests that it served to confirm Derham's measure in his mind (*17*).

Now he had a solid figure on which to exercise his mathematical talents. The annotated and interleaved copies of the first edition in which he prepared the corrections for the second edition show him manipulating the "crassitude" of the particles before he finally chose the size that allowed him to increase the velocity by one-ninth. After he lit on the further correction due to vapors, he fiddled with its magnitude as well (*15*, p. 533). Finally he decided to return to the density of the air for the fine adjustment, and setting the wandering ratio at 1 : 870 he emerged in the second edition with a calculated velocity of 1142 feet per second. At one point in the calculation, although not in the conclusion where it belonged, he did insert a modest *circiter*. That hardly covered the extent of the illusion he was attempting to foster.

Doctoring the Correlation

In the case of the correlation of *g* with the moon, much more was at stake. The correlation was the linchpin of the entire argument for universal gravitation, and we can understand why Newton wanted it to appear precise. Nevertheless, in the first edition he had been content to present it within a reasonable margin of error. After listing available calculations of the moon's mean distance, Newton chose 60 times the earth's radius as a just average. At such a distance, he calculated, the moon would fall $15\frac{1}{12}$ feet in 1 minute; hence, at the surface of the earth a body should fall $15\frac{1}{12}$ feet in 1 second, the figure Huygens had established. While the new edition was being prepared, Newton sent the editor, Roger Cotes, a scholium to Proposition IV in which he carried out the correlation substantially as it finally appeared. Since it assumed material from Propositions XIX and XXXVII, Newton eventually broke the scholium up and inserted it in those two propositions, most of it in the latter. In that position the character of the finely corrected correlation is clear. Proposition XXXVII, on the tides, compares the force of the sun in attracting the sea to the force of the moon. A series of corollaries then apply the conclusion (and earlier calculations of the comparative masses of the sun and planets) to computing the comparative densities of the earth and the moon and their comparative masses. Corollary 7, which contains the correlation, is actually a calculation of the mean distance of the moon from the value of g. In Proposition IV Newton had assumed that the moon orbits the center of the earth. In Proposition XXXVII, with the relative masses of the two known, he corrected the distance of the moon by referring its orbit to the common center of gravity of the two bodies. If the mean distance of the moon is $60\frac{1}{4}$ (in edition three, $60\frac{2}{5}$) times the maximum radius of the earth, then its centripetal acceleration will correspond to a value of $\frac{1}{2}g$, in Paris, of 15 feet, 1 inch, 5.32 lines. When the measurement by the pendulum is increased by 3.267 lines for the centrifugal effect in Paris, it yields a value for $\frac{1}{2}g$ of 15 feet, 1 inch, 5.32 lines, slightly different figures from those in the third edition with an even higher degree of precision (*18*). What had Newton demonstrated? He had shown that he could calculate the distance of the moon from the value of g, and he had shown, what should be obvious, that he could start the calculation with a value of g stated with whatever degree of precision he might choose. Even in these terms the correlation is delusory since it employs the comparative masses of the moon and the earth drawn from a dubious calculation of the tides that I shall discuss in a moment. Beyond this, the correlation of g with the moon in Proposition XXXVII demonstrates nothing whatever.

Indeed, Proposition XXXVII serves to indicate that even the modest correlation of the first edition is somewhat deceptive. To be valid, the correlation must start with two quantities that are measured independently, the distance of the moon and g. Huygens had provided the latter with high precision. For the former Newton had a range of varying measurements. Most astronomers, he said, set it at 59 radii of the earth. Vendelin set it at 60, Copernicus at $60\frac{1}{3}$, Kircher at $62\frac{1}{2}$, and Tycho at $56\frac{1}{2}$. Tycho's result was founded on a false theory of refraction, however, and when his observations were corrected they gave a distance of 61. From this set of numbers Newton extracted an average of 60 which yielded in turn a centripetal acceleration that correlated with a value of $\frac{1}{2}g$ of 15 feet, 1 inch. It is difficult to believe that the value of $\frac{1}{2}g$ did not influence his averaging of the lunar measurements. Significantly, in edition two he omitted Kircher's measure and found that the correction of Tycho's came, not to 61, but to $60\frac{1}{2}$ times the radius of the earth.

When he placed it in Corollary 7 to Proposition XXXVII, Newton did not indeed explicitly present the correlation as anything more than an exact calculation of the moon's mean distance. When he thought originally of making it a scholium to Proposition IV, however, he seemed to be trying implicitly to present it as something else. Cotes, who had caught the spirit of the enterprise rather well, saw as much. "In y^e^ Scholium of IV^th^ Proposition," he suggested, "I think the length of y^e^ Pendulum should not be put 3 feet and $8\frac{2}{3}$ lines; for the descent would then be 15 feet 1 inch $1\frac{1}{3}$ line. I have considered how to make y^t^ Scholium appear to the best advantage as to y^e^ numbers, & I propose to alter it thus" (*19, 20*). He went on to select values for the distance corresponding to 1° on the earth's surface and for the latitude of Paris that led to a very precise correlation. Cotes need not have worried. Newton was also considering how to make the correlation appear to the best advantage as to the numbers. Ultimately he settled on different values for the elements of the calculation, but by treating the distance of the moon as terminus ad quem instead of terminus a quo, he reached a correlation quite as satisfactory as Cotes'. In both calculations it was more public relations than science.

Manipulating the Precession

Newton's concern that the numbers appear to best advantage in his correlation with the moon, however, was as nothing beside his efforts on the precession of the equinoxes. Precession, in his treatment of it, was a quintessential demonstration of the ultimate advantage of Newtonian science over the reigning mechanical philosophy, a quantitative derivation of a perturbation which qualitative mechanical explications did not even dare to mention. The very claim of Newtonian science on the allegiance of natural philosophers depended on quantitative precision in such cases. In the case of precession, moreover, the correction of a faulty lemma in edition one imposed the necessity of an adjustment of more than 50 percent in the remaining numbers. Without even pretending that he had new data, Newton brazenly manipulated the old figures on precession so that he not only covered the apparent discrepancy but carried the demonstration to a higher plane of accuracy.

Like his derivation of the velocity of sound, Newton's demonstration of the precession was a brilliant insight tempered by limitations that the level of the 17th-century dynamics imposed. He identified the same cause of precession that celestial dynamics still employs—the attraction of the sun and moon on the bulge of matter about the equator. He lacked the conceptual tools to treat it adequately, however. The *Principia* itself established the basic equation of the dynamics of linear motions relating force to acceleration. Although the concept of moment was present in the law of the lever, dynamics possessed as yet no equation corresponding to Newton's second law that related moment to angular acceleration, and it possessed no concept similar to moment of inertia that played the role corresponding to mass, which was also Newton's creation, in the equation of linear motions. His demonstration treated precession on the analogy of the lunar nodes, the motion of which he attempted to derive by extending both the principles used to derive perturbations in the shape of the orbit and the quantities thus found. To understand how limited the basic concepts relevant to rotational motion were, consider his correction of the precessional motion by the cosine of $23\frac{1}{2}°$, the inclination of the equator to the ecliptic. As it appears in his demonstration, the correction amounts to taking the component of attraction parallel to the equator, just as he had used the effective component of force in computing the tides. In this case, however, in which the very effect depends on the inclination of the equator to the ecliptic, his procedure treated the effect as though it would be maximized if the inclination were zero. Since the result of this correction reduced the effect to 0.91706 of the value it would otherwise have had, it may appear to be an application of the fudge factor. In this instance, however, which appeared unchanged in all the editions of the *Principia* and escaped comment even by the sharp-eyed Cotes, we have to do more probably with a technical limitation. Correspondingly, nothing in his derivation utilized the different inclination of the equator from the inclination of the lunar orbit, although the precession was calculated directly from the motion of the lunar nodes.

Although the alteration involved a considerable refinement in his treatment of the tides, the refinement was necessitated by the need to make the numbers appear to best advantage as a result of other changes. As I have explained, the demonstration began with the mean motion of the lunar nodes, and adjusted the value first in the ratio of the two periods of rotation and then in the ratio of the total mass of the earth to the mass of the ring of matter responsible for the precession. At a later step, he reduced the motion further in the proportion of the cosine of $23\frac{1}{2}°$. All of this was a straightforward matter of ratios that allowed no margin for maneuver. Two major adjustments that remained supplied the principal substance of calculation. On the one hand, the comparison with the lunar nodes assumed the matter causing the precession to be concentrated in a ring around the equator, whereas it is in fact spread unevenly over the surface of an oblate spheroid. A subsidiary demonstration (or lemma) established the ratio of the further reduc tion thus entailed. On the other hand, the comparison with the lunar nodes involved only the attraction of the sun. The attraction of the moon is several times more potent in causing precession, however, and the ratio in this case was established from the variation between spring and neap tides. The nub of Newton's derivation of the precession lay in the relation of these two factors, and the problem he faced in the new edition lay in the major revision he introduced in the first of them, the ratio between the effect of matter concentrated in a ring and that of matter spread over an oblate spheroid. Once that revision was made, Newton fell back on the tides, which became in his hands as fluid as the water composing them.

In the first edition, Lemma I, Book III, provided the adjustment from a ring of matter at the equator to an oblate spheroid. Suffice it to say that the demonstration is perplexed, and anyone interested in the difficulties that rotational motion presented to early dynamics will find it a revealing case study. In attempting to compare the *vis et efficacia tota* of the two distributions of matter to rotate the earth about a particular axis, he employed among other things the moment (or "efficacy of the forces") of a vector passing through that axis. The lemma arrived at a ratio of 1 : 4. Applied to the motion derived from the lunar nodes, this ratio yielded an annual precession of $6''12'''2^{\text{iv}}$, due to the attraction of the sun. From the tides he computed that the moon is $6\frac{1}{3}$ times more effective than the sun, so that the two together produce a precession of $45''24'''15^{\text{iv}}$. Clearly a further adjustment was needed, and Newton found one in the density of the earth. The earth is denser at the center; as a consequence, it is higher at the equator than the computation from centrifugal force alone reveals, with the result that the precession is increased in the ratio of $10 : 8\frac{1}{3}$ or 6 : 5. When the resulting figure was reduced by the cosine of $23\frac{1}{2}°$, introduced in edition one at this point, it yielded a precession of $49''$-$58'''$, in sufficient agreement with the observed value of $50''$ (*8*, pp. 467–473).

By 1694 he recognized the fault in Lemma I (*21*). In the second edition two lemmas replaced it; they arrived at a ratio, not of 1 : 4, but of 2 : 5. Without any further adjustments, the new ratio would have left the precession more than 50 percent too large.

Fortunately, the other major correction, the ratio of the attractions of sun and moon as established by the tides, was amenable. This adjustment worked out so well that he abandoned the additional specious correction from the density of the earth despite the explicit statement in edition two that, because of the greater rarity of matter near the surface of the earth, the diameter at the equator may be nearly 15 miles greater than the calculation from centrifugal force alone would suggest (*22*).

In all three editions, Newton relied on the same body of data about the tides, two sets of observations published in the *Philosophical Transactions* in 1668 in response to the Royal Society's early diligence in collecting a complete history of nature. In light of the conclusions Newton ultimately drew from the observations, it is worthwhile to savor their precision in their own words. Samuel Colepresse measured the tides at Plymouth. He found that "the Water usually riseth about 16 Foot (I say *usually*, because it may vary in this Part from the lowest Neap to the highest Annual Spring above 7 or 8 Foot) . . ." (*23*). If the title to the paper is correct, the statement rested on observations made during the year 1667 alone. Since the Royal Society began to solicit such information only in the autumn of 1666, there is no reason to think the measurements were more extended. The same strictures apply to Samuel Sturmy's observations of the tides in the mouth of the Avon below Bristol (*24*). The highest spring tide there "flows in height about $7\frac{1}{2}$ fathoms, or 45. foot; the lowest Neap-tydes flowing in height 25. foot." Sturmy's paper included a table of the flow of a spring tide measured every quarter hour through its total span of 5 hours and of the ebb measured every hour through its span of 7 hours. Each column is summed the same, "45. feet circiter"; the numbers in one add, in fact, to 44 feet, 1 inch, and in the other to 45 feet, $10\frac{1}{2}$ inches. (Of course, the measurement of the total tide was not reached by adding the two columns.) Although entries in the table include measurements to half inches, he remarked that making them always that accurate "is neither easie, nor material, or usefull" (*25*).

In the first version of Book III, what is known as "The System of the World," Newton concluded from these observations that the ratio of attractions is $1 : 5\frac{1}{3}$. The spring tide is produced by the combined attractions of moon and sun [L(una) + S(ol)], the neap tide, when the moon is in quadratures, by the excess of the moon's attraction over the sun's (L − S). Hence the basic equation was $(L+S)/(L-S) = 9/5$. On this occasion, he did not even mention Colepresse's observations; the ratio of 9/5 comes from Sturmy alone. Since the moon declines $23\frac{1}{2}°$ from the plane of the equator at the time of the neap tide, whereas the two luminaries act in the same plane at spring tide, the force of the moon must be diminished correspondingly. The equation became $(L+S)/(.841L-S)=9/5$, or $L=5\frac{5}{11}S$, a ratio he adjusted minimally to $5\frac{1}{3}$ (*1*, p. 593). In "The System of the World" the ratio played no further function; although he had stated the cause of the precession, he did not attempt to derive it. In edition one he did, and the computed amount of the sun's effect required a larger ratio. Colepresse's measurements were summoned to the aid of Sturmy's; they produced a ratio of $7\frac{1}{3}$. Newton averaged the two at $6\frac{1}{3}$ and extracted the value of precession stated above (*8*, pp. 464–465).

Mending the Numbers

Then came the new lemmas and the need for a radical reduction in the calculated precession. In February 1712, Cotes' painful progress through the text he was editing arrived at Proposition XXXVII, on the tides. Apparently the ratio had returned to $5\frac{1}{3}$ by this time, probably by preferring Sturmy's measurements to Colepresse's as the later editions of the *Principia* did. After correcting Newton's calculation to $5\frac{3}{28}$, Cotes noted that this ratio would change the figures in the corollaries to the proposition, especially the comparative masses of the moon and the earth. "This alteration," he continued, "will very much disturb Your Scholium of y^e^ 4^th^ Proposition [the correlation of *g* with the moon] as it now stands; neither will it well agree with Proposition 39^th^ [the precession] . . ." (*26*). In his reply, Newton confessed that he had lost his copy of the amended Proposition XXXIX and did not know how to make the further correction. "If you can mend the numbers," he added candidly, "so as to make y^e^ precession of the Equinox about 50″ or 51″, it is sufficient" (*27*).

Cotes had fully grasped the nature of the game by now; and as long as they chose to play it, he intended to play it well. He pointed out to Newton that the greater bulge at the equator explicitly considered in edition two, so that the diameter through the equator would exceed that through the poles by nearly 32 miles instead of $17\frac{1}{6}$, raised problems. The passage in the first edition correcting the precession by the ratio 6 : 5 because of variations in density had been dropped, but Newton now proposed a brief statement that the increase in precession due to the added length of the equatorial diameter was compensated by the decrease due to the greater rarity. "You have very easily dispatch'd the 32 Miles in Prop. XXXIX^th^ . . .," Cotes commented with a muted note perhaps of irony, perhaps of admiration, perhaps of both (*28*).

He had hardly begun to see the extent of Newton's dexterity. The basic equation establishing the ratio of S to L had been rather crude. Newton began to refine it. The highest tides do not occur exactly at syzygies but somewhat after, when the luminaries are approximately 15° out of conjunction or opposition. Thus, the force of the sun must be reduced at both its appearances in the equation. With the new equation, Newton got a ratio of $4\frac{5}{7}$, and in a manuscript revision of Proposition XXXIX, he calculated from that ratio to a precession of $51''58'''40^{\mathrm{iv}}$. "If the force of moon in moving the sea were to the force of the sun as $4\frac{4}{7}$ to 1," he continued, "(for the proportion of these forces cannot yet be defined very accurately from the phenomena), an annual precession of the Equinoxes of $50''40'''43^{\mathrm{iv}}$ would result" (*29*). Later he took the calculation a step further. If the ratio were $4\frac{1}{2}$, the precession would be $50''14'''45^{\mathrm{iv}}$ (*30*).

Before the full implications of this line of thought could be explored, Cotes produced a new problem. According to Newton's lunar theory, the moon approaches nearer to the earth in syzygies than in quadratures. "But this allowance would increase the number $4\frac{5}{7}$ so much as to give some disturbance to the XXXIX^th^ Proposition & the Scholium of the IV^th^ as they now stand, unless," he added discreetly, "You think fit to ballance it some other way, for there is a latitude in that XXXVII^th^ Proposition" (*31*). When Newton seemed to demur, he presented the arithmetic. If the new factor were introduced without other changes, the ratio would increase to $5\frac{2}{7}$. In order to save the value $4\frac{5}{7}$, the ratio of spring tide to neap would have to be set at 11/6, but 11/6 fell outside the measurements at Plymouth and Bristol. "I shall therefore leave it to Your self to settle y^e^ whole Proposition as You shall judge it may best be done" (*32*).

Over a month passed before Newton sent Cotes a revision of Proposition XXXIX, a revision which not only incorporated the varying distances of the moon but achieved the ratio of $4\frac{1}{2}$ as well. He had discovered the secret, which lay in the fact that high tides occur after the luminaries are in syzygies and quadratures. The observations were sufficiently vague that he could adjust the exact angles at will to yield whatever ratio he chose. In February he was reducing the force of the sun by 6/7 (corresponding to an angle of $15\frac{1}{4}°$). Now he increased the reduction nearly to 4/5 (an angle of $17\frac{1}{2}°$) and, beginning to savor the possibility of a truly impressive demonstration, he shifted to the decimal 0.819152 (the cosine of 35°, twice the angular distance between the sun and the moon at the spring tide). With the ratio of $1:4\frac{1}{2}$ in hand, he went on to correlate *g* with the moon, placing the correlation now in Corollary 7 of the proposition, and with the same ratio he calculated the precession given above. Cotes was not yet satisfied. He thought that the correction for the distance of the moon was incorrect, but he hoped equally to retain what had been gained. "I could wish when the whole is settled that the proportion of $4\frac{1}{2}$ to 1 may be retain'd for the sake of Proposition XXXIX. I think there is no Proposition in Your Book which does more deserve Your care" (*33*). Newton was willing enough; he had found the key. He not only accepted Cotes' correction, but in compensating by increasing the angular distance again, he pushed the ratio below $4\frac{1}{2}$ and improved on the calculated precession still more. Cotes was delighted. "I am very glad to see the whole so perfectly well settled & fairly stated, for without regard to the conclusion [sic] I think y^e^ distance of $18\frac{1}{2}$ degrees ought to be taken & is much better than $17\frac{1}{2}$ or $15\frac{1}{4}$ & the same may be said of y^e^ other changes in y^e^ principles from which the conclusion is inferr'd" (*34*). The equation which had started out as $(L+S)/(.841L-S)=9/5$ now read (*35*)

$$\frac{1.017342L+.7986355S}{.9828616\times .8570328L-.7986355S}=9/5$$

$$L=4.4824S$$

With that ratio he carried through the correlation of *g* with the moon and calculated the precession of the equinoxes, both with an ostensible precision of about 1 part in 3000. Some might consider it a rather ambitious conclusion to draw from the measurements of a retired sea captain who had summed up two unequal columns to "45. feet circiter."

The emendations to the second edition cast a special light on the revolution in scientific discourse that Newton concluded. "I often say," Lord Kelvin asserted more than a century and a half later, "that when you can measure what you are speaking about and express it in numbers you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science . . ." (*36*). What Kelvin had in mind was not what Cotes called "making it appear to the best advantage as to the numbers," but the history of the second edition suggests that the one may frequently be indistinguishable from the other. Undoubtedly the new pattern of natural science that Newton more than any other one man raised to dominance rested on profound epistemological insights. Undoubtedly the *Principia* displayed them apart from the alterations introduced in edition two. Undoubtedly the displacement of the world of more or less by the universe of precision was a remorseless and irreversible process. Nevertheless, successful polemics are the necessary condition of every intellectual revolution. "I am satisfied that these exactnesses . . . are inconsiderable to those who can judge rightly of Your book": Cotes wrote to Newton as they polished the final decimal points of the correlation, "but y^{e} generality of Your Readers must be gratified wth such trifles, upon which they commonly lay y^{e} greatest stress" (*37*). Whether or not he considered them trifles, Newton comprehended perfectly the nature of the polemic he deployed. And among the other factors that provoked Lord Kelvin's dictum, and all that it represents in modern science, not the least was the fudge factor, manipulated with unparalleled skill by the unsmiling Newton.

References and Notes

1. I. Newton, *Mathematical Principles of Natural Philosophy*, A. Motte, Transl.; F. Cajori, Ed. (Univ. of California Press, Berkeley, 1934). [This is the standard English version in use today; it is based on the third Latin edition (*10*).]
2. Galileo, *Dialogues Concerning Two New Sciences*, H. Crew and A. de Salvio, Transl. (Macmillan, New York, 1914), pp. 252–253.
3. I. Newton, *Principia* (*1*, pp. 407–409; 482–483). The computation of the centrifugal effect used in Proposition XXXVII is found in Proposition XIX (*1*, p. 425).
4. W. Derham, "Experimenta & observationes de soni motu, aliisque ad id attinentibus," *Phil. Trans.* **26**, 2–35 (1708–9).
5. Newton also mentioned the work of Joseph Sauveur, whose measurement of the lengths of organ pipes sounding known tones seemed to confirm Derham's measurement. See Sauveur's articles "Sur la détermination d'un son fixe," *Hist. Acad. Roy. Sci.* (1700), pp. 166–178; "Systême générale des intervalles des sons, & son application à tous les systèmes & à tous les instrumens de musique," *Mem. Acad. Roy. Sci.* (1701), pp. 390–482; and "Application des sons harmoniques à la composition des jeux d'orgues," *ibid.* (1702), pp. 411–437.
6. T. Birch, Ed., *The Works of the Honourable Robert Boyle* (A. Millar, London, 1744), vol. 1, pp. 101–102.
7. There were three editions of the *Principia* during Newton's life (*8–10*).
8. I. Newton, *Philosophiae naturalis principia mathematica* (Royal Society, London, ed. 1, 1687).
9. ———, *Philosophiae naturalis principia mathematica*, R. Cotes, Ed. (Cambridge, ed. 2, 1713).
10. ———, *Philosophiae naturalis principia mathematica*, H. Pemberton, Ed. (G. & J. Innys, London, ed. 3, 1726).
11. *Commercium epistolicum D. Johannis Collins, et aliorum de analysi promota* (Royal Society, London, 1712).
12. See E. W. Strong ["Newtonian Explications of Natural Philosophy," *J. Hist. Ideas* **18**, 49–83 (1957)] where the methodological positions of a number of Newtonians are described in detail.
13. See B. S. Finn ["Laplace and the Speed of Sound," *Isis* **55**, 8 (1964)] for a list of the various measurements of the velocity of sound in the 17th century. A paper by a Mr. Walker of Oxford, "Some Experiments and Observations Concerning Sounds" [*Phil. Trans.* **20**, 433–438 (1698)], is interesting for its technique, which was very similar to Newton's, though it led him to quite a different result. His lowest single measurement was 1150 feet per second, and he averaged 11 measurements to 1305.
14. H. W. Turnbull and J. F. Scott, Eds., *The Correspondence of Isaac Newton* (Cambridge Univ. Press, Cambridge, 1961), vol. 3.
15. I. Newton, *Philosophiae naturalis principia mathematica*, A. Koyré and I. B. Cohen, Eds. (Harvard Univ. Press, Cambridge, Mass., ed. 3 with variant readings, 1972), vol. 1.
16. Nearly all of the figures have been written over and altered—that is, fiddled with—and it is difficult to know what numbers Newton meant at any given stage. See the original manuscript, Cambridge University Library, manuscript Adv. b. 39.1, folio 370A.

    To the extent that edition two employs a different measure of the velocity of sound, it contradicts my assertion that the changes in it were manipulations of the same basic body of data. It will become apparent that the new data in this case, far from justifying the higher level of precision, only perplexed the numerical manipulations that led to it.
17. J. Sauveur, "Applications des sons harmoniques . . ." [see (*5*)]. Planche XII, inserted after p. 436, gives a table of frequencies with corresponding pipe lengths.
18. In Proposition XIX, edition two, $\frac{1}{2}g$ is set equal to $1274\frac{1}{18}$ lines (15 feet, 1 inch, $2\frac{1}{18}$ lines), corresponding to a seconds pendulum 3 feet, $8\frac{5}{9}$ lines in length. In edition three the seconds pendulum is quoted at 3 feet $8\frac{1}{2}$ lines, from which $\frac{1}{2}g$ is calculated as $2173\frac{7}{9}$ lines. Newton now decided that the buoyancy of the air subtracted $\frac{3}{5}$ of a line. Hence, he adjusted the value of $\frac{1}{2}g$ back up substantially to that of edition two, and for the rest of Proposition XIX he employed that figure. In Proposition XXXVII, where the slightly different figures of edition three lowered the value of $\frac{1}{2}g$ required for a good correlation, he simply forgot the buoyancy of the air, which would have made the correlation less precise.
19. Cotes to Newton, 16 February 1711/12 (*20*, pp. 61–62). It transpired that the fraction $\frac{5}{9}$ in the length of the pendulum had been a slip of Newton's pen. He had intended $\frac{1}{2}$.
20. J. Edleston, Ed., *Correspondence of Sir Isaac Newton and Professor Cotes* (John W. Parker, London, 1850).
21. Newton to Gregory, 14 July 1694 (*14*, p. 380).
22. Proposition XX (*9*, pp. 386–387).
23. S. Colepresse, "Of Some Observations made by Mr. Samuel Colepresse at and nigh Plymouth, An. 1667, by way of Answer to some of the Quaeries Concerning Tides," *Phil. Trans.* **3**, 632–633 (1668).
24. See R. Moray, "Patternes of the Tables Proposed to be Made for Observing of Tides," *ibid.* **1**, 311–313 (1666), and H. Oldenburg, "An Account of Several Engagements for Observing of Tydes," *ibid.*, pp. 378–379. Oldenburg's squib mentioned that Colepresse had agreed to observe the tides at Plymouth, and appealed for observations from Bristol, suggesting that Sturmy had not yet been set to work.
25. S. Sturmy, "An Account of Some Observations, made this Present Year by Capt. Samuel Sturmy in Hong-road within four Miles of Bristol, in Answer to Some of the Quaeries Concerning the Tydes," *ibid.* **3**, 814–815 (1668).
26. Cotes to Newton, 7 February 1711/12 (*20*, p. 58).
27. Newton to Cotes, 12 February 1711/12 (*20*, p. 61).
28. Cotes to Newton, 23 February 1711/12 (*20*, p. 68).
29. Cotes to Newton, 23 February 1711/12 (*20*, p. 65).
30. Newton to Cotes, 19 February 1711/12 (*20*, p. 67).
31. Cotes to Newton, 23 February 1711/12 (*20*, p. 73).
32. Cotes to Newton, 28 February 1711/12 (*20*, p. 76).
33. Cotes to Newton, 15 April 1712 (*20*, p. 94).
34. Cotes to Newton, 26 April 1712 (*20*, pp. 100–101).
35. Newton to Cotes, 22 April 1712 (*20*, pp. 95–96). The calculation that Newton gave in this letter is substantially that which appeared in the second and subsequent editions. In his customary careful manner Cotes recalculated it, altering the numbers slightly. The final numbers were his.
36. W. Thomson, *Popular Lectures and Addresses* (Macmillan, London, ed. 2, 1891), vol. 1, p. 80.
37. Cotes to Newton, 23 February 1711/12 (*20*, p. 68).