NORDITA publications

No. 173

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Archive for History of Exact Sciences 2 (1965) 365-386

NORDITA
Nordisk Institut for Teoretisk Atomfysik
Blegdamsvej 17, København Ø, Danmark

Offprint from "Archive for History of Exact Sciences", Volume 2, Number 5, 1965, P. 365—386

Springer-Verlag, Berlin · Heidelberg · New York

Newton and the Law of Gravitation

L. Rosenfeld

There has been recently a welcome revival of Newtonian studies. The Royal Society has at long last made a start with the publication of the correspondence[1]. Various scholars have brought to light much interesting new evidence from the too long neglected Newton manuscripts[2]. For the first time a thorough and competent study[3] has been made of a side of his activity which had hitherto remained obscure: his historical researches, coloured by theological considerations, to which he himself attached great importance. By piecing together all this new evidence with long known, but little understood facts, one arrives, as I intend to show, at a view of Newton's personality rather different from the traditional one. The latter is very much influenced by hero worship[4], but it does not help to react to this — as recent biographers are inclined to do — by hero debunking. Newton's personality is not easy to understand: secretive and suspicious as he was, one has to catch him, so to speak, in unguarded moments to get a glimpse of his thoughts and of his passions. To reconstruct a coherent portrait from the scraps of evidence gleaned from his papers, his letters and his actions is a hard detective work, but a rewarding one.

In Newton's thought, the problem of understanding the construction of the universe, which led him to the discovery of the law of gravitation, doubtless occupied a central position, although it was by no means the problem which he regarded as the most decisive and to which he devoted the greatest effort. Indeed, it is strange how casually he dealt with it before Halley with much difficulty managed to wring out of him the work which we esteem his greatest. There is certainly something in this attitude which demands explanation. The circumstances of the discovery of the law of gravitation present a further puzzling feature, which early attracted the attention of historians: after he had the

[1] Three volumes have so far appeared, covering the period up to 1694 (refs. [*1*—*3*]). In the following, documents published in the *Correspondence* will be quoted by their number, followed, for greater convenience, by the reference to the volume; thus no. 288 [*2*] means the letter no. 288, published in volume 2.

[2] Especially A. R. Hall [*4*, *5*], J. W. Herivel [*6*—*9*], A. Koyré [*10*], D. T. Whiteside [*29*, *30*].

[3] See Manuel's book [*11*] and my review of it [*12*].

[4] This is very conspicuous in Brewster's biography [*13*], but in fairness to this author, it should be stressed that he is remarkably accurate and reliable in his account of the facts. Much worse is the case of Thomson & Tait, whose judgement of the formulation of the fundamental laws of mechanics in the *Principia* (ref. [*14*], chapter II, especially § 263, 264) is very uncritical and vitiated by a quite unhistorical interpretation of "actio" as mechanical work.

first idea of the identity of the force of gravity on the earth with that governing the planetary motions, why did NEWTON not follow up the clue at once? Why did he allow twenty years to elapse before enunciating the law of universal gravitation?

To answer this question, a tale was put up, on slender evidence[5], to the effect that NEWTON's first estimate did not exhibit the expected identity between the two forces, because he used a wrong value of the earth's radius (on which, as we shall see presently, the result sensitively depends); he had to wait until a more accurate value of this radius became available, as a result af PICARD's triangulation, before he could repeat the calculation, which then allegedly confirmed him in his previous surmise: in order that the dramatic touch should not fail, it is even added that when he made this last calculation, his hand trembled so much that he had to ask a friend to finish it for him. How unlikely this whole story was did not escape the perspicacity of such competent scientists as ADAMS (the co-discoverer of the planet Neptune) and GLAISHER, a distinguished mathematician: they suggested a seemingly more plausible explanation, which could be supported by NEWTON's own declaration[6]. In a famous letter to HALLEY[7] of June 20, 1686, NEWTON alludes to "a certain demonstration I found the last year" which first gave him full assurance that the inverse square law was accurately valid down to the surface of the earth: obviously, what is here meant is the theorem on the attraction of a spherical shell, without which the argument leading to the identity of the force of gravity and the attraction on the moon has indeed no firm foundation. However, the question of this identity of the two forces cannot be the whole story: NEWTON had to struggle with many more issues, and the only way to elucidate the matter is to retrace all the stages of his long quest. In doing so, we shall at the same time gain insight into NEWTON's deeper motivations and the workings of his powerful mind.

Let us start at the beginning, in the autumn of 1665, when the young Cambridge scholar, having sought refuge from the plague in the family mansion of Woolsthorpe, passed the time in studious meditation[8]. For the authenticity of the story of the falling apple starting the decisive train of thought, we have the guarantee of NEWTON's own testimony, reliably reported by his friend STUKELEY [17]. Let us first try to reconstruct the argument: this is inevitably conjectural to some extent, since unfortunately no trace of it has been found so far in NEWTON's papers. However, there is indirect evidence to tell us that we are on safe ground: thus we know[9] that HOOKE, just about this time, was entertaining speculations about a power emanating from the celestial bodies, by which they would attract other bodies and influence their motion; HOOKE conceived that when acted

[5] See the account in BREWSTER's biography [13], vol. 1, p. 290—292; BREWSTER, however, is not entirely uncritical. A full survey of the problem has been given by CAJORI [15].

[6] See CAJORI [15].

[7] This is letter no. 288 [2]. The theorem in question is proposition 71 of book 1 of the *Principia* [16].

[8] See BREWSTER [13], vol. 1, p. 25—26.

[9] On HOOKE's views about gravitation, see especially BREWSTER [13], vol. 1, p. 283—288, and LOHNE [18]; in the latter's paper, the relevant texts are reproduced.

upon by such a force a body would deviate from its inertial motion and be constrained to revolve in a closed orbit in a similar way as a conical pendulum or a body attached to a rotating wheel; and in the latter types of motion, he was aware of the interplay of the force deflecting the body from its inertial motion and an "endeavour of recess" or centrifugal force, the two balancing each other along the actual path of the body. Assuming, then, that the young student (whose extraordinary gifts had already impressed his teacher ISAAC BARROW[10]) started from a similar conception of the nature of the moon's motion, we see that the main problem for him was to evaluate the centrifugal acceleration of this body on its very nearly circular orbit: for it would give him directly the acceleration due to the attraction from the earth. We shall therefore have to enquire how NEWTON came to know the expression for the centrifugal acceleration

$$A = \left(\frac{2\pi}{T}\right)^2 R \tag{1}$$

(or some equivalent one) in terms of the period of revolution T and the radius R of the orbit.

The next step is to assume for the attraction the inverse square law: one then finds its value at the surface of the earth from the proportion $a: A = R^2 : r^2$, where the small letters refer to the earth, the capitals to the moon; and one expects this acceleration a to be the same as that of a freely falling body. This step was not difficult to make for NEWTON if he knew the formula (1): for the inverse square law is an immediate consequence of it, when it is combined with KEPLER's third law of planetary motion $T \propto R^{\frac{3}{2}}$. We have direct evidence[11] that NEWTON did draw this conclusion at the time; indeed, as we shall see, it was also drawn at a later date, independently, by HOOKE and other *virtuosi*. It needed NEWTON's critical acumen, however, to be aware of its possible limitations: but of this later. For a numerical estimate of the acceleration a on the basis of the preceding argument, NEWTON needed, besides the well-known values of the moon's period of revolution T and the ratio $R:r$ between the radius of the moon's orbit and the earth's radius, also the absolute value of the latter: in fact, the value found for a depends linearly on that adopted for the earth's radius. Here we meet the question alluded to above: which value did NEWTON use? Let us now look into his early investigations of the law of centrifugal force; these will also suggest us a plausible answer to the last question.

The analysis of circular motion in terms of a centrifugal tendency goes back to GALILEI. In the second day of his *Dialogo* [19] of 1632, devoted to the examination of the objections raised against the earth's motion, the question comes up whether all bodies, however heavy, would not be hurled into space from the

[10] To be quite precise, BARROW was not too pleased with NEWTON when he examined him in geometry in 1664 (BREWSTER [13], vol. 1, p. 24), but he must have soon formed a better opinion of him, since NEWTON's discovery of the method of fluxions is recorded as early as May 20, 1665 (*ibid.* p. 25). The first written expressions of BARROW's esteem for NEWTON date from 1669 (letters nos. 5—7 [1]). More evidence on NEWTON's early intellectual development, as well as indirect information on the precise nature of his relations with BARROW in his student years, is found in a well-documented article by D. T. WHITESIDE [29].

[11] Letter no. 288 [2].

surface of the moving earth as a stone is hurled from a sling. With refined irony GALILEI contrives to put the refutation in the very mouth of Simplicio, who is driven by clever questioning to recognize that a body on the earth is not carried away along a tangent to the surface with the full velocity of the earth's motion, but only lifted along the radius towards the tangent at the immediately preceding position. GALILEI is not able, however, to give a quantitative analysis of the effect: he just has the correct feeling that it must be much smaller than the force of gravity.

HUYGENS [20] was the first to bring GALILEI's argument to completion. This work dates from 1659, when HUYGENS, who was then thirty, had reached the maturity of his genius. His treatment of the problem is masterly — better in fact than many a modern textbook exposition. GALILEI's point is made most

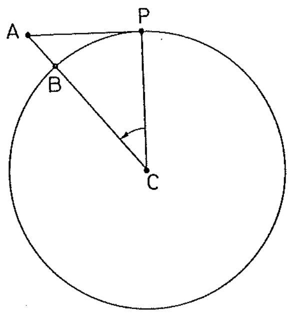

Fig. 1. Analysis of circular motion by HUYGENS

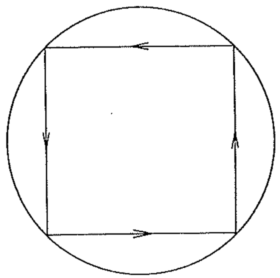

Fig. 2. Analysis of circular motion by NEWTON

elegantly by considering the motion from the point of view of an observer participating in the rotation: for such an observer the deviation from the inertial motion during a very short time interval may indeed be approximated by a motion towards the centre. This is shown by a minute kinematical analysis, exhibiting HUYGENS' skill in dealing with problems of continuity by the methods of the ancient geometers. If (Fig. 1) the arc PB is very small, the radial distance AB is easily seen to be approximately equal to the square of the arc PB, divided by the diameter, and thus proportional to the square of the time. This is precisely the same law as that of the free fall, so that the centrifugal acceleration is immediately seen to be given by the product of the radius and the square of the angular velocity, as expressed by the formula (1).

However, HUYGENS did not publish the law of centrifugal acceleration until 1673, when it appeared as an appendix to the *Horologium oscillatorium* [21]. NEWTON knew it in 1665 because he had discovered it the year before (at the age of twenty-two) by his own exertions. Early papers recently brought to light disclose the devious path by which he arrived at the goal: there is no trace here of the scholarly elegance of the Dutch physicist; NEWTON's approach appears by contrast curiously simple-minded and uncouth. It bears the mark of his teacher BARROW, whose didactic works herald the final break with the ancient geometrical tradition in favour of the modern analytical methods; a tendency still more evident, of course, in NEWTON's first attempts, from the same period, at a systematic representation of geometrically defined functions by infinite series.

In his analysis of circular motion[12], NEWTON discusses the case of a globe moving along a great circle inside a hollow sphere: the inertial motion of the globe is continuously impeded by the spherical surface, which experiences a pressure from the globe; this centrifugal pressure is measured by the change of momentum corresponding to the deviation of the globe's inertial motion. NEWTON first obtains a lower limit for the effect by observing that in a half-turn the velocity of the globe is just reversed: its total "endeavour from the center" in a half-turn is thus at least twice its momentum. A better estimate follows from the case (Fig. 2) in which the globe, with the same velocity as in the actual circular motion, just bounces four times in a complete turn against the sphere, thus describing the four sides of an inscribed square: this gives the proportion

$$\frac{\text{endeavour from centre at each reflexion}}{\text{momentum}} = \frac{\text{side of inscribed square}}{\text{radius of sphere}},$$

and further

$$\frac{\text{total endeavour from centre in one turn}}{\text{momentum}} = \frac{\text{perimeter of trajectory}}{\text{radius of sphere}}.$$

It is easily seen that this argument holds for any regular inscribed polygon as well as for the square, so that by a passage to the limit familiar from ARCHIMEDES, NEWTON could conclude that in the actual circular motion, the total endeavour from the centre in one revolution is to the momentum as the circumference to the radius: this gave him the value π instead of the lower limit 2 for the same ratio for a half revolution. Also, in view of the perfect uniformity of the process, we may say that the total endeavour from the centre during the time that the body describes an arc equal to the radius is just equal to the momentum. How can one pass from this "integral" law to an expression for the instantaneous force? For NEWTON's powerful intuition, the continuous change of direction of the centrifugal force is no embarrassment: its effect must be the same as if the motion, instead of being constrained to the spherical surface, were allowed to proceed on a plane: but then we have the problem solved by GALILEI of the effect of a constant force, like gravity, acting perpendicularly to the trajectory of a uniformly moving body. The total effect of the constant acceleration A during a time t is to produce a velocity At: now, we have found that if the time t is R/V (R denoting the radius of the sphere, V the velocity of the circular motion), the acquired velocity is just V. Therefore, $A = V^2/R$, an expression for the centrifugal acceleration equivalent to the formula (1) above.

It is not sure what incited NEWTON to this highly original study of the circular motion: it may have been, as certain of his notes suggest[13], a reading of DESCARTES' *Principia philosophiae*. However, while the latter's influence on HUYGENS as well as NEWTON remained paramount for their general conception of the transmission of force by contact, Cartesian dynamics was too crude and erroneous to be in the taste of such acute and independent minds. For both of them, the true source of inspiration in their dynamical thinking was GALILEI.

[12] My account is based on the documents published by HERIVEL [6], but differs in some particulars from his own interpretation of them [6, 8].

[13] See on this point HERIVEL's [6, 8] remarks. An early essay by NEWTON pertaining to DESCARTES' *Principia philosophiae* is published in ref. [5], p. 89—156.

Other manuscripts[14] from the same time give evidence of NEWTON's reading the *Dialogo*, an English translation of which by SALUSBURY had been available since 1661. In one of these[15], one finds a new derivation of the law of centrifugal force very similar to HUYGENS'; but it was clearly an afterthought. Arguments based on the principle of relativity, of which GALILEI and HUYGENS made such brilliant use, never appealed to NEWTON: what he took from GALILEI was rather, as exemplified by his analysis of circular motion, the more dynamical aspect of the law of inertia, the idea of investigating the forces determining the motion of bodies by the changes of momenta they produce.

One has also retrieved[16], and HERIVEL has very skilfully deciphered, the scrap of paper on which NEWTON jotted down the numerical computations leading to the quantitative estimates which were still wanting, as I pointed out above, in GALILEI's argument. We see NEWTON applying his newly acquired knowledge of the law of centrifugal force to calculate this force at the surface of the earth and compare it with the force of gravity. This precious document answers rather definitely the question which value NEWTON did adopt for the earth's radius in his famous meditation under the apple tree. A learned paper has been written on this question by CAJORI [*15*]: he painstakingly collected all the data he could find in the books on navigation of the time and went on to speculate to what extent NEWTON could be expected to have cognisance of this lore. He overlooked, however, one item, the one that was perhaps the most obvious and that at any rate has now turned out to give a decisive clue, to wit GALILEI's *Dialogo*. In the calculation just mentioned, NEWTON naturally enough borrows from the book he was studying the value he needed: it is unmistakably quoted as 3500 Italian miles. Admittedly, we cannot be absolutely sure that NEWTON used the same value in the other calculation, but the proximity of dates and circumstances makes it overwhelmingly probable. Now, this value is very rough indeed, about 16% too small: obviously, GALILEI regards it just as a round number easy to memorize and sufficient for rapid estimates. It is noteworthy that the value quoted by GALILEI for the acceleration of gravity is much worse: it is about half the true value. This NEWTON finds unacceptable, and he determines himself a more correct value by means of experiments on the times of oscillation of simple and conical pendulums; by itself another remarkable achievement. He is apparently unconcerned, however, about taking over GALILEI's value of the earth's radius.

If he used this value, he found a discrepancy of the order of 16% between the calculated attraction at the surface of the earth and the force of gravity[17].

[14] These are the manuscripts first published by HALL [*4*] and reproduced as documents no. 117 [*1*] and 347 [*3*]. A comparison of the latter with SALUSBURY's translation of GALILEI's *Dialogo* was performed by the late Prof. H. W. TURNBULL, first editor of the *Correspondence*, and is reported by HERIVEL [*7*]; it reveals convincing analogies.

[15] This is no. 117 [*1*].

[16] This is no. 347 [*3*], which is interpreted by the editor of the third volume of the *Correspondence*, Dr. J. F. SCOTT, but more fully by HERIVEL [*7*].

[17] This is in agreement with the figures quoted by BREWSTER [*13*], vol. 1, p. 26, in his account of NEWTON's work at Woolsthorpe: according to BREWSTER, he found for the deflexion in one second due to the attraction at the surface of the earth a value of 13.9 feet, whereas that due to gravity is 16.1 feet.

What did he think of such a result? We have the testimonies of PEMBERTON and WHISTON, who knew NEWTON well in later years[18]. They give the impression that he regarded the outcome as condemning the idea he wanted to test; they mention the wrong value of the earth's radius as the cause of the failure, but this is a retrospective consideration which throws no light on NEWTON's possible motive for being so casual about this constant. On the other hand, NEWTON himself, in a memorandum[19] of 1714, writes that he found his calculations "answer pretty nearly", which would suggest that he was not so dissatisfied with the result. After all, none of the commentators, prone to dramatization, has ever raised the simple-minded question: how nearly did NEWTON actually expect that the two accelerations would agree? WHISTON gives a further piece of information[20] which appears highly relevant in this respect: he states that NEWTON inclined to the conclusion that besides gravitation, some other cause, such as a Cartesian vortex, might contribute to determine the moon's motion. This is not as unplausible as it might seem: more direct evidence of the influence of Cartesian cosmology on NEWTON's early views of the moon's motion has indeed recently been brought to light[21]. Only by pursuing the story can we hope to find further clues. So far, however, it will be clear to every scientist that NEWTON at this stage had opened up for himself an exciting prospect, but had nothing fit to be published.

During the decade following his return to Cambridge, in 1667, we find NEWTON engrossed in his optical investigations, as well as busily engaged in mathematical correspondence with COLLINS. There is important evidence from these years, however, that far from losing sight of the problem of gravitation, he developed about the nature of this force speculations of extraordinary depth and boldness. The first piece of evidence, although somewhat indirect, is significant enough to be quoted. When in 1673 he received from HUYGENS a copy of the *Horologium oscillatorium*, NEWTON did not fail, in his message of thanks, to intimate in a covert way (as was the custom among the *virtuosi*) that he also had long known all about the centrifugal force: this he contrived to do by giving as an example of its usefulness the application he had made of it to the comparison of the respective attractions exerted by the earth on the moon and by the sun on the earth. Alluding much later[22] to this message, NEWTON believed that he had even mentioned explicitly in it that such a comparison would lead (in connection with KEPLER's third law) to the inverse square law for the attraction; in fact, this is

[18] See BREWSTER [*13*], vol. 1, p. 290—292 and CAJORI [*15*].

[19] Quoted *e.g.* by CAJORI [*15*], p. 160.

[20] This is quoted by BREWSTER [*13*], vol. 1, p. 290.

[21] See an important paper by D. T. WHITESIDE [*30*], who analyses early astronomical manuscripts of NEWTON and annotations found in books he read and traces their relation to the contemporary background, of which he makes an extensive study. It is noteworthy that NEWTON showed as little appreciation as the practical astronomers of his time for the significance of KEPLER's two first laws. In particular, he ignored the second law (the law of areas) until he found that it was a consequence of the inverse square law of attraction.

[22] See the beginning of letter no. 116 [*1*] to OLDENBURG, with the editors' comment. Newton refers to this letter in his correspondence with HALLEY in 1686 (nos. 288 and 291 [*2*]).

not in the letter, as he soon could ascertain when he found a copy of it; the omission surprised him: obviously, he remembered his original intention of communicating to HUYGENS a fuller account of his old investigations. This shows at any rate that he had not given them up.

Another curious fact emerges from an exchange of letters[23] he had in January 1680/1 with THOMAS BURNET at the occasion of the publication of the latter's book *Telluris theoria sacra*, which represented one of the earliest attempts at a scientific theory of the formation of the earth. BURNET having asked, among other things, for NEWTON's opinion about the shape of the earth, the latter replies that to the best of his judgement, based on the analogy with the other planets, it is spherical; he intimates that the effect of rotation must be negligible. Besides, he cannot tell what to make of the evidence of geodetic measurements, "not knowing how exactly those measures were made or the latitudes of places taken". This statement helps us at least to understand why he did not show even at this later date more eagerness to look for the best available value of the earth's radius.

The discussion with BURNET is interesting from another, more general, point of view. It gives us a glimpse of an aspect of NEWTON's thinking so uncongenial to us that it has been mostly neglected or misunderstood by biographers: I mean his attitude to theological problems. In the present instance, while BURNET dismisses the first account of creation in *Genesis* as purely "ideal", without relation to "physical reality", NEWTON defends it, most ingeniously, as a consistent description of physical phenomena as they would have appeared to a human observer if any such could have witnessed them from a terrestrial vantage point. As to the phenomena themselves, he imagines the gradual formation of the sun and planets as local condensations of the primeval chaos, perhaps by an entirely natural cause, such as the action of gravitation. The length of the successive "days" he conceives as gradually decreasing as the earth acquires its present state of rotation: but here he allows for an immediate divine intervention, estimating natural causes insufficient to produce such a rotating motion. This mixture of rationalism and theology is hard for us to appreciate; but I hope to show in the last part of this essay that it can be made more accessible by taking due account of all circumstances. At any event, it would be quite wrong methodically to disregard a side of NEWTON's activity to which he himself attached perhaps more importance than to his scientific work.

Of still greater interest for our enquiry is another document which reveals to us NEWTON the scientist engaged in considerations of a character quite different from the penetrating inductive enquiries into the laws of nature for which he is commonly celebrated: it is a long paper[24], written some time about 1675, and in which NEWTON develops the hypothesis of a universal aether, as the agent by which not only the various forces acting on matter are propagated, but even muscular motion is initiated by the "soul". There are several other manuscripts[25], as well as material published in the form of "queries" at the end of the *Opticks*, which extend and modify the views contained in the paper we are considering; but it is the latter that plays a decisive part in the course of events we are trying to analyse. It was communicated to OLDENBURG, the secretary of the Royal Society, on December 7, 1675, the immediate occasion being NEWTON's controversy with HOOKE on the nature of light. NEWTON had vainly tried to avoid this controversy by arguing that his theory of colours was independent of any assumption about the physical constitution of the "rays" carrying these colours. Now, he wanted to show HOOKE that he had nothing to learn from him on this last question either, and that indeed he had thought more thoroughly about the constitution of the universe than any of those *virtuosi* whose heads "run much upon hypotheses".

The deeper motivation, of course, was to acquaint at least the fellows of the Royal Society with a grand conception of the workings of the universe on purely mechanical principles, in the best Cartesian spirit. NEWTON's attitude about it is curiously ambiguous: he feels that it is not mature and is accordingly reluctant to commit himself; but he clearly regards it as so fundamental that he is impatient to present it to the judgement of his peers. He does not let OLDENBURG publish it[26], but he allows it to be "registered", *i.e.* copied in the register of the Society, which is only accessible to the fellows. NEWTON's fondness for these speculations, which so sharply belie his "*hypotheses non fingo*", is a challenge to the historian. I shall take it up in a little while, and have occasion then to stress again the intimate relationship between NEWTON's scientific speculations and his theologically coloured metaphysics.

For the moment, let us only retain the remarkable interpretation of the force of gravitation proposed by NEWTON in the framework of his aether hypothesis. In the paper itself, there is only a brief and not too clear indication of it; when, however, in 1686, NEWTON has occasion, as we shall see, to refer HALLEY to it, he adds some comments which greatly clarify the idea without modifying it in any way[27]. The idea is that a body like the earth, or the sun, is the seat of a cyclic process of transformation of the aether: a stream of aether falls continually upon the earth and pervades all its parts, its density increasing as it loses momentum in interaction with the gross matter of the earth; this condensed aether would then continually escape from the earth to form the atmosphere and further to disperse into the "aethereal spaces" where it would resume its original form, thus completing the cycle. In modern terms, a constant inward stream of S aether particles per unit time moving with radial velocity v, has at a distance R from the centre, a density $S/4\pi R^2 v$, increasing in the inverse ratio of the velocity. Now, such a stream will exert on gross matter a pressure $S\,m\,v/4\pi R^2$ directed towards the centre and, so long as the velocity does not change appreciably, inversely proportional to the square of the distance, just as is required for the attraction of the sun on the planets by KEPLER's third law. Thus we see that in his bold speculations about the constitution of the material world, NEWTON thought of gravitation as a universal force having all the appearance of an attraction obeying the inverse square law, although it actually proceeded from a contact interaction between aether and matter. Again we understand why he did not publish his ideas at such an immature stage, although his allowing them to be

[23] Letters nos. 244, 246, 247 [*1*].
[24] Letter no. 146 [*1*].
[25] See especially ref. [*5*], part III.

[26] NEWTON's cautious directions to OLDENBURG are found in his letters nos. 147, 151 (postscript), 153 [*1*].
[27] See the end of the letters 288 and 291 [*2*].

"registered" is an indication of how confident he was to be on the track of essential truths.

On the other hand, as we have noted, he fully upheld the conclusion of his reflexions in the Woolsthorpe garden, but apparently did not think it worth his while to try and improve the result, for example by resorting to a better value of the earth's radius. I think this behaviour is not so puzzling as it looks. Scientists do not relish the strenuous work which any detailed investigation demands; they only engage in such work under compulsion. NEWTON was not different in this respect from the rank and file of his craft. Now, it so happened that in 1679 and the following years the moment of compulsion arose from an extraordinary succession of casual events, and literally drove NEWTON to the completion of his great work.

In December 1679 HOOKE, who had succeeded OLDENBURG as secretary of the Royal Society, took over the care of the scientific correspondence, previously assumed by a second secretary. He used the opportunity to make another effort to restore his personal relations with NEWTON, which had been badly strained by the light controversy. A previous attempt, immediately after the dispute, in 1675/6, had been received by NEWTON (who was HOOKE's junior by seven years) with haughty condescension[28], and the kind man fared no better this time *(tantaene animis caelestibus irae!)*. In inviting[29] NEWTON to communicate to the Society "what shall occur to you that is philosophical", he made the diplomatic blunder of soliciting also NEWTON's "objections against any hypothesis or opinion of mine", and of mentioning particularly his ideas about "compounding the celestial motions of the planets of a direct motion by the tangent and an attractive motion towards the central body". NEWTON excused himself[30] by the pretext, outrageously improbable, that he had been lately so much out of touch with "philosophy" as to be ignorant of recent productions: thus, he did not remember having ever heard of HOOKE's hypothesis about celestial motions.

Nevertheless, NEWTON — to "sweeten" his answer, as he later wrote[31] to HALLEY — did insert in it a philosophical communication. He pointed out that a proof of the earth's rotation could be obtained by observing the deviation of a falling body from the vertical towards the east. In the figure (Fig. 3) illustrating the argument, he prolonged the path *AD* of the falling body to the centre *C* of the earth as a smooth curve *DEC*. This remark elicited a prompt reply[32] from HOOKE: if the body could proceed inside the earth, he said, it would not describe such a curve as Newton had sketched, but rather an ellipse-like trajectory around the centre (the curve *AFGH* on Fig. 4) bringing it back to its starting point, if there was no resistance to the motion; allowing for such resistance, the path would be a kind of spiral like *AIKLMNOP* ultimately reaching the centre *C*. Moreover, the deviation from the vertical would only be exactly to the east at the equator; in our northern latitudes, however, it would rather be to the south-east, and in London even rather more south than east.

[28] This alludes to the oft-quoted exchange of letters nos. 152 and 154 [*1*].
[29] HOOKE's letter to NEWTON no. 235 [*2*].
[30] NEWTON's reply no. 236 [*2*].
[31] In his letter no. 288 [*2*].
[32] Letter no. 237 [*2*].

Very much against his wish[33], NEWTON was thus forced to continue the correspondence, and, worse still, to admit that he had been in error in the shape of the inside part of the trajectory. However, he does not deign to comment on HOOKE's solution, but proceeds to discuss the shape that the trajectory would have if one assumed the force of gravity to be constant over the whole inside of the earth. To this HOOKE retorts in the next letter[34], which ends the debate, that he had in mind the case in which the attraction is assumed to obey the inverse square law down to the centre. The problem of gravitation was one on which HOOKE, in the midst of his busy life, never ceased to meditate in his own acute and imaginative fashion[35]; he has also stated quite plainly[36] the motive for this sustained interest:

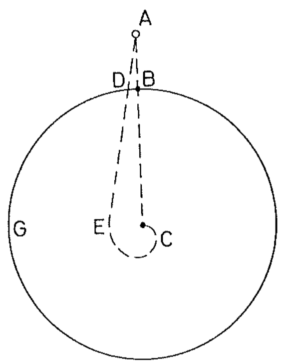

Fig. 3. Path of falling body prolonged to the centre of the earth, according to NEWTON

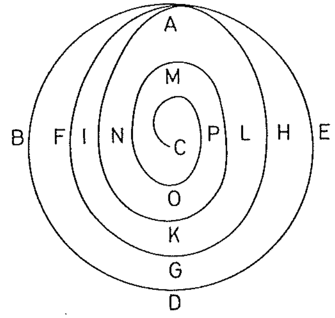

Fig. 4. Path of gravitating body inside the earth, according to HOOKE

the utility of a good theory of planetary motions for solving the great problem of navigation, to determine the longitude of a ship's position. He must have realized that the attraction of the sun on the planets must obey the inverse square law as soon as he learned from HUYGENS' book the law of centrifugal force; we know from entries in his diary that he read the book in November 1675 and at once started thinking about the motion of the planets. Now, we find from this memorable exchange with NEWTON that by 1679 he had made the correct guess about the form of the orbit resulting from this law.

More than a guess it could not be, for HOOKE totally lacked the mathematical powers needed for tackling such a problem; one may even say (as NEWTON later actually did, in a fit of anger[37]) that it was an easy guess, since after all it was

[33] NEWTON's annoyance is vividly conveyed by his later account of this correspondence with HOOKE to HALLEY (letters nos. 286 and 288 [*2*]). His second reply to HOOKE is the letter no. 238 [*2*]. It must be observed that the figure belonging to this letter is incorrectly reproduced in the *Correspondence;* a photograph of the original figure is given in the paper [*18*] by LOHNE, in which the present controversy between HOOKE and NEWTON is discussed in great detail.
[34] Letter no. 239 [*2*].
[35] All relevant documents about HOOKE's studies on gravitation may be found in LOHNE's excellent paper [*18*]. In this paper, LOHNE takes HOOKE's side with as much ardour as if the two contending worthies were still alive! Even though I am not able to accept all his arguments or to subscribe to all his conclusions, I derived much help from his careful analysis.
[36] In letter no. 239 [*2*].
[37] In the passionate postscript of the letter to HALLEY no. 288 [*2*].

known since KEPLER'S time that elliptic orbits fitted the observations satisfactorily. But to have clearly recognized that the shape of the orbit is determined by an attractive force from the sun obeying a given law of dependence on the distance, and that the fundamental problem of astronomy was the mathematical derivation of the orbit from the law of attraction, represented a remarkable intellectual feat which is entirely to HOOKE's credit. Even NEWTON, however guardedly he behaved towards him, is candid enough to acknowledge[38] the stimulation he received from HOOKE: "Your acute letter having put me upon considering thus far the species of this curve, I might add something about its description by points *quam proxime*."

This last sentence reveals at the same time that NEWTON was so well prepared to tackle the problem that in response to HOOKE's challenge he could rapidly master it, at least in its essential features. As he explains elsewhere in the same letter, he considers motion "according to the method of indivisibles", as a succession of "innumerable and infinitely little motions ... continually generated by gravity"; and this leads him to a construction of the trajectory point by point. If NEWTON here refers to the method of indivisibles, it is no doubt because he did not think that HOOKE could be acquainted with his own method of fluxions, of which nothing was then published. To arrive at a global characterization of the trajectory is of course a much tougher problem, whose general solution was reserved to the Leibnizian school. However, when NEWTON himself tells us in later memoranda[39] that he did deduce the elliptic form of the orbit from the inverse square law towards the end of the year 1679, we may well trust him to have solved this particular case "by the inverse method of fluxions" even in its rudimentary form.

A touch of comedy is added to this dramatic development by NEWTON'S acquiescence to HOOKE'S erroneous statement about the southern deviation of the falling body[40]. It is curious that NEWTON did not notice the error and persisted in it until he took up the whole problem of the form of the earth when writing the *Principia*.

During the month of November 1680, a spectacular comet was observed moving towards the sun, and another one was seen in the latter part of December and in the following January moving away from the sun in the opposite direction with about the same velocity. This prompted FLAMSTEED, the recently appointed Astronomer Royal, to put forward the view[41] that the two comets were actually one and the same, whose path had undergone a complete reversal in the vicinity of the sun. He endeavoured to account for this effect by some magnetic action of the sun, changing from attraction to repulsion as the comet entered the sun's vortex and was thus deflected from its direct path towards the sun; he naively imagined that the magnetic axis of the comet would follow the deflexion of the trajectory, so that it would eventually present its other pole to the sun: just as a bullet, as gunners apparently believed, always kept the same side forward along its path. Solicited for his opinion, NEWTON gave a civil, but devastating reply[42]. It would be tempting to dwell on its refutation of FLAMSTEED's magnetic hypothesis, since it shows us NEWTON at his best as a natural philosopher; but it is a side issue. I shall only stress one point, because of its interest for the social aspect of the evolution of science: on the opinion just recorded about the behaviour of a bullet, NEWTON commented that it "may be a tradition of the gunners, but I do not see how it can consist with the laws of motion, and therefore dare venture to say that upon a fair trial it will not succeed excepting sometimes by accident". Such an utterance marks the point at which the science of mechanics emancipates itself from the empiricism of craftsmen and enough confidence is felt in its laws to make predictions about the outcome of yet untried experiments. More to our point, however, is NEWTON's extraordinary attitude to FLAMSTEED's identification of the two comets: he is most reluctant to accept it[43], because it would make this comet "paradoxical"; all comets known so far have been observed to move in the same sense on both sides of the perihelion "in a line almost straight". Not until 1685, when he was working out the theory of cometary orbits for the *Principia*, did he recognize the correctness of FLAMSTEED's skilful interpretation of the observations[44].

Nevertheless, he is willing to discuss the possibility of a curved orbit and explains[45] to FLAMSTEED that no repulsive force is needed to bring it about: an attraction from the sun, such as that "whereby the planets are kept in their courses about him", could produce such an orbit. Only the comet would then turn around the sun, and not (as FLAMSTEED supposed) be deflected before reaching it; and by a direct method he has of computing an orbit (of whatever shape) from exact observations, he finds that the last such observations (including some of his own in February and March 1681), when extrapolated backwards, indicate December positions well beyond the sun. Here then we witness NEWTON in possession of all the theoretical tools enabling him to assimilate cometary motion to that of the planets, but severely refraining from taking the step because he is not satisfied that it is granted by the data of observation. However convinced he may have been of the universality of the force of gravitation, the strict rules of his natural philosophy forbade him to draw rash conclusions.

NEWTON and FLAMSTEED held each other in high esteem; in spite of all disagreement, the tone of their correspondence is a model of courtesy and serenity. When he started work on the *Principia*, at the end of 1684, NEWTON consulted FLAMSTEED on various points of astronomical observation[46]. Among other things, he was worried about the fact that KEPLER's determination of Saturn's orbit was not in agreement with the third law; he suspected that the discrepancy

[38] Letter no. 238 [*2*] (last paragraph).

[39] These are quoted in LOHNE's paper [*18*]. Nevertheless the latter contends, in my opinion on insufficient grounds, that NEWTON was not able to solve the problem; he repeats this assertion in another interesting paper [*22*] on NEWTON's theory of colours.

[40] The nature of the error is very well explained by LOHNE [*18*].

[41] FLAMSTEED's views may be inferred from the surviving part of his correspondence on this topic with HALLEY (letter no. 250 [*2*]) and NEWTON (letters nos. 251 and 254 [*2*]).

[42] Letters nos. 251 and 254 [*2*].

[43] Letter no. 255 [*2*].

[44] See NEWTON's letter to FLAMSTEED of 19 September 1685 (no. 281 [*2*]).

[45] Letters nos. 254 and 255 [*2*].

[46] Especially letters nos. 274, 275, 276 [*2*].

could be due to the perturbation of this orbit at conjunction with Jupiter, and asked FLAMSTEED whether he had observed deviations from KEPLER's tables at such conjunctions, corresponding to the estimated perturbation. In his reply FLAMSTEED expressed surprise that the two planets could be thought to influence each other to any appreciable amount, and gave NEWTON indications about his own observations of their orbits. Thereupon NEWTON explained that in computing the perturbation of Saturn's orbit, he had assumed an inverse square law of interaction between the two planets; but he could see from the new data that he had overestimated the "virtue" of Jupiter. "Your information," he adds, "about the error of KEPLER's tables for Jupiter and Saturn has eased me of several scruples. I was apt to suspect there might be some cause or other unknown to me, which might disturb the sesquialterate proportion" (by which is meant KEPLER's third law). At this late date, as we learn from this incident, he is not yet sure whether the third law "fills the heavens", he still keeps an open mind with regard to its universal validity and still contemplates the possibility of some other "cause" besides gravitation influencing the planetary motions.

We are now coming to the last act of the drama. In spite of his renewed interest in the problem of gravitation, there is no saying how long NEWTON would have gone on withholding publication of his powerful methods and the momentous conclusions he had already reached with their help, had it not been for another fortunate circumstance which brought into the picture one of the most gifted men of the new generation, EDMOND HALLEY. Grown up in the midst of those who had first shaken off the shackles of tradition and shaped the modern spirit of enterprise and enquiry, he was following their lead with all the eagerness of youth and the vigour of an uncommon intelligence. Admitted to the Royal Society in 1678, at the age of twenty-two, he was alert to the questions of the day and discussed them with the most distinguished *virtuosi*. Thus it happened[47] that on one Wednesday in January 1683/4, HALLEY having met Sir CHRISTOPHER WREN and HOOKE in town, the conversation turned as so often before on the great problem of the planetary motions. How the motion had to be analysed as inertial displacement modified by attraction, and how one could conclude that the attraction obeyed the inverse square law, was common knowledge to the three of them, but the *crux* of the matter was how from this knowledge to derive the form of the orbit. HOOKE boasted that he could do it, but the others were not to be contented with mere assertions. Sir CHRISTOPHER offered a prize of a book worth 40 shillings to the one who would produce the solution within two months: needless to say he had not to incur the expense.

HALLEY, however, did not let the matter rest. In the following May[48], he visited NEWTON in Cambridge and put the question to him. NEWTON replied at once

[47] The story is told by HALLEY in his letter to NEWTON no. 289 [2]. NEWTON himself (letter no. 286 [2] to HALLEY) mentions a conversation he had with WREN about the problem of the planetary orbits as early as 1677, when it seems that WREN was already acquainted with the inverse square law.

[48] About the dates of HALLEY's visits to NEWTON, there is some uncertainty due to discordant testimonies. The question has been ingeniously discussed by HERIVEL [9], whose conclusions I adopt.

that he had shown the orbit to be an ellipse, but he could not immediately reproduce the argument. He promised HALLEY to send it, and indeed he was hard at work during June and July drafting a treatise[49] *De Motu* in which he enunciated in the traditional style, as a succession of definitions, axioms and propositions, the laws of motion and their application to the case of the inverse square law of attraction: this was the kernel of the future *Principia*, and also, more immediately, the text of his Lucasian lectures for the following Michaelmas term. HALLEY probably visited him again in August[50], when he "learnt the good news" that NEWTON "had brought this demonstration to perfection", and in November received at last the promised paper. HALLEY thereupon paid another visit to NEWTON and persuaded him that he ought to write up a full-scale book on the whole subject. In order to allow him the required leisure while securing his priority, he proposed to have the tract *De motu* presented to the Royal Society and entered upon their register. With NEWTON's consent, he could make this announcement at the meeting of the 10th December, and about the middle of the following February, the copy destined to the Society was received and duly registered[51].

HALLEY had thus not only procured the fellows of the Society the possibility of acquainting themselves without delay with the gist of NEWTON's ideas, but he had even managed to launch the latter on the elaboration of a complete exposition of them. NEWTON's interest and energy were now thoroughly aroused, and the work progressed with remarkable speed: on April 28, 1686 the manuscript of the first book of the *Principia* was presented to the Society[52]; the second book followed in the autumn of that year and the third in April 1687; the finished work appeared about midsummer 1687: it was HALLEY again who had assumed all the chores, as well as the financial burden, of seeing it through the press[53]. How could HALLEY succeed so easily in overcoming NEWTON's bent to procrastination and bringing him to such momentous decisions ? I think the answer is simple: NEWTON was a man of very sensitive disposition, who must have felt very lonely among the Boeotian crowd of fellows and students; no wonder that he at once fell under the charm of a brilliant and enterprising young man, to whom he could explain his thoughts with the assurance of an intelligent response. Later, he behaved towards FATIO DE DUILLIER with fatherly kindness and solicitude, no doubt for the same reason[54].

Before he could help the undertaking to its happy completion, HALLEY had still to weather a storm which put to a severe test his diplomatic talent[55]. At the

[49] One of the four extant versions of this treatise is published in ref. [5], pp. 237—292. The moot problem of ascertaining the relation of these manuscripts with the text communicated to the Royal Society is thoroughly treated by HERIVEL [9].

[50] Letter no. 289 [2].

[51] NEWTON expresses thanks for the registering in his letter no. 278 [2] of 23 February 1684/5, in which he declares his intention "to finish it" (*i.e.* the proposed book), after he has returned from a journey to Lincolnshire, "as soon as I can conveniently".

[52] Letter no. 285 [2].

[53] Letters nos. 300, 303, 304, 306, 309 [2].

[54] The attitude of NEWTON towards FATIO DE DUILLIER is revealed by their letters newly published in the third volume of NEWTON's *Correspondence*. See my review of this volume [23].

[55] The acts of this memorable incident are the oft-quoted letters nos. 285, 286, 288, 289, 290, 291 [2].

memorable meeting of April 28, 1686 when much praise was being lavished on NEWTON's discoveries, HOOKE was piqued not to hear any mention of his own contributions to the problem, which indeed, as we have seen, were entirely in the same line as NEWTON's. He may well be forgiven the human weakness of giving his own ideas a higher estimation than they deserved and even of fancying that NEWTON had borrowed from him the inverse square law. At the coffee-house where the society adjourned after the meeting, he voiced his claims without success, the others being of opinion that he had only himself to blame "for having taken no more care to secure a discovery which he puts so much value on". The incident was reported to NEWTON, very tactfully by HALLEY, but less so by others. It incensed him so much that he threatened to suppress the third book of his work, that which had to treat of the astronomical applications of the theory. He soon relented, however, after another epistolary masterpiece[56] of HALLEY's, which soothed him to the extent that he expressed regret for his outburst of anger, and touchingly declared[57] that he would acknowledge having learned from HOOKE that the deviation of a falling body would be south-east in our latitudes.

NEWTON's exasperation was indeed out of proportion with the objective facts of the case: HOOKE's boastfulness and reckless priority claims, arising from a fiery and uncritical imagination, were well-known, and nobody took them more seriously than could be helped. NEWTON had never quite forgiven HOOKE's questioning of his optical discoveries, and one understands that he might feel irritation at being once more the object of a futile accusation; but in his present position he could well have afforded to ignore it. Instead, he fills passionate pages in defence of the originality of his conceptions, retracing their origin to his student days: precious pages indeed that disclose to us, besides details not otherwise known, how he himself judged the successive stages of his investigations.

It his here that we learn, in the first place, the circumstance mentioned at the beginning of this essay, that NEWTON had never thought the inverse square law to remain valid down to the surface of the earth, until he had found the theorem on the attraction of spherical shells some time in 1685, while he was writing up the first book of the *Principia*. This would seem to settle the question of the famous discrepancy in the early comparison of gravity and attraction: he could not ascribe to this estimate more than an indicative value, and there was thus little point, he thought, in improving it by using a better value of the earth's radius; he would not have expected the error in the value he adopted to be so large, and was rather inclined to regard the discrepancy in question as the measure of a real physical effect. Only after discovering the theorem on the attraction of the spheres would he suspect the discrepancy to be spurious and verify that it actually disappears if one adopts PICARD's value for the earth's radius[58]. By that time

[56] This is letter no. 289 [2].
[57] Letter no. 290 [2].
[58] In his letter no. 288 [2] to HALLEY, NEWTON mentions, in connection with his message to HUYGENS alluded to above, the early paper (document no. 117 [1]) containing the computations of centrifugal accelerations on which this message was based. However, he also states that in this paper the ratio of the acceleration of gravity to the centrifugal acceleration of the moon "is calculated" (a calculation unfortunately not found in the document as we have it) and then adds, casually, "though not accurately enough".

however, he had started writing the *Principia*, and the correction of the error had therefore no influence on his decision to publish the results of his studies. In fact, he had by then so many proofs that the law of gravitation "fills the heavens" that the particular argument which had first oriented his thoughts to this enquiry receded into comparative insignificance. In the third book of the *Principia*, it is merely used to show that no other attraction than the universal gravitation acts at the surface of the earth; and another source of inaccuracy appears: the non-sphericity of the earth, itself a consequence of the combined action of gravitation and rotation[59]. We have here an example of a frequent occurrence in the history of science: the degradation, as I would call it, of heuristic arguments which, once they have played their role in guiding the discoverer to some truth of wider scope, appear in a dimmer light when they are contemplated from the higher viewpoint they have helped to reach.

There is, however, in NEWTON's review of the circumstances manifesting his early understanding of the inverse square law, a remarkable and, I think, very revealing feature: it is the way in which he adduces[60] as evidence the paper of 1675 on the aether hypothesis, for which he refers HALLEY to the register of the Royal Society. He is a bit embarrassed by the fact that in the short passage on gravitation inserted there as an afterthought (it was "interlined" at the last moment in the original manuscript) no mention is made of the inverse square law; he is so uneasy about this that he returns to the matter in a further letter[61] to HALLEY, in which he gives the explicit derivation of the law from the hypothesis of an aether stream which I tried to formulate in modern language earlier in this essay. But his reason for invoking this hypothesis is interesting: he points out that it leads to the inverse square law only "upward" from the surface of a planet, *i.e.* in free space, where the velocity of the aether stream remains constant, but not "downwards", inside the body, where the aether loses momentum. This is why in the correspondence with HOOKE about the falling body he was careful not to assume the validity of the inverse square law inside the body — in contrast to HOOKE, the "bungler", who was not aware of this limitation.

Although he describes[62] the hypothesis, rather misleadingly, as "one of my guesses which I did not rely on", he obviously regards it as sufficient to establish the limitation in question — otherwise there would be no point in invoking it to expose HOOKE's "error". People who want to look upon NEWTON as the great master of the inductive method (which he is) may be surprised, but there it is: *habemus confitentem reum*. The half-hearted admission that he should not rely on a mere hypothesis while he is in fact relying on it reveals a tension between two tendencies equally powerful in NEWTON's mind, of which many instances can be found in his writings. There was a contradiction between the rigorous requirements of rational analysis and the urge for a comprehensive, intuitive synthesis,

[59] It is with this complication in mind that NEWTON, in his letter no. 290 [2] to HALLEY, returning to the early paper mentioned in the preceding note, says that the calculation of the centrifugal force arising from the earth's rotation "is a thing of far greater difficulty than I was aware of". See further *Principia* [16], book III, proposition IV.
[60] In the letter no. 288 [2], both in the body of the letter and in the postscript.
[61] In the letter no. 290 [2].
[62] Letter no. 288 [2], at the end of the postscript.

that NEWTON could never overcome, and whose roots must be sought much deeper than in any question of scientific method. Indeed, the profound meaning of NEWTON's conception of the aether, and the explanation of the extraordinary value he attached to it in all his life can only be appreciated against the general philosophical and religious background of his activity.

Of paramount influence on NEWTON's philosophical outlook was HENRY MORE[63], who with BARROW represented at Cambridge, in NEWTON's student days, the progressive current, open to the modern spirit in science and philosophy. Both men came from the same district of Lincolnshire, a Puritan stronghold, and it was a pupil of MORE who taught NEWTON mathematics at Grantham school. At Cambridge they remained close friends until MORE's death in 1687. MORE brought to England the ideas of the Italian humanists of the XVIth century, who were fighting the scholastics by playing up PLATO against ARISTOTLE: hence he became known as the "Cambridge platonist". This is a misnomer, however, for little of PLATO's ideal world remains visible under the strong Puritan colouring MORE applied to it. Indeed, of the two key concepts of his system, space and God, the first is derived from the Italian school, the second decidedly British. The Italians under the influence of the Copernican view of the world made space infinite and conceived the stars as so many systems similar to ours freely roaming through it; thus space as the substratum of moving things was thought of as existing independently of them and itself eternally at rest. All things were conceived as animated, whether material or immaterial, moving in this substantial space, in which God also was everywhere present. All this cosmology was taken over by MORE, and through him by NEWTON as well.

Characteristic for the English aspect of MORE's system is his conception of God as the absolute master of the world, creating things at will and capable of acting upon them, or even destroying them, at any time according to his own designs. In particular, man's relation to God is that of a servant to his master: it is governed by God's absolute power, not by any of its other attributes: as NEWTON remarks[64], we say "my God", "my master", but not "my infinite", "my eternal". Yet, God's omnipotence is compatible with man's free will, as well as with the regulation of all natural phenomena by the laws he has designed. This insistence on the personal aspect of God as the ruler of the world is a striking feature of religious thought in England in the latter part of the XVIIth century, and its historical origin is not far to seek: it is indeed a faithful transposition onto the theological plane of the political ideology developed by the bourgeoisie after the failure of its first experiment in self-government and the recall of the king. Incapable of finding a source of sufficient authority among themselves, they put all authority in the hands of a ruler not belonging to their own class, but hedged the king's authority by law in order to preserve their individual liberty.

[63] This point is most forcefully made in an important paper by M. FIERZ [*24*], which also contains a detailed exposition of MORE's ideas and of their origin in Italian philosophy.

[64] In the *scholium generale* at the end of the second edition of the *Principia*. This edition appeared in 1713, but NEWTON was working on it long before; there is a draft of the *scholium* dating probably from before 1697 (about which see ref. [*25*] and BREWSTER [*13*], vol. 2, p. 154).

Thus the subject submitted to the king's will, but expected the latter to act according to laws designed for the common good and guaranteeing the freedom of the individual.

This is not a far-fetched theoretical interpretation. We have direct evidence that NEWTON's political philosophy was precisely what I have just outlined. When King JAMES in February 1686/7 tried to force the University to admit an unqualified monk to the degree of master of arts, NEWTON advised resistance[65], not in a rebellious spirit, but because it was a clear legalistic issue: "all honest men are obliged by the laws of God and man to obey the king's lawful commands. But if his Majesty be advised to require a matter which cannot be done by law, no man can suffer for neglect of it." And he confidently concludes: "An honest courage in these matters will secure all, having law on our sides."

If the balance of political relations between the king and his subjects was precarious, dealings with the divine power could also be troublesome. Especially the pioneers of modern science, the natural philosophers, were dangerously exposed to the allurements of materialism, and had a difficult course to steer between this Charybdis and the Scylla of pantheism or deism. Against the latter, NEWTON is careful to warn us: God is not duration or space, he is everlasting and everywhere present; he is not the world-soul, but the world-ruler. The main argument for upholding the conception of a personal God was the old one of design: the regularities of the natural phenomena could not have been produced by chance, they betrayed the existence of a supremely wise and intelligent being who had designed everything according to the function it had to fulfil in the grand harmony of the whole creation. At a time when the wonders of nature were just beginning to reveal themselves to critical scrutiny, this argument carried a great weight: for HUYGENS, it was indeed the only reason to retain a belief in a deity[66]. To the generation following that of NEWTON, it was already losing its glamour, and atheism became the fashion. BOYLE was so alarmed at this deterioration of morality that he instituted by bequest annual lectures on the evidences of Christianity. The first Boyle lectures were delivered in 1692 by BENTLEY, who in particular adduced as evidence of design the constitution of the solar system, newly reduced to law by NEWTON: he obtained the latter's active support in getting his arguments straight, and the BENTLEY letters[67] illustrate the curious mixture of caution and assurance with which NEWTON handled finalistic causality.

God has not laid bare his design to man, but he has endowed him with reason so as to enable him to discover it. The great goal of NEWTON's life was to discover God's design[68], by studying his works and following the clues he had given mankind through his prophets. This motivation throws light on NEWTON's whole activity and gives it unity and consistency. It is at the root of his choice of method: only by rational analysis of the natural phenomena and rational interpretation of the scriptures can we hope to read God's message, since reason is the tool he

[65] Letter no. 301 [*2*]. Similar views were again expressed by NEWTON on another occasion two years later: see the letter no. 328 [*3*].

[66] Ref. [*26*], especially p. 524—528 and more particularly p. 363 (§ 42).

[67] Letters nos. 398, 399, 403, 406 [*3*].

[68] NEWTON's preoccupation with the relation of nature and God is already apparent in the student's essay already quoted about the Cartesian system (published in ref. [*5*], p. 89—156). MORE's influence is very noticeable in this essay.

has given us for this purpose. It explains his life-long endeavours in search of the meaning hidden in the sacred books (which he treated as a rational problem of de-coding) and his erudite and painstaking historical investigations, which aimed at establishing the great antiquity of the Hebrew people and the authenticity of the prophecies[69]. For him there was no essential difference, either in purpose or in method, between the derivation of the laws of nature from the analysis of the phenomena and the ascertaining of God's intentions about man's fate by a reconstruction of the history of mankind.

We are now better prepared to judge the real place that the aether hypothesis occupied in NEWTON's mind. In the philosophy of MORE and NEWTON, space was occupied by God as well as by the created things: but how did God perceive the things and how could he act upon them? A direct interaction of God and gross matter was out of the question: God has no sense organs and is not affected by the motions of the bodies; the latter do not experience any resistance from God's omnipresence[70]. But could there not be a finer kind of substance providing the missing link? MORE, as we know from his famous controversy with DESCARTES, had contemplated such a solution, but shrunk from it because of its materialistic flavour: he was in consequence driven to a mystical conception of "spirits" emanating from God and animating the created things. Mysticism, however, was wholly averse to NEWTON's rationalistic turn of mind, Cartesianism much less so. An aethereal fluid filling all space could, as DESCARTES wanted it, transmit various forces between the bodies by appropriate cyclic motions: "for nature", says NEWTON[71], "is a perpetual circulatory worker". It could also be the agent of transmission of the sensations from the sense organs to the "sensorium", in which — according to the rudimentary physiology of the time — these sense impressions were directly perceived by the "sensible substance" of the animal. Likewise, by a bold analogy[72], space would be as God's sensorium, in which he would perceive the aethereal motions and through them (if such was his will) influence those of the material bodies.

If the aether hypothesis played such a central part in NEWTON's view of the world, why did he not publish it in the first edition of the *Principia*? Why at least did he not present his tentative theory of the cause of gravitation, which would have spared him the imputation of introducing action at a distance as a primary quality of matter? By a cruel irony of fate, the proof of the universal validity of the inverse square law of gravitation, his greatest triumph, had dealt a severe blow to his whole aethereal construction. He had all the time imagined the celestial bodies moving in the aethereal medium would encounter some resistance from the latter, and accordingly expected the inverse square law to be only approximate. Now, the planets and the comets were found to move through the heavens in all directions without revealing the least presence of any resisting medium. From his own studies of the motion of bodies through fluids, which form the

[69] See especially refs. [11] and [12].
[70] See the *scholium generale*.
[71] Letter no. 146 [1].
[72] About the analogy of space as God's sensorium, see especially the query 31 at the end of the *Opticks* [27], and the controversy between LEIBNIZ and CLARKE (the latter acting as NEWTON's spokesman); the documents relating to this controversy have been edited with outstanding accuracy by A. ROBINET [28].

second book of the *Principia*, NEWTON was forced to conclude that the density of the aether ought to be extremely small: and how could it then fulfil the role for which it was primarily conceived? Confronted with this difficulty, NEWTON did not relinquish entirely his conception of the aether, but he had to restrict its scope considerably, confining its actions essentially to the inside and immediate vicinity of the material bodies; and the confident assurance of the early days was gone[73]. When, many years later, as an old man, he decided at last to publish his speculations about the aether, together with his unfinished optical studies, the queries in which he propounded them had the pathetic ring of renouncement. There is an undertone of renouncement also in the last page of the *scholium generale* which concludes the second edition of the *Principia*. The arrogance of the sentence "And it is enough that gravitation actually exists and acts according to the laws we have exposed" does not conceal the fact that this "experimental philosophy" is a position of retreat.

NEWTON failed in his double quest. His historical constructions fared no better than his aether hypothesis: they were based on conjectural identifications which eluded any possibility of conclusive examination. The common error of his time was to underestimate the magnitude of these problems. The scale of time was hopelessly distorted by the unquestioned acceptance of the biblical tradition; and even though the narrow frame of the medieval world had been shattered by COPERNICUS, the true dimensions of the universe were still far from being realized. NEWTON believed that the history of mankind held in the four "ancient kingdoms" of the classical authors, and that the celestial motions were confined to the solar system. And after a long life of unceasing toil, when he had unravelled the laws of these motions, he found himself on the shore of an "ocean of truth undiscovered" and wrote, regretfully, "*hypotheses non fingo*".

Bibliography

[1] The Correspondence of Sir Isaac Newton, vol. 1 (1661—1675). Cambridge: Cambridge University Press 1959.
[2] The Correspondence of Sir Isaac Newton, vol. 2 (1676—1687). Cambridge: Cambridge University Press 1960.
[3] The Correspondence of Sir Isaac Newton, vol. 3 (1688—1694). Cambridge: Cambridge University Press 1961.
[4] HALL, A. R., Annals of Science **13**, 62 (1957).
[5] HALL, A. R. & M. BOAS HALL, Unpublished scientific papers of Isaac Newton. Cambridge: Cambridge University Press 1962.
[6] HERIVEL, J. W., Isis **51**, 546 (1960).
[7] HERIVEL, J. W., Isis **52**, 410 (1961).
[8] HERIVEL, J. W., Revue d'histoire des sciences **15**, 105 (1962).
[9] HERIVEL, J. W., Archives Intern. d'hist. des sciences **13**, 63, 67, 71 (1960).
[10] KOYRÉ, A., Archives Intern. d'hist. des sciences **13**, 3 (1960).
[11] MANUEL, F. E., Isaac Newton historian. Cambridge: Cambridge University Press 1963.

[73] LOHNE [22] exaggerates the difficulty arising for NEWTON's aether conception from the absence of resistance to planetary motions when he states that it also overthrew his "alchemical cosmogony and essential parts of his theory of light and colours": NEWTON did uphold the functions of the aether in chemical and optical processes, which only involve short-range interactions; but he had to abandon the hope of a comprehensive synthesis.

[12] ROSENFELD, L., Nature **202**, 43 (1964).
[13] BREWSTER, D., Memoirs of the life, writings and discoveries of Sir Isaac Newton. Edinburgh: Thomas Constable 1855.
[14] THOMSON, W. & P. G. TAIT, Treatise on natural philosophy. Cambridge: Cambridge University Press 1879.
[15] CAJORI, F., in: Sir Isaac Newton 1727—1927, p. 127—188. Baltimore: Williams & Wilkins 1928.
[16] NEWTON, I., Philosophiae naturalis principia mathematica. Londini: Reg. Soc. 1687.
[17] STUKELEY, W.: Memoirs of Sir Isaac Newton's life [1752]. London: Taylor & Francis 1936.
[18] LOHNE, J., Centaurus **7**, 6 (1960).
[19] GALILEI, G., Dialogo dei due massimi sistemi del mondo. Firenze: Landini 1632. Giornata seconda (from: "La vertigine veloce ha facultà di estrudere e dissipare").
[20] HUYGENS, C., Oeuvres complètes, tome 16 (La Haye: Nijhoff 1929): De vi centrifuga (1659), p. 237—328.
[21] HUYGENS, C.: Horologium oscillatorium. Parisiis: Muguet 1673. (Oeuvres complètes, tome 18. La Haye: Nijhoff 1934.)
[22] LOHNE, J., Archive for the History of Exact Sciences **1**, 389 (1961).
[23] ROSENFELD, L., Nature **195**, 414 (1962).
[24] FIERZ, M., Gesnerus **11**, 62 (1954).
[25] GREGORY, J. C., Transactions of the Royal Soc. of Edinburgh **12**, 64 (1829).
[26] HUYGENS, C., Oeuvres complètes, tome 21. La Haye: Nijhoff 1944.
[27] NEWTON, I., Opticks, 2nd edition. London: W. Innys 1717.
[28] ROBINET, A., Correspondance Leibniz-Clarke. Paris: Presses Universitaires de France 1957.
[29] WHITESIDE, D. T., Notes and Records of the Royal Society **19**, 53 (1964).
[30] WHITESIDE, D. T., British Journal for Hist. of Science **2**, 117 (1964).

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(Received April 4, 1965)