An Unpublished Letter of Robert Hooke to Isaac Newton

BY ALEXANDRE KOYRÉ *

ROBERT HOOKE'S letter to Isaac Newton of 9 December 1679 forms a part of a very interesting correspondence exchanged between the two great scientists during the winter months of 1679–1680. This correspondence, which played an important, perhaps a decisive, role in the development of Newton's thought,[1] was discovered, some sixty years ago, by W. W. Rouse Ball in the Library of Trinity College, Cambridge, and was published by him in his precious *Essay on Newton's Principia*.[2] Unfortunately, the collection in Trinity College was not complete, and contained only five of the seven letters written by Newton and Hooke; the remaining two — namely, Hooke's letter to Newton of 9 December 1679 and Newton's reply of 13 December 1679 — were missing.

The latter turned up at a public sale at Messrs Sotheby and Co., on 29 June 1904, and was acquired by the British Museum. It was published with an extremely careful and scholarly commentary by Professor Jean Pelseneer, in 1929, in this journal.[3]

The former also appeared at a sale at Sotheby's, in April 1918, came into the possession of Dr Erik Waller of Stockholm, and finally was acquired by the Yale University Library, New Haven.[4] With the kind permission of the librarian, Mr James T. Babb, I am printing it here for the first time.[5] Thus the gap that remained open even after Professor Pelseneer's publication seems now to be definitely closed.[6]

The relationship between Newton and Hooke was never friendly, though it is only after the last — the third — clash, which followed the publication of Newton's *Principia*, that it degenerated into a bitter and burning hatred.[7] The second clash occurred in 1679, and is the subject matter of this paper. As for the first — and in many respects the most important one — it took place at the very beginning of Newton's public career, in 1672, when Robert Hooke produced a somewhat hasty and rather sharp criticism of Newton's optical discoveries, claiming, moreover,

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[1] In his letter to Halley of 14 July 1666, Newton writes: "This is true, that his letters occasioned my finding the method of determining figures, which when I had tried in the ellipsis, I threw the calculations by, being upon other studies." Cf. W. W. Rouse Ball, *An essay on Newton's "Principia,"* London, Macmillan, 1893, p. 165.

[2] W. W. Rouse Ball, *Op. cit.*, Appendix A: Correspondence between Hooke and Newton and memoranda relating thereto, pp. 139–153.

[3] Jean Pelseneer, Une lettre inédite de Newton, *Isis 12*, 237–39, 1929.

[4] Cf. Ernest Weil, Robert Hooke's letter of 9 Dec. 1679 to Isaac Newton, *Nature 158*, 135, 1946.

[5] One passage only — "I could add many other considerations consonant to my theory of circular motions compounded by a direct motion and an attractive one to the center . . ." — has been preserved; (by Hooke himself, cf. his *A true state of the case and controversy between S^r Isaak Newton and Dr. Robert Hooke as to the priority of that noble hypothesis of motion of the planets about the sun as their centre*, W. W. Rouse Ball, *op. cit.*, pp. 151 sq.); the contents of this letter, however, were not completely unknown, since Hooke had read it to the Royal Society at their meeting on 4 Dec. 1679, and inserted a short report of it in the *Minutes* of the R.S., which has been published by Th. Birch, *History of the Royal Society*, London, 1757, *3*, pp. 512 sq. Cf. infra, p. 327.

[6] An entry in Hooke's *Journal*, quoted by Prof. Pelseneer, *op. cit.*, p. 238, seems to imply that there may have been two more letters; "peut-être de simples billets," says Prof. Pelseneer. No trace has ever been found of them, nor have they ever been mentioned by anybody, not even by Newton.

[7] It is well known that Newton obstinately refused to publish his *Opticks* during the lifetime of Hooke. He, therefore, held back his manuscript, awaiting patiently and confidently the disappearance of his foe, and printed it in 1704, the year of Hooke's death.

priority for the best part of them.[8] It is natural that this unexpected attack, as well as the tone adopted by Robert Hooke — already a well-known man, the celebrated author of the very famous *Micrographia* [9] — towards the obscure Cambridge professor could not fail to engender a deep resentment in Newton's proud and sensitive mind. The wounds inflicted in this long-drawn and heated polemic are doubtless responsible for the nearly pathological aversion to all publications, the nearly invincible resistance to being drawn out of his shell, that Newton developed in his mature years.

In the year 1675/6 the bitterness of the polemic reached its climax; Hooke asserted that the main part of Newton's work "was contained in his *Micrographia*, which Mr. Newton had only carried farther in some particulars." [10] Newton replied by pointing out Hooke's indebtedness to Descartes and others and his inability to apply exact measurement to the problems of optics, especially to that of the colours in the thin plates:

> He left me to find out and make such experiments about it as might inform me of the manner of the production of those colours, to ground an hypothesis on; he having no further insight to it than this, that the colour depended on some certain thickness of the plate, though what that thickness was at every colour, he confesses in his *Micrography*, he had attempted in vain to learn; and therefore, seeing I was left to measure it myself, I suppose he will allow me to make use of what I took the pains to find out. And this I hope may vindicate me from what Mr. Hooke has been pleased to charge me with.[11]

Yet, instead of meeting Newton's attack with a counter-charge, Hooke, though maintaining the superiority of his own theory over that of his rival, quite unexepectedly made a step towards reconciliation. Professor L. T. More assumes "that pressure was put on Hooke to appease the wounded feelings of the younger man." [12] An assumption that goes far towards explaining the otherwise unintelligible fact that Hooke sent Newton a letter which Professor Pelseneer, who likewise attributes it to

[8] Cf. Sir David Brewster, *Memoirs of the life, writings and discoveries of Sir Isaac Newton*, Edinburgh, Constable, 1855, *1*, pp. 78–9; Louis Trenchard More, *Isaac Newton, a biography*, New York, Scribner's, 1934, pp. 82–9. As a matter of fact, Hooke's criticism was rather profitable to Newton; it made him improve his theory by incorporating undulatory components into it.

[9] *Micrographia: or some physiological descriptions of minute bodies made by magnifying glasses, with observations and inquiries thereupon* by R. Hooke, Fellow of the Royal Society, London, Jo. Martin and Ja. Allestry, 1665. The *Micrographia*, a first-rate work of quite outstanding importance, is characterized by the *Dictionary of national biography* as: "a book full of ingenious ideas and singular anticipations. It contained the earliest description of the 'fantastical colours' of thin plates with a quasi-explanation by interference (p. 66), the first notice of the 'black spot' in soap-bubbles, and a theory of light as a 'very short vibrative motion,' transverse to straight lines of propagation through a 'homogeneous medium.' That was defined as 'a property of a body arising from the motion or agitation of its parts' and the real nature of combustion was pointed out in detail eleven years before the publication of Mayow's similar discovery." (*DNB*, *32*, 284.) Professor E. N. da C. Andrade, in his Wilkins Lecture, Robert Hooke, *Proceedings of the Royal Society, A*, *201*, 1950, 439–73, says: "The plates of the *Micrographia* are beautiful in themselves but also record a number of fundamental discoveries. Sachs, the historian of botany, puts Hooke with Malpighi, Grew and Leeuwenhoek as 'endeavouring by earnest reflection to apply the powers of mind to the objects seen with the assisted eye, to clear up the true nature of the microscopic objects, and to explain the secrets of their constitution.' The figures of the gnat, the flea, and the louse were long famous. But microscopic pictures and their discussion form but a small part of the book. In it we find important theoretical discussions of the nature of light and heat . . .; further discussion of capillarity on the lines of his earlier tract [*An attempt for the explication of the phenomena observable in an experiment published by the Honorable Robert Boyle*, London, 1661]; experiments on the thermal expansion of solids and liquids; shrewd speculations on tempering of metals; observations on crystal structure; astronomical discussions, including attempts to form artificially craters like those of the moon; and accounts of the magnitudes of stars, in which occurs the statement that more powerful telescopes would discover fresh stars. . . . Further, we must note that the book contains a very full discussion of the colors of thin plates, such as flakes of mica, air films between glasses, and bubbles not only of soapy water but of rosin and several other substances. These observations were a cause of subsequent dispute with Newton. The *Micrographia* gained Hooke considerable fame at home and abroad."

[10] Cf. Birch, *op. cit.*, *3*, p. 169.

[11] *Ibid.*, p. 279.

[12] Cf. L. T. More, *op. cit.*, p. 175.

pressure — on the part of Oldenburg — characterises as "so curteous and so humble that it looks like a child's letter of excuses." [13]

Sir, [writes Hooke] [14] The hearing a letter of yours read last week in the meeting of the Royal Society, made me suspect that you might have been some way or other misinformed concerning me; and this suspicion was the more prevalent with me, when I called to mind the experience I have formerly had of the like sinister practices.[15] I have therefore taken the freedom, which I hope I may be allowed in philosophical matters to acquaint you of myself. First, that I doe noe ways approve of contention, or feuding or proving in print, and shall be very unwillingly drawn to such kind of warre. Next, that I have a mind very desirous of, and very ready to embrace any truth that shall be discovered, though it may much thwart or contradict any opinions or notions I have formerly embraced as such. Thirdly, that I do justly value your excellent disquisitions, and am extremely well pleased to see those notions promoted and improved which I long since began, but had not time to compleat. That I judge you have gone farther in that affair much than I did, and that as I judge you cannot meet with any subject more worthy your contemplation, so I believe the subject cannot meet with a fitter and more able person to enquire into it than yourself, who are every way accomplished to compleat, rectify, and reform what were the sentiments of my younger studies, which I designed to have done somewhat at myself, if my other more troublesome employments would have permitted,[16] though I am sufficiently sensible it would have been with abilities much inferior to yours. Your design and mine are, I suppose, both at the same thing, which is the discovery of truth, and I suppose we can both endure to hear objections, so as they come not in the manner of open hostility, and have minds equally inclined to yield to the plainest deductions of reason from experiment. If, therefore, you will please to correspond about such matters by private letters, I shall very gladly embrace it; and when I shall have the happiness to peruse your excellent discourse, (which I can as yet understand nothing more of by hearing it cursorily read), I shall, if it be not ungrateful to you, send you freely my objections, if I have any, or my concurrences, if I am convinced, which is the more likely. This

[13] Cf. Jean Pelseneer, Lettres inédites de Newton, *Osiris* 7, p. 541, 1939.

[14] Cf. Brewster, *op. cit.*, *1*, pp. 140–41; More, *op. cit.*, pp. 175–76.

[15] An obvious hint at Oldenburg, and not an unjust one.

[16] It is perfectly true that Hooke who, as Curator of Experiments of the Royal Society, was supposed "to furnish the Society every day [they met once a week] with three or four considerable experiments," never enjoyed the blessing of leisure of which Newton, at least in his Cambridge years, had so large a share. Yet, it was certainly not only outward pressure that prevented Hooke from thinking out his extremely numerous and original ideas; it was just as much, or even more, the inner pressure of a feverish and ebullient mind. Let us quote once more the *DNB* and Professor E. N. da C. Andrade. The *DNB*, p. 284:

"The registers of the Royal Society testify to the eagerness with which Hooke hurried from one inquiry to another with brilliant but inconclusive results. Among those which early engaged his attention were the nature of the air, its function in respiration and combustion, specific weights, the law of falling bodies, the improvement of land-carriage and diving bells, methods of telegraphy and the relations of barometrical readings to changes in the weather. He measured the vibrations of a pendulum two hundred feet long attached to the steeple of St. Paul; invented a useful machine for cutting the teeth of watch-wheels; fixed the thermometer zero at freezing-point of water; and ascertained (in July 1664) the number of vibrations corresponding to musical notes." This characterisation of Robert Hooke is not so very different from that of Professor E. N. da C. Andrade, who says (*op. cit.*, p. 439): "Probably the most inventive man who ever lived, and one of the ablest experimenters, he had a most acute mind, and made astonishingly correct conjectures, based on reason, in all branches of physics. Physics, however, was far from being his only field: he is the founder of scientific meteorology; as an astronomer he has observations of great significance to his credit; he did fundamental work on combustion and respiration; he was one of the founders of modern geology." And (*ibid.*, p. 441), "From now on [1660] we are to be confronted with the difficulty of coping with the stream of inventions, notions, brilliant suggestions, accurate observations, daring speculations and prophetic conjectures that poured from Hooke's fertile brain and contriving hands. It will be impossible even to mention them all; to classify them will be difficult; in many cases, in view of the scanty record, it will be hard to decide what exactly was done. Practically everything, however, will bear witness to a truly extraordinary inventiveness and a truly modern outlook. Sometimes Hooke is wrong, but he is wrong in a strictly scientific and not a medieval way. Very often the ideas which he tumbled out in such profusion were taken by others; sometimes his findings were reached quite independently by others, which Hooke found hard to believe. At every stage we are witnessing the workings of a mind so active, so fertile in expedients, so interrupted at every hour, at every endeavour, by the inrush of new concepts, new projects, that it is hard to disentangle his doings. Newton said that he made his discoveries by keeping the subject constantly before him and waiting until the first dawnings opened little by little into the full light. This Hooke was quite unable to do: he totally lacked Newton's powers of concentration. His mind was restless, continually disturbed by fresh ideas, but they were nearly all good, and many were of first importance."

way of contending, I believe, to be the more philosophical of the two, for though I confess the collision of two hard-to-yield contenders may produce light, [yet] if they be put together by the ears by other's hands and incentives, it will [produce rath]er ill concomitant heat, which served for no other use but . . . kindle — cole. S^{r}, I hope you will pardon this plainness of, your very affectionate humble serv^{t},

ROBERT HOOKE[17]

1675/6

Newton's reply, likewise written, most probably, under pressure, though very courteous and even conciliatory, is by no means as meek and humble as Hooke's letter. Quite the contrary: while recognising the merits of his predecessors, Descartes and Hooke, even calling them "giants," he, quite unmistakably, insists on his own:[18]

Dear Sir, —

At the reading of your letter I was exceedingly pleased and satisfied with your generous freedom, and think you have done what becomes a true philosophical spirit. There is nothing which I desire to avoyde in matters of philosophy more than contention, nor any kind of contention more than one in print; and, therefore, I most gladly embrace your proposal of a private correspondence. What's done before many witnesses is seldom without some further concerns than that for truth; but what passes between friends in private, usually deserves the name of consultation rather than contention; and so I hope it will prove between you and me. Your animadversions will therefore be welcome to me; for though I was formerly tyred of this subject by the frequent interruptions it caused to me, and have not yet, nor I believe ever shall recover so much love for it as to delight in spending time about it; yet to have at once in short the strongest objections that may be made, I would really desire, and know no man better able to furnish me with them than yourself. In this you will oblige me, and if there be any thing else in my papers in which you apprehend I have assumed too. . . .

. . . If you please to reserve your sentiments of it for a private letter, I hope you [will find that I] am not so much in love with philosophical productions, but that I can make them yield. . . .

But, in the mean time, you defer too much to my ability in searching into this subject. What Descartes did was a good step.[19] You have added much several ways, and especially in considering the colours of thin plates. If I have seen farther, it is by standing on the shoulders of giants.[20] But I make no question you have divers very considerable experiments beside those you have published, and some, it's very probable, the same with some of those in my late papers. . . .[21]

This celebrated correspondence has been, since its publications, greatly admired and praised by the historians and biographers of Newton. Thus Brewster exclaims: "These beautiful letters, emulous of good feeling and lofty principle, throw some light on the character and position of two of the greatest of our English philosophers. . . ."[22] I must confess that I do not share this common admiration. Both letters seem to me to be too full of rhetoric. The mutual praise (though very carefully graded by Newton) and the subtle distinction between contention in public and friendly discussion in private (a commonplace since Prodicos, or at least, since Plato) give much more the impression of conforming to a conventional pattern than of following a free inspiration. Or, to quote Professor More: "These two letters have all the earmarks of an attempt towards a formal reconciliation which had been urged by others, and recognised as proper by themselves. Each of the writers expresses great admiration for the other's ability; each deprecates the public and partisan discussion of their opinions; and each requests the other to criticise rigourously his work, but to do it privately."[23] — Neither, of course, availed himself of the magnanimous and high-sounding invitation.

[17] This letter is addressed "to my much esteemed friend, Mr Isaak Newton, at his chambers in Trinity College in Cambridge."

[18] Cf. Brewster, *op. cit.*, *1*, p. 141; More, *op. cit.*, p. 176. Newton's letter is dated: Cambridge, 5 February 1675/6.

[19] If one considers that Descartes established the law of refraction and gave a complete theory of the rainbow, one must confess that the praise bestowed on him by Newton is not very lavish.

[20] As pointed out by L. T. More, this celebrated saying which is usually quoted as being original with Newton and as expressing his magnanimous modesty is, as a matter of fact, a commonplace. It is used by Burton in his *Anatomy of melancholy* as a quotation from Didacus Stella, *In Luc.* 10 tom. 2: *Pigmaei Gigantum humeris impositi plusquam ipsi gigantes vident*. Cf. More, *op. cit.*, p. 177, note 28.

[21] The rest of this letter deals with special optical questions of no interest in the present context.

[22] Cf. Brewster, *op. cit.*, *1*, p. 143.

[23] Cf. More, *op. cit.*, p. 177.

The official reconciliation did not heal the wounds inflicted by the conflict. Bitterness and resentment on both sides, and especially on Newton's side, remained; to quote Professor More once more:

Two such men may indulge in general sentiments of a high and abstract order, and use elaborate expressions of personal esteem; but there could not be found two men, who were so temperamentally incapable to form a lasting friendship. Both were suspicious and sensitively vain. In Hooke these qualities showed themselves by wrathful explosions and by reiterated accusations that he had been robbed of the fruits of his work; in Newton, when opposed, they were equally apparent in a cold assumption of a disdain for fame and a silent retirement into his ivory tower. It is needless to say that their correspondence was limited to official communications; the embers of hostility still existed and needed only a new occasion to make them blaze in public. They never forgave each other: Hooke continued to claim that he had anticipated Newton's work, and Newton maintained his aloof attitude towards the Society, till Hooke's death relieved him from the fear of his insinuations.[24]

Yet, outwardly and formally, cordial, or at least courteous relations were re-established and, though no real scientific correspondence had followed the exchange of the letters of reconciliation, still, as has been shown by Professor Pelseneer, in the years 1677–1688, Newton and Hooke actually exchanged a couple of, rather insignificant, letters.[25] Newton even went as far as to congratulate Hooke on his election, after Oldenburg's death, to the secretaryship of the Royal Society.[26]

Thus it was perfectly natural that, having been, two years later, entrusted with holding and promoting the correspondence of the Royal Society with its members as well as with foreign scientists, Hooke, on 24 November 1679, addressed to Newton an invitation to resume his former relations with the Royal Society, and to participate in the exchange of scientific information with its members.

Sir, [wrote Hooke][27] — Finding by our registers that you were pledged to correspond with Mr. Oldenburg, and having also the happiness of receiving some letters from you my self make me presume to trouble you with this present scribble — Dr. Grew's more urgent occasions having made him decline the holding correspondence. And the Society hath devolved it on me. I hope therefore that you will please to continue your former favours to the Society by communicating what shall occur to you that is philosophicall, and for returne, I shall be sure to acquaint you with what we shall receive considerable from other parts or find out new here. And you may be assured that whatever shall be soe communicated shall be noe otherwise further imparted or disposed of than you yourself shall praescribe. I am not ignorant that both heretofore, and not long since also, they have been some who have indeavoured to misrepresent me to you, and possibly they or others have not been wanting to doe the like to me,[28] but difference in opinion if such there be (especially in philosophicall matters where interest hath little concerne) me thinks should not be the occasion of enmity — 'tis not with me I am sure. For my part I shall take it as a great favour if you shall please to communicate by letter your objections against any hypothesis or opinion of mine; and particularly if you will let me know your thoughts of that of compounding the celestiall motions of the planetts of a direct motion by the tangent and an attractive motion towards the centrall body,[29] or what objections you have against my hypothesis of the lawes or causes of springynesse.[30]

I have lately received from Paris a new hypothesis invented by Mo^r^ Mallement de Messanges,[31] D^r^ of the Sorbon, who desires much to have what can be objected against it. He

[24] *Ibid.*

[25] Cf. Jean Pelseneer, Lettres inédites de Newton, *Osiris* 7, 536 sq., 1939.

[26] Letter of Newton to Hooke, 18 Dec. 1677: "I wish you much happiness in yo^r^ new employmt & that the R. Society may flourish yet more by the labours of so able a member." Cf. Pelseneer, *op. cit.*, p. 541. Hooke was elected secretary of the Royal Society on 25 Oct. 1677.

[27] Cf. W. W. Rouse Ball, *op. cit.*, pp. 139 sq.; More, pp. 220 sq.

[28] Once more a hint at Oldenburg.

[29] Hooke's ideas on celestial mechanics were expounded at the end of his *Attempt to prove the motion of the earth by observation*, London, 1674. I quote the relevant passage *infra*, p. 318.

[30] Hooke deals with the laws of elasticity (springiness) in his *Lectures de potentia restitutiva, or, Of the spring: Experiments on the power of springing bodies*, London, 1678; reissued in the *Lectiones Cutlerianae* of 1679.

[31] Claude Mallemont (or Mallemans) de Messanges, professor of philosophy in the Collège de Plessis. He published a *Nouveau système du monde inventé par M. Mallemont de Messanges*, 4°, pp. 22, Paris, 1678, followed by a *Nouveau système du monde, par lequel, sans excentricité, trépidation et autres inventions d'astrologues on explique mécaniquement tous les phénomènes*, in fol., pp. 114, Paris, 1679; a *Dissertation sur les comètes*, in 1681, and a solution of the quadrature of the circle in 1686. All his writings are a tissue of absurdities.

supposes then a center of this our vortex about which all the primary planets move in perfect circles, each of them in his aequall spaces in aequall times. The next to it he places the Sun; and about the Sun, Mercury as a satellit; the next Venus; the next the earth, about which the Moon as a satellit; then Mars; then Jupiter and his satellits; and Saturn with his. He supposes the Sun to make its revolution in about half the time the earth makes its, and the plaine of it to be inclined to the plaine of the ecliptick as much as the trepidation requires. He is not precise in defining any thing, as reserving a liberty to himself to help him out where objections might stick.

I am informed likewise from Paris that they are there about another work, viz. of setling the longitude and latitude of the most considerable places: the former of those by the eclipses of the satellites of Jupiter. M^r Picart and De la Hire travell, and Mo^r Cassini and Romer observe at Paris. They have already found that Brest in Britaigne is 18 leagues nearer Paris than all the mappes make it. I have written to a correspondent in Deavonshire to see if we can doe somewhat of that kind here, and I should be glad if by perpendicular observations, we could determine the difference of latitude between London and Cambridge. If you know of any one that will observe at Cambridge, I will procure it to be done here very exactly.

M^r Collins shewed me a book he received from Paris of De la Hire containing first a new method of the conick sections [32] and secondly a treatise *De locis solidis*. I have not perused the book but M^r Collins commends it. M^r Flamstead by some late perpendicular observations hath confirmed the paralax of the orb of the earth.

But I fear I have too much trespassed, and therefore to put an end to your further trouble I shall subscribe myself, Sir,

Your very humble Servant

R. H.

Gresham College, Nov. 24. 1679

Hooke's letter barely needs a comment. Having been, as I have already mentioned, newly entrusted with the correspondence of the Royal Society, he is performing his duty — or playing his part — in (a) informing Newton of his new appointment, (b) asking him, in a very courteous and dignified way, to resume his scientific cooperation with the Society, and (c) giving to Newton news about the recent developments in the field of science. Of course, one could ask oneself why Hooke finds it necessary to inform Newton about the obviously worthless "hypothesis" of Mallemont de Messanges. Yet it is possible that its worthlessness was somewhat less striking in 1679 than it is now, or even only a hundred years later: the *Philosophical Transactions* are full of things as manifestly worthless and absurd or even worse. Absurdity, as well as truth, is a daughter of time.

On the other hand, we should ask ourselves why Hooke asks Newton for a criticism of his work, quite particularly of his theory of elasticity and his celestial mechanics. Does he really want to learn the objections which Newton could formulate against them? This does not seem very probable: Hooke bears criticism as badly as Newton himself; to me it appears much more like a piece of rhetoric aimed at convincing Newton of the sincerity of his friendly feelings and at dissipating the apprehension and mistrust that Newton may still feel against him. We may assume, too, that Hooke is trying to induce Newton to acknowledge the value of his brilliant pioneer work in these two fields of study that seemed to lay outside of Newton's own preoccupations; Hooke did not — and could not — know that Newton was interested in celestial mechanics since about 15 years earlier and that in this field, as in optics, Newton was already far ahead of him. Finally it is even not impossible that Hooke expected, or at least hoped, that Newton should be able to bring his celestial mechanics to a completion, that is, to work out mathematically the ratio of the variation (as function of the distance) of that attractive power of which he was the first to assert the cosmical universality and fundamental role.[33]

Hooke's merits in the development of the theory of elasticity have been recognised by history and rewarded by the naming of its cardinal law: Hooke's law; his contributions to celestial mechanics, on the other hand, are so completely overshadowed

[32] Probably Philippe de la Hire's *Nouveaux élemens des sections coniques, les lieux géometriques, la construction ou effection des équations*, Paris, A. Pralard, 1679.

[33] Cf. my paper, La gravitation universelle, de Kepler à Newton, *Archives internationales d'histoire des sciences 16*, 638 sq., 1951.

by the work of Newton that it is nearly impossible for us to appreciate them justly and to determine their value and importance in, and for, their own time.[34] In order to do so, we should compare Hooke's attempts not with Newton's achievements — there is between them no common measure —, but with those of *his* contemporaries, or immediate predecessors, *e.g.* with those of Borelli.[35]

Such a comparison would show that in the very first paper — read by Hooke before the Royal Society on 23 May 1666 — in which he deals with the problems of planetary motions, a paper in which Borelli's influence is unmistakeable, Hooke's superiority quite obvious.[36] The replacement of Borelli's "tendency" or "natural instinct" of the planets to move towards the sun (or, in the case of the satellites, towards their primary planet) by an attractive power of the central body that draws the planets (or the satellites) to itself, enables Hooke to make a decisive step, and to consider this attractive power not as a constant force — as Borelli's "tendency" or "instinct" — but as a force which is some function of the distance. Hooke, it is true, does not know the exact law of variation of this force. Yet we must not overlook the fact that his conception is only incomplete and no longer simply false, as was Borelli's. In 1670, in his *Cutlerian Lectures*, Hooke seems to have taken a step farther: a step of tremendous importance. The attractive power is now conceived not as a special force (or set of forces) that binds the planets to the sun or the satellites to their planet, but as a universal factor which binds all the celestial bodies (at least those of our solar system) together, and which moreover, is identical with our terrestrial gravity.[37]

In 1674, in his *Attempt to prove the motion of the Earth by Observation*, which according to Hooke, reproduces the text, or the contents, of his 1670 lectures,[38] he announces [39] "a system of the world differing in many particulars from any yet known answering in all things to the common rules of mechanical motions. This depends upon three suppositions."

First, that all celestial bodies whatsoever have an attraction or a gravitating power towards their own centers, whereby they attract not only their own parts, and keep them from flying from them, as they may observe the earth to do, but that they do also attract all the other celestial bodies that are within the sphere of this activity;[40] and consequently that not only the

[34] A very able and scholarly attempt to vindicate for Hooke a much more important role in the development of celestial mechanics than is usually attributed to him has been made recently by Miss L. D. Patterson (Hooke's gravitation theory and its influence on Newton, *Isis 40* and *41*, 1949 and 1950). Unfortunately, Miss Patterson — who, in order to magnify Hooke (as a matter of fact, Hooke has been rather badly treated by Newton-inspired historians) charges Newton with all the capital sins, including plagiarism and falsification of documents — does not seem to me to appreciate at its just value the difference between an *idea* and a *theory*. A much more balanced and just account of Hooke's scientific work — the best that we have today — has been given by Professor E. N. da C. Andrade, in his Wilkins Lecture, Robert Hooke, *Proceedings of the Royal Society*, *A*, *201*, 439–73, 1950.

[35] Cf. A. Armitage, "Borell's hypothesis" and the rise of celestial mechanics, *Annals of science 6*, 268–282, 1950 and my paper, La mécanique céleste de Borelli, *Revue d'histoire des sciences*, 1952.

[36] Cf. Birch, *op. cit.*, *2*, 90; R. T. Gunther, The life and work of Robert Hooke, *Early science in Oxford 6*, 265, Oxford, 1930.

[37] Miss L. D. Patterson gives Hooke the credit for having discovered the inverse square law as far back as 1664, though not having stated it explicitly in his *Micrographia* (*Isis 40*, 330, 1949) and the law of centrifugal force nearly at the same time, in any case, prior to the experiments of 23 Dec. 1666. In my opinion, such is by no means the case.

[38] Robert Hooke, *Lectiones Cutlerianae*, London, 1679, preface: "I have begun with a Discourse composed and read in Gresham College in the year 1670, when I designed to have printed it, but was diverted by the advice of some friends to stay the repeating of the Observation, rather than publish it upon the Experience of one year only. But finding that sickness has hitherto hindered me from repeating the trials, and that some Years Observations have already been lost by the first delay: I do rather hast it out now, though imperfect, then detain it for a better compleating, hoping it may be at least a Hint to others to prosecute and compleat the Observations, which I much long for."

[39] *An attempt to prove the motion of the earth by observation*, London, 1674, pp. 27 sq. According to Hooke, it was read to the Royal Society in 1671.

[40] The sphere of activity of the attracting or gravitating power is thus considered by Hooke as finite.

sun and moon have an influence upon the body and motion of the earth; and the earth upon them, but that Mercury, Mars, Saturn and Jupiter by their attractive power have a considerable influence upon its motions, and in the same manner the corresponding attractive power of the earth hath a considerable influence upon every one of their motions also.

The second supposition is this, that all bodies whatsoever that are put into a direct and simple motion will so continue to move forward in a straight line, till they are by some other effectual powers deflected and bent into a motion describing a circle, ellipsis or some other more compound curve line.

The third supposition is that these attractive powers are so much the more powerful in operating by how much the nearer the body wrought upon is to their own centers. Now what these several degrees are, I have not yet experimentally verified;[41] but it is a notion which, if fully prosecuted as it ought to be will mightily assist the astronomer to reduce all the celestial motions to a certain rule which I doubt will never be done without it. He that understands the nature of the circular pendulum and circular motion[42] will easily understand the whole ground of this principle and will know where to find direction in nature for the true understanding thereof, etc. This, I dare promise the undertaker, that he will find all the great motions in the world to be influenced by this principle, and that the true understanding thereof will be the true perfection of astronomy.

The boldness and the clarity of Hooke's thought and the depth of his intuition are nothing less than admirable; the near similarity of his world-view with that of Newton is striking — Hooke certainly is perfectly right in insisting on his priority. Yet it cannot be denied that the lacuna which we discovered in his earlier work has not been filled up: Hooke still does not know, "what the several degrees are" by which the attractive power varies with the distance. In 1678 when he publishes his *Cometa* he is as far from the solution of that problem as in 1674[43] and that is probably why, feeling that he is unable to keep his promise and to "explain" his "system of the world," he simply reissues, in 1679, his old *Attempt* under the new cover of *Lectiones Cutlerianae*.

Did he still believe in the possibility of determining the law of attraction "experimentally"? — In any case, when in the same year 1679 he finally found out the inverse square law, he certainly did not do it by experiment. It is even possible that his appeal to astronomers and to those who "understand the nature of the circular pendulum and the circular motion" reveals some doubt about the value, in this case, of purely experimental research.

I have said already and I want to repeat: it is only justice to recognise the outstanding value of Hooke's vision and to defend him against Newton's accusation of having only plagiarised Borelli.[44] And yet one can well understand Newton's outburst

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[41] In his letter to Newton of 6 Jan. 1680, Hooke writes that Halley "when he returned from S^t^ Helena, told me that his pendulum at the top of the hill went slower than at the bottom" and thus "had solved me a query I had long desired to be answered but wanted opportunity, and that was to know whether the gravity did actually decrease at a greater height from the center. To examine this decrease of attraction I have formerly made many experiments on Paule's steeple and Westminster Abby, but none that were fully satisfactory." Cf. W. W. Rouse Ball, *op. cit.*, p. 148. Besides the experiments at St Paul's and Westminster, Hooke also made experiments in a deep mine at Banstead Downes. Cf. R. T. Gunther, *op. cit.*, *Early science in Oxford 6*, p. 257.

[42] In spite of Miss L. D. Patterson's able defense of Hooke (cf. A reply to Professor Koyré's note on Robert Hooke, *Isis 41*, 304, 1950) I still believe that, as I pointed out (Note on Robert Hooke, *Isis 41*, 195, 1950), it is not the "conatus to descend" but the "conatus of returning to the centre" in the plane of the motion — as, besides, Miss Patterson states it herself in her paper on Hooke's gravitational theory, *Isis 40*, 333, 1949 — that Hooke assumes to be proportional to the sine of the vortex angle, and that, therefore, he does not belong to those who "understand the nature of the circular pendulum and circular motion."

[43] The assertions (in the *DNB 37*, 286, and elsewhere) that the inverse square law is stated in the *Cometa*, p. 286, are based on a misinterpretation of a passage in a letter of Newton to Halley of 20 June 1686 (W. W. Rouse Ball, *op. cit.*, p. 157): "I am almost confident by circumstances, that Sir Chr. Wren knew the duplicate proportion when I gave him a visit; and then Mr. Hooke (by his book Cometa written afterwards) will prove the last of us three that knew it." Newton does not mean that the "duplicate proportion" is to be found in the *Cometa*, but, on the contrary, that it does not appear even there.

[44] Newton to Halley, 20 June 1686 (W. W. Rouse Ball, *op. cit.*, p. 159): ". . . I cannot forbear, in stating the point of justice, to tell you further, that he has published Borell's hypothesis in his own name."

when, having completely worked out the *Principia* he was confronted with Hooke's claims:

> Borell did something in it, and wrote modestly. He has done nothing, and yet written in such a way, as if he knew and had sufficiently hinted all but what remained to be determined by the drudgery of calculations and observations, excusing himself from that labour by reason of his other business, whereas he should rather have excused himself by reason of his inability. For 'tis plain, by his words, he knew not how to go about it. Now is not this very fine? Mathematicians, that find out, settle, and do all the business, must content themselves with being nothing but dry calculators and drudges; and another, that does nothing but pretend and grasp at all things,[45] must carry away all the invention, as well of those that were to follow him, as of those that went before.[46]

If, as I am inclined to believe, Robert Hooke, in writing to Newton, expected, or at least hoped, to start a friendly discussion, and to obtain some help, the answer must have been deeply disappointing to him. It is even possible that it was this disappointment — and irritation — that determined his subsequent behaviour; namely the fact that, though he promised Newton to keep his correspondence to himself, he immediately made it public by a reading at a meeting of the Royal Society: both Newton's letter and his own answer. Most probably, though he knew only too well — by experience — that Newton, to quote the expression of Locke, was a man "nicely to deal with," he could not resist the temptation of publicly correcting and thus humiliating his rival.

Indeed, Newton's answer to Hooke's invitation to correspond with him and the Royal Society, though by no means as harsh and as forbidding as, some years later, in a letter to Halley, he pretended it to be [47] — quite on the contrary, it is, in form, extremely courteous and urbane — aimed obviously at discouraging Hooke's attempts. It is quite clear that Newton does not want to resume his former relationship with the Royal Society: perhaps less than ever, since it means dealing with Hooke, whom he still dislikes and does not trust.[48] Thus in order to cut short all further approaches, he tells Hooke that he has completely renounced philosophy and has never even heard

[45] Newton is certainly more than unjust in not recognizing the amazing fecundity of Hooke's restless mind. Hooke is not a mere "pretender" and "grasper"; if he was nicknamed "the universal claimant" because "there was scarcely a discovery made in his time which he did not conceive himself entitled to claim" (*DNB* 37, p. 286), it was because his mind was "so prolific" (*ibid.*), that he had really some reason to claim a great number of these discoveries, or at least, the ideas on which they were based. Yet, it was this very restlessness, the inability of concentration, and therefore, of obtaining conclusive results, that made him unacceptable to Newton. Newton, to speak with Professor Pelseneer, was a "classical" mind and must have shuddered when reading Hooke's "profession de foi" (cf. *Lectiones Cutlerianae*, London, 1679, preface) where he explained that "there is scarce one Subject of millions that may be pitched upon, but to write an exact and compleat History thereof, would require the whole time and attention of a man's life, and some thousands of Inventions and Observations to accomplish it. So on the other side no man is able to say that he will compleat this or that Inquiry, whatever it be (The greatest part of Invention being but a luckey bitt of chance, for the most part not in our own power, and like the wind, the Spirit of Invention bloweth where and when it listeth, and we scarce know whence it came, or whether 'tis gone.) 'Twill be much better therefore to imbrace the influences of Providence, and to be diligent in the inquiry of everything we meet with. For we shall quickly find that the number of considerable Observations and Inventions this way collected, will a hundred fold out-strip those that are found by Design. No man butt hath some luckey hints and useful thoughts on this or that Subject he is conversant about, the regarding and communicating of which, might be a means to other Persons highly to improve them." cf. *supra* p. 16.

[46] Cf. W. W. Rouse Ball, *op. cit.*, p. 159.

[47] Newton to Halley, 20 June 1686 (W. W. Rouse Ball, *op. cit.*, p. 157): ". . . in my answer to his first letter I refused his correspondence, told him I had laid philosophy aside, sent him only the experiment of projectiles (rather shortly hinted than carefully described), in compliment to sweeten my answer, expected to hear no further from him."

[48] According to Professor More (*op. cit.*, p. 297), "Newton . . . with great ingenuity, relieved his feelings of resentment for past injustice, and insinuated every reason for making Hooke so angry that he would drop any further correspondence."

about Hooke's theories of celestial motions; that he has no time to lose in correspondence, though perfectly willing to "communicate in oral discourses" with him — should they ever have "familiar converse." Still, being a well-bred man and, besides, a Fellow of the Royal Society, Newton feels that he cannot give Hooke's request, made in the name of the Society, a purely negative answer and, to sweeten the pill, he proposes to him a carefully thought-out project of a very interesting experiment which should enable one "to prove the motion of the earth by observation." [49] It is in describing this experiment that Newton made the fateful blunder [50] which occasioned the flare-up of his second polemic with Hooke and finally led him to the elucidation of the inverse square law of universal gravitation.

Newton's letter to Hooke has been considered by all Newton's historians — even by such careful and critical ones as Professor Pelseneer and Professor More — as an invaluable document about its author's spiritual development. In my opinion it is by no means worthy of such confidence. Newton — a suspicious and secret mind — had no reason whatever to be sincere and "candid" with Hooke. Most probably, he was not. Thus, all he says — even, or perhaps just, the famous phrases in which he describes his aversion for science, though seemingly supported by comparable assertions in 1676 and confirmed by his letters to Halley in 1686 — are not to be taken as gospel truth. It is, of course, quite possible that, at the time when he received Hooke's letter, he was "busy," "upon other things," and "thought no farther of philosophical matters than his letters put me up." [51] He may have been occupied with chemical experiments, or with theology or even with something else,[52] but it is impossible to admit that this aloofness lasted long years and was as strong as he tells Hooke. Indeed, some months before (in February 1679), he sends to Locke a very elaborate paper, in which he develops — as a hypothesis — a physical explanation of gravitation. Moreover he contradicts and betrays himself: at the same moment when he tells Hooke (in the letter of 28 Nov. 1679) that he "shook hands with philosophy" and that he never as much as heard of Hooke's "hypothesis of compounding the celestial motions of the planets, of a direct motion by the tangent to the curve" (which means that he not only has never heard of the famous experiment of 1666 but also has never read his *Attempt to prove the motion of the earth by observation* of 1674 and of 1679), he informs him of having ordered two pieces of metal for a reflecting tube and congratulates him for the confirmation by Flamsteed of the discovery of the earth's parallax which Hooke announced in his book.[53]

[49] Though Newton calls it "a fancy" and, in his letter to Halley (quoted *supra*, n. 47), pretends it to be "rather shortly hinted than carefully described," it is, as Professor Pelseneer rightly remarks (*Une lettre inédite de Newton*, pp. 240 sq.), "en dépit de la négligence de l'exposé, un magnifique exemple de la conception d'un problème scientifique chez Newton"; besides, adds he (*ibid.*, p. 240, n. 11) "cette négligence concerne surtout la forme; au contraire, certains détails de l'expérience proposée par Newton révèlent un sens admirable de l'importance relative des causes d'erreurs dont Hooke allait avoir à tenir compte au cours de l'expérimentation, par exemple la dissymétrie causée dans les couches d'air du puits par la chute de la bille" (cf. *infra*, p. 323). I would go even farther: in my opinion, Newton, in instructing Hooke about the manner in which the proposed experiment is to be performed, and in analysing the possible sources of error, wants to give a lesson to Hooke, and to show him his own ability as an experimenter. Moreover, he is, in a certain sense, reversing the roles: it is he, Newton, who gives the idea, and Hooke who has the drudgery of finding out. . . .

[50] In describing the trajectory of a falling body, he told Hooke that it would be a spiral.

[51] Newton to Halley, 20 June 1686. Cf. W. W. Rouse Ball, *op. cit.*, p. 157.

[52] Professor Pelseneer, Une lettre inédite de Newton, *Isis 12*, p. 240, suggests that he was studying law.

[53] Cf. *An attempt to prove the motion of the earth by observation*, p. 25: "'Tis manifest then by the observations of July the Sixth and Ninth: and that of the One and twentieth of October that there is a sensible parallax of the Earths Orb to the first Star in the head of *Draco*, and consequently a confirmation of the Copernican System against the *Ptolomaick* and *Tychonic*." Cf. *infra*, p. 322.

It is difficult to admit that these assertions of ignorance of Hooke's work are anything else but irony.

Small wonder that Hooke did not believe him.[54] It was not so much a lack of affection for philosophy, as a lack of affection for Hooke — and fear of being "embroiled" in discussions -- that inspired Newton. But let the reader judge for himself.

Sir, (writes Newton),

I cannot but acknowledge my self every way by the kindness of your letter tempted to concur with your desires in a philosophical correspondence. And heartily sorry I am that I am at present unfurnished with matter answerable to your expectations — for I have been this last half year in Lincolnshire cumbered with concerns amongst my relations till yesterday when I returned hither; so that I have had no time to entertain philosophical meditations, or so much as to study or mind any thing else but country affairs. And before that, I had for some years last been endeavouring to bend myself from philosophy to other studies[55] in so much that I have long grutched the time spent in that study unless it be perhaps at idle hours sometimes for a diversion; which makes me almost wholy unacquainted with what philosophers at London or abroad have of late been imployed about. And perhaps you will incline the more to believe me when I tell you that I did not, before the receipt of your last letter, so much as heare (that I remember) of your hypothesis of compounding the celestial motions of the planets, of a direct motion by the tangent to the curve, and of the laws and causes of springyness, though these no doubt are well known to the philosophical world. And having thus shook hands with philosophy, and being also at present taken of with other business, I hope it will not be interpreted out of any unkindness to you or the R. Society that I am backward in engaging my self in these matters, though formerly I must acknowledge I was moved by other reasons to decline, as much as M^r^ Oldenburg's importunity and ways to engage me in disputes would permit, all correspondence with him about them. However I cannot but return my hearty thanks for your thinking me worthy of so noble a commerce and in order thereto francly imparting to me several things in your letter.

As to the hypothesis of Mons^r^ Mallemont, though it should not be true yet if it would answer to phaenomena it would be very valuable by reason of its simplicity. But how the orbits of all the primary planets but Mercury can be reduced to so many concentric circles through each of which the planet moves equal spaces in equal times (for that's the hypothesis if I mistake not your description) I do not yet understand. The readiest way to convince the world of this truth would be I conceive to set forth first in some two of the planets, suppose Mars and earth, a specimen thereof stated and determined in numbers.[57]

I know no body in the University addicted to making astronomical observations: and my own short sightedness and tenderness of health makes me something unfit. Yet it's likely I may sometime this winter when I have more leisure than at present attempt what you propound for determining the difference of latitude between Cambridge and London.

I am glad to hear that so considerable a discovery as you made of the earth's annual parallax is seconded by M^r^ Flamstead's observations.

In requital of this advertisement I shall communicate to you a fancy of my own about discovering the earth's diurnal motion. In order thereto I will consider the earth's diurnal motion alone, without the annual, that having little influence on the experiment I shall here propound. Suppose then, *BDG* represents the globe of the earth [*see figure 1*] carried round once a day about its centre *C* from west to east according to the order of the letters *BDG*; and let *A*

[54] Hooke was perfectly right in disbelieving Newton, and in inserting before the last paragraph of Newton's letter the words: "he here pretends he knew not H's hypothesis." There seems to be very little doubt, if any, about the fact that Newton *knew* "Hooke's hypothesis," as besides the slip I have already pointed out, he quite definitely says so in his letter to Halley of 20 June 1686 where, protesting against Hooke's claim of having taught him "the duplicate proportion," he adds, "That by the same reason he concludes me then ignorant of the rest of the duplicate proportion, he may as well conclude me ignorant of the rest of that theory I had read before in his book" (W. W. Rouse Ball, *op. cit.*, p. 157); and "That when Hugenius [in 1673] put out his Horol[ogium] Oscil[atorium] . . . I had then my eye upon comparing the forces of the planets arising from their circular motion, and understood it; so that a while after, when Mr. Hooke propounded the problem solemnly, in the end of his Attempt to prove the Motion of the Earth, if I had not known the duplicate proportion before, I could not but have found it now." In the postscript to this letter (*ibid.*, p. 160) Newton writes: "For his extending the duplicate proportion down to the centre (which I do not) made him correct me, and tell me the rest of his theory as a new thing to me, and now stand upon it, that I had all from that his letter, notwithstanding that he had told it to all the world before, and I had seen it in his printed books, all but the proportion."

[55] Cf. *supra*, p. 315, "Philosophy" in the language of the XVIIth century includes natural science (*Philosophia naturalis*), but not mathematics.

[56] Newton believed himself to have been unfairly treated by the Royal Society in general and by Hooke in particular.

[57] Professor Pelseneer comments on this passage as follows (*op. cit.*, p. 240), "Ces derniers mots expriment de fort heureuse façon l'idée qui est à la base de l'oeuvre Newtonienne: la traduction dans le langage mathématique des faits d'expériences et le contrôle des hypothèses ainsi réalisé en toute sûreté."

be a heavy body suspended in the air, and moving round with the earth so as perpetually to hang over the same point thereof *B*. Then imagine this body *A* let fall, and its gravity will give it a new motion towards the center of the earth without diminishing the old one from west to east. Whence the motion of this body from west to east, by reason that before it fell it was more distant from the center of the earth than the parts of the earth at which it arrives in its fall, will be greater than the motion from west to east of the parts of the earth at which

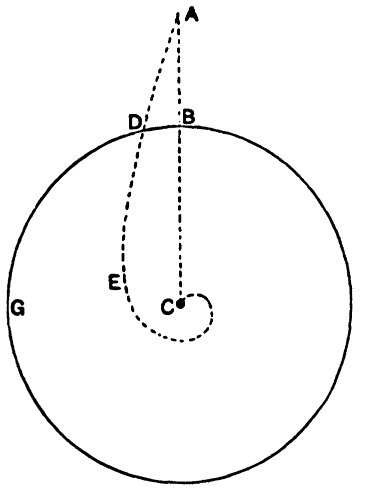

FIG. 1

the body arrives in its fall; and therefore it will not descend the perpendicular *AC*, but outrunning the parts of the earth will shoot forward to the east side of the perpendicular describing in its fall a spiral line *ADEC*, quite contrary to the opinion of the vulgar who think that, if the earth moved, heavy bodies in falling would be outrun by its parts and fall on the west side of the perpendicular. The advance of the body from the perpendicular eastward will in a descent of but twenty or thirty yards be very small, and yet I am apt to think it may be enough to determine the matter of fact. Suppose then in a very calm day a pistol bullet were let down by a silk line from the top of a high building or well, the line going through a small hole made in a plate of brass or tinn fastened to the top of the building or well, and the bullet when let down almost to the bottom were setled in water so as to cease from swinging, and then let down further on an edge of steel lying north and south to try if the bullet in setling thereon will almost stand in aequilibrio but yet with some small propensity (the smaller the better) decline to the west side of the steel as often as it is so let down thereon. The steel being so placed underneath, suppose the bullet be then drawn up to the top and let fall by cutting, clipping or burning the line of silk, and if it fall constantly on the east side of the steel it will argue the diurnall motion of the earth.[58] But what the event will be I know not, having never attempted to try it.[59] If any body would think this worth their trial, the best way in my opinion would be to try it in a high church or wide steeple, the windows being first well stopped; for in a narrow well the bullet possibly may be apt to receive a ply from the straitened air neare the sides of the well, if in its fall it come nearer to one side than to another. It would be convenient also that the water into which the bullet falls be a yard or two deep or more, partly that the bullet may fall more gently on the steel, partly that the motion which it has from west to east at its entering into the water may by meanes of the longer time of descent through the water, carry it on further eastward and so make the experiment more manifest.

If I were not so unhappy as to be unacquainted with your hypothesis abovementioned[60] (as I am with almost all things which have of late been done or attempted in philosophy) I should so far comply with your desire as to send you what objections I could think of against them, if I could think of any. And on the other hand I could with pleasure heare and answer any objections made against any notions of mine in a transient discourse for a divertisment.[61] But yet my affection to philosophy being worn out, so that I am almost as little concerned about it as one tradesman uses to be about another man's trade or a country man about learning, I must acknowledge my self averse from spending that time in writing about it which I think I can spend otherwise more to my own content and the good of others: and I hope neither you nor any body els will blame me for this aversness. To let you see that it is not out of any shyness, reservedness, or distrust that I have of late and still do decline phi[losophi]call commerce but only out of my applying my self to other things, I have communicated to you the notion above set down (such as it is) concerning the descent of heavy bodies for proving the motion of the earth; and shall be as ready to communicate in oral discourse anything I know, if it shall ever be my happiness to have

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[58] The reader may judge if these elaborate prescriptions are really "rather hinted than carefully described."

[59] Newton, of course, cannot doubt his analysis of the movement of the falling body and its "outrunning" the parts of the earth that are below it, and he does not need an experiment in order to be certain of it; the only thing he can doubt is the possibility of ascertaining this outrunning by experiment.

[60] Newton rubs it in!

[61] ". . . in a transient discourse for a divertisement" — i.e., not taking any serious account of them. It was by no means what Hooke aimed at.

familiar convers frequently with you.[62] And possibly if any thing usefull to mankind occurs to me I may sometimes impart it to you by letter. So wishing you all happiness and success in your endeavours, I rest,

Sir,

Your humble Servant

to command

Is. Newton

P.S. Mr. Cock has cast two pieces of metal for me in order to a further attempt about the reflecting tube which I was the last year inclined to by the instigation of some of our Fellows. If I do any thing you may expect to hear from me. But I doubt the tool on which they were to be ground, being in the keeping of one lately deceased who was to have wrought the metals, is lost.

Cambridge.

Novemb. 28, 1679

Endorsed: For his ever Hon^{d} ffriend M^{r} Robert Hooke at his Lodgings in Gresham College in London

The problem Newton is dealing with in the experiment he suggests to Hooke, i.e. the problem of the trajectory of a heavy body falling down to the earth, or to the center of the earth, has a very long and intricate story.[63] Edmund Hoppe, in his history of physics,[64] seeks the source of "the opinion of the vulgar who think that, if the earth moved, heavy bodies in falling would be outrun by its parts and fall on the west side of the perpendicular" in Tycho Brahe, who "in his *De mundi aetherei recentioribus phenomenis* (1588–1610) presented as the principal objection against the rotation of the earth the fact that a stone falling down on the west side of a tower would deviate to the west, for, during its fall the earth flees beneath it to the West." It is perfectly true that Tycho Brahe used this argument as well as several others, based on the same fundamental conception — that of the Aristotelian dynamics. Yet he did not invent them, but only clothed them, sometimes, in modern garb.[65] As to the argument of the body falling down from a tower, it belongs to the stock-in-trade of the objections against the movement of the earth and can be traced back to its discussion, and rejection, by Ptolemy, and farther back, by Aristotle himself, who asserts that, if the earth were moving, a stone thrown perpendicularly upwards would never fall down on the place wherefrom it departed because that place would, meanwhile, move away from beneath it.[66]

The Aristotelian (Ptolemaic, Tychonian) argument is by no means stupid. Quite the contrary: on the basis of the Aristotelian dynamics, or even, more exactly, on the basis of the Aristotelian conception of motion, according to which the motion of a body, especially its natural motion, is perfectly independent of, and not influenced by, the motion of its point of origin — *we* believe it to be the case in the propagation of the rays of light — it is perfectly sound and even irrefutable. In order to disprove it, a new conception of motion (and of space, physical reality, and so on) was needed and before it has been developed — by Galileo and Descartes — the attempts made by the Copernicans to answer the argument in question were bound to be weak and rather unconvincing.[67] Copernicus, for instance, asserted that the circular motion of the earth being a "natural" and not a "violent" one, it would be "participated in" by all the earthly objects; Kepler explained that all the "earthly" bodies were drawn from west to east by the same "magnetical" attractive power or strains that drew them towards the earth.[68] It is, therefore, hardly surprising, that the anti-Copernicans — and anti-Copernicanism was by no means supported only by the condemnation

[62] As Newton lived in Cambridge and practically never went to London, the probability of such a "familiar converse" was, obviously, not very great.

[63] It is unfortunately too long and too intricate to be dealt with here.

[64] Cf. Edmund Hoppe, *Histoire de la physique*, Paris, Payot, 1928, p. 54.

[65] Cf. my *Etudes galiléennes*, Paris, Hermann, 1939, vol. 3 (Galilée et la loi d'inertie), pp. 22 sq.

[66] Cf. Aristotle, *De Coelo* I, 2; *Physica* II, 1 and V, 2; Ptolemy, *Almagest* I, 7.

[67] By far the best defense of the Copernican position, from the point of view of the theory of *impetus*, was devised by Giordano Bruno; cf. my book, quoted *supra*, in n. 65, pp. 11 sq.

[68] Cf. *ibid.*, pp. 26 sq.

of the heliocentric system by the Roman church and restricted to Catholic countries [69] — continued to make use of the old objection throughout the 17th century. Thus, among countless others, this objection is raised by the celebrated author of the widely read, and very influential, *Almagestum Novum*, the Jesuit J. B. Riccioli.[70]

The Galilean "New Science" destroyed, of course, the very basis of the Aristotelian reasoning. Yet, as a matter of fact, Galileo himself did not give a correct solution of the problem. He asserted, indeed, in his *Dialogue on the two greatest world systems*, that, whether the earth moved or stood still, all the phenomena that may happen on it, with the sole exception of the tides (which he explained by a combination of the effects of the earth's diurnal and annual motion), would take place in a perfectly identical manner. A rather sad conclusion — it precluded the finding out of a physical proof of the Copernican doctrine — which seemed unbelievable; and, besides, was false. Moreover, in his deduction of the true ("absolute") motion of the falling body, as distinguished from its motion relative to the moving earth (a question that every Copernican had to consider) he made an error — which, it is true, he recognised later of having made — stating it to be *circular*.[71]

The error of the Galilean solution was discovered by Mersenne,[72] who subjected it to a very searching criticism, and tried to devise a better one. This, in turn, led to a very interesting discussion about the trajectory of a falling body: a discussion in which Fermat took a prominent part.[73]

On the other hand, the partial acceptance of Galileo's erroneous theory by Riccioli induced the latter to present a new objection against the motion of the earth: an objection that gave birth to a heated polemic in Italy, of which polemic Newton's friend James Gregory published a very carefully written report in the *Philosophical Transactions* in 1668.[74]

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[69] Even Isaac Barrow, the master of Newton, was by no means sure of the verity of the Copernican doctrine and, on his deathbed, expressed the hope that he would learn the truth in the other world. About the spread of Copernicanism in England, cf. F. R. Johnson, *Astronomical thought in renaissance England*, The Johns Hopkins Press, Baltimore, 1937.

[70] Johannes Baptista Riccioli, S. J., *Almagestum novum*, Bononiae, 1651; *Astronomia reformata*, Bononiae, 1665.

[71] Cf. *Dialogo dei due massimi sistemi del mondo* (Opere., Ed. Naz., 7, pp. 190 sq.).

[72] Cf. R. P. Marin Mersenne, *Harmonices mundi*, Parisii, 1636; *Harmonie universelle*, Paris 1636; *Cogitata physico-mathematica*, Parisii, 1644.

[73] The very interesting discussions about the trajectory of a body falling down on a rotating earth are unfortunately too intricate as to be dealt with here. I am studying this history elsewhere. Here I shall only mention that according to Fermat this trajectory will have the form of a spiral, and that this view was held also by Stephano degli Angeli (see note 74). Fermat developed his theory in a letter to Galileo which has remained unpublished. Yet, since he communicated it to Mersenne, the latter gave an account of it in his *Cogitata physico-mathematica*, Parisii, 1644, pp. 57 sq. & even added to the text a drawing which is not without some resemblance to that of Newton; in both of them, for instance, in spite of the fact that the deviation of the falling body from the perpendicular is to the east, the spiral is drawn from the right to the left. Newton may have been acquainted with Fermat's thesis and with Mersenne's drawing. (See fig. 2.)

[74] Cf. An Account of a controversy betwixt Stephano de Angelis, professor of the mathematics in Padua, and Joh. Baptista Riccioli, Jesuite; as it was communicated out of their lately Printed Books by that learned mathematician, Mr. Jacob Gregory, a Fellow of the R. Society, *The Royal Society, Philosophical Transactions 1*, pp. 693 sq., 1668. Gregory does not quote the titles of the books he is reporting about. It seems worthwhile to reproduce them in full:

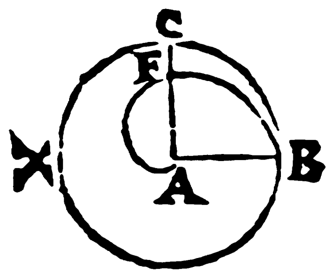


Fig. 2

[i.] Stefano degli Angeli: *Considerationi sopra la forza / di alcune raggioni / fisicomattematiche / addotte dal M. R. P./Gio. Battista Riccioli della Compagnia di Giesù nel suo Almagesto Nuovo / et Astronomia Riformata contro il / Sistema Copernicano / espresse in due dialogi da F. / Stefano degli Angeli / Venetiano, Mattematico nello Studio di Padova*, Apreso Bartolo Bruni, Venetia 1667.

[ii] Michele Manfredi, replying to Angeli in the name of Riccioli, who did not want to

Thus it is not particularly surprising that Newton too, perhaps as early as when reading Gregory's paper, turned his attention to the problem; nor is it astonishing that having done it, he found the true answer: the body falling from a high tower will not "lag behind," but "outrun" it, i.e. fall not to the *west* but to the *east* of its initial position.

Now let us go back to Newton and to Hooke.

Upon receiving Newton's letter above discussed, Hooke immediately presented it to the Royal Society. At its meeting on the 4th of December 1679,

> Mr. Hooke produced and read a letter of Mr. Newton to himself, dated 28th November, 1679, containing his sentiments of Mons. Mallemont's new hypothesis of the heavens; and also suggesting an experiment, whereby to try, whether the earth moves with a diurnal motion or not, viz. by the falling of a body from a considerable hight, which, he alledged, must fall to the eastward of the perpendicular, if the earth moved.
>
> This proposal of Mr. Newton was highly approved of by the Society; and it was desired, that it might be tried as soon as could be with convenience.[75]

Newton's proposal was not only approved, but also discussed. And nothing is more illuminating than this discussion; it shows us the scientific climate, or if one prefers, the level of the scientific understanding — or lack of understanding — of even the best minds of the time. Thus we read that:

> Sir Christopher Wren supposed, that there might be something of this kind tried by shooting a bullet upwards at a certain angle from the perpendicular round every way, thereby to see whether the bullets so shot would all fall in a perfect circle round the place, where the barrell

enter himself in the polemics, or, at least to do it under his own name. (According to Carlos Sommervogel, S.J., *Bibliothèque de la compagnie de Jesus*, *s.v.* "Riccioli," vol. 6, p. 1803, Bruxelles-Paris, 1895, "Manfredi" is only a pseudonym of Riccioli): *Argomento fisicomattematico / del padre Gio. Battista Riccioli Della Compagnia di Giesù / contro il moto diurno della terra, / Confirmato di nuovo con l'occasione della Risposta alle Conside-/razioni sopro la Forza del dello Argomento, etc. / Fatte dal M. R. Fr. Stefano De Gli Angeli, / Mattematico nello Studio di Padova, / All'Illustriss. Signore il Sig. Co: Francesco Carlo Caprara, / Conte di Pantauo, / Gonfaloniere di Giustizia / del Popolo et Commune di Bologna*, Per Emilio Maria, Fratelli de' Manolesi, in Bologna, 1668.

[iii.] Angeli, defending himself against Manfredi, and counterattacking: *Seconde / considerationi / sopra la forza / dell' argomento fisicomattematico / del M. Rev. P. / Gio. Battista Riccioli / della Compagnia di Gésù, / contra il moto diurno della terra, / spiegato dal Sig. Michel Manfredi nelle sue "Risposte, e / Riflessioni sopra le prime Considerationi / di / F. Stefano degl' Angeli / Venetiano / Mattematico nello Studio di Padova" / Espresse da questi in due altri Dialoghi III, e. IV.* / Per Mattio Bolzetta de Cadorini, in Padova, 1668.

Besides the books reported about by Gregory there are four others on the same subject.

[iv.] *Risposta / di Gio: Alfonso / Borelli / Messinese Matematico dello Studio di Pisa / Alle considerazioni fatte sopra alcuni luoghi del suo / Libro della Forza della Percossa / Dell R. P. F. Stefano De Gl. Angeli / Matematico nello Studio di Padova, / All' Illustrissimo, e Dottissimo Sig. / Michel Angelo Ricci.* Messina, 29 Febraio, 1688.

[v.] *Terze / Considerationi / Sopra una lettera / Del Molto illustre, et eccelentissimo Signor / Gio: Alfonso Borelli Messinese Mattematico nello Studio di Pisa / Scritta da Questi in replica / Di alcune dottrine incidamente tocche / Da Fra / Stefano degli Angeli / Venetiano / Mattematico Nello Studio di Padova / Nelle sue prime considerationi sopra la forza di certo Argomento contro il moto diurno della Terra / Espresse da questo in un Dialogo / Quinto in ordine*, In Venetia M.DC.LXVIII, Apresso li Heredi Leni con licenza de' Superiori.

[vi.] *Confermazione / d'una sentenza / del Signor / Gio Alfonso / Borelli M. / Matematico dello Studio di Pisa / di nuovo contradetta / Dal / M. R. P. Fra Stefano / de Gl' Angeli / Matematico dello Studio di Padova / nelle sue terse considerazioni / Prodotta da /* Diego Zerilli. / In Napoli, per Ludovico Cauallo, 1668.

[vii.] *Quarte / Considerationi / Sopra la Confermatone / D'una Sentenza dal Sig. Gio. Alfonso Borelli M. / Matematico nello Studio di Pisa / Prodotta da Diego Zerilli / contro le terze Considerationi / Di Stefano degli Angeli / E sopra l'Apologia del M. R. P. Gio. Battista Riccioli / Della Compagnia di Giesù / A favore d'un suo Argomento detto Fisico-Matematico / Contro il sistema Copernicano / Espresse dal medesimo de gl'Angeli Venetiano Matematico / nello Studio di Padova in due Dialoghi VI. e. VII.* In Padova, Per Mattio Cadorin detto Bolzetta, 1669, con Licenza de' Superiori.

According to Sommervogel (*op. cit., loc. cit.*), the *Apologia* of R. P. G. B. Riccioli is the same book as that of Manfredi quoted under [ii.].

[75] Minutes of the Royal Society, 4 Dec. 1679; of. Birch, *op. cit.*, 3, pp. 512 sq.; W. W. Rouse Ball, *op. cit.*, p. 145.

was placed. This barrell he desired might be fixed in a frame upon a plain foot, and that foot placed upon a true plain every way, and the mouth of the gun be almost in the same point over the plain which way soever shot.

Mr. Flamstead hereupon alledged, that it was an observation of the gunners, that to make a ball fall into the mouth of the piece, it must be shot at eighty-seven degrees; and that he knew the reason thereof; and that it agreed with his theory: and that a ball shot perpendicularly would never fall perpendicularly: and he mentioned the recoiling of a perpendicular jet of waters. But this was conceived to arise from some mistake of the gunners, in not well taking notice of all circumstances; since a body shot perpendicularly would also descend perpendicularly; and a body shot at eighty-seven degrees would fall considerably distant from the place where it was shot.[76]

A week later, on the 11th of December 1679, Hooke, once more, deals with Newton's letter. This time it is not the experiment, but Newton's solution of the problem of the trajectory of the falling body that is in question:

Upon the mentioning of Mr. Newton's letter, and the experiment proposed in it, Mr. Hooke read his answer to him upon that subject, wherein he explained what the line described by a falling body must be supposed to be, moved circularly by the diurnal motion of the earth,[77] and perpendicularly by the power of gravity: and he shewed, that it would not be a spiral line, as Mr. Newton seemed to suppose, but an excentrical elliptoid, supposing no resistance in the medium: but supposing a resistance, it would be an excentric ellipti-spiral, which, after many revolutions, would rest at last in the centre: that the fall of the heavy body would not be directly east, as Mr. Newton supposed; but to the south-east, and more to the south than the east. It was desired, that what was tryable in this experiment might be done with the first opportunity.[78]

The problem, whether by "elliptoid," Hooke meant an ellipse or simply some kind of oval curve, has always been a *crux* for the historians.[79] The finding of Hooke's letter to Newton at last enables us to give a definitive answer to this vexing question: Hooke did *not* mean the curve to be an ellipse.[80]

To the second *questio vexata*: by what kind of reasoning did Hooke arrive at the conviction that the falling body — supposing, as always has been done, no resistance in the medium — would describe a curve closed upon itself, and thus assert, *for the first time and in a violent opposition to the whole preceding tradition*,[81] that a body. falling down on a moving earth will *not* arrive at its centre, in contradistinction to what would happen if the earth remained immobile? — this letter, unfortunately, gives us no information, and we are still reduced to hypotheses.

It is nevertheless extremely interesting to see that Hooke, though deploring Newton's "desertion of philosophy," is more than skeptical about the reality of this desertion. But, of course, the chief value of this letter lies in its scientific part, as it gives us the first, though not quite correct — but we can not blame him for that[82] — application to the problem of the trajectory of falling bodies of Hooke's theory of "compounding a curve by a direct |tangential| Motion and an attractive one to the centre."

Hooke's letter is dated 9 December 1679 and addressed "to his much honoured Friend, Mr. Isaac Newton, Lucasian Professor at Cambridge."[83]

[76] *Ibid.* The criticism of Wren's and Flamsteed's opinions is, probably, due to Hooke.

[77] Even Hooke, in spite of the fact that he had given to the principle of inertia a pretty good formulation, falls into the error of considering the falling bullet as moved circularly by the rotation of the earth.

[78] Cf. Birch, *op. cit.*, 3, p. 516; W. W. Rouse Ball, *op. cit.*, p. 146.

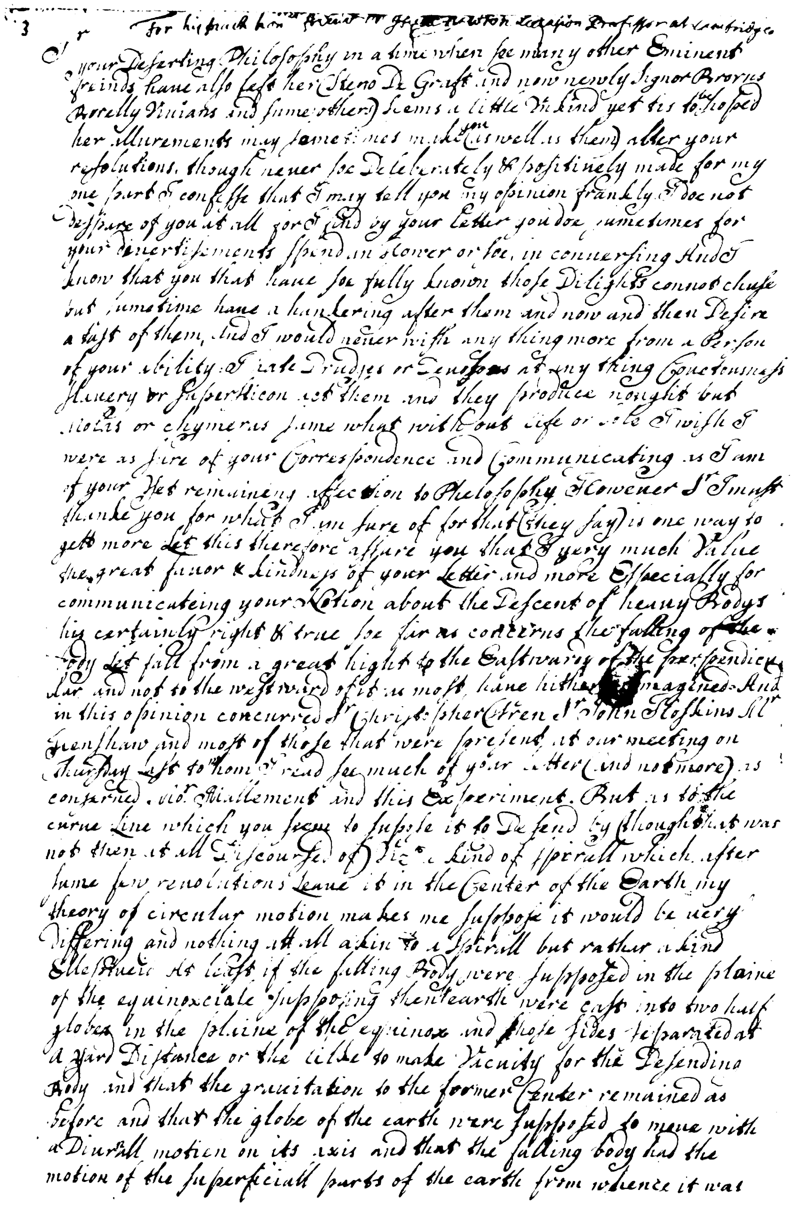
[79] Cf. Miss L. D. Patterson, *op. cit.*, *Isis 41*, pp. 39 sq. and 42, 1950.

[80] Cf. *infra*, p. 329.

[81] Even Borelli, who asserted that a planet, gravitating towards the sun and animated by a motion along the tangent, will not fall down to the sun but will move around and describe an ellipse, never asserted that a heavy body on the earth will behave in the same manner.

[82] The problem that Hooke and Newton are dealing with is extremely difficult and was solved only in 1835 by Coriolis.

[83] Of this letter only the subscription and the signature are in Hooke's own handwriting. The rest is written by an amanuensis, and a very bad and ignorant one. I am reproducing it as faithfully as possible, without correcting either the spelling, or the punctuation, even where the words are obviously misspelt or make no sense.


For his much hon[ore]d friend Mr Isaac Newton Lucasian Professor at Cambridge

Sr
your Deserting Philosophy in a time when soe many other Eminent friends have also left her (Steno De Graft and now newly Signor Brorus Borelly Vivians and some others) seems a little unkind yet tis to be hoped her allurements may sometimes make you (as well as them) alter your resolutions, though never soe Deliberately & positively made for my one part I confesse that I may tell you my opinion frankly I doe not despaire of you at all for I find by your letter you doe sometimes for your divertisements spend an houre or soe in conversing And I know that you that have soe fully known those Delights cannot chuse but sometime have a hankering after them and now and then Desire a tast of them. And I would never wish any thing more from a Person of your ability. I hate Drudges or Deniers at any thing Covetousness Slavery or Superstition act them and they produce nought but noises or chymeras some what without life or sole. I wish I were as sure of your Correspondence and Communicating as I am of your Yet remaining affection to Philosophy However Sr I must thanke you for what I am sure of for that (they say) is one way to gett more. Let this therefore assure you that I very much Value the great favour & kindness of your Letter and more Especially for communicateing your Notion about the Descent of heavy Bodys tis certainly right & true soe far as concerns the falling of the Body let fall from a great hight to the Eastwards of the perpendicular and not to the westward of it as most have hitherto imagined. And in this opinion concurred Sr Christopher Wren Dr John Hoskins Mr Henshaw and most of those that were present at our meeting on Thursday last to whom I read soe much of your Letter (and not more) as concerned viz. Allegement and this Experiment. But as to the curve Line which you seem to suppose it to Desend by (though that was not then at all Discoursed of) viz. a kind of spirall which after some few revolutions Leave it in the Center of the Earth my theory of circular motion makes me suppose it would be very differing and nothing at all akin to a spirall but rather a kind Ellipsoeid. At least if the falling Body were supposed in the plaine of the equinoxciale supposing then the earth were cut into two half globes in the plaine of the equinox and those sides separated at a yard Distance or the like to make Vacuity for the Descending Body and that the gravitation to the former Center remained as before and that the globe of the earth were supposed to move with a Diurnall motion on its axis and that the falling body had the motion of the superficiall parts of the earth from whence it was

Fig. 3. First page of Hooke's letter to Newton, 9 December 1679, reproduced through the courtesy of the Yale University Library.

Sr.

your deserting Philosophy at a time when soe many friends have also left her (Steno De Graft and now newly Signor Brorus Borelli Vivians [84] and some others) seems a little unkind yet tis to be hoped her allurements may sometimes make you (as well as them) alter your resolutions, though newer soe Deliberately and posi-

[84] I do not understand the meaning of Hooke's assertion that Steno (Nicolaus Stenonius), De Graft (doubtlessly Regnerus de Graaf), Signor Brorus (?), Borelli (J. A. Borelli) and Vivians (Vincenzo Viviani) left philosophy. De Graaf, of course, did it when he died on 17 Aug. 1673; Borelli died on 31 Dec. 1679, but at the time of Hooke's letter he was still alive. Steno lived until 25 Nov. 1686, and Viviani until 22 Sept. 1703; both were active until their death. As for Signor Brorus (?), I do not know who he is. It is possible that the name is misspelt.

tively made for my one part I confesse that I may tell you my opinion frankly. I doe not despare of you at all for I find by your letter you doe sumetimes for your divertisements spend an hower or soe, in conversing. And I know that you that have so fully known those Dilights cannot chuse but sumetime have a hankering after them and now and then Desire a tast of them, and I would neuer wish any thing more from a Person of your ability: I hate Drudges or Devosons [?] at any thing Covetousness Slavery or Supersticon act them and they produce nought but Molas or chymeras sume what with out life or sole. I wish I were as sure of your Correspondence and Communicating as I am of your yet remaining affecshon to Philosophy. However S^r I must thanke you for what I am sure of for that (they say) is one way to gett more. Let this therefore assure you that I very much value the great favor and kindness of your Letter and more Especially for communicateing your Notion about the Descent of heavy Bodys his certainly right and true soe far as concerns the falling of the body Let fall from a great hight to the Eastward of the perpendicular and not to the westward of its as most have hitherto Imagined. And in this opinion concurred S^r Christopher Wren S^r John Hoskins M^r Glenshaw and most of those that were present at our meeting on Thursday Last to whom I read soe much of your letter (and noe more) as concerned Mr Mallement and this Experiment. But as to the curue line which you seem to suppose it to Desend by (thought that was not then at all Discoursed of) Viz^o a kind of spirral which after sume few revolutions Leave it in the Center of the Earth my theory of circular motion makes me suppose it would be very differing and nothing at all akin to a Spirall but rather a kind Elleptueid. At least if the falling body were supposed in the plaine of the equinoxiale supposing then y^e earth were cast into half globes in the plaine of the equinox and those sides separated at a yard Distance or the lilke [?] to make Vacuity for the Descending Body and that the gravitation to the former Center remained as before and that the globe of the earth were supposed to move with a Diurnall motion on its axis and that the falling body had the motion of the superficiall parts of the earth from whence it was Let fall Impressed on it I conceive the line in which this body would move resemble An Elleipse for Instance Let A B D E represent the plaine of the equinox limited by the superficies of the earth C the Center thereof to which the lines of Gravitation doe all tend. Let A represent the heavy Body let fall at A and attracted towards C but Moued also by the Diurnall Reuolution of the earth from A towards B D E etc I conceive the curve that will be described by this descending body A will be A F G H and that the body A would neuer approach neerer the Center C then G were it not the Impediment of the medium as Air or the like but would continually proceed to move round on the Line A F G H A F G etc. But were the Medium through which it moues has a power of impeding and destroying its motion the curve in wch it would move would be some what like the line A J K L M N O P etc and after many resolutions [*sic*] would terminate in the center C. But if the Body litt [?] fall be not in the aquinochill plain as here in London 51° 32′ the elleipsed will be made in a plain as inclined to the plaine of the Equinox : 51·32 Soe that the fall of the Ball will not be exactly east of the perpendicular but south East and indeed more to the South then the east as lett N L Q S represent y^e Meridian of London and Q the equinox L London and P L the parrallel in w^ch it moves about the Axis N S the body let fall at L would desend in the plaine L C supposed at right angles with the plaine of the Meridian N L Q S R and not in the superificies of the cone P L C whose apex is C the Center of the Earth and whose base is the plaine of the parrallel circle P L.[87] I could adde many other conciderations which are consonant to my Theory of Circular motions compounded by a Direct motion and an attractiue one to a Center. But I feare I have already trespassed to much upon your more Usefull thoughts with these my Impertinants yet I would desire you not to look upon them as any prouacations to alter your ***mind*** more mature and serious Resolutions. Goe on and Prosper and if you succeed and by any Freind let me understand what you think fit to impart any thing from you will be Extremely Valued by

S^r Your very Humble Servant

RT HOOKE

Gresham Colledg Dec 9th 1679

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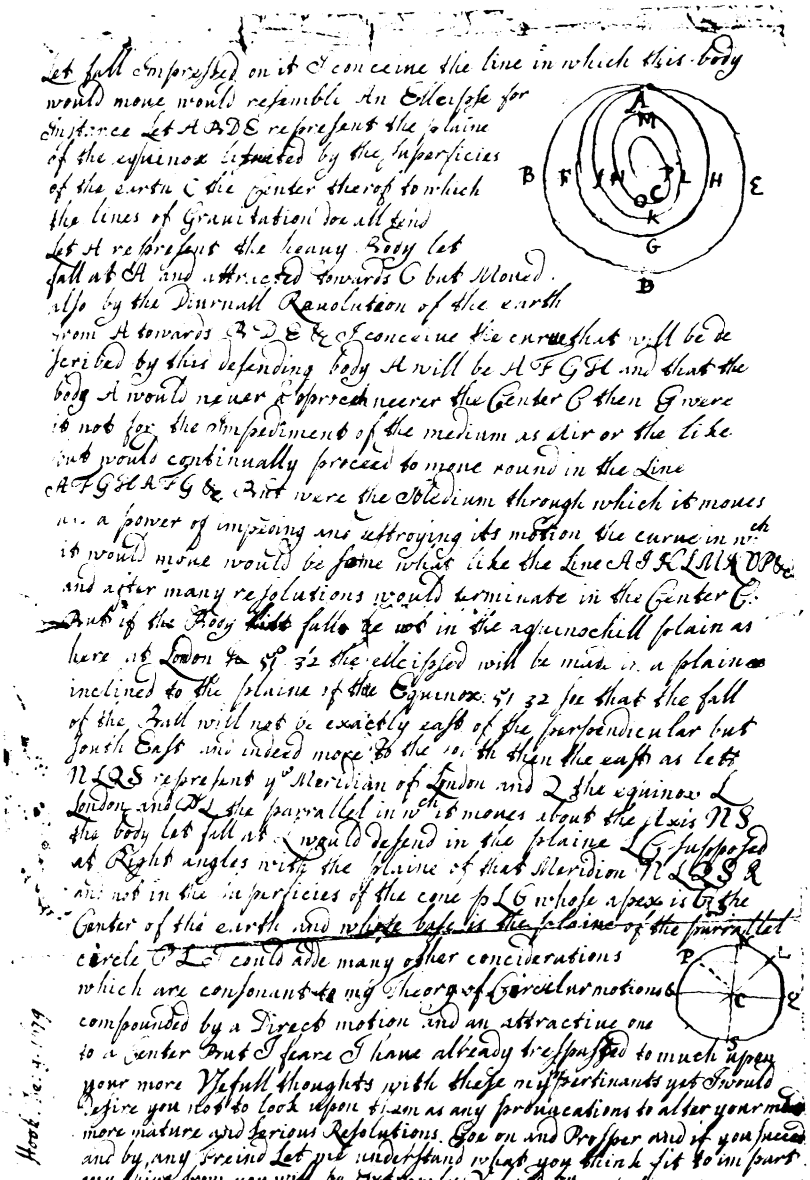
[85] Devosons or Devotons. I do not know what is meant. Perhaps, devotees.

[86] "lilke" — probably, like.

[87] It is interesting to note that Newton did not assert that the falling body will describe a spiral on a cone. Hooke misunderstands him or, better to say, reconstructs Newton's views. He obviously believes Newton to hold a certain theory about the fall of heavy bodies on a rotating earth according to which these bodies will describe spirals: a plain one when falling on the equator, and conical ones, when falling from a point placed on some parallel. This theory, reported by Mersenne and Galileo, goes back to John George Locher's *Disquisitiones mathematicae*, Ingolstatii, 1614. The *Disquisitions* of Locher, having been held *sub praesidio Christophori Scheineri*, are usually misquoted as a work of this latter.



let fall impressed on it I conceive the line in which this body
would move would resemble An Ellipse for
instance Let A B D E represent the plaine
of the equinox limited by the superficies
of the earth C the center thereof to which
the lines of Gravitation doe all tend
let A represent the heavy Body let
fall at A and attracted towards C but Moved
also by the Diurnall Revolution of the earth
from A towards B D E &c I conceive the curve that will be de
scribed by this descending body A will be A F G H and that the
body A would never approach neerer the Center C then G were
it not for the Impediment of the medium as Air or the like
but would continually proceed to move round in the Line
A F G H A F G &c. But were the Medium through which it moves
has a power of impeding and destroying its motion the curve in which
it would move would be some what like the Line A I K L M N O P &c
and after many revolutions would terminate in the Center C.
But if the Body be let fall not in the aequinoctiall plain as
here at London in 51 32 the ellipsoid will be made in a plaine
inclined to the plaine of the Equinox 51 32 so that the fall
of the Ball will not be exactly east of the perpendicular but
South East and indeed more to the south then the east as lett
N L Q S represent ye Meridian of London and Q the equinox L
London and P L the parallel in which it moves about the Axis N S
the body let fall at L would descend in the plaine L C supposed
at Right angles with the plaine of that Meridion N L Q S
and not in the superficies of the cone P L C whose apex is C the
Center of the earth ~~and whose base is the plaine of the parralled~~
circle P L. I could adde many other considerations
which are consonant to my Theory of Circular motions
compounded by a Direct motion and an attractive one
to a Center But I feare I have already trespassed to much upon
your more Usefull thoughts with these impertinants yet I would
desire you not to look upon them as any provocations to alter your more
more mature and serious Resolutions. Goe on and Prosper and if you succeed
and by any freind Let me understand what you think fit to impart
any thing from you will be Extreamly Valued by
Gresham Colledg Dec: 9th 1679 Sr Your very Humble Servant Ro: Hooke

Hook. Dec. 9. 1679

FIG. 4. Second page of Hooke's letter to Newton, 9 December 1679, reproduced through the courtesy of the Yale University Library.

Hooke's letter to Newton is — or at least pretends to be — written in an amiable and friendly spirit. But it was by no means in this spirit that it was received by the latter; quite the contrary, it made him exceedingly angry.[88] After all, it is easy to understand Newton's reaction; nobody, and Newton less than anybody, likes his blunders to be pointed out to him, and "corrected," even if the corrections are based

[88] "Could scarce persuade myself to answer his second letter; did not answer his third," reports Newton to Halley, 20 June 1686; cf. W. W. Rouse Ball, *op. cit.*, p. 157. This "third," by the way, was perhaps the most important of the lot: it was the letter where Hooke told Newton that "the attraction always is in duplicate proportion to the center reciprocall. . . ."

— at least partially — upon a misunderstanding. And Hooke not only "corrected" Newton, but also exposed his blunder to the Royal Society, i.e., to the world.[89]

Small wonder, therefore, that Newton's answer should be as dry and terse as a solicitor's writ. He wants to make his blunder good somehow and, at the same time, to teach Hooke a lesson, show him his own error, tell him what, in the case imagined by him (a fall through a void space which yields without resistance), the real path of the falling body would be.

Newton admits that if there is no resistance, the body in question will not arrive at the center of the earth. But in that case, and contrary to Hooke's supposition, it will not describe a closed curve "resembling an ellipse" but an open [90] and very complicated one — a curve which he, Newton, is able to determine, but not Hooke. Thus, he writes:

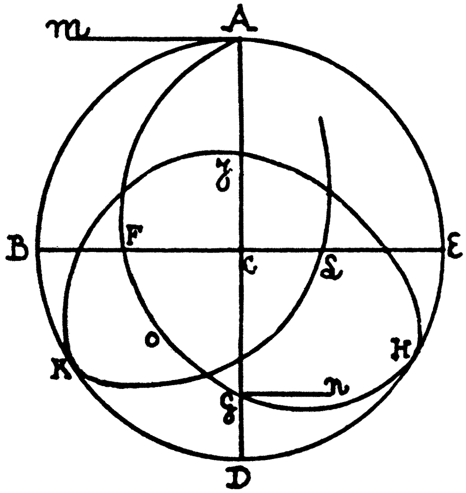


FIG. 5

"S^{r}

I agree wth you y^{t} y^{e} body in o^{r} latitude will fall more to y^{e} south then east if y^{e} height it falls from be any thing great. And also that if its gravity be supposed uniform it will not descend in a spiral to y^{e} very center but circulate wth an alternate ascent & descent made by its vis centrifuga & gravity alternately overballancing one another. Yet I imagin y^{e} body will not describe an Ellipsoeid but rather suit a figure as is represented by A F O G H J K L etc. . . .[91]

At the end of the letter, Newton, imitating Hooke's manner, concludes:

Your acute Letter having put me upon considering thus for y^{e} species of this curve,[92] I might add something abouts its description by points quam proxime. But the thing being of no great moment I rather be[g] yo^{r} pardon for having troubled you thus far wth this second scribble wherin if you meet wth any thing inept or erroneous I hope you will pardon y^{e} former & y^{e} latter I submit & leave to yo^{r} correction remaining S^{r}

Yo^{r} very humble Servant
Is. NEWTON

[89] From a formal point of view, Hooke's actions were perfectly correct: he did not read to the Royal Society the "personal part" of Newton's letter (about his estrangement from philosophy, etc.); as for the scientific part, it was addressed to Hooke as the secretary of the Royal Society, and had to be presented to its members, as well as the answer (scientific) that Hooke sent to Newton. Still, having repeatedly asked Newton for a *private* correspondence and having assured him of secrecy, Hooke, in making this correspondence public, certainly demonstrated a lack of tact. As for Newton's reaction, it is expressed in the following passage of the postscript of his letter to Halley quoted *supra*, n. 88. (p. 161): "Should a man who thinks himself knowing, and loves to show it in correcting and instructing others, come to you, when you are busy, and notwithstanding your excuse press discourses upon you, and through his own mistakes correct you, and multiply discourses; and then make this use of it, to boast that he taught you all he spake, and oblige you to acknowledge it, and cry out injury and injustice if you do not; I believe you would think him a man of strange unsociable temper. M^{r} Hooke's letters in several respects abounded too much with that humour, which Hevelius and others complain of. . . ."

[90] As a matter of fact, Newton does not say that the curve will be an open one; nor does he say that it will be closed; he does not say anything about it. His very careful drawing is made in such a way as to leave the question open.

[91] Cf. J. Pelseneer, Une lettre inédite de Newton, *Isis 12*, p. 243 sq., 1929. This letter is dated 13 Dec. 1679.

[92] A moment of very great importance, because, as Newton himself later told Halley (letter of 27 July 1686, W. W. Rouse Ball, *op. cit.*, p. 167): ". . . his correcting my spiral

Newton's solution is not quite correct. Which, as a matter of fact, is not surprising. The problem he deals with is very difficult,[93] and its solution implies the use of mathematical methods that Newton, probably, did not possess at the time, perhaps not even later. Much more surprising is the very problem Newton is treating — the problem of a body submitted to a *constant* centripetal force. In other words, Newton assumes, or at least seems to assume, that gravity is something *constant* — an assumption that enabled Hooke to "correct" him once more by pointing out that Newton misunderstood the question.

"Your calculation of the curve described by a body attracted by an aequall power at all distances from the center, such as that of a ball rolling in an inverted concave cone," writes Hooke to Newton on 6 Jan. 1680, "is right,[94] and the two auges will not unite by about a third of a revolution; but my supposition is that the attraction always is in duplicate proportion to the distance from the center reciprocall. . . ."[95]

Years later, at the height of a new quarrel with Hooke — the last one, the quarrel about the priority in the discovery of the inverse square law — Newton tried to explain (or to explain away) his blunder in the following way or ways:

> The summe of what past between Mr Hooke and me (to the best of my remembrance) [writes he[96] to Halley on May 27th, 1686] was this. He solliciting me for some philosophicall communications or other I sent him this notion, that a falling body ought by reason of the earth's diurnall motion to advance eastward and not fall to the west as the vulgar opinion is. And in the scheme wherein I explained this I carelessly described the descent of the falling body in a spirall to the center of the earth:[97] which is true in a resisting medium, such as our air is. Mr Hooke replyed it would not descend to the center but at a certaine limit returne upwards againe. I then took the simplest case for computation, which was that of gravity uniform in a medium not resisting — imagining he had learned the limit from some computation, and for that end had considered the simplest case first.[98] And in this case I granted what he contended for, and stated the limit as nearly as I could. He replyed that gravity was not uniform but increased in descent to the center in a reciprocall duplicate proportion of the distance from it, and thus the limit would be otherwise than I had stated it, namely, at the end of every intire revolution, and added that according to this duplicate proportion the motions of the planets might be explained and their orbs defined.

And in a letter of 20 June 1686 (which I have already and repeatedly quoted) Newton adds that[99] at the time of this correspondence he "thought no further of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time."

Newton's explanations, to tell the truth, do not seem very convincing. It is hard to believe that, if he ever had had "thoughts" about the problem, that is, if he had really thought out a theory about the complete trajectory of the falling body, he could have forgotten it so much as to form, on the spur of the moment, another and quite a different one.

It is difficult to admit too that it was mere "carelessness" that made Newton describe (that is : draw) the descent of the falling body in a spiral; the drawing only

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occasioned my finding the theorem, by which I afterwards examined the ellipsis . . . ," and (letter of 14 July 1686, *ibid.*, p. 165): "his letters occasioned my finding the method of determining figures, which when I had tried [it] in the ellipsis, I threw the calculations by, being upon other studies; and so it rested for about five years, till upon your request I sought for that paper; and not finding it, did it again. . . ."

[93] Cf. J. Pelseneer, *op. cit.*, pp. 250 sq., for a discussion of the problem and its solution.

[94] Cf. J. Pelseneer, *op. cit.*, p. 251: "Hooke a donc observé qu'on peut ramener au problème traité par Newton l'étude du mouvement d'un point pesant assujetti à se mouvoir sur un cône de révolution, d'axe vertical et de rayon CA, reposant sur la pointe en C. On sait que cette équivalence des deux problèmes est une conséquence des équations intrinsèques du mouvement du point sur la surface conique. Si l'on songe que Hooke ne s'est probablement livré à aucun calcul, sa remarque donne une mesure exacte de la profondeur et de la sûreté de son intuition."

[95] Hooke's letter to Newton of 6 Jan. 1680; cf. W. W. Rouse Ball, *op. cit.*, p. 147.

[96] Cf. W. W. Rouse Ball, *op. cit.*, p. 155.

[97] This is not quite true: the drawing is by no means careless and is backed by the text.

[98] Newton probably thought that Hooke was following Borelli who, though admitting a constant power of gravity, believed that the resulting curve will be an ellipse.

[99] Cf. W. W. Rouse Ball, *op. cit.*, p. 157.

illustrates the text of the letter. A more careful one would, perhaps, add some spires to the line; it would not have changed its essential nature. Besides, that the falling body would describe a spiral in the interior of the earth was, as I have said, a rather widely held belief. Moreover, as Newton himself points out, for a resisting medium it is quite a correct one.

It seems to me, therefore, most probable that Newton never had given many "thoughts" to the problem.[100] It may have appeared to him not only devoid of reality — bodies do not fall through the earth — but also of no importance whatever (he tells us so himself). He may even have felt that it lacks any (or, at least, any definite) meaning. How was the earth to be considered in this case — as a material body that would differ from our earth only by being penetrable as our air? In this case, the medium would oppose resistance. Or, should all matter be thought away, leaving behind it pure space and its former center? But in this case, why should the body go down towards it? The answer was quite clear for those who, as Fermat, still believed that bodies "seek" the center of the earth, but for those who did not and who believed that bodies were "attracted by" or "pushed towards" the earth, the situation was quite different. They had to find out in which way this attraction, whether a pull or push, would vary in the interior of the earth. And that was a problem that nobody could master, not even Newton, who, as he confesses to Halley, held pretty uncertain views about it until 1685; [101] thus he "never extended the duplicate proportion lower than to the superficies of the earth, and before a certain demonstration I found the last year, have suspected it did not reach accurately enough down so low. . . ." [102]

Newton was interested in gravity as a cosmical factor. He endeavoured to find a physical explanation of this "force," because he never, as well we know, believed in an "attractive power." He did not achieve his purpose. Indeed, he found out something else, *viz.*, the impossibility of doing it: [103] a discovery of tremendous importance (though usually not recognized as such) which liberated his mind to transform "attraction" from a physical into a "mathematical" force.

As for terrestrial gravity, though he suspected, and even believed in, its identity with the cosmical one, he never, before Hooke put it to him, made it a subject of special study. As to terrestrial gravity, he was even less sure than in the case of that "gravity" or "attraction" which regulated the movements of the planets, that the inverse square proportion which he deduced from Kepler's third law was anything more than a mere approximation. Nay, he was rather certain that it was just that:

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[100] For the solution of the problem of the trajectory of a body falling down *to the earth* from a point placed above its surface — the problem he deals with in his first letter to Hooke — he did not need such a theory. It is perfectly true that, as he says to Halley (letter of 20 June 1686, postscript, W. W. Rouse Ball, *op. cit.*, p. 162), "In the small ascent and descent of projectiles above the earth, the variation of gravity is so inconsiderable, that Mathematicians neglect it. Hence the vulgar hypothesis with them is uniform gravity." Yet, of course, this did not entitle Newton to admit, as in his second letter to Hooke, that gravitation was constant below the earth's surface, down to the center of the earth. And to say as Newton does (*ibid.*): "And why might not I, as a Mathematician, use it frequently without thinking on the philosophy of the heavens, or believing it to be philosophically true?" does not explain nor justify his procedure. Nor does it explain away his blunder and thoughtlessness.

[101] Cf. Letter to Halley, 20 June 1686; W. W. Rouse Ball, *op. cit.*, p. 157.

[102] Cf. *De motu* (W. W. Rouse Ball, *op. cit.*, p. 56): "Ex horologii oscillatorii motu tardiore in cacumine montis praealti quam in valle liquet etiam gravitatem ex aucta nostra a terrae centro distantia diminui, sed qua proportione nondum observatum est." Newton does not tell us why he suspected that the "duplicate proportion" did not reach accurately down to the superficies of the earth, but it seems to me not to be impossible to guess the reasons: (*a*) the near parts of the earth could (and at the first glance, should) play a greater influence than those that are far away, and (*b*) a body which is in the atmosphere of the earth is already, in a certain sense, below its surface.

[103] He found that in order to explain attraction mechanically, as an action of the surrounding medium (aethereal push) he had to postulate an elastic aether; and this implied the postulation of a force of repulsion between the particles of the aether. Thus, attraction could only be explained by repulsion, that is, by something philosophically just as bad.

a mere approximation — because, as he tells Halley,[104] "There is so strong an objection against the accurateness of this proportion, that without my demonstrations, to which Mr Hooke is yet a stranger, it cannot be believed by a judicious philosopher." [105]

As for the inside of the earth, he only knew that the inverse square proportion could not be applied there. But he was not able — being probably not sufficiently interested in the problem — to determine what was the one that had to be applied. And it was only when Hooke — erroneously — asserted that the inverse square law was valid even there (though at the same time acknowledging that he did not believe it seriously), that Newton applied himself to solving the problem. This he did, as we know, in 1685 when he found out (solving two problems at the same time) that the inverse square law had to be applied primarily not to the wholes, but to the particles which compose them, and that the resulting (mathematical) attraction of a spherical body, such as the earth, had to be computed as if its whole mass was concentrated in its center.[106] There was the surprising, but nevertheless necessary and evident, result that a particle placed outside of a sphere will be attracted by a force inversely proportional to the square of its distance from the center,[107] and a particle placed inside a sphere will be attracted by a force proportional to its distance from the center.[108] Of these deliberations Newton tells us:

> After I had found that the force of gravity towards a whole planet did arise from and was compounded of the forces of gravity towards all its parts, and towards every one part was in the inverse proportion of the squares of the distances from the part, I was yet in doubt whether that proportion inversely as the square of the distance did accurately hold, or but nearly so, in the total force compounded of so many partial ones; for it might be that the proportion which accurately enough took place in greater distances should be wide of the truth near the surface of the planet, where the distances of the particles are unequal, and their situation dissimilar. But by the help of the Prop. LXXV and LXXVI, Book I, and their Corollaries, I was at last satisfied of the truth of the Proposition, as it now lies before us.
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> [From Book III, proposition VIII, theorem VIII: *In two spheres gravitating each towards the other, if the matter in places or all sides round about and equidistant from the centres is similar, the weight of either sphere towards the other will be inversely as the square of the distance between their centres.*] [109]

But in 1679, when proposing to Hooke an experimental proof of the motion of the earth, he seems to have simply followed the lead of the tradition in considering gravity

104 Letter to Halley, 20 June 1686, W. W. Rouse Ball, *op. cit.*, p. 101. The importance of this passage has been pointed out by F. Cajori, who drew from it the conclusion that "before 1685 Newton suspected that the inverse square law was a mere approximation to the truth." Cf. F. Cajori, Newton's twenty years' delay in announcing the law of gravitation, in *Sir Isaac Newton, a bicentenary evaluation of his work*, Baltimore, Williams & Wilkins, 1928, p. 182.

105 Newton once more does not tell us what this objection is, nor what demonstration he has in mind; once more we are obliged to guess. I think that the reason why Newton felt that his deduction of the inverse square proportion from Kepler's third law was only valid *quam proxime* and not absolutely was that it had been made under the assumption that the orbits of the planets are circular, and not elliptic. Now, how could a proportion valid for circles be at the same time valid for ellipses? Besides the eccentric character of the orbits, the asymmetry resulting from the fact that the sun is to be found not in the center but in one of the foci of that ellipse, could seem to contradict a fundamental principle of Galilean physics, according to which a body descending from a certain altitude acquires a speed, or *impetus*, or moment, *i.e.*, a quantity of motion, sufficient to bring it back to the same height; it is obvious that these objections could only be met by a factual demonstration that from attraction following the inverse square law, "compounded with a direct motion by the tangent," there would result a motion in an ellipse, and *vice versa* that such a motion implied an attraction inversely proportional to the square of the distance and directed to one of the foci of the elliptical trajectory. It is this demonstration which constitutes, according to Newton himself, his great discovery, and not the invention of the inverse square law as such — an easy thing since the work of Huygens and even earlier.

106 The decisive role played by this discovery was recognized already by J. A. Adams and J. W. L. Glaisher in 1887 (cf. F. Cajori, *op. cit.*, p. 127 sq.), and in 1927, by H. H. Turner (*ibid.*, p. 186) who points out that this result "came as a complete surprise" to Newton.

107 *Sir Isaac Newton's Mathematical principles of natural philosophy*, Andrew Motte's translation revised by Florian Cajori, 2nd printing, Berkeley, Cal., 1946, p. 199: Proposition LXXXIV, Theorem XXXIV.

108 *Ibid.*, p. 196, Proposition LXXXIII, Theorem XXXIII.

109 *Ibid.*, pp. 415 sq.

as constant,[110] and in assuming that a heavy body, if it were not arrested by the earth, would finally arrive at its center. Each of these two conceptions is strictly incompatible with the other.

Let us now go back to Hooke. It cannot be denied that his attempt to deal with the problem of the trajectory of the falling body is extremely ingenious; the device of cutting the earth in two in order to give to this body the space it needs for moving down towards the center (and around it) is plainly brilliant; the endeavour to combine the idea of attraction, inherited from Gilbert, Kepler and Bacon, with the principles of the Galilean mechanics, already announced in the *Attempt to prove the motion of the earth by observation*,[111] and to apply the resulting pattern not only to celestial but also to terrestrial physics, points in the right direction. Once more we have to admire Hooke's depth of vision and energy of thought. Once more we are obliged to recognize that he is unable to arrive at a precise, quantitative, mathematical solution. Once more, though at this time he is in possession of the inverse square law, he misses the point.

He does not recognize that, in the case imagined by him, the falling body will describe not an "elliptoid," that is, some kind of oval curve; but an exact ellipse. This is all the more surprising since the situation in which he places his heavy body (falling down through a split made through the equator of the earth) is very nearly similar to that which he realized in his famous experiments performed before the Royal Society on 23 May 1666. The imagined case leads to a result exactly similar to that which, at that time, he believed the Royal Society experiments to indicate — the power by which the body is attracted towards the center is directly proportional to its distance from that center.[112]

It is rather strange that Hooke seems not to have recognized this analogy. It is all the more strange since he knew (or, more precisely, assumed) — as anyone who accepted the explanation of gravity by attraction was bound to assume — that the attractive power was at its maximum on the earth's surface, and that it diminished above as well as below it.[113] He even tried, as we know, to determine the rate of this diminishing by experiment.

One could argue, of course, that this analogy did not escape Hooke, and that it was just because he had no means of ascertaining the ratio of the variation of the force of attraction according to the distance of the attracted body from the center of the earth — he could not determine it theoretically and the experiments failed to give a result [114] — that he confined himself to stating in a vague manner that the curve in question will be a kind of "elliptoid," resembling an ellipse, but not an ellipse. But in this case he would have had no reason to speak of an "excentrical elliptoid"; [115] moreover, he would not be able to assert to Newton, as he did in his letter of 6 Jan. 1680, that his "supposition is that the attraction always is in duplicate proportion to the distance from the center reciprocall." Indeed, in his device of cutting the earth in two "in the plaine of equinox" he supposed "that the gravitation to the former center remained as before."

One could assume, on the other hand, that having discovered the inverse square law (as did everybody else) from Kepler's third law, and the law of centrifugal force

[110] It is a strange paradox of history that the strongest supporters of this conception have been Galileo and the Galileans.

[111] Cf. *supra*, p. 319.

[112] Newton, therefore, is perfectly right in reproaching Hooke (cf. letter to Halley, 20 June 1686, W. W. Rouse Ball, *op. cit.*, pp. 159 sq.) for extending the inverse square proportion down to the center of the earth.

[113] Cf. Cornelis de Waard, *Introduction* to the supplementary volume of the *Oeuvres* of Fermat, Paris, 1922.

[114] This was not Hooke's fault. With the instruments at his disposal, a successful direct measurement was impossible.

[115] In the experiments of 23 May 1666, the ellipses described by the ball of the conical pendulum were, of course, not eccentric.

— which, as Newton did not fail to point out,[116] was rather easy, this latter having been published by Huygens in 1673 [117] — Hooke transferred to his falling body the scheme of motion prevailing in the skies. The mention of Kepler [118] in the very letter in which the inverse square law is formulated by Hooke makes this assumption by no means improbable.[119] It is all the more probable in that it would give an explanation also of Hooke's avoidance of the simple transfer of the astronomical scheme to terrestrial phenomena. Indeed, in this same letter (an answer to Newton's letter of 13 December 1679, published by Professor Pelseneer), he writes:

> What I mentioned in my last concerning the descent within the body of the earth was but upon the supposall of such an attraction, not that I really believe there is such an attraction to the very center of the earth, but on the contrary I rather conceive that the more the body approaches the center the lesse will it be urged by the attraction, possibly somewhat like the gravitation on a pendulum or a body moved in a concave sphere where the power continually decreases the nearer the body inclines to a horizontal motion which it hath when perpendicular under the point of suspension or in the lowest point. . . . But in the celestiall motions the sun, earth, or centrall body are the cause of the attraction, and though they cannot be supposed mathematicall points yet they may be conceived as physicall, and the attraction at a considerable distance may be computed according to the former proportion as from the very center.[120]

In the celestial motions, because of the great distance that separates the attracted bodies, they can be conceived as points: but this is obviously impossible in the case of a terrestrial motion,[121] even of such an impossible motion as that of a body falling down to the center of the earth. In this case, the real variation of the attractive "urge," that becomes weaker and not stronger when the body approaches the center, should be taken into account. How? Hooke does not know, no more than he knows

[116] In his letter to Halley of 20 June 1686, postscript (W. W. Rouse Ball, *op. cit.*, p. 160), Newton says that to find the inverse square law from Kepler's third law was pretty easy and something that any mathematician could have done (and told Hooke) five years ago; "For when Hugenius had told how to find the force in all cases of circular motion, he has told 'em how to do it in this as well as in all others."

[117] In his *Horologium oscilatorium* of 1673 Huygens announced the laws of centrifugal force (which he had possessed since 1659), but without giving their demonstrations — a nasty trick played on his contemporaries, since it obliged them to find the proofs by themselves. It is well known that Newton recognized Huygens' priority and that his demonstrations — one of which he found, independently, in 1665/1666 — are quite different from those of Huygens.

[118] Hooke to Newton, 6 Jan. 1680 (W. W. Rouse Ball, *op. cit.*, p. 147): ". . . my supposition is that the attraction always is in duplicate proportion to the distance from the center reciprocall, and consequently that the velocity will be in a subduplicate [proportion] to the attraction, and consequently as Kepler supposes reciprocall to the distance." Hooke does not recognize the error committed by Kepler, which shows sufficiently well the very imperfect character of his "Theory of circular motion compounded by a direct motion and an attractive one to the center" and explains his inability to solve the problem of deducing the elliptical trajectory from the inverse square law of attraction. Cf. Halley's letter to Newton of 29 June 1686 about Sir Christopher Wren's challenge to Hooke (W. W. Rouse Ball, *op. cit.*, p. 162).

[119] It is to be mentioned, however, that Newton suggests another explanation of Hooke's discovery of the inverse square law, not improbable either: "Nor do I understand by what right he claims it [the inverse square proportion] as his own; for as Borell wrote, long before him, that by a tendency of the planets towards the sun, like that of gravity or magnetism, the planets would move in ellipses, so Bullialdus wrote that all force, respecting the sun as its centre, and depending on matter, must be reciprocally in a duplicate ratio of the distance from the centre, and used that very argument for it, by which you, sir, in the last Transactions, have proved this ratio in gravity. Now if Mr. Hooke from this general proposition in Bullialdus, might learn the proportion in gravity, why must this proportion here go for his invention?" Letter to Halley, 20 June 1680, postscript; cf. W. W. Rouse Ball, *op. cit.*, p. 160.
The passage of Bullialdus (Ismael Bouillaud) which Newton evidently has in mind is contained in his *Astronomia Philolaica*, Paris, 1645, pp. 21 sq., and his explanation of the origin of Hooke's views on gravitation seems to be confirmed by a passage of the *Posthumous Works*, London, 1705, p. 185 (dated 1682) where the inverse square law of gravitation is deduced from an analogy with the inverse square law of the intensity of illumination. I have to point out, however, that Bullialdus does not assert the inverse square law of attraction, but uses the argument reported by Newton, adding to it, moreover, the analogy between magnetic forces and light, as a refutation of Kepler's celestial mechanics.

[120] Hooke to Newton, 6 Jan. 1680; W. W. Rouse Ball, *op. cit.*, p. 147.

[121] As I have already pointed out, to discover that this was not only not impossible, but on the contrary, necessarily true, was one of the main achievements of Newton.

what the resulting line will actually be. It is not an ellipse, of course, but something resembling it.

Yet it would be very interesting and even very useful (for astronomy as well as for navigation) to know it. And once more, in his letter of 17 Jan. 1680 Hooke urges Newton to solve the problem: "It now remains to know the propriety of a curve (not circular nor concentricall) made by a central attractive power which makes the velocities of descent from the tangent line of equall straight motion at all distances in a duplicate proportion to the distances reciprocally taken.[122] I doubt not that by your excellent method you will easily find out what that curve must be, and its proprieties, and suggest a physicall reason of this proportion." [123]

I must confess that this appeal for help, backed by an honest and straightforward admission of Newton's mathematical superiority, should have met a better reception on Newton's part than it did. Newton, indeed, solved the problem, but never said a word about it to Hooke.

[122] Hooke to Newton, 17 Jan. 1680; W. W. Rouse Ball, *op. cit.*, p. 149. Hooke, once more, repeats his error: an attractive power which conforms to the inverse square law will not make "the velocities of descent from the tangent line or equall straight motion at all distances in a duplicate proportion to the distances reciprocally taken." Moreover, this proposition, if taken *verbatim*, implies the proportionality of the velocity — and not of the acceleration — to the force acting on the body.

[123] As we see, even in this pure case, Hooke does not suggest that the resulting curve will be a conic section.

How Old is the *Bergbüchlein*?

BY ANNELIESE SISCO *

IT is generally believed that the oldest treatise on mining geology ever printed in any language is a small German book known as the *Bergbüchlein*. Nobody knows exactly when this little work was first published, and it is in the hope of stimulating a search for its first edition that this article is written.

Students of the history of the arts of mining and metallurgy, or the history of technology in geenral, may have read about the *Bergbüchlein* in the Appendix of the famous Hoover translation of Agricola's *De Re Metallica* [1] or may have seen the present writer's English translation of the little work itself.[2]

Both translations contain a discussion of the various editions of the *Bergbüchlein*, and both agree in principle on the shape of its "family tree." The characteristics of this tree, without paying attention to detailed relationships, are that the stem forks not far from the ground, and that of the two tines of the fork the shorter and weaker one actually is the continuation of the stem, while the stronger one is a branch. Considerably more is known about the branch than about the stem. Expressed differently: there are two distinct series of editions of the *Bergbüchlein*; but information on the common source of these series is lacking (see Fig. 1, top).

In June 1951, the French magazine *La Nature* published an article by P.-M. Guelpa,[3] assistant to the librarian of the École Supérieure des Mines at Paris, who believes that she may have found this information. Reorganization at her library

* New York City.

[1] Herbert Clark Hoover and Lou Henry Hoover: Georgius Agricola *De Re Metallica*, London, 1912; reprinted, New York, 1950; pp. 610–612.

[2] Anneliese Sisco and Cyril Stanley Smith: *Bergwerk- und Probierbüchlein*, New York, 1949.

[3] P.-M. Guelpa: Le plus ancien livre sur les gîtes minéraux, Le Bergbüchlein, *La Nature*, June 1951, pp. 187–191.