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THE PREHISTORY OF THE *PRINCIPIA* FROM 1664 TO 1686*

by

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Since S.P. Rigaud's pioneering *Historical essay* appeared in 1838¹ there have been many, from Rouse Ball, Cajori and Beth down to I.B. Cohen, A.R. Hall, J.W. Herivel and R.S. Westfall in our own day, who have explored how Newton's *Principia* came to be.² Surely there can be nothing profoundly new to be said about its progress from first conception as an inchoate idea in its author's mind to the maturity of its first publication in 1687? No and yes. There is now a broad balance of agreement over the main stages in its evolution: one no longer set greatly awry by the nuggets of *Principia* gold still (if with decreasing frequency and size) to be sieved from Newton's papers by those willing laboriously to do the panning. Anyone not of the fraternity, however, would surely be surprised to see how much Newton scholars can still at times find to disagree upon in assessing what is now in itself known in such abundance, sometimes even at the most basic level of dating a manuscript.³ As for the changes that must now be made in the accepted account, these only slowly filter through. How often am I still asked: 'Did Newton use calculus to obtain the theorems in his *Principia*?' How, without seeming to patronize, do you lay the groundwork on which you can reply that the question is ill-formed and therefore meaningless? I will not here go into the reasons why.⁴ But I would like briefly to tell anew the tale of how Newton wrote his *Principia*, embellishing it with some of the freshnesses of insight that have come out of recent research.

Not too many years ago I could have begun my story in August 1684 when

* This combines the texts of two talks given in 1987 to commemorate the tercentenary of the publication of the first edition of the *Principia*: one a Public Lecture given on 5 May in the University of Cambridge, and the other a contribution to the Discussion Meeting held at the Royal Society on 30 June more broadly honouring the tercentenary. (The other papers from this meeting were published in *Notes and Records*, vol. 42, part 1, January 1988.)

Edmond Halley visited Newton in Cambridge to ask if he had any thoughts on how to demonstrate the accuracy of the Kepler's hypothesis that the planets orbit in ellipses about the Sun at a common focus. Newton in his old age claimed that the *Principia* rested on foundations that he had begun to lay in the middle 1660s,⁵ but there would have been no obvious way to confirm or refute this. But no longer! We may choose or not to take his word that he had already, in the winter of 1679/80, obtained a proof of what Halley sought (even though he could not reproduce it for him).⁶ But the truth of his wider assertion may plentifully be documented from his private papers.

Let me go back, therefore, some 15 years more to that *biennium mirabilissimum* of his youth which began in autumn 1664 when, not quite 22, Newton was still an undergraduate at Cambridge, and continued through to October 1666: a time when, so he himself put it* in a now universally cited recollection in old age, he was 'in the prime' of his life for 'invention', and 'minded philosophy [*lege* science] more then at any time since'. The reminiscence whose last sentence I have half-quoted is of course merely the well-honed punch-line to a long list of discoveries that he claimed to have then made⁸: one that does not, unfortunately, entirely agree with the *prima facie* witness of his original writings (many dated by him) as they would seem fairly completely to have survived. These provide unshakeable evidence that a major part of his studies in the mid-1660s centred on the mechanics of a moving body. By pursuing the notion of the 'fluxion' of a 'fluent' quantity – its rate of change in 'time', that is – he quickly penetrated far, as I can here no more than mention, into the subtleties of what to modern eyes is geometricized calculus. But what of his parallel researches into the motion of bodies, and especially his explorations of the enforced dynamic** 'motus corporum' which is the main concern of the *Principia* itself? How widely and how deeply did they range?

Herivel has published these in their entirety,⁹ so I need stress only what I see as the important. Newton's earliest thoughts on mechanics are recorded on a dozen folios in his 'Waste Book' dated between September 1664 and early the next year.¹⁰ No one would expect these to be vastly original. In fact, they mostly organize and refine the notions and axioms of motion which Descartes had set

* In an early draft of a letter to Desmaizeaux in August 1718.⁷ Newton was, however, clearly dissatisfied with what he wrote, because he has crossed the passage out in the manuscript. Only a watered-down version of its content survived into the letter later sent (if indeed one was).

** I slip already into using a *terminus technicus* which Leibniz was not to coin till the 1680s. Anachronistic though it may be, it is hard to evade employing what is now the *mot juste*, and I freely do so passim in sequel.

out in 1644 in Book 2 of his *Principia philosophiæ*. From it in particular he took what we still know as 'Newton's' First Law, decreeing that unforced motion is uniform and in a straight line.¹¹ Of his own first discoveries he was later proudest, rightly so, of his derivation (independently of its first 'inventor', Christiaan Huygens, who announced it to the world only in 1673 in appendix to his *Horologium oscillatorium*)¹² of a way of measuring the 'centrifugal' force induced outwards by uniform motion in a circle. The 'Demonstration' of the press of this 'endeavour from the center' (to use his Englishing of Descartes's Latin technical term *conatus à centro*), which Newton entered in January 1665 on the first page of his Waste Book,¹³ well manifests his growing dynamical insight, while illustrating the difficulties that he faced and somewhat crudely mastered in elaborating his variety of *ad hoc* reasonings.

Newton here supposes (I clear away the thorns and contractions befogging his text) that a body moves uniformly in an 'equilaterall polygon abcd ... [of] 6, 8, 12, 100, 1000 sides &c' – in the first instance he chooses a square, but his argument needs only slight correction in the general case¹⁴ – at whose corners it is instantly 'reflected' (bounced) from one side into the next by its collision with the circumscribing circle. Then he asserts that

If [a] ball revolves about the center n, the force by which it endeavours from [it] would beget soe much motion ... as there is in [the body] in the time that [it] moves the length of the semidiameter an.

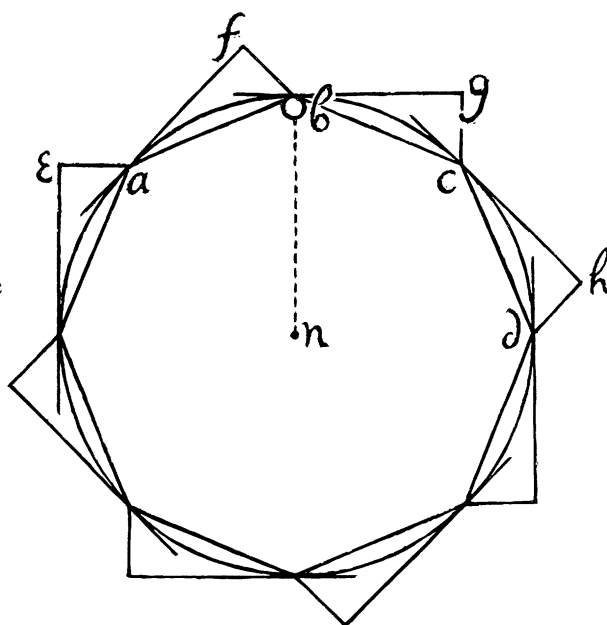
And in 'Demonstration' he argues:

If the globe [by its reflection at a] move from a to b, then
 $2f[b]^{15} : ab :: ab : [bn]$
 :: the force or
 pression of [the globe]
 upon [the circle] at its
 reflecting

: the force of its motion.

[And] soe if the body [revolve in] the sides of an equilaterall ... polygon of an infinite number of sides (i: e: ... the circle it selfe).

The subtleties here are not evident at first inspection. On taking r to be the circumradius $an = bn = cn = dn \dots$ of the polygon abcd ... let the 'globe'



travelling along it cover each equal side $ab = bc = cd = \dots = s$ in time t at uniform speed $v = s/t$. Newton supposes that when it reaches each corner a, b, c, d, \dots in turn an inwards ‘pression’, f say, to the centre n instantly ‘reflects’ it from one side to the next, altering the direction but not the magnitude of its ‘force of motion’ v . It is the *impulse ft* of the ‘pression’ which Newton compares to this ‘force’, namely via the proportion $(ft : v : :) ft^2 : s :: s : r$, whence at once $f = v^2/r$. Equally, therefore, in the limit where the polygon’s sides become infinite in number (and so infinitely small) the outward ‘endeavour from the center’ which acts upon a body constrained to move in a circle of radius r at speed v is v^2/r . Newton’s assertion is merely the corollary that the ‘motion’ – the total impetus we would say – ‘begotten’ by the ‘endeavour from the center’ acting over an arc of length r in time $t = r/v$ is $ft = v$.¹⁶

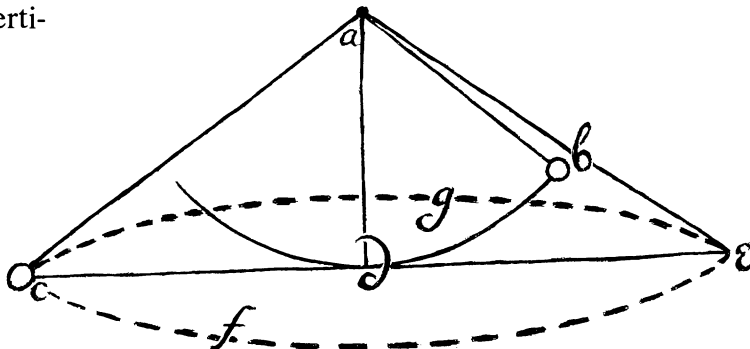
In a sentence squeezed in by him immediately afterwards he adjoins what might well seem to us obvious, observing that

If the body b moved in an Ellipsis, then its force in each point (if its motion in that point bee given) may bee found by a tangent circle of equall crookednesse with that point of the Ellipsis.¹⁷

Not only, however, was this remark original, but Newton’s path to dynamical discovery might have been very different had he pursued it. In a Waste Book entry dated December 1664 he had already roughed out a method for constructing the centre of curvature, and so the ‘quantity of crookednesse’ inverse to it, in an ellipse.¹⁸ Six years later, to jump ahead, he would in Problem 5 of his 1671 fluxions treatise derive the elegant result that the radius ρ of curvature at any point on a conic is proportional to the cube of the normal at the point, down to the axis.¹⁹ Because this (as is easily shown) varies inversely as the sine of the angle, α say, between the conic and one or other focal radius vector there, the force $f = v^2/\rho \cdot \sin \alpha$ acting through the focus to sustain motion in a conic orbit varies as $(v \cdot \sin \alpha)^2$. At once by Kepler’s area law for their motion, positing in equivalent the constancy of $rv \cdot \sin \alpha$, the planets can orbit under a force to a focus varying as the inverse-square of the distance r . But I talk of a deduction that Newton never made till the 1690s. Quickly back to January 1665.

Newton on his Waste Book folio²⁰ went on to cite ‘Galileus’ as his authority for asserting of the vertical pendulum that

If a body undulate in the circle bd , all its undulations of any altitude are performed in the same time with the same radius.



He then (again treading in the steps of Huygens had he known it)²¹ compares its period with that of a horizontally beating circular pendulum of equal height, claiming that

If *c* undulate in the circle *cgef*, to whose diameter *ce*, *ad* = *ab* being perpendicular, then will the body *b* undulate in the same time that *c* circulates.

For if the bob *c* of the conical pendulum of given length *ac* continues to beat at the same constant height *ad*, maintaining the radius *cd* of its horizontal 'circulation', the outwards thrust of the force, *f* say, of that circulation must counterbalance the downwards pull, *g*, of gravity upon it.

Whence, in Newton's words in corollary, there must be *cd* : *ad* :: the force [*f*] from *d* : the force [*g*] from *a*. Or $cd/f = ad/g$. And therefore, because the period of one circulation of *c* about the centre *d* at speed *v* is $2\pi \cdot cd/v = 2\pi \sqrt{cd/f} = 2\pi \sqrt{ad/g}$, this is indeed equal (for small vibrations) to that of the pendulum bob *b* beating to and fro under gravity in the circular arc *bd* of radius *ad*.

No less importantly, Newton observes in the same corollary,

hence may the force of gravity & the motion of things falling were they not hindered by the aire [*bee*] very exactly found.

Yet again like Huygens before him,²² he made several such calculations on this basis on the back of a vellum sheet originally used as a land lease,²³ having timed the 'ticks' (complete revolutions) of 45° conical pendulums of differing lengths. Where such a pendulum of horizontal radius *r* inches swings round *n* times an hour, its bob will cover the distance $2\pi r$ inches in $1/n$ hours at the uniform speed $v = 2\pi nr$ in/h, its swing maintained by the balance of the (here equal) forces $g = f = v^2/r$ in/h². Again, because the time *t* in which a body falls from rest through $\frac{1}{2}r = \frac{1}{2}gt^2$ inches is $\sqrt{r/g} = r/v = 1/2\pi n$ h = $3600/2\pi n$ s, its fall in the first second is $\frac{1}{2}r \cdot (2\pi/3600)^2$ inches. And so Newton computes, putting $\pi = 22/7$ as near enough. In the first instance, taking the string-length to be 81 in and so the base radius [$r = 81/\sqrt{2} =$] 57.28 in, he finds from trial that it makes [$n =$] 1512 'ticks in an hour'; whence – after correcting a shaky bit of arithmetic – he computes that a body falls from rest under gravity some '200^{inches} ... = 5^{yds}' in the first second. In sequel, finding that one with a base radius $56\frac{1}{2}$ inches makes 1500 'ticks' in 'an hower + 1/30' (62 minutes) he computes a smaller fall of not quite 197 inches. For one, lastly, of radius 57 inches 'circulating' at 1500 'ticks an hower' – suspect such integral numbers! – he calculates a lesser fall still of about $195\frac{1}{2}$ inches, but then rounds this upwards to assert that in the first second of its fall from rest 'a weighty body' covers 196 inches, or (just under) $5\frac{1}{2}$ yards.²⁴ At the outset he had cited Galileo as his authority for stating that it would fall only about '4^{cubit}. id est 3^{yds}'.

The pattern of Newton's early dynamical studies will now be clear. I will merely mention a paper of his²⁵ where half a dozen years later, having perhaps heard some report of this still unpublished discovery by Huygens,²⁶ he deftly showed even as its 'second inventor' that a cycloid is the curve in which a pendulum beats to and fro in a time independent of its arc of swing.²⁷ (He knew already that its bob is made to move in a such an arc by curving its thread along the 'cheeks' of a congruent cycloid.)²⁸

Before leaving Newton's 1665 vellum sheet, let me notice one more thing about it. At its top he writes out the calculation whereby – after removing a deep-set error – he deduces that 'the force of the Earth from its center is to the force of gravity as ... 1 : 300,²⁹ or thereabouts'. In its corrected form the computation merely applies his new-found measure of the 'endeavour from the center' in a straightforward way.³⁰ I would, however, stress that, ignoring arithmetical error, the accuracy of his value for this ratio of terrestrial gravity to the centrifugal force engendered by the Earth in its daily rotation depended crucially upon the closeness to truth of the astronomical data that he assumed in it. This leads me to the second strand that was, or so I have claimed, an essential prerequisite to Newton's writing the *Principia*.

That both the manuscript pages from which I have quoted cite Galileo – indirectly via Thomas Salusbury's 1665 English translation of his *Two new sciences*, in fact – is misleading. Was Newton's chief reason for putting his present trust in Galileo's value of 3500 miles for the Earth's radius really any more than that it coupled with the Archimedean approximation $22/7$ for π to yield a round figure of 22 000 miles for its circumference? Further to accept, as Newton here did (elsewhere he knew better), Galileo's far too low estimate of $17\frac{1}{2}$ million miles³¹ for our mean distance from the Sun – implying in turn a value of more than $40''$ for the Earth's solar parallax – could only have been for him a textbook convenience. But, if not from Galileo, from whom did he gain his working knowledge of technical astronomy? Not from studying any of the works of Kepler or the other great astronomers of the first two-thirds of the 17th century, or guided by any teacher. His surviving early writings on the topic uniformly reveal that he took his first uneasy steps in the subject by reading for himself standard textbooks of the period, long forgotten though these and their authors now are, with one exception, other than by professional historians.

But what sparked his interest in things astronomical? The appearance of a comet in late 1664, seemingly, which when he first saw it on 9 December in (we now know) its inward path to perihelion already had a 'taylor' 20° in length.³² This he again observed several times some weeks afterwards as it egressed,³³ but without realizing it was the same one. Other comets had, he knew, often appeared in the past. Where could he find out about these? He first, we know,

consulted Willebrord Snell's tract on the comet of late 1618,³⁴ to discover that many of the technical terms there used were unknown to him. A crash course in basic astronomy was clearly called for.

Of the several textbooks on the topic then in use in England two were pre-eminent: Vincent Wing's bulky *Harmonicon cæleste* and Thomas Streete's more modest *Astronomia Carolina* (in English despite their Latin titles).³⁵ The first Newton bought and lavishly annotated,³⁶ and he closely studied the latter in a copy not his own. Together they gave him a sound grounding in the working astronomy of the day. From Streete in particular he learnt of Kepler's first and third planetary 'hypotheses' (he would afterwards not a little sniffily declare that Kepler himself had only 'guessed' them);³⁷ but neither Wing nor Streete mentioned the area law on whose generalization he was to found his proof that they are exact. In lieu Newton took careful note³⁸ of a standard equant model outlined by Streete which approximately constructed the mean motion of the planets in Keplerian elliptical orbit.

But his deep interest in workaday astronomy was to endure little longer than the comet which provoked it.³⁹ Afterwards he would prefer always to go to better observers of the night sky than he could ever be, to furnish him with his astronomical data. What underlay the visible appearance of the heavens came instead to fill his mind. Would it be possible to embrace the motions of the planets and the Moon (and maybe comets too?) within one single dynamically structured scheme? It was natural that, just as he had founded his previous researches into circular motion on the second book of Descartes's *Principia philosophiæ*, he should here look to the notions set out in its third one for like guidance. And to be sure, as I have written,⁴⁰ he soon 'quickly ... familiarised himself with a universe in which *vortices* (deferent whirlpools) of matter fill space and all moving bodies are borne along in the swirl, the planets ... trapped in the solar whirlpool'. It is perhaps still not fully appreciated how much such images coloured and structured his thoughts upon astronomy during the next 15 years.

Two still not widely known examples may be cited. The lunar theory that Newton sketched out on a folio sheet once loose in his copy of Vincent Wing's *Astronomia Britannica* of 1669⁴¹ (which must therefore be the *terminus ante quem non* of its composition) is wholly expounded in terms of vortices. In particular he would there explain the inequalities of reflection and evection by having the solar vortex compress the terrestrial one.⁴² Again, more than a decade later, in a paper of the early 1680s largely devoted to discussing comets, he could still assert that 'the material of the heavens is fluid, and revolves round the centre of the cosmic system'.⁴³ However, not the least eccentric thing about comets is that many – including the one Newton first glimpsed early in December 1664 – are in retrograde motion around the Sun. This is contrary to

the 'natural vortex' of the planets, and ever remained a major worry for the Cartesian Newton.

But how was this 'natural state' of vortical motion in the heavens to be reconciled with Descartes's parallel dictate that at the surface of the Earth, as Newton had made it axiomatic,⁴⁴ 'unlesse it bee interrupted by some externall cause ... A body once moved will always keep the same celerity, quantity and determination [rectilinear direction] of its motion': a notion which, as I have outlined, was basic to his deriving a measure of Descartes's outwards 'endeavour from a center', effectively in terms of the deviation from inertial straight motion that it induces. If celestial and terrestrial motions could not directly be compared, there is an end to it. But can they? The Moon, orbiting nearly uniformly around the Earth all but in a circle, constrained therein against an outwards 'endeavour from the centre', which Newton now knew how to compute, afforded an obvious test. For the force of the 'endeavour' will be to that of gravity at the Earth's surface as the Moon's deviation from straight in a given time is to the fall of a body under gravity from rest in an equal time, which he likewise now knew. Or was it quite so simple? ...

We should not take for gospel the tales that Newton was wont to tell in his old age of how a falling fruit (be it an apple or no)⁴⁵ had led him as a young man to ponder whether all motion, in the heavens no less than on Earth, is governed by some principle of universal gravitation. Maybe so, but nothing in his own papers supports it. The notion, certainly, would have been an alien intruder in the Cartesian whorl of ideas that otherwise filled his mind. All such stories, even when told by Newton about himself, must be submitted to the usual canons of historical evidence, and when they do not pass, then they must be demoted to being mere unsupported anecdote.

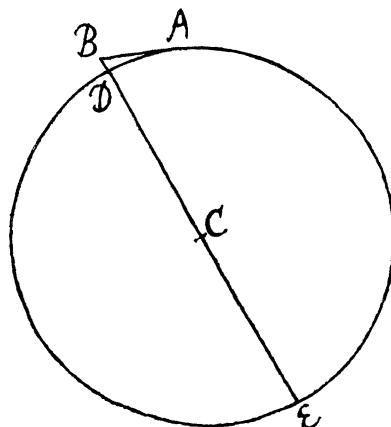
One of these, most fully recounted by Whiston,⁴⁶ has it that soon after Jean Picard published his near-exact value of slightly more than 69 miles for a degree of longitude at the Earth's equator,⁴⁷ yielding some 3960 miles for its radius, Newton used this to correct an 'old imperfect Calculation' where he had thought to compare the Moon's motion with the Earth's gravity. No such revised computation by him has survived. Whiston adds, however, of Newton's abortive earlier computation that, having there failed to show that the Moon's outwards 'endeavour' in its circular orbit about the Earth was even roughly in proportion to the pull of terrestrial gravity as the inverse-square of the lunar distance to the Earth's radius, he then began to 'suspect that the Power ... that restrained to Moon in her Orbit ... was partly that of Gravity, and partly that of Cartesius's vortices', and so 'threw aside' his calculation.⁴⁸ However, when Newton later did make such test of the Moon in his 1687 *Principia* (there taking Huygens's value of 15 Paris feet for free fall from rest in the first second at the Earth's surface), to confirm the 'precise' truth of the inverse-square law of

decrease in terrestrial gravity with distance, he had no scruple in slightly stretching the radius of the Moon's orbit to be 'virtually' 61 Earth radii.

Almost certainly, the 'imperfect Calculation' to which Whiston refers is a quarto sheet now with other early papers of his in Cambridge University Library.⁴⁹ First brought to attention by A.R. Hall in 1957, its text has been published more than once in recent years.

The measure v^2/r of the *conatus recedendi à centro* pressing outwards on a body rotating in a circle of radius $CA = CD = r$ at a constant speed v is here, however, derived by a variant argument (that used by Huygens in 1659 in fact,⁵⁰ though Newton could not have known this), which posits the arclet AD to be vanishingly small, and so too, therefore, its tangent AB ; whence the deviation

$$BD = AB^2/BE \approx AB^2/2r.^{51}$$



There was a subtlety in this argument that Newton was not to appreciate till 15 years later. In the numerical computations that follow there are several roundings-off made *en route*, one so drastic that it draws attention to itself. The data that he employs, moreover, vary a deal in their accuracy. His value of 27d7h43m 'or 27.3216 days, whose square is $746\frac{1}{2}$ ', for the Moon's period is all but exact, as nearly so is his estimate that the (mean) distance of the Moon from the Earth is about 60 of its 'semi-diameters'. What thoroughly faults his calculation is his implicit adoption yet again of Galileo's value of a mere 3500 miles (each of 5000 feet) for the Earth's radius. Though he gives only a bare outline of it, his ensuing argument is straightforward. Because a body continuously accelerated from rest by the force of the outwards 'endeavour' generated by the Earth's daily rotation would cover ($2\pi^2 =$) 19.7392 radii⁵² 'or 69087 miles' in a day, that is, some 120 miles in the first hour, or 5/9 inches in the first second; and this *conatus recedendi* is ($746\frac{1}{2}/60$, or about) $12\frac{1}{2}$ times greater than that pressing outward upon the Moon in her all but circular orbit about the Earth; then, because the Earth's outwards 'endeavour' is, taking free fall from rest to be about 16 feet in the first second, virtually 35053 times less than the pull of terrestrial gravity, it follows that the Moon's 'endeavour' to recede from the Earth is some ($12\frac{1}{2} \times 350 =$) 4375 times less than it: a figure which Newton himself with economy of truth rounded to be a broad '4000 times and more'.

Though Newton added no comment regarding this ratio,⁵⁴ he must have been disappointed that it did not more closely approach the ($60^2 =$) 3600 that an inverse-square law of decrease in the 'power' of the Earth's 'endeavour of

receding from the centre' would entail. What he does spell out in an ensuing paragraph in the manuscript is a corollary to Kepler's Third Law of planetary motion (Newton does not name it so after its author, but merely enunciates it), namely, that the cubes of the mean distances of the 'primary' planets orbiting the Sun are proportional to the squares of their periods of revolution.⁵⁵ It follows at once that the squares of their mean speeds are in inverse proportion to their mean distances, and hence on dividing through by the distances, their 'endeavours' from the Sun are (or nearly so) inversely as the squares of those distances.⁵⁶

But this is to see only the trees and not the forest. In a Cartesian vortex theory such as Newton espoused right through the 1670s all 'natural' uniform motion is implicitly explained through the supposition that the outwards 'endeavour from the center' and the contrary inwards 'force of gravity' are at every instant in balance. Where, however, this is not so, then, to anticipate a phrase Newton used to Hooke in December 1679, 'the body will circulate with an alternate ascent & descent made by its *vis centrifuga* & gravity alternatively overballancing one another'.⁵⁷ (Notice that for Newton, Huygens's now familiar equivalent name had already ousted Descartes's *conatus recedendi à centro* just six years after its inventor introduced it to the public in his 1673 *Horologium*.) But how to flesh out this intuitively acceptable notion that the radial acceleration in an orbit is compounded of an outwards pull of 'centrifugal force' directly opposed in continuous imbalance to an inwards 'centripetal' (centre-seeking) one of gravity, as Newton would himself rename it in 1684 in his tract 'De motu Corporum'? Is the 'centre-fleeing force' he here posits precisely Huygens's circular *vis centrifuga*? Or is it just a convenient tag for some other yet ill-defined outwards 'push', the only demand upon which is that it shall continuously counteract an equally unspecified inwards 'gravitation' to produce the desired elliptical orbits of the planets? If either, was the idea original with him? And, most important of all, what at any time up to the early 1680s could he have done to pluck mathematical fruit from it?

With these questions we are mostly in the realm of the unknowable past, if not in the reaches of one that never was. The notion here favoured by Newton had in fact been proposed by Giovanni Alfonso Borelli in the very year of his own *annus mirabilis* as a 'theory deduced from physical causes' of the motion of the 'Medicean' satellites about their parent planet Jupiter.⁵⁸ Though he propounded it only in words, Borelli's hypothesis* was that the non-circular

* I neglect an additional *impetus* posited by him to act instantaneously at right angles to the radius vector, so as to maintain uniform circular motion (of zero radial acceleration) about the centre of force.

orbits of the Jovian satellites ensue from compounding 'two motions directly contrary each to the other: one, perpetual and uniform, whereby the planet impelled by its own magnetic virtue moves towards the Sun's body; and a second, ... continually decreasing [outwards], whereby the planet is driven out from the Sun by the force of its circular motion'. When 20 years later, early in summer 1686, Edmond Halley reported to Newton that Robert Hooke was making 'great stir' in London by claiming to be the first to have solved the problem of planetary motion by postulating an inverse-square solar gravity, everyone knows the bitter sneer that he hurled back, angrily complaining that 'he has done nothing & yet written ... as if he knew & had sufficiently hinted all but what remained to be determined by y^e drudgery of calculations & observations, excusing himself from that labour by reason of his other business: whereas he should rather have excused himself by reason of his inability ... to go about it'.⁵⁹ But he also recalled for Halley that in contrast, long before Hooke, 'Borell ... did something in it & wrote modestly'.

This is something more than a distant memory of casually reading the copy of Borelli's book which he had in his library. But did Newton ever think seriously of mathematicizing what he calls 'Borell's Hypothesis'?

No surviving evidence indicates so. Which could just mean simply that the papers in which Newton did try to develop a coherent Borellian explanation of the motion of the planets and their satellites about their parent bodies have not survived. Had he done so, he would surely have posited that the '*vis centrifuga*' (even if he failed to see that this is merely the radial acceleration induced by the body's instantaneous inertial motion) shall ever vary inversely as the cube of the radial distance.⁶⁰ But would he have grasped the crucial necessity further to premise (no need to prove it) that Kepler's law of areas shall hold true, whatever the central force? The kiss of death to any such speculation must be his repeated insistence in his later years⁶¹ that it was not until the winter of 1679/80 that he first appreciated the fundamental role that the generalized 'Keplerian' law must play in dynamical theory. Had he done so earlier, he might already by 1670 have been wending the path that Leibniz would tread in late 1688 when he proved in his own fashion that a planet can be sustained in an elliptical orbit around the Sun at a focus under the radial action of an inverse-cube outwards 'endeavour' teetering in instant imbalance with the counter thrust of an inverse-square 'solicitation' (as he named it) of gravity.⁶²

During the 1670s in fact, with other matters scientific (and theological) occupying his mind, Newton's interest in the motion of bodies, terrestrial and celestial, died down to a smoulder. Only at the very end of the decade were its embers stirred into a new fire, one then quickly banked down for four and a half years more before at length breaking into the fierce blaze out of which the

Principia was, phoenix-like, born.

The man who poked the fire awake was, need I say it, Hooke. He in a flurry of letters to Cambridge over several weeks from late November 1679⁶³ specified to Newton the basis on which he thought it should be demonstrated that planetary motion in Keplerian ellipses implies an inverse-square force of ‘attraction’ to the Sun. Granted that he failed dismally to appreciate that this in no way provided its solution, I no less excuse Newton for his later refusal to give due credit to Hooke not merely for reawakening his interest, but for urging him to consider the enforced deviations from tangential straight which indeed paved the way to his solving the problem.

Let me cite only the salient points of the correspondence, because I have analysed it elsewhere.⁶⁴ Hooke’s reason for opening it was part of his aim, as its recently appointed Secretary,⁶⁵ to stir new life into a Royal Society moribund since the death of Henry Oldenburg two years before. So it was that he began on 24 November 1679 by requesting Newton ‘please to continue your former favours to the Society by communicating what shall occur to you that is Philosophicall’, though he made it clear that ‘For my own part I shall take it as a great favour ... particularly it you will let me know your thoughts of [my hypothesis] of compounding the celestial motions of the planetts of a direct’ – straight – ‘motion by the tangent & an attractive motion towards the centrall body’.⁶⁶ Had he added that Kepler’s area law was to be understood, he would have handed to Newton a complete skeleton solution of the problem, leaving him no more than the job of geometrically fleshing it out with pertinent theorems on the ellipse. Six years later Hooke clearly thought he had done just that.

When Newton wrote back four days later, however, he stated that ‘I did not before ... 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 sought to change the topic to the different one, so he thought, of tracking the path of a body dropped from rest at the top of a tower, to fall freely under (constant) gravity ‘towards the center of the Earth’, which ‘will not descend in the perpendicular ... but outrunning the parts of the Earth will shoot forward to the east ...’.⁶⁷ In his rough accompanying sketch – the original is so often badly reproduced – the path taken by the falling body is shown as starting tangent to the vertical before reversing its curvature to continue thereafter (so Hooke puts it) as ‘a kind of spirall which after some few revolutions [ends] in the Center of the Earth’. What Newton had not made clear is that his curve is the path of fall relative to the tower viewed as it *rotates* about the Earth’s centre. An external observer *at rest*, however, would see the body drop in a smoothly inwards spiralling ‘Elleptueid’,⁶⁸ of the kind ‘my theory’, Hooke wrote back in would-be confutation on 9 December,⁶⁹ ‘makes me suppose it’. But when it is

still rare for modern commentators to notice this nicety, we should perhaps not take Hooke too much to task for his blindness to it.

Newton was of course never one to be rebuked. In what is to me the most interesting letter of the brief correspondence, he responded on 13 December (I have already partially quoted the passage) that

I agree with you that the body ... if its gravity be supposed uniform will not descend in a spiral to the very center but circulate with an alternate ascent & descent made by it's *vis centrifuga* & gravity alternately overballancing one another. Yet I imagine the body will not describe an Ellipsœid but rather such a figure as [this]⁷⁰

And he drew a nearly trefoil path – how often since Koyré first published the letter has it confidently been depicted as one, often with praise for Hooke's superior insight here! – that, despite all the care Newton put into 'considering [its] species', much over-estimates the angles between its successive apogees. It is not so much that the mathematical techniques for summing (in Newton's words) 'according to the method of indivisibles' the 'innumerable & infinitely little' deviations of a body from tangential straight 'continually generated by ... the impresses of gravity [upon] it's passage' were not yet to his hand. Rather, as he must quickly have found, no exact solution⁷¹ of this 'simplest' case of tracing the path of free-fall under constant gravity to a finite centre is possible within (as we would say) the elementary functions at his disposal.

The imprecisions in Newton's solution apart, the subtleties that lay beneath it were certainly beyond Hooke's capacity to grasp. In his reply on 6 January 1679/80 he compliantly accepted that indeed: 'Your Calculation of the Curve [traversed] by a body attracted by an æquall power at all Distances ... is right and the two auges will not unite by about a third of a Revolution.' But then he swept the problem aside, insisting instead that

my supposition is that the Attraction always is in a duplicate proportion from the Center Reciprocall, and Consequently ... as Kepler supposes ... with such an attraction the auges will unite ... and the nearest point of accesse to the center will be opposite to the furthest Distant. Which I conceive doth very Intelligibly and truly make out all the Appearances of the Heavens ... 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 [ones] and the attraction at a Considerable Distance may be computed according to the former proportion as from the very Center.⁷²

Newton replied neither to this nor to a last letter of Hooke's 11 days later where he urged once more that it 'now remaines to know the propriety of a Curve line made by a centrall attractive power which makes the Velocity of Descent from the tangent Line or equall straight motion at all Distances in a Duplicate proportion to the Distances Reciprocally taken'.⁷³ But the challenge had been squarely put to him to frame a general theory of the motion of a body

‘attracted’ from moment to moment out of uniformly traversed tangential straight towards some given central point: one which shall, on positing that the force of attraction varies as the inverse-square of the distance, yield confirmation of Kepler’s hypothesis that the planets orbit in exact ellipses about the Sun at a focus. The breakthrough came soon after when, to cite a memorandum by de Moivre,⁷⁴ ‘he laid down this proposition that the areas described in equal times were equal, which tho[ugh] assumed by Kepler was not by him demonstrated’. With this sole addition to the guide-lines that Hooke had set, Newton passed to verify that elliptical orbits may be traversed in an inverse-square force field centred on the Sun at one or other focus;⁷⁵ and then laid his discovery aside, telling no one of it (so far as we know) for almost five years.

Although the calculation he then made has not survived – or at least he could not locate it in August 1684 when Halley asked to see it⁷⁶ – its form surely did not basically differ from the demonstration that he was to set out for Halley in November 1684 in his tract ‘De motu corporum in gyrum’, and ultimately publish as Propositions 1 and 11 of Book 1 of his *Principia* in 1687. Only once in the interim did he have any contact with the outside world on anything to do with motion, terrestrial or celestial: this for a few months from December 1680, when he corresponded with John Flamsteed about the great comet – or rather two distinct ingoing and outgoing ones (on either side of its perihelion on 8 December) as he was first inclined to believe – whose giant tail was for many weeks visible even in daytime until the early spring of 1681.⁷⁷ Interesting though Newton’s letters are, however, it would take me too far out of my way to discuss them here.

To fill in the background to what follows, let me jump ahead a moment to 22 May 1686 when Halley, who had replaced Hooke as Secretary, wrote officially to Newton on the Royal Society’s behalf to announce that ‘[the first book of] your incomparable treatise intituled *Philosophiæ Naturalis Principia Mathematica* was by Dr Vincent presented ... on the 28th [April] past’ and that a ‘Councill’ was to be ‘summon’d to consider about the printing thereof’. He took the cream off his news, however, by adjoining:

There is one thing more that I ought to informe you of, viz, that M^r Hook has some pretensions upon the invention of the rule of the decrease of Gravity being reciprocally as the squares of the distances from the Center. He sais you had the notion from him, though he owns the Demonstration of the Curves generated therby to be wholly your own, [and he] seems to expect you should make some mention of him, in the preface, which, it is possible, you may see reason to præfix.⁷⁸

Newton replied in (for him) a mild tone five days later that he wished that a ‘good understanding’ be kept between himself and Hooke, and went on to give Halley a reasonably accurate ‘summe of what past between M^r Hooke & me (to the best of my remembrance)’ in their exchange of letters some six years

before. He said that in his letter of 28 November 1679 he had 'carelessly' described the path of fall of a body to be 'in a spirall to the center of the earth: which is true in a resisting medium such as our air is'; and on 13 December 'took the simplest case for computation, ... of Gravity uniform in a medium not Resisting' and 'stated the Limit as nearly as I could ...'. But upon Hooke's claim to have been the first to suppose that the Earth's gravity 'increased in descent to the center in a reciprocally duplicate proportion, and ... that according to this duplicate proportion the motions of the planets might be explained, and their orbs defined', he would not be drawn.⁷⁹ However, he went on,

I remember about 9 years since [the date would be around Easter of 1677] S^r Christopher Wren upon a visit D^r Done and I gave him at his lodgings, discoursed of this Problem of determining the heavenly motions upon philosophical principles. This was about a year or two before I received M^r Hooks letters. You are acquainted with S^r Christopher. Pray know when & whence he first learnt the decrease of the force in a duplicate ratio of the distance from the Center.⁸⁰

Just three weeks later, in a letter to Halley of 20 June 1686 whose most celebrated passage I have already cited, he was prepared to thunder.

In order to let you know the case between M^r Hooke & me ... I am almost confident by circumstances that S^r Chr. Wren knew the duplicate proportion when I gave him a visit, & then M^r Hook will prove the last of us three that knew it. I never extended the duplicate proportion lower then to the superficies of the earth & before a certain demonstration I found the last year have suspected it did not reach accurately enough down so low.⁸¹ ...

I hope I shall not be urged to declare in print that I understood not the obvious mathematical conditions of my own Hypothesis 10 & 11 years ago ... wherein I hinted a cause of gravity in which the proportion of the decrease of gravity from the superficies of the Planet ... can be no other then reciprocally duplicate of the distance from the center. But grant I received it afterwards from M^r Hook, yet have I as great a right to it as to the Ellipsis. For as Kepler knew the [planetary] Orb to be not circular & guest it to be Elliptical, so M^r Hook without knowing what I have found out since his letters to me [in the early winter of 1679/80] can know no more but that the proportion was duplicate *quam proximè* at great distances from the center, & only guest it to be so accurately, & guest amiss in extending that proportion down to the very center.⁸²

Halley's reply on 29 June served not merely to smooth Newton's by now considerably ruffled feathers, but also sheds light on the cul-de-sac at which discussion of planetary motion at London in the early 1680s had arrived. 'According to your desire', he reported, he had indeed

waited upon S^r Christopher Wren, to inquire of him, if he had the first notion of the reciprocally proportion from M^r Hook. His answer was, that he himself very many years since had had his thoughts upon making out the Planets motions by a composition of a Descent towards the sun, & an imprest motion; but that at length

he gave over, not finding the means of doing it. Since which time M^r Hook had frequently told him that he had done it, and attempted to make it out to him, but that he never satisfied him, that his demonstrations were cogent.⁸³

But ‘this I know to be true’, he continued:

... in January 83/4, I, having from the consideration of the sesquialter proportion of Kepler concluded that the centripetall force [to the Sun] decreased in the proportion of the squares of the distances reciprocally, came one Wednesday to town, where I met with S^r Christ. Wren and M^r Hook, and falling in discourse about it, M^r Hook affirmed that upon that principle all the Laws of the celestial motions were to be demonstrated, and that he himself had done it. I declared the ill success of my attempts; and S^r Christopher to encourage the Inquiry said that he would give M^r Hook or me 2 months time to bring him a convincing demonstration thereof, and besides the honour, he of us that did it, should have from him a present of a book of 40 shillings. M^r Hook then said that he would conceale [his] for some time that other triing and failing, might know how to value it, when he should make it publick. ... I remember S^r Christopher was little satisfied that he could do it, and though M^r Hook then promised to show it him, I do not yet find that in that particular he has been as good as his word.⁸⁴

With so much commotion in London, one might have expected that someone there would have written to Cambridge’s Lucasian Professor of Mathematics to ask his opinion of the matter. Had anyone done so, of course, he would have been vastly surprised to find just how far Newton himself had gone on the way to giving the requisite ‘convincing’ proof: namely, that the three hypotheses governing the motions of the planets in their elliptical orbits round the Sun at a focus can be accounted for by positing a pull of gravity to it, drawing them from their uniformly traversed onward tangential paths, which varies inversely as the square of the distance. But no one seems to have thought to do so. And if Newton himself heard any report of what was current scientific gossip in the capital, he kept his thoughts to himself. It was, Halley proceeded to remind, only in ‘August following’ – seven months later – ‘when I did myself the honour to visit you’, that

I then learnt the good news that you had brought this demonstration to perfection, and you were pleased to promise me a copy thereof, which the November following I received with a great deal of satisfaction from M^r Paget;⁸⁵ and [I] thereupon took another Journey down to Cambridge on purpose to conferr with you about it, since which time it has been entered upon the Register Books of the Society.

To which he adjoined that ‘all this [time] past M^r Hook was acquainted with it; and according to the philosophically ambitious temper he is of, he would, had he been master of a like demonstration, no longer have conceald it, the reason he told S^r Christopher & I now ceasing...’

As a return for this reassurance Newton outlined to Halley in what way (as

he saw it) Hooke had helped him to achieve his breakthrough in the winter of 1679/80. 'This is true', he wrote back on 14 July, that

his Letters occasioned my finding the method of determining Figures, which when I had tried in the Ellipsis, I threw the calculation by being upon other studies. & so it rested for about 5 years till upon your request I sought for that paper, & not finding it did it again & reduced it into the Propositions shewed you by M^r Paget; but ... the duplicate proportion ... I gathered ... from Keplers [third] Theorem about 20 yeares ago ...⁸⁶

The 'calculation' that Newton made in late 1679, offhandedly to lay it aside because he had things more important on his mind, has long intrigued scholars. Although Herivel has claimed to have located the original, he is supported only (and then somewhat waveringly) by Westfall.⁸⁷ I myself see no need to go against the common assumption that it no longer exists. But whatever his 'calculation' was, Newton here testifies that he deduced by it only that a planet can be maintained in its elliptical orbit under the pull of inverse-square gravitation to the Sun. And everyone, even Herivel, agrees that it could only reasonably have been substantially the same as the one that he set out in revised form in the 'Propositions' which Paget bore to London for him in November 1684.

But why was Newton unable at once to reproduce this 'calculation' for Halley when he visited him in Cambridge the previous August? Although we should never rely too heavily upon unconfirmed anecdote, there is a passage in de Moivre's 1727 memorandum relating to this⁸⁸ which is so full of detail that its ultimate source can only have been Newton himself. Here it is reported that, when 'D^r Halley came to visit him at Cambridge' (only the year '1684' is stated),

... after they had been some time together, the D^r asked him what he thought the Curve would be that would be described by the Planets supposing the force of attraction towards the Sun to be reciprocal to the square of their distance from it. S^r Isaac replied immediately that it would be an Ellipsis. The Doctor struck with joy and amazement asked him how he knew it. Why saith he I have calculated it, whereupon D^r Halley asked him for his calculation without any farther delay.

When, however, Newton 'looked among his papers' he 'could not find it', but in lieu 'he promised him to renew it, & then to send it him'.

Who will blame Halley if, as he made the (then) long journey back to London, his thought was that he had met another Hooke claiming to have achieved what he had not, and now promising to supply what he could not? If so, in just three months he would find out how wrong any such hasty judgement was. For, to cite de Moivre once more, back in Cambridge

in order to make good his promise [Newton] fell to work again, [to find that he]

could not come to that conclusion which he thought he had before examined with care. However he attempted a new way which thou[gh] longer than the first, brought him again to his former conclusion, [and] then he examined carefully what might be the reason why the calculation he had undertaken before did not prove right. [H]e found that having drawn an Ellipsis coarsely [*lege* coarsely] with his own hand, he had drawn two Axes of the Curve, instead of ... two Diameters somewhat inclined to one another, whereby he might have fixed his imagination to any two conjugate diameters, which was requisite he should do. That being perceived, he made both his calculations agree together.

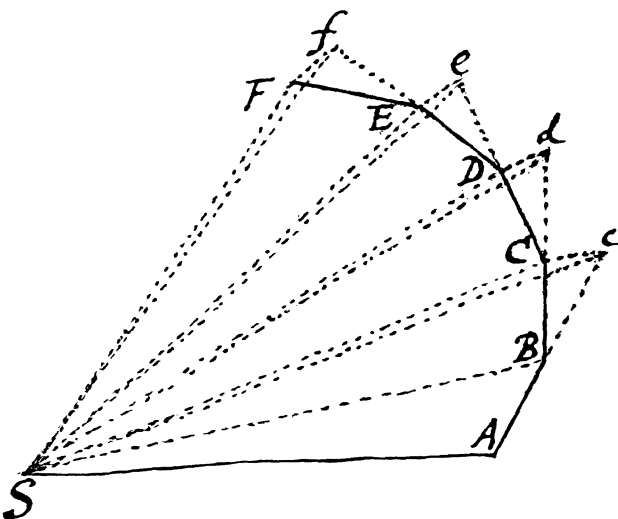
The innate plausibility of this story is in itself reason good enough to recount it anew. But if it is true, what might the variant ‘new way ... longer than the first’ have been that Newton first contrived when he was unable either to locate his original ‘calculation’ or to recover its argument on the spot for Halley? It could be that its essence survives in a simplified ‘Demonstration that the Planets by their gravity towards the Sun may move in Ellipses’ which Newton let John Locke copy in March 1690.⁸⁹ Without my further pressing the suggestion, anyone who looks at this text, and especially the lightly augmented copy of it in Newton’s own hand (here, as ever, in Cambridge University Library), should see why I make it.

The original of the ‘copy’ of Newton’s ‘perfect’ demonstration (in the hand of his newly engaged secretary Humphrey Newton, I take it) which Paget took up to London in November 1684 has disappeared since he passed it on to Flamsteed early in January 1685.⁹⁰ But before then, as Halley was to inform Newton, some unknown clerk ‘enterd’ a copy it, making many errors in his transcription, ‘upon the Register books of the [Royal] Society’, where it is still to be seen.⁹¹ If we compare this with Newton’s much reworked autograph original, titled by him ‘De motu Corporum in gyrum’ (On the motion of bodies in orbit), now in Cambridge University Library, it will be seen that the Royal Society copy, except for spelling out a ‘Hyp. 4’ which in the draft is marooned in the margin on its first page,⁹² is effectively the same text. Using the first to emend the draft, I have elsewhere⁹³ edited what is probably the nearest we shall now come to having the tract ‘De motu Corporum’ that Newton sent to London in 1684.

It is impossible here fully to recount its content. But it would make a damp finale to what has gone before if I did not give some impression of the ‘De motu’. In its opening Definition 1 – the parallel with Huygens’s notion of an outwards *vis centrifuga* surely cannot be accidental – Newton introduces his preferred technical appellation ‘centre-seeking force’ (*vis centripeta*) for that ‘whereby a body is driven or attracted towards some point regarded as its centre’. Definition 2, where he posits a ‘force innate to a body (*vis corpori insita*)’ as one ‘whereby it endeavours to persevere in its motion along a straight line’,

together with Hypothesis 2, where he postulates that 'Every body proceeds uniformly along a straight line indefinitely unless prevented by something without', are a first version of the principle of inertial motion which he later set out much more clearly in the First Law of Motion in the *Principia* to come, though we might find some initial difficulty in recognizing it. Hypothesis 3 states the then entirely novel application of the parallelogram rule to compounding force vectors. But the Second Law as we know it is no more than adumbrated in Hypothesis 4 (a last-minute addition not in the draft), which propounds that 'The space which a body under the press of any centripetal force at the very beginning of its motion describes is in the doubled ratio of the time';⁹⁴ that is, under the impact of a force, f say, at the start, a body is over a short 'moment' of time, Newton would call it o , thereafter diverted (from inertial straight) through the distance $\frac{1}{2}f.o^2$. Let me sketch how he goes on to demonstrate that the central force maintaining orbit in an ellipse round a focus is as the inverse-square of the distance from it.

Theorem 1 sets forth the crucial generalization of Kepler's area law for the planets that 'All orbiting bodies' – under any 'centripetal' force instantly diverting them from uniformly traversed straight paths inward to the centre of force, that is – 'describe by the radii drawn [from them] to the centre areas proportional to the times'. And so (I use Newton's own figure) if a body that initially sets out along AB to B, instead of then carrying straight



on to c, is at B forced towards S by an impulse which compels thereafter it to travel along BC to C, where a second impulse forces it out of its onward inertial path Cd to move along CD to D, and so on; then, because the deviation cC from BC is parallel to the direction BS of the impulse of force at B towards S, and likewise dD to CS, eE to DS, ... the triangles BSc and BSC, CSd and CSD, DSe and DSE, ... it follows, where t_{AB} is the time of passage along AB, t_{BC} that along BC, and so on, that

$$t_{AB} : t_{BC} :: AB : Bc :: \Delta ASB : (\Delta BSc =) \Delta BSC,$$

$$t_{BC} : t_{CD} :: BC : Cd :: \Delta BSC : (\Delta CSd =) \Delta CSD,$$

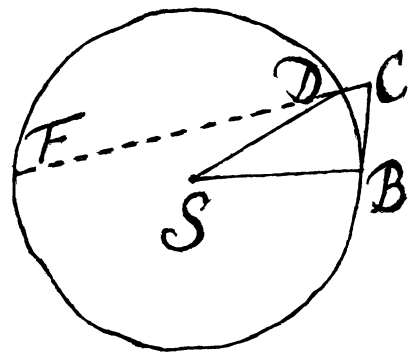
$$t_{CD} : t_{DE} :: CD : De :: \Delta CSD : (\Delta DSe =) \Delta DSE,$$

and so forth. (For convenience of future argument Newton himself puts the

times to be equal, but this is here unnecessary. At once, the times of passage of a body over successive sides AB, BC, CD, DE, ... of the chain ABCDE... are, independently of the size of the impulses of force at the corners B, C, D,... in turn which beget it, proportional to the respective triangular areas ASB, BSC, CSD, DSE, ... which the body sweeps out about the centre S of force. In the limit, then, as each of the sides of the chain comes to be vanishingly small and the chain is, under successive impulses of force, each infinitesimal in magnitude, acting at the infinity of its 'corner' points, smoothed out to be the curving arc (A)BF, the sum of the areas of the 'infinitely many', 'infinitely small' triangles BSC, CSD, ... traversed in equal times as the body orbits about the vertex S will be the sector (BSF): which therefore measures the time of orbit over the arc BF, into which a body will from instant to instant be diverted from tangential straight by a force acting towards S 'without intermission'. In the phrase now familiar, in any central-force field the time of orbit over an arc is proportional to the area swept out by the radius vector that joins the moving body to the force-centre.

But it is here too blithely said that the aggregate of the infinity of discrete infinitesimal impulses of force acting 'instantly' towards S at 'every' point of the arc BF becomes a force acting 'without break' over the arc BF 'ever' towards the centre S. How does one compare such impulses at one end-point B of the orbital arc with those at the other one F? Why was this generalized Keplerian law of areas so fundamental a breakthrough in the dynamics of central forces which Newton was so elegantly to expound in his *Principia*? And how does it fruitfully connect with Hooke's insistence in late 1679 that he should consider the deviations made from inertial tangential straight under the action of a force that Hooke had directed Newton to attend to in late 1679? The matter is much clarified if you go to the manuscript of the 'De motu Corporum' and look carefully not just at its text, but at the figures that Newton drew to illustrate it.

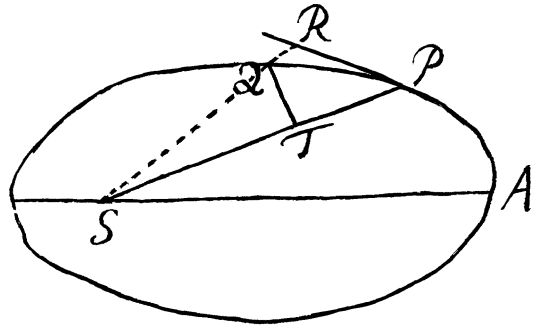
In this Theorem 1 Newton omits mention of the total deviation from initial tangential straight as a body orbits under a force acting 'without intermission' by infinitesimal impulses, one each at the 'infinity' of its 'successive' points. His argument in Theorem 2 ensuing, treating uniform motion in a circle, merely hones the argument by which he had around 1669 derived the measure v^2/r of the central force maintaining constant orbital speed v at radial distance r with one significant difference. Where a body B rotates uniformly in a circle of centre S, Newton argues:



Let the body B 'instantly' (*simul*) describe the arc BD. It would by its 'innate force' (inertially) describe the tangent equal to it. The central force perpetually draws the body back from the tangent to the circumference, and hence is as the distance CD travelled; that is, where CD is produced to [meet the circle again in] F, as BC^2/CF and so [its double] $BD^2/\frac{1}{2}CF$.

'But', he adds, 'I speak of the minutest arcs infinitely to be diminished, and so in place of $\frac{1}{2}CF$ it is allowed to write the radius SB of the circle'.⁹⁵ And therein is the crux. In Theorem 1 even the smooth arc BF engendered when the force acts 'continuously', via infinitesimal impulses 'instantly' applied at successive points, must itself be put to be infinitely small.

But what of the deviations from inertial straight when the motion is other than uniform in a circle about its centre? Newton in his Theorem 3 discusses the general case. Here (the figure is once more his, though the orbit need not of course, as he supposes in the 'De motu', be closed *in gyrum*) a body is put to move in 'any curved line' AP, from any point P of which it travels along the arc PQ to Q, being ever further driven out of the tangent line PR of initial uniform motion by a force continuously acting towards the centre S. Look closely at the deviation RQ: it is rightly depicted as curving from R, where it is parallel to PS, to Q, where it is in line with QS; such that indeed, by Theorem 1, the triangle PSR (if RS be drawn) is equal in area to the sector PSQ. It will be evident that the slope of RQ at any point r is parallel to pS, where pr is tangent to the orbit PQ at p.



Newton could now give general solution of the direct problem of central forces. By what 'centripetal' force, varying solely as the distance from the centre to which it is continually directed, can a body be made to travel in any given orbit? As in Theorem 2 it is sufficient to consider the force acting over the infinitesimal time $t = t_{PR}$ in which it would otherwise uniformly cover the tangent linelet PR, continuously diverting it therefrom towards S in the arclet PQ during the same time $t = t_{PQ}$, which by Theorem 1 is proportional to the area of the sector (PSQ) contained between PS, QS and the arc PQ. Because as Q comes to coincide with P, and so SQ with SP, the magnitude of the force acting at any point of PQ to S will be equal to that, f_{SP} say, of the force at P; and therefore the length of the deviation RQ, as it 'flattens out' into a linelet ultimately parallel to PS, will be $\frac{1}{2}f_{SP}.t^2$. Replace the vanishingly narrow sector (PSQ) by the triangle PSQ, that is, by $\frac{1}{2}SP.QT$ where QT is let fall perpendicu-

$$(BC^2/AC.PC) + (AC/PC) + (PG/VG) + (PC/CK)^2 + (QV/QX)^2 + (AC/PF)^2,$$

which, because at any point P of an ellipse there is $PF.CK = AC.BC$,⁹⁸ reduces to be the 'sum' of the ratios (PG/VG) and $(QV/QX)^2$: each of which is unity in the limit as Q, and therefore V and X, come to coincide with P.

It follows that the central force to a focus S which maintains motion in the arc PQ immediately from P, and so at every point P, varies inversely as the *latus rectum* L and the square of the distance SP. On this basis Newton went on not only to demonstrate that Kepler's Third Law holds precisely true for motion in a closed conic orbit, but in his Problem 4 to show how, given the speed and direction of motion of the orbiting body at any point P, together with the magnitude of the gravity there which ever deviates it from tangential straight to S, the elliptical path PBD that it travels thereafter may be constructed.

This sampling of the riches of the 'De motu Corporum in gyrum' must here be enough. Let me close with a rapid survey of what else came to be before the magic day in April 1687 when Halley wrote to Newton acknowledging that he had received the script of Books 2 and 3 of the *Principia*, and could at last hasten the whole into print.⁹⁹

For some weeks after the 'De motu Corporum' reached London in November 1684 only a few people, it would seem, were allowed to see it. Halley made a copy for himself (I will return to it in a moment) to attempt to master what was in it; but only after paying a second visit to Cambridge that month did he, at the meeting of the Royal Society on 10 December (or so the rather confused minute in its Journal Book records), give

... an account that he had lately seen Mr Newton ... who had shewed him a curious treatise, *De Motu*, which upon Mr Halley's desire, was, he said, promised to be sent to the Society to be entered upon their register.

And so of course it was done. But we must not think that even thereafter more than two or three – Wren, I presume, was one, but Hooke certainly was not,¹⁰⁰ and Flamsteed tried hard not to be in their circle¹⁰¹ – studied the 'De motu', either in its original secretary 'copy' or in the transcript of this then entered into the Society's Register Book. Of the handful who did see it, however, how many understood its niceties? Not even he, I have to say, who had pestered it out of Newton.

Except for slightly paraphrasing it in places, the text of Halley's copy¹⁰² mostly follows Newton's original word for word. (The five Corollaries to Theorem 2 are omitted, but probably through an oversight because they are included in the table of contents that he adjoined to it.) His redrawings of Newton's figures are something else. Halley's failure to comprehend the deeper subtleties of what was before him appears nowhere more plainly than in his 'improvement' of the diagram that Newton set to illustrate Theorem 1. In

Halley's revamping of it the chain-polygon is *already* smoothed to be an ellipse ACD... about the S[un] at a focus, but with the corner-point B 'strayed' out of orbit. All the care that he took precisely to describe this 'planetary' orbit was to no purpose when he did not appreciate that in Newton's argument the chain-polygon ABCD... ceases to be angular only in the limit as its component linelets come each to be 'infinitely' small.

It is, indeed, a notion difficult enough even today to grasp. Three centuries on, when elementary classical dynamics has become one more topic in our textbooks, we should never forget how surpassingly original Newton's tract was in its day. Even Hooke, who five years before had started him on his way to writing it, would have found little of his own wide knowledge of the motion of bodies helpful in coming to grips with the sophisticated reasonings and technicalities of the 'De motu Corporum'.¹⁰³

Already by the beginning of 1685 Newton's rush of research into the motion of bodies had outgrown any possibility that it might be contained within the pages of a small tract. During that year, he began to write a treatise 'De motu Corporum Liber primus' treating, in more than 70 propositions (because only 32 of its probable 72 folios survive we cannot be exactly sure how many),¹⁰⁴ the dynamics of free, constrained and resisted motion under the action of a central force in greater breadth than before. A companion 'De motu Corporum Liber secundus' treats, for the most part in a discursive and popular style, how the technical propositions developed in the 'Liber Primus' can be applied to explain the system of the world as conceived by Newton.¹⁰⁵ Here is not the place even broadly to appraise their content, though I itch to say much about the first in particular. Within a few short months the 'Liber primus' was yet further augmented by Newton and split into two to be Books 1 and 2 'De Motu Corporum', and the 'Liber secundus' soon after was all but wholly rewritten to be a highly technical 'De Mundi Systemate Liber Tertius', in a text¹⁰⁶ which needed only to be copied, checked, and sent up to Halley in London as it became ready for the printer. It is a matter of small importance that the transmission was made in two batches, that of the new 'De Motu Corporum Liber Primus' in April 1686, succeeded not quite a year later by the 'Liber Secundus' and the 'Liber Tertius'. The book's title of *Philosophiæ naturalis principia mathematica* was a catchpenny one contrived to 'help ye sale of ye book'¹⁰⁷ (rather than a hint at Newton's immense debt to Descartes's own *Principia philosophiæ* in forming many of his basic ideas on motion), as is well known.

When there have been numerous attempts, some in many volumes each, to assess Newton's master-work, I should not try to emulate them by giving my own sketchiest of summaries of what is in it. But there is no one who cannot be impressed by Newton's sustained attempt to achieve the utmost generality

possible. Some of the techniques that he developed remain basic in any exposition of classical 'Newtonian' dynamics. I think above all of Proposition XLI of its Book 1: one too often nowadays called after its second discoverer Johann (I) Bernoulli.¹⁰⁸ There Newton gave a complete solution of the general two-body problem, when the mass of one body is so much greater than that of the other that it may be taken to be the centre of force. I can only guess at the long hours of hard and lonely struggle that it must have taken to make all come right.

I could give you fulsome instance of my stint of the scholar's work in tracking down, or at least conjecturing the dates when Newton wrote this or that part of his *Principia*. I could say something of the role that Halley played – very much under Newton's thumb, though he made a handsome profit, by my calculation¹⁰⁹ – in seeing it through press in London. It has not been unknown for me to superimpose photo images of its two slightly variant title-pages to show precisely how they differ.¹¹⁰ But I spare you this.

On the tercentenary of the first publication in 1687 of his master work on 'The mathematical principles of science' my final words are simple:

I give you its onlie begetter: Isaac Newton !

NOTES

- 1 Stephen Peter Rigaud, *A historical essay on the first publication of Sir Isaac Newton's 'Principia'* (Oxford University Press, 1838). This rare book was reissued in 1972 in facsimile by Johnson Reprint Corporation as no. 121 in their *Sources of science* series.
- 2 Even a decade ago it was a commonplace to call the thriving research then still massively pursued into Newton's scientific papers an 'industry'. Now that what once thrived has shrivelled to be little more than a cottage one, I ought perhaps to fill in the background.

In 1888 the fifth Lord Portsmouth made over to Cambridge University the mathematical and scientific part, including many letters, of Newton's papers (from the all but complete corpus of his writings that had been in the family's ownership since the mid-18th century). In response the University published *A catalogue of the Portsmouth Collection of books and papers written by or belonging to Sir Isaac Newton, the scientific portion of which has been presented ... to the University of Cambridge*, before returning its non-scientific part. (The cataloguing was performed enthusiastically by John Couch Adams unaided except, in the respective areas of their expertise, by the Professor of Chemistry G.D. Liveing and the University Archivist H.R. Luard. The resulting *Catalogue* heavily reflects the interests and predilections of those who compiled it.) On being presented, the 15 000 or so sheets of these papers were deposited in the University Library, not as a separate 'Portsmouth Collection' which it was their right to be (and as everyone

now affectionately calls them), but unadornedly as Add[itional] MSS 3958–4006.

The papers documenting Newton's first steps in his study of dynamics and astronomy are mostly to be found in the early papers loosely packeted in Add. 3958, the pages of the notebook Add. 3996 in which he jotted a variety of entries on scientific matters in his late student and first graduate year, and above all Add. 4004, the 'Waste Book / Feb 1664[/5]' whose folios (all but blank when he inherited it from his step-father) he used as scrap paper, detaching those on which he wrote out many of his early mathematical investigations of tangency and curvature of curves. (With what papers he put them we can only conjecture because of Adams's mistaken diligence in returning these to their 'parent' volume.)

Since 1888 it has increasingly been accepted that any serious student of Newton's scientific achievement needs to be familiar with the corpus of his papers in Cambridge. Newton himself deposited in the University Library in 1674 and in the mid-1680s four sets of what he claimed were his Lucasian professorial lectures during 1670–1672 and 1673–1686 respectively. Many of the papers returned to Lord Portsmouth, only to be auctioned at Sotheby's in 1936, were bought by J.M. Keynes and are now in King's College. Newton's former College, Trinity, owns *inter alia* several of the letters exchanged between him and Hooke in 1679 and the greater part of his correspondence with Cotes in the early 1710s. Of the names mentioned, W.W. Rouse Ball was author of *An essay on Newton's 'Principia'* (London, Macmillan, 1893 → facs. repr. 1972 by Johnson Reprint). F. Cajori's name endures for his (occasionally quixotic) amended reissue of Andrew Motte's 1729 English translation of the *Principia*. H.J.E. Beth's Dutch commentary on *Newton's 'Principia'* (2 vols. Groningen, Noordhof, 1932) remains, quite unjustly, all but unknown outside Holland. Cohen, Hall, Herivel and Westfall are the names that will spring soonest to mind when anyone is asked to name those who have contributed most to deepening our understanding of the *Principia* since the renaissance in its study beginning in the mid-1950s.

- 3 For example, the differing opinions that Herivel, Westfall, Cohen and myself hold regarding the date of composition of the autograph, ULC Add. 3965.1 (now published in facsimile as Part 3 of *The preliminary manuscripts for Isaac Newton's 1687 'Principia': 1684–1685* (Cambridge University Press, 1989).
- 4 I have done so in 'The mathematical principles underlying Newton's *Principia Mathematica*', *J. Hist. Astron.* 1, 116–38 (1970); and more generally in pertinent footnotes in vol. 6 of my edition of *The mathematical papers of Isaac Newton* (Cambridge University Press, 1974).
- 5 In particular, Newton gave William Whiston (his successor as Lucasian Professor at Cambridge) and Henry Pemberton (the editor of the *Principia*'s third edition in 1726) somewhat differing versions of this. Pemberton's, published by him a year after Newton died, begins: 'The first thoughts, which gave rise to his *Principia*, he had, when he retired from Cambridge in 1666 on account of the plague. As he sat alone in a garden, he fell into a speculation on the power of gravity: that as this power is not found sensibly diminished at the remotest distance from the center of

the earth, to which we can rise, ... it appeared to him reasonable to conclude, that this power must extend much farther than was usually thought, why not as high as the moon? ...' (*A view of Sir Isaac Newton's philosophy*, (London, 1728), Preface). For Whiston's recollection of what Newton had told him 'long ago' about 'discovering his amazing Theory of Gravity' see his *Memoirs of the life and writings of Mr. William Whiston ... written by himself ...* (London, 1749 [→21753]), pp. 37–9. Both these accounts are conveniently reprinted by Rouse Ball on pages 9–11 and 8–9 respectively of his 1893 Essay (see note 2).

- 6 Already, in the 'Memorandum relating to S^r Isaac Newton' which he gave to John Conduitt in November 1727, the mathematician Abraham de Moivre records of the correspondence that ensued from Hooke's invitation to Newton 'in 1673[!]' to discourse about falling bodies, that Hooke 'took occasion to imagine' that he meant their trajectory would be 'a Spiral', and instead 'writt to him that the Curve would be an Ellipsis & that the body would move according to Kepler's notion'. Which, de Moivre went on, gave Newton 'an occasion to examine the thing thoroughly, and for the foundation of the Calculus he intended [he] laid down this proposition that the areas described in equal times were equal, which th[ough] assumed by Kepler was not by him demonstrated ...'. In the privacy of Newton's papers there are many rough drafts, particularly of prefaces to an edition of the *Principia* that he intended to bring out in the mid-1710s, where he makes the same forthright claim to discovery. Perhaps the fullest of these is ULC Add. 3968.41, 101r in a paragraph discussing the exchange of letters between Hooke and himself 'in the end of the year 1679' on the paths of falling bodies: he there readily allows that Hooke's assertion that bodies 'would not fall down to the center of the earth, but rise up again and describe an Oval as the Planets do in their orbs' was his stimulus '[t]hereupon' to compute what the planetary orbits are. 'For I had found before by the sesquialterate proportion of the *tempora periodica* of the Planets with respect to their distances from the Sun, that the forces which kept them in their Orbs about the Sun were as the squares of their mean distances reciprocally; and I found now that whatsoever was the law of the forces which kept the Planets in their Orbs, the areas described by a Radius drawn from them to the Sun would be proportional to the times in which they were described' (ibid.; I have expanded contractions).
- 7 The most finished draft of the letter, now ULC Add. 3968.27, 393r–5r, is printed in (ed. A.R. Hall) *The correspondence of Isaac Newton* vol. 6, pp. 454–7 (1976), with extensive citations of widely variant preceding ones in notes 1, 7, 8 and 15 thereto. Of these, Add. 3968.27, 389r/390r (whose plain text is given in note 15) ends with a flourish, 'And the testimony of these ... knowing & credible witnesses may suffice to e[x]cuse me for saying ... that ... in the years 1665 & 1666 I was ... in the prime of my age for invention & most intent upon mathematicks & philosophy', which is greatly expanded on the stray sheet that is now Add. 3968.41, 85. Ignorant of the latter's provenance, but unable to resist making Newton's forthright claims there public, Adams published its text on page xvii of his preface to the *Catalogue of the Portsmouth collection ...* (see note 2); whence it was reprinted by Rouse Ball in his

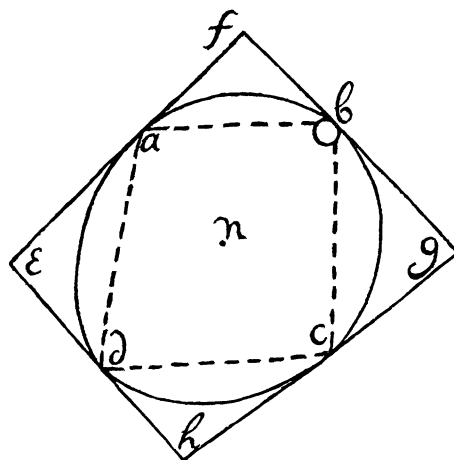
1893 *Essay* (page 7) and is now universally quoted, all but always without any *caveat* about its historical truth. In a short essay on ‘Newton’s marvellous year: 1666 and all that’ (*Notes Rec. R. Soc. Lond.* 21, 32–41 (1966)) I outlined which of Newton’s claims in his ‘Portsmouth Draft Memorandum’ (as Herivel has called it) we may accept, and which we should treat with a deal of circumspection. For a careful transcription of the manuscript text, deletions and all, see pages 291–292 of Cohen’s *Introduction to Newton’s ‘Principia’*.

- 8 As I go on to show, Newton’s assertion that he had ‘the same year’ – namely 1665 as in his preceding sentence, and not 1666 as it is always taken to be – ‘found out how to estimate the force with which [a] globe revolving within a sphere presses the surface of the sphere’ is confirmed by his yet surviving manuscript. But he advances historical truth by half a decade when he also places in ‘the two plague years of 1665 & 1666’ – when (as he wrote in draft of the phrase cited in my previous note) ‘I was in the prime of my age for invention & minded Mathematicks & Philosophy more then at any time since’ – his ensuing deduction ‘from Keplers rule of the periodical times of the Planets being in a sesquialterate proportion of their distances from the centers of their Orbs’ that ‘the forces which keep the Planets in their Orbs must [be] reciprocally as the squares of their distances from the centers about which they revolve’. As we shall see, he was unable to do the same some years afterwards, so we need take no notice of his final claim that he ‘thereby compared the force requisite to keep the Moon in her Orb with the force of gravity at the surface of the earth, and found them answer pretty nearly’.
- 9 J.W. Herivel, *The background to Newton’s ‘Principia’. A study of Newton’s dynamical researches in the years 1664–84, based on original manuscripts [principally] from the Portsmouth Collection in the Library of the University of Cambridge* (Oxford University Press, 1965 [1966]). I have checked his transcriptions against every one of Newton’s original papers. Such a close comparison inevitably turns up numerous misprints, but there are also a few non-trivial misreadings that cannot be the printer’s fault. But the book’s figures, many of them erroneous in ways not found in Newton’s originals, are its poorest feature. It remains, none the less, the unique published source for so much of what we know of Newton’s early ventures in both kinematics and dynamics.
- 10 See ULC Add. 4004, 1r, 10r–15r and 39r (+ 38v), transcribed by Herivel with commentary and footnotes on pages 129–82 of his *Background* ...
- 11 Specifically, in Axiom 100 on Add. 4004, 12r (the first of 23 on the direct and oblique impact of bodies) he laid down that ‘Every thing doth naturally persevere in that state in which it is unlesse it bee interrupted by some externall cause, hence ... A body once moved will always keepe the same celerity, quantity and determination [understand instant direction] of its motion.’ (Compare, Herivel *Background* ..., p. 153.)
- 12 To safeguard his priority and ‘protect’ his proofs Huygens gave the enunciations only of the 13 propositions of his treatise *De vi centrifuga ex motu circulari, theorematata* when he appended it (on pages 159–61) to his *Horologium oscillatorium*,

sive de motu pendulorum ad horologia aptato demonstrationes geometricæ (Paris, 1673). He had written the tract long before, however, in the autumn of 1659. Like all others who have never looked at Huygens's papers in Leiden University Library, I had assumed that the version of the *De vi centrifuga* published with full proofs in Huygens's *Opera varia posthuma* in 1703, and reprinted with corrections in his *Œuvres complètes* (22 vols. The Hague, 1888–1950) vol. 16, pp. 255–311, closely followed Huygens's original revised manuscript. But J.G. Yoder has now enlightened me, in note 2 on page 186 of her *Unrolling time. Christiaan Huygens and the mathematization of nature* (Cambridge University Press, 1988 [1989]), regarding just how much editorial 'tailoring' went on behind scenes.

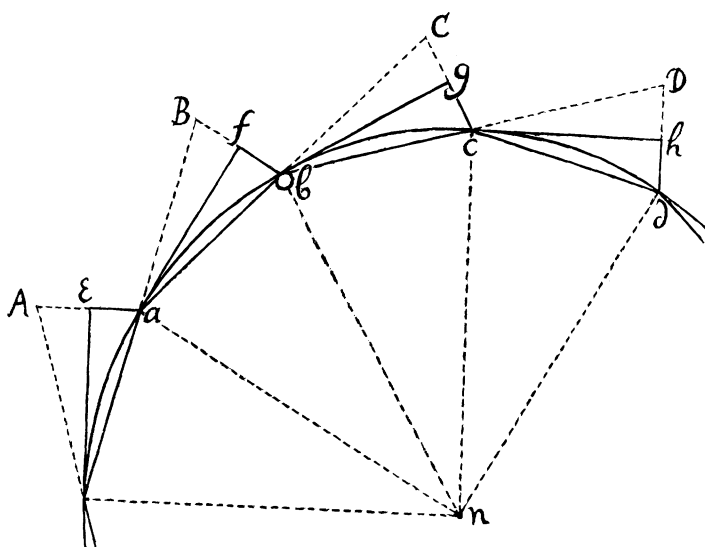
13 Add. 4004, 1r; more precisely its bottom half. Compare Herivel's *Background ...*, 129–31 and also his 'Newton's discovery of the law of centrifugal force', *Isis* 51, 546–53 (1960).

14 Where a 'body' bounces inside a circle along the square abcd, Newton quietly assumes that the impulses of 'pression' repelling it on impact at the corners a, b, c, d are parallel to fb, gc, hd, ea tangent to the circle at b, c, d, a where it next hits. This is true for no other path, but the few adjustments that I make to Newton's argument render it general. That he himself replaces the circle by the circumscribed polygon tangent at a, b, c, d is a dubious refinement which I here ignore.



15 That is, the linear deviation Bb (= Cc = Dd = ...) from the point B(C,D,...) which the body would have reached in its onward path aB(bC,cD,...) had it not been bounced back by the circle at the corner a(b,c,...).

The fuller figure here is mine, but even so let me leave it as a *verbum sapienti* (with the hint, if need be, to consider its mirror-image ...) that Newton was already come close at this moment to being able to prove the generalization



of Kepler's area law which was in the winter of 1679/80 to be his fundamental breakthrough in his researches of the motion of a body under the action of an arbitrary central force.

- 16 It follows at once that the distance $\frac{1}{2}ft^2$ through which a body falls from rest accelerated by the force of 'gravity' f is $\frac{1}{2}r$. This is Theorema V of Huygens's *De vi centrifuga*, namely, 'Si mobile in circumferentia circuli feratur ea celeritate, quam acquirit cadendo ex altitudine, quæ sit quartæ parti diametri æqualis; habebit vim centrifugam suæ gravitati æqualem' (*Horologium oscillatorium* [note 12], p. 160). Newton will make implicit use of this corollary in computing the ratio of the outwards thrust engendered at the Earth's surface by its daily rotation to the (some three hundred times greater) inward pull of its gravity.
- 17 In the transcription that he gives of this in his *Background ...*, p. 130 Herivel misreads Newton's ('yⁿ' =) 'then' as ('y^t' =) 'that'. He comments, unexceptionably, that 'Newton is already pondering' – 'adumbrating' would perhaps be the *mot juste* for what is merely an incidental observation? – 'the more difficult problem of motion in an ellipse' (see *ibid.*, p. 132, note 3).
- 18 See my edition of *The mathematical papers of Isaac Newton* (8 vols. Cambridge University Press, 1967–81) vol. 1, pp. 252–5.
- 19 *Ibid.* vol. 3, p. 158 (where the case of the ellipse is, unusually for Newton, treated as a 'Coroll.' to the hyperbola that is 'Exempl: 1' preceding).
20. Add. 4004, 1r / bottom lines; compare Herivel's *Background ...*, p. 131.
21. In his initial draft 'De vi centrifuga' in late October 1659 (*Œuvres complète* vol. 16, 309–10); compare Yoder's *Unrolling time ...*, pp. 26–7.
22. In course of writing his 'De vi centrifuga' in late October 1659; see his *Œuvres complètes* 16, p. 306, and Yoder's *Unrolling time ...*, pp. 27–32. To be more precise, Huygens began by reworking a shaky computation of Marin Mersenne in his 1647 *Reflexiones physico-mathematicæ* (compare Yoder, pp. 12–14) to determine with scarcely greater confidence a free fall of 3ft 5in from rest under gravity in the first $\frac{1}{2}$ second, and therefore 13ft 8in in the first second. (See his *Œuvres* vol. 17, p. 278.) On approximating π , as Newton after him, by its Archimedean upper bound $22/7$, he then calculated that the base radius of the 45° conical pendulum that 'ticks' exactly once a second ought to be $(2 \times 164 \times (7/44)^2)$ or 8.3in, and so its cord-length $11\frac{1}{2}$ in (See *Œuvres* vol. 16, pp. 306–7; and compare Yoder, p. 27.) Using the same value of 13ft 8in for fall in the first second, Huygens went on to calculate what we would call bench-tests which a suitably geared mechanism maintaining the pendulum's bob in motion at constant speed must satisfy. (See Yoder, pp. 27–8 and her related footnotes 27 and 28 on page 190.) Specifically, he rightly computes therefrom that the cord-lengths of the conical pendulums geared by him to complete $(12 \times 15 \times 14 \times 2 =)$ 5040 and $(12 \times 15 \times 12 \times 2 =)$ 4320 revolutions per hour need to be 6in and $8\frac{1}{2}$ in respectively.

Like Yoder, I see no reason to doubt that Huygens did in fact early in November 1659 build a working model of the improved clock-driven circular pendulum (of which only the sketch she reproduces on her page 30 survives, if it ever was more)

- by which uniform motion is maintained in a horizontal circle in equilibrium at a constant height and hence, for a given length of its cord, determines the period and vertex angle in an equivalent conical pendulum. What is certain is that on 15 November he for the first time (as Newton a few years afterwards here) established by physical trial 'ex motu conici penduli' the distance that a body covers in free-fall from rest in a given time. Having thereby arrived at the more nearly true value of 8ft 9½ in for fall in the first $\frac{3}{4}$ -second – equal, on multiplying up by $(4/3)^2$, to one of some 15.6ft in the first second – and being bucked that the latter agreed with the rounded-off one of 15ft given by Giambattista Riccioli in Lib. 9 of his *Almagestum novum* (Bologna, 1651), Huygens went through his 'De vi centrifuga' changing the '7' to be '9½' in all occurrences of the inferior figure (amended from Mersenne) of 8ft 7in which he had there initially entered for it. (See Yoder, p. 32.)
- 23 Now in the Portsmouth papers in Cambridge University Library (folios 45/46 of Add. 3958.4). Newton has there crowded into what was (except for a nasty blemish) the hitherto blank top portion, some 9 inches square, a skein of textual argument and related calculations not everywhere either immediately legible or easily sorted out. H.W. Turnbull first drew attention to the manuscript in a letter in 1953 to the *Manchester Guardian*, and a few extracts from it were subsequently published in 1958 by A.R. Hall in editing the related Latin text Add. 3958.5, 87r–88r. Turnbull himself, however, was the first who attempted a full analysis of its riches: one that appeared only after his death, in his edition of *The correspondence of Isaac Newton* vol. 3, pp. 46–54 (1961). The same year J.W. Herivel published his own independent 'Interpretation' of Newton's manuscript in *Isis* 52, 410–16, subsequently revised in his *Background ...*, pp. 183–91. R.S. Westfall contributes only a few minor honings of their findings in 'Newton and the acceleration of gravity', *Arch. Hist. exact Sci.* 35, 255–72 (1986).
- 24 More exactly, 16ft 4in of course, close to the true fall in normal atmospheric pressure. I should, however, warn the unwary against assuming that Newton's units of linear measurement are precisely our modern ones. An additional complication is that he here follows Thomas Salusbury's lead (in his 1661 translation of Galileo's *Dialogo* from which Newton elsewhere here takes several of his data) in slightly increasing the length of the English cubit, namely half a yard or 18 inches, by identifying it with the Italian *bracchia*, of which there are 3000 in a mile of 5000 feet, and which is consequently 2 in longer. Correspondingly, Newton's foot would perhaps be a little greater than ours.
- 25 Portsmouth papers ULC (Add. 3958.5. 89–91). The piece was first edited by A.R. and M.B. Hall in their *Unpublished scientific papers of Isaac Newton* (Cambridge University Press, 1962), pp. 170–180; and again by Herivel in his *Background ...*, pp. 199–203. In neither instance was it mentioned that this was but an addendum to Newton's 1671 fluxions tract, and so I yet further printed it in *The mathematical papers of Isaac Newton* vol. 3, pp. 420–31 (1969), specifying the narrowness of its ties to Example 4 of Problem 5 of the 1671 treatise (compare *ibid.*, 160–66).
- 26 When David Gregory saw this 'M.S. [quo] equiditurnitatem pendulum intra

cycloides suspensi [demonstratur]' while visiting Newton at Cambridge in 1694, he was told that it had been written 'ante editum [sc. in 1673] *Horologii oscillatorii* Hugenii'. (See *The correspondence of Isaac Newton* vol. 3, p. 331.) In agreement, the writing in the manuscript is all but identical to that in the 1671 tract. Whether or not Newton then knew of Huygens's prior discovery that a body beats to and fro in a cycloid, under constant downwards gravity, in a period independent of its swing, matters less, perhaps, than that his own researches into isochronism were of a generality to which Huygens himself began to attain only in late 1674, a year and a half after he published his *Horologium* (See his *Œuvres complètes* vol. 18, p. 489.) In the *Horologium* only kinematical arguments are used. It was Newton who first resolved the general problem of isochronous motion dynamically by laying down the criterion that the period of beat of a body in a curve whose general arc-length s is traversed in time t is independent of its arc of swing if, and only if, the motion at every point is 'simple harmonic' as we say, that is, the instantaneous deceleration, $-d^2s/dt^2$, shall vary everywhere as s . (Compare my remarks on *Mathematical papers* ... vol. 3, p. 390.) Using this criterion, the discovery that the cycloid is the isochrone under constant gravity, which Huygens laboured so hard to achieve in December 1659 (see Yoder's *Unrolling time* ..., pp. 48–61) and equally mightily to give water-tight demonstration of in his *Horologium*, took Newton (compare my next note) little more than a dozen lines to duplicate. I cannot but think that his generalized result in Proposition 53 of the *Principia*'s first book would have been beyond Huygens in 1673.

- 27 If a body moves to and fro in any curve under the continuous action of constant downwards gravity, its motion is decelerated at any point by the component of gravity acting in the instantaneous direction of motion; if the time in which it makes a full swing is independent of its length, then the deceleration in its motion must also be at every point proportional to the length of the arc from the base point. If g is the gravity, s the arc-length, t the time in which it is traversed, and y the vertical distance of the point above the base, then $g \cdot dy/ds = -d^2s/dt^2 \propto s$; from which at once $s^2 \propto y$. In the cycloid generated by a circle of radius r rolling 'upside down' along a horizontal line there is correspondingly $s = 2\sqrt{2ry}$, with the tangent $y \cdot ds/dy = \sqrt{2ry}$ drawn from any point of it to the base equal and parallel to the related chord in its generating circle.
- 28 Because the cycloid's general arc-length s is twice that of the related chord $\sqrt{2ry}$ in its generating circle, it will be equal in length to the sum of that chord and the corresponding one of the circle of same radius r which generates a congruent cycloid by rolling, upside down, along the tangent at its base. The latter cycloid will therefore be its evolute as it 'unwraps' itself from its curving arc along its tangent. This was indeed the argument used by Huygens in late 1659, though I need not stress that its simplicity was not at once evident to him. (Compare Yoder's remarks in her *Unrolling time* ..., pp. 74–75.)
- 29 Newton perhaps meant '350'; see my next note.
- 30 Taking the radius of the Earth to be 3500 miles, each of 5000 ft, and rounding down

to 16 ft the value of 196 in which he has just computed for fall from rest in the first second at its surface under gravity, Newton first calculates (taking $\pi = 22/7$) that at its equator the Earth rotates through a distance that is equal to its radius in $(24 \times 7/44 \text{ h} =) 229.09 \text{ min}$, in which time a body will fall $16 \times (229.09 \times 60)^2$ ft under gravity; but a body continually impressed by the centrifugal 'force' from the centre will (see note 16 above) fall from rest through only half the Earth's radius in that time. Accordingly, the ratio of the force of terrestrial gravity to the 'endeavour' from the Earth's centre created by its diurnal rotation is

$$16 \times 229.09^2 \times 60^2 / \frac{1}{2} \times 3500 \times 5000 \approx 345 \frac{1}{2}.$$

Huygens had made an equivalent calculation in late October 1659 (see his *Œuvres* vol. 16, p. 304; and Yoder, *Unrolling time ...*, pp. 188–189, note 17), assuming the distance of fall in the first second under gravity to be 14 (Rhenish) feet and taking on trust Willebrord Snell's estimate that there are some 342 000 such feet in 1° of longitude at the Earth's equator (equivalent to taking its radius 19 595 160 ft). From the first, were the centrifugal force engendered by the Earth's daily spin equal to the counter pull of its gravity, its radius would need to be approximately $\frac{1}{2} \times 14 \times (60^2 \times 24 / \pi)^2 \approx 5290 \times 10^6$ ft. Dividing this by Snell's value for it (some 3900 miles, only slightly inferior to the true one) yields a ratio of some 265 only. The discrepancy arises, of course, from Huygens taking too low a value for the fall in the first second, and from Newton's assuming much too short a one for the Earth's radius.

- 31 More explicitly 52 500 000 000 braces, of which there are (compare note 24) 3000 in a mile. Taking '365 $\frac{1}{4}$ days' – or 525 960 min – in a 'yeare', Newton (mistaking a '2' for a '9' in copying a draft of the calculation he made elsewhere?) multiplies by 7 to find 3 681 790, which he then divides by 44 ($\approx 7 \times 2\pi$) to find that 'The Earth in about 83 677 [min] moves the length of the solar distance'. Because the 'vis terræ a sole' engendered by the Earth's (near-circular) annual orbit about the Sun would in those 83 677 min accelerate a body from rest to cover half the radius of the Earth's orbit, it would in the first minute travel $26\,250\,000\,000/83\,677^2 \approx 3.749\dots$ braces, or some $6 \frac{1}{4}$ ft. The corresponding free-fall from rest under gravity is of course 60^2 times that in the first second, or many thousand times greater. Newton's own figure of '3749 or thereabouts' (does he just take out the decimal point' here?) is a curious 'rounding out' of 3600 as 1000.
- 32 Of this sighting by his naked eye 'at 4 of the clock in the Morning' he otherwise thought to give only its latitude and longitude. This and eight observations that he afterwards made of the comet in its egress (see the next note) were recorded by him, under the sub-heading 'Of Comets', on pages 55–57 of a 95 page section on 'Questiones quædam Philosoph[i]cæ' in a small notebook of his now in the Portsmouth papers in Cambridge University Library (Add. 3996, ff. 87r–135r, especially 114v–116r); and in an *addendum* on page 12 (*ibid.*, f. 93v) he gives, without specifying its time, a second observation made by him the next night, locating the comet only loosely by its distance 'from the center of the Moone'. The *addendum* also records an observation made by him on 17 December when, he estimated, the

comet's tail had grown to be some 34° or 35°.

These now justly famous 'Queries', drawn by Newton from his wide reading in Hobbes, Walter Charleton and above all Descartes, have been edited by J.E. McGuire and M. Tamny (with a facing modern 'translation' of Newton's archaic English) as *Certain philosophical questions* (Cambridge University Press, 1983); see pages 296–304, 350–358 and 412–416. McGuire and Tamny rightly comment (pages 298–299) that 'it is fairly clear that [in early December 1664] Newton was almost a complete novice in astronomy [but] also ... quickly recognized his deficiencies and immediately moved to correct them'. (See also their subsequent survey of 'Newton's astronomical apprenticeship: notes of 1664/5' in *Isis* 76, 349–63 (1985).)

33 Clouds and Fen fog permitting, of course, Newton's last sight of the comet, as ever without telescope to aid his naked eye, was 'On munday Jan 23^d at 8^h at night' when its tail was 'scarse discernable' ('Questiones', 57 = Add. 3996, 116r; see *Certain philosophical questions*, p. 416). Newton was not seriously to consider whether the ingoing and outgoing comets might be one and the same, moving towards and later departing from perihelion respectively, till after he began to correspond with John Flamsteed about the 'great' comet of 1680/81. J.A. Ruffner has examined this question in his doctoral thesis (University of Indiana, 1966) on 'The background and early development of Newton's theory of comets'.

34 *Descriptio cometæ quæ apparuit anno 1618* (Leyden, 1619). On page 54 of his 'Questiones' (Add. 3996, 114v = *Certain philosophical questions*, pp. 410–12) Newton has copied out not only five December sightings of the 1618 comet (see *Descriptio*, pp. 76–78), but also three dozen observations of the comet of autumn 1585 over the month from 8 October (ibid. pp. 88–89). Snell made no attempt in his tract to give any theory of comets.

35 Vincent Wing, *Harmonicon cæleste: or, The celestial harmony of the visible world ...* (London, 1651). Thomas Streete, *Astronomia Carolina: a new theory of the cælestial motions* (London, 1661).

36 I have not myself seen Newton's copy of the *Harmonicon cæleste* (now in the Butler Library at Columbia University, New York), but according to H. Zeitlinger in the Second Supplement to his *Bibliotheca Chemico-Mathematica* (London, Sotheran, 1937) it has notes, some extensive, on 46 of its pages.

37 It was, to be precise, the first only of which Newton spoke when he asserted in a letter to Halley on 20 June 1686 that, when Kepler 'knew the planetary Orb to be not circular', he 'guest it to be Elliptical'. I have no doubt that Newton would have made the same remark about Kepler's other two laws. Certainly, after he contrived his own generalized form of the area law, he gave no credit to Kepler for first formulating it in the particular instance of the planets orbiting the Sun.

38 On an earlier page (f. 30r) of the same pocket-book (now Add. 3996) in which he had, slightly earlier if I judge his handwriting correctly, set down his 'Questiones quædam philosophicæ'. Transcriptions of the passage are given on pages 123–124 of my 'Newton's early thoughts on planetary motion' in *Br. J. Hist. Sci.* 2, 117–37 (1964), and also by McGuire and Tamny, *Isis* 76, 364 (1985). Streete does not

mention that the equant hypothesis which Newton copied out had been contrived by Ismaël Boulliau in the mid-1650s (and published by him in his *Astronomiæ philolaicæ fundamenta clariùs explicata, & asserta*, (Paris, 1657)), pp. 16–17, in response to an 'impugnation' made by Seth Ward in Book 1 of his *Astronomia Geometrica ...* (Oxford, 1656) that the simple 'empty focus' equant hypothesis for constructing mean motion, which Boulliau had earlier made public in his original *Astronomia philolaicæ* (Paris, 1645), was not exact enough to be of practical use. Ward had in mind the planets, and not the Earth, whose eccentricity is small enough to render the simple equant hypothesis adequate to derive true motion in elliptical orbit from mean. Newton knew as much when (not long afterwards by the hand-writing) he set it down on the recto of a sheet at the end of his Waste Book 'paper pad' (f. 1191), there making it the foundation of an elegant (if scarcely practical) method of determining the eccentricity of the Earth's solar orbit from observations of the varying apparent diameters of the Sun's disc.

- 39 Immediately below his last sighting of the winter 1664/65 comet in late January (see note 33) he adjoined, it is true, note of three observations that he made of a second one which became visible to the naked eye a little before the following Easter ('Questiones', 57 = Add. 3996, 115r; see *Certain philosophical questions*, p. 416). In some effort to be systematic, he made each of these at 3.20 a.m. at two-day intervals, beginning 'at 20' after 3 in the morning' of 1 April.
- 40 See page 10 of my preferred version 20 years ago (I no longer cleave to its every fine detail) of what happened 'Before the 'Principia': the maturing of Newton's thoughts on dynamical astronomy' *J. Hist. Astron.* 1, 5–19, (1970).
- 41 Newton's lavishly annotated copy of Wing's folio book (published, with a page-long title, at London) is now in Trinity College, Cambridge (shelved at NQ.18.36) along with that greater part of his library presented by the Pilgrim Trust in 1944.
- 42 Specifically, by about 1/43rd of its width. In Newton's words, 'Luna defertur in Ellipsi æquabili motu circa Centrum [medii motus] ... nisi quod per [illam] compressionem vorticis impellitur versus tangentem Orbis magni: ... debes potiùs .. ad id referre lunares irregularitates quas Reflectionem at Evectionem vocant'. (Compare my 'Newton's early thoughts on planetary motion ...' [note 38], 127.)
- 43 'Materiam cælorum fluidam esse [et] circa centrum systematis cosmici secundum cursum Planetarum gyrate', to combine the second and third of 16 lemmas on cometary motion (now ULC Add. 3965.14, 613r) which he wrote around early 1681 shortly after the appearance of the 'Great Comet' of 1680/1 sparked his interest anew in the matter.
- 44 See note 11 for the source of this excerpt.
- 45 For what it is worth, the two 'best' recounters of Newton's story, his nephew-in-law John Conduitt and the antiquarian William Stukeley, in fact agree that the fruit was an apple and that Newton was 'musing in a garden' when he saw it fall. See the memorandum that Conduitt jotted down on pages 10/11 of the little green notebook now King's College, Cambridge MS Keynes 130.4, printed by R.S. Westfall in his biography *Never at rest* (Cambridge University Press, 1980), p. 154; and

Stukeley's 1752 *Memoirs of Sir Isaac Newton's Life* (ed. A. Hastings White) (London, Taylor & Francis, 1936), pp. 19–20. Tradition has it that it was during his enforced retreat to Lincolnshire in the mid-1660s because of the outbreak of plague in Cambridge that such a 'contemplative mood ... occasion'd by the fall of an apple' prompted him to ponder whether (in Stukeley's words) 'a drawing power ... like that we here call gravity ... extends its self thro' the universe'. Whatever truth there is in this – in fact Cambridge University shut down for two periods of some nine months only each from the early summers of 1665 and 1666 – we should not presume that his musing took place on the small, isolated Woolsthorpe farm where he had been born, and where his mother still lived. He was now an educated young man, all his undergraduate years at Trinity behind him, and we would not expect that he would bury himself away in the rural wilds. By his own record (see ULC Add. 4000, 14v) he was in summer 1665 with his uncle Humphrey Babington (a Fellow of Trinity College with whom he had a close relationship) at his Squire's house in Boothby, four miles or so away to the northeast. At the back of the house was an extensive orchard ... The next year in June, when the Heralds visited Grantham, he was listed as resident there. He must have visited his mother, but when and for how long is not known.

When in 1797 Edmond Turnor inherited the by then badly run-down Woolsthorpe farmhouse and its land, he found decayed but still alive at its west front a 17th-century Flower of Kent apple tree, and propagated it. Over the past two centuries a goodly trade has developed in selling scions from it, even to Newton's Cambridge college. It was also Turnor who zealously and with blind faith went on to refurbish the two-up, two-down cottage – pulling down an unsightly ramshackle barn on its south side and bricking two sun-dials into the uncovered wall, building a 'study' into the sunny southeast corner of the bedroom where he decided Newton 'must' have slept and studied, and (this in total anachronism) setting a stone plaque over the outer door in which he incised his hero's knight's arms of crossed shin bones – to be the 'Manor House' it never was of what, in Newton's time, was an insignificant hamlet without its own church. Such are our icons of scientific worship.

46

47

In the autobiographical *Memoirs* ... which he wrote in his own old age; see note 5. In his *La mesure de la terre* (Paris, 1671, reprinted without change in 1676 as the second of his *Recueil de plusieurs Traitez*). Newton, who was a regular reader of the *Philosophical Transactions*, must have at least glanced through the accounts of Picard's book which appeared there in 1675 (10, 261) and the year after (11, 591). Cajori first pointed this out in his 1928 essay, where he sought to account for 'Newton's twenty years delay in announcing the law of gravitation' *Sir Isaac Newton* ... (Baltimore, 1928), and he cites some dozen other contemporary estimates for the length of 1° at the Earth's equator, varying upwards from the 56.8 miles of J.J. Leurechon (= 'H. van Etten'); but none except Picard's (of about 69.1 miles) is greater than Snell's of some 66.9 miles. In comparison, the Galileian value of 3500 miles for the Earth's radius which (see note 24) Newton took over from Salusbury's 1661 English translation of the *Dialogo*, silently identifying Italian miles with

English ones, yields 1° at the equator to be 61 miles only. The common practice of the day made it a round 20 leagues, and so Conduitt assumed Newton took it to be in his account of how the latter stumbled in his first test of his 'musing' whether the 'power of gravity [on] earth' might 'extend' – understand decreasing inversely as the square of the distance – 'as high as the moon ... & perhaps retain her in her orbit', and 'fell a calculating ...'; but 'being absent from books [took] the common estimate in use among Geographers & our seamen ... that 60 English miles were contained in one degree of latitude on the surface of the Earth, he found that his computation did not agree with his theory'. (See King's College, Cambridge. Keynes MS 130.4. 11; the passage is quoted in full by Westfall in his *Never at rest*, p. 154.)

48 'and went to other Studies', Whiston added (*Memoirs* [note 5], p. 37). Conduitt tells all but the same tale in immediate sequel to the sentence cited in the previous note, namely, that when Newton could not make his computation exactly justify his theory, his disappointment 'inclined him then to entertain a notion that with the force of gravity there might be a mixture of that force which the Moon would have if it was carried along in a vortex'. Whiston, too, identifies the reason for Newton's failure in his taking 'a Degree of a great Circle on the Earth's Surface ... to be 60 measured Miles only, according to the gross Measures then in Use', so it is very clear that they each recount their preferred versions of a virtually identical original tale.

49 MS Add. 3958.5, 87r/v. To anticipate my next sentence, A.R. Hall printed this now widely known Latin piece of around – maybe perhaps a little before – 1670 in his discussion of 'Newton on the calculation of central forces' in *Ann. Sci.* 13, 62–71 (1957). Two years later H.W. Turnbull somewhat awkwardly placed his own version of it in his edition of *The correspondence of Isaac Newton* vol. 1, pp. 297–303 (1961). It appears in Herivel's *Background to Newton's 'Principia'*, pp. 193–195.

50 See his *Œuvres complètes* vol. 16, pp. 297–299, and compare Yoder, *Unrolling time* ... [note 12], pp. 19–22, especially 20, where Huygens's original worksheet, dated by him '21 Oct. 1659', is reproduced.

51 If f be the 'endeavour from the centre' induced by constraining a body to move uniformly in the circle that presses outwards upon it, and t be the time of its passage over the 'infinitely' small arc $AD \approx AB$, then $BD = \frac{1}{2}ft^2$; and $AB = vt$. Thus the equation $BD = AB^2/2r$ gives $f = v^2/r$. Newton himself states the equivalent result that by taking the third proportional $ADEA/DE$ to the circle's diameter DE and its circumference $ADEA$ 'I obtain the line through which the endeavour from the centre would, when constantly applied, propel a body in the time of one revolution'. The truth of this will perhaps more readily appear on setting T to be the time of one revolution; whence the distance covered (from rest) under the constant (linear) action of the *conatus* f will be $\frac{1}{2}fT^2 = (vT)^2/2r$.

52 Namely (circumference)²/diameter; see the previous note.

53 Strictly, $16 \times 12 \times 9/5 = 345.6$. By his equivalent 'vellum sheet' calculation Newton would (see note 30) have found $345\frac{1}{2}$.

54 He did, however, go on to frame an argument from it whose basis I do not properly understand. Because in a sidereal year the Moon makes (on average) $365\frac{1}{4}/27.3216 \approx 13.369$ circles about the Earth (whereas of course the latter makes but one about the Sun), it follows that the centrifugal ‘endeavour’ of the Moon from the Earth is $(13.369)^2 \approx 178.73$ times greater than that of the Earth from the Sun. ‘Whence it is agreed’ (says Newton) that the distance of the Moon from the Earth ‘ought to be’ greater than $1/178.73 = 0.00559\frac{1}{2}$ times that of the Earth from the Sun; and therefore the maximum angle subtended by the Earth–Moon distance at the Sun should be $\sin^{-1}0.00559\frac{1}{2} \approx 19'$, and so (on taking the Moon’s distance to be 60 Earth radii) the maximum solar parallax is $19''$. ‘But’, he continues, ‘put the parallax to be in fact $24''$, and the Moon’s distance from the Earth will then’ – on scaling up by $24/19$ or ‘around 5 to 4’ – ‘be $[0.00]706\frac{3}{4}$ [of the Earth’s distance from the Sun], and so the force of gravity [some] 5000 times greater than the Earth’s endeavour from the Sun’.

Where Newton found this ‘true’ value of $24''$ for solar parallax I do not know. In his ‘vellum sheet’ calculations he had (compare note 31 above) used Galileo’s value of $17\frac{1}{2}$ million miles = 5000 Earth radii for the Sun’s distance from the Earth, which is equivalent to a maximum parallax of $\sin^{-1}0.0002$ or some $41\frac{1}{4}''$. Conversely, a parallax of $24''$ implies that the Sun’s distance is $\operatorname{cosec} 24'' \approx 8594$ Earth radii or about 30 million miles.

55 ‘... in Planetis primarijs cū cubi distantiarum a Sole reciprocè sunt ut quadrati numeri periodorum in dato tempore’. On one of the sheets in his copy of Wing’s *Astronomia Britannica* Newton did spell out that ‘Est ... regula Kepleriana quod cubi diametrorum (maximarum scilicet) sunt ut quadrata temporum revolutionis’.

56 ‘[Itaque] conatus à Sole recedendi erunt ut quadrata distantiarum a Sole’, to cite Newton’s words.

57 Newton invoked the same notion a year and a half later when he put to John Flamsteed that by a like perpetual imbalance between an outwards ‘force of circular motion’ and the contrary pull of the Sun’s ‘magnetism’ a comet ‘attracted all the time of its motion [may] by this continuall attraction [be] made to fetch a compass about the Sun ... the *vis centrifuga* at [perihelion] overpowering the attraction & forcing the Comet there ... to begin to recede from [it]’. (Newton to Crompton for Flamsteed, ca. April 1681; printed in his *Correspondence* vol. 2, p. 361 (1960).)

58 *Theoricæ medicorum planetarum ex causis physicis deductæ* (Florence, 1666); Newton’s lightly dog-eared copy of it (a gift from John Collins in July 1671; see *The correspondence of Isaac Newton* vol. 1, p. 66 (1959)) is now bound in Trinity College, Cambridge. NQ.16.79. The instance of orbit in a circle about its centre (when the *vis centrifuga* and gravity are precisely in balance) had earlier been treated by Descartes in Part 3, Proposition 120 of his *Principia philosophiæ*: there it is laid down that a body can circulate in a vortex only when its gravity to the centre counters its ‘tendency’ to fly off instantaneously in the direction of its motion. But Descartes said nothing of those ‘unnatural’ motions that are not circular, and we

should continue to acclaim Borelli's originality, and indeed his daring, in asserting that the ellipticity of the orbits of the planets/Moon/Jovian satellites arises from an ever teetering imbalance between the two opposed radial forces that from instant to instant press upon them: the varying 'endeavour to recede from the centre' induced by their vortical 'circulation' about it, and their 'magnetic attraction' to the Sun/Earth/Jupiter at that centre.

59 Newton prefaced this magisterial rebuke in postscript to his letter to Halley on 20 June 1686 with the words: 'Since my writing this letter, I am told by one' – would it be Edward Paget? – 'who had it from another lately present at one of your meetings, how that Mr Hooke should there make a great stir, pretending that I had all from him, and desiring they would see that he had justice done him' (*Correspondence of Isaac Newton* vol. 2, p. 435). Though the form in which he cast it says much about Newton himself, I can see no basis in his counter-charge that Hooke had merely 'published Borell's hypothesis in his own name; and the asserting of this to himself, and completing it as his own, seems to me the ground of all the stir he makes' (ibid.).

60 Assuming – whence my next sentence – some convenient equivalent to the generalized Keplerian area law. Because it is natural here, no less for Newton when he came to write his *Principia* (see especially Proposition 41 of its Book 1) than for us three centuries on, to use polar coordinates (r, ϕ) with the centre of force as origin, the law can be formulated as the differential condition that $r^2 \cdot d\phi/dt = c$, constant, where t is the time. (Newton in fact put the constant to be Q , but I use standard present-day notation.) Because the polar equation of any straight line can be written as $r = R/\cos \phi$, where R is the perpendicular distance of the origin from the line, the radial acceleration generated by motion in any line can be derived in two steps: first,

$$\frac{dr}{dt} = \frac{c}{r^2} \frac{dr}{d\phi} = \frac{cR}{r^2} \frac{\sin\phi}{\cos^2\phi} = \frac{c \sin\phi}{R},$$

and thence

$$\frac{d^2r}{dt^2} = \frac{c}{r^2} \frac{d}{d\phi} \left(\frac{c \sin \phi}{R} \right) = \frac{c^2 \cos \phi}{r^2 R} = \frac{c^2}{r^3}.$$

That the Borellian equation of motion $d^2r/dt^2 = c^2/r^3 - f(r)$ holds true for any central force $f(r)$ acting at distance r from the centre of force may perhaps be not wholly evident. It is, however, equivalent to the other accepted measures of central force: for instance, that defining it (this Newton would do in Proposition 41 of Book 1 of the *Principia*) as that whose component in the direction of orbital motion acts to generate the instant change in orbital speed. Here, taking s to be any arc of the orbit (r, ϕ) traversed by a body in time t , and $v = ds/dt$ to be the body's speed, there comes $dv/dt = -f(r) \cdot dr/ds$, whence $f(r) = -v \cdot dv/dr = -\frac{1}{2} d(v^2)/dr$. Because $v^2 = (ds/dt)^2 = (r \cdot d\phi/dt)^2 + (dr/dt)^2 = (c/r)^2 + (dr/dt)^2$ on eliminating ϕ , there is $f(r) = -c^2 \cdot \frac{1}{2} d(r^{-2})/dr - (dt/dr) \cdot \frac{1}{2} d((dr/dt)^2)/dt$.

61 Especially in unfinished draft statements, nearly all now gathered in ULC MS Add. 2968, which he wrote with various intentions during the 'war' that he conducted

virtually single-handedly with the Leibnizians in the 1710s over his priorities of ‘invention’ in calculus. The most informative of these – allowing that Newton often wrote ‘1677’ and ‘1683’ when he meant ‘1679’ and ‘1684’ – are published by I.B. Cohen in Supplement 1 (pp. 289–98) to his *Introduction to Newton’s ‘Principia’* (Cambridge University Press, 1971). Nothing could be clearer (if we are prepared to trust him!) than his firm declaration on Add. 3968.9, 101r (*Introduction*, p. 293) that ‘In the end of the year 1679 in answer to a letter from D^f Hook ... I computed what would be the Orb in described by the Planets ... & I found now that whatsoever was the law of the forces w^{ch} kept the Planets in their Orbs, the areas described by a Radius drawn from them to the Sun would be proportional to the times in w^{ch} they are described.’

62 What in essence Leibniz did in the short ‘Tentamen de motuum cælestium causis’, which he made public in *Acta Eruditorum*, 82–6 (February 1689), with correction of a flaw in his argument made good in *ibid.* 446–51 (1706) (C.I. Gerhardt printed both texts together in his now obsolescent edition of *Leibnizens mathematischen schriften* (Halle, 1859), vol. 6, pp. 146–61), was twice to ‘geometrically differentiate’ the polar equation of an ellipse with focus as origin, $r/a = (1 - e^2)/(1 - e \cos \phi)$, where a is its semi-major axis and e its eccentricity, taking the ‘Kepler’ constant $c = r^2 \cdot d\phi/dt$ as a datum. It is nowadays merely a school exercise to deduce, first, that

$$dr/dt = -\{c/a(1 - e^2)\} e \sin \phi,$$

and thence

$$d^2r/dt^2 = -\{c^2/a(1 - e^2)r^2\} e \cos \phi = c^2/r^3 - c^2/a(1 - e^2)r^2.$$

On identifying c^2/r^3 to be, as Leibniz would have it, the outwards *conatus* generated by the *circulatio harmonica* of the vortex, the other component k/r^2 , where $k = c^2/a(1 - e^2)$, of the radial acceleration d^2r/dt^2 ‘must’ patently be the gravitational *solicitatio* which, for him, attracts the planet at every point (r, ϕ) of its orbit to the Sun sited at that focus of it which is taken as the origin of coordinates.

In his paper Leibniz states that he had not yet seen Newton’s *Principia* when he wrote his paper (during a visit to Italy in autumn 1688). Although there have been those in the past not convinced by this disclaimer, Leibniz’s mathematical argument is certainly original, and he has usually been given the benefit of any doubt (though not by Newton!). But who else but Borelli in his *Theoricæ* could have directly influenced him to interpret the second-order difference equation which he obtained as a proof that the solar planets are held in orbit by inverse-square gravity? Or so it seemed to be to Eric Aiton in the several percipient articles that he published in *Annals of Science* in the early 1960s (see 16, 65–82 (1960); 18, 31–41 (1962); 20, 111–23 (1964); and 21, 169–73 (1965)). The truth, as we now know it since Domenico Bertoloni Meli began to study Leibniz’s original papers, now in the Niedersächsische Landesbibliothek at Hanover, is far different. Leibniz *did* see a copy of Newton’s *Principia* in Italy in autumn 1688, and the elaborate notes that he then made upon it – notably upon Lemmas 1–11 and the opening Propositions of its Book 1 – were the stimulus for and foundation of the variant proof that he gave of its Proposition 11 in his 1689 ‘Tentamen’. In the Cambridge doctoral thesis

on 'The formation of Leibniz's techniques and ideas about planetary motion in the years 1688 to 1690' (soon to be published by Oxford University Press) where he wrote up his findings (with careful transcription of the relevant MSS from the Leibniz *Nachlass*), Bertoloni Meli also briefly considered to what extent he may have drawn on Borelli. He tentatively concludes (page 58) that, although their 'analysis of motion along the radius under the action of gravity and of an outwards tendency due to the rotation [of the vortex is similar,] at present evidence that Leibniz saw Borelli's *Theoricæ* is lacking, although it is not impossible that Leibniz was indirectly influenced by it. Orbital motion had already been seen as the resultant of two opposite tendencies in Descartes's *Principia Philosophiæ*, Part 3, Proposition 120' – compare note 58 – 'which Leibniz carefully studied and excerpted in the early 1680s ... it is more probable that this was Leibniz's source'.

63 Newton replied only to Hooke's first two letters, of 24 November and 9 December 1679. As he told Halley in June 1686 (see *Correspondence* vol. 2, p. 436), he 'never answered' two others from him of 6 and 17 January 1679/80.

64 See pages 131–135 of my 'Newton's early thoughts on planetary motion' [note 38].
65 More precisely, one of the two secretaries. Nehemiah Grew had earlier been delegated to deal with the Society's foreign correspondence: a post which, to be charitable, he took lightly. I find it commendable of Hooke that he approached Newton for help in his endeavour 'philosophically' to reactivate the Society, and not at all laudable of the latter that he declined to do so: even though (as Hooke could not know) he was just back from a lengthy stay in Lincolnshire, where he had nursed his mother on her death-bed and then settled her affairs.

66 See *Correspondence* vol. 2, p. 297. As Newton probably knew, Hooke had already published this notion, likewise without specifying what the variation of the central attraction with distance might be, in a 1670 Cutler lecture on 'An attempt to prove the motion of the Earth by observation', printed under the same title at London in 1674. (See R.T. Gunther, *Early science in Oxford 8: The Cutler Lectures of Robert Hooke* (Oxford, 1931).) The basis that he there adduces for motion in the heavens well show how inchoate his notions on planetary dynamics then were, supposing as he did 'That all Cælestial Bodies whatever, have an attraction or gravitating power towards their own Centers, whereby they attract not only their own parts, ... but also all the other ... Bodies ... within the sphere of their activity. ... That all bodies whatsoever that are put into a direct and simple motion, will so continue to move forward in a streight line, till they are by some other effectual powers deflected and bent into a Motion, describing a Circle, Ellipsis, or some other more compounded Curve Line. ... 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' (ibid., pp. 27–28).

67 Newton to Hooke, 28 November 1679 (*Correspondence* vol. 2, pp. 300–301).

68 In my drawing on page 132 of my 'Newton's early thoughts on planetary motion', I somewhat bloated out this everywhere convex path of inwardly spiralling fall, as seen by a stationary external observer.

- 69 See *Correspondence* vol. 2, pp. 305–306. The redrawn figure on page 133 of my ‘Newton’s early thoughts ...’ perhaps better conveys Hooke’s preferred ‘Elleptueid’ than the roughly scrawled one in the amanuensis copy which alone now survives of Hooke’s letter.
- 70 *Correspondence* vol. 2, pp. 307–308.
- 71 For an approximate one, building on Jean Pelseneer’s analysis on pages 250–253 of the article in *Isis* 12, 237–54 (1929), where he was the first to print Newton’s letter, see note 55 of my ‘Newton’s earliest thoughts ...’.
- 72 *Correspondence* vol. 2, p. 309.
- 73 *Correspondence* vol. 2, p. 313.
- 74 That which he gave to Conduitt in November of that year; see note 6.
- 75 More fully (see note 61) to quote Newton’s statement on Add. 3968.9, 101r, he there wrote (this would be about autumn 1714): ‘In the end of the year 1679 in answer to a Letter from D^r Hook, ... I computed what would be the Orb described by the Planets. For I had found before by the sesquialterate proportion of the *tempora periodica* of the Planets with respect to their distances from the Sun, that the forces w^{ch} kept them in their Orbs about the Sun were as the squares of their mean distances from the Sun reciprocally: & I found now that whatsoever was the law of the forces ..., the areas described by a Radius drawn from them to the Sun would be proportional to the times in w^{ch} they were described. And by the help of these two Propositions I found that their Orbs would be such Ellipses as Kepler had described.’
- 76 Newton would remind Halley in July 1686 that ‘when I had tried [my method] in the Ellipsis, I threw the calculation by being upon other studies. & so it rested ... till upon your request I sought for that paper, & not finding it [I] did it again ...’.
- 77 Compare my summaries in footnote to *The mathematical papers of Isaac Newton* vol. 5, pp. 524–525 and vol. 6, pp. 481–482. Here is not the place to say more.
- 78 *Correspondence of Isaac Newton* vol. 2, p. 431.
- 79 *Correspondence* vol. 2, pp. 433–434.
- 80 *Ibid.*, p. 434.
- 81 Newton implicitly cites Proposition 40 of his 1685 ‘De motu Corporum Liber primus’. The text of this, now printed in *Mathematical Papers* vol. 6, pp. 180–185, would appear virtually unaltered in his 1687 *Principia* as Proposition 71 of its Book 1. Let me comment that all who read this passage assume that it means that Newton found it difficult to prove the result (as he certainly did not). It is the unexpectedness of the result which he here insists upon, confirming the exactness of a seeming approximation, without which no one may make any accurate test of terrestrial gravity against the distant motions of the Moon and the planets.
- 82 See *Correspondence* vol. 2, pp. 435–436.
- 83 *Correspondence* vol. 2, p. 441.
- 84 *Ibid.*, p. 442.
- 85 Edward Paget, a young Fellow at Trinity College in Cambridge, had just set foot on the long path to dissipation in India which was his ultimate lot. In April 1682,

with strong support from Newton, he had been appointed Master of the Mathematical School at Christ's Hospital, at that time just to the North of St Paul's and so hard by Gresham College, the then meeting place of the Royal Society, of which he soon became a Fellow. For several years thereafter he maintained not merely his scientific interests – even as he bore Newton's 'demonstration' up to London in November 1684 he was (see Newton to Francis Aston, 23 February 1684/5 = *Correspondence* vol. 2, p. 415) himself fresh from promoting a 'designe' for establishing a 'Philosophick meeting' in Cambridge; by close contact with his Trinity colleagues. He made an ideal intermediary for Newton to entrust his 'copy' to.

86 See *Correspondence* vol. 2, pp. 444–445. What Newton had in fact 'gathered' from Kepler's 'Theorem' 20 years before was the very different conclusion that the centrifugal 'endeavours' of the planets from the Sun are in 'the [inverse] duplicate proportion' of the distances.

87 What Herivel sought to identify as the original 'calculation' made by Newton in the winter of 1679/80 (see *Arch. Int. Hist. Sci.* 14, 23–33 (1961)) is a considerably revised autograph, lacking any title, which now occupies pride of place in the box (Add. MS 3965) of his 'Principia' papers in Cambridge University Library. (Rouse Ball has printed its text, shorn of the proofs of three minor lemmas, in his 1893 *Essay* [note 2], pp. 116–120 and it is reproduced in facsimile in Part 3 of *The preliminary manuscripts for Isaac Newton's 1687 'Principia': 1684–1685* (Cambridge University Press, 1989).) When he did so, however, Herivel was seemingly unaware that the latter, except for an inserted Prop. 2, is all but identical with the text of a simplified 'Demonstration that the planets by their gravity towards the Sun may move in ellipses' which Newton let John Locke see (and may well have specially composed for him) in March 1690, shortly after they first met. (This was first printed, from the copy – now Bodleian Library, Oxford. MS Locke c.31, 101r–103r [+ 104r] – then made by Locke's secretary, by Lord Peter King in his *Life of John Locke, with extracts from his correspondence, journals and common-place books* (London, 21830) vol. 1, pp. 389–400.) This indeed demonstrates, at elaborate to tedious length, precisely the two propositions – that Kepler's area law holds true for any central force, and hence that a 'planet' may orbit in an ellipse about its 'Sun' set at a focus under the pull of inverse-square gravity to it – that Newton afterwards stated he had first derived in the winter of 1679/80. But these are also the core of what anyone would wish to know about the *Principia*, and there seems no reason to look for an origin for Newton's paper other than in Locke's requesting Newton for a simple proof of what by 1689/90 were in print as Propositions 1 and 11 of the first book of the *Principia*.

There are other reasons that seem to exclude what I may presume to call Herivel's backdating by ten years of Newton's paper. Even after A.R. and M.B. Hall (in *Arch. Int. Hist. Sci.* 16, 23–28 (1963)) first drew Herivel's notice to the Locke 'copy' – so it is now known, though it is more probably one of an antecedent version – Herivel did not alter his opinion (see *Arch. Int. Hist. Sci.* 16, 13–22 and his *Background to Newton's 'Principia'* [note 9], pp. 108–117), but rather became

- aggressively convinced that he was right in a series of scholarly scuffles which I will not tiresomely here document. However, R.S. Westfall in 1969 found the evidence in favour of his interpretation ‘impressive’ (see his note in *Arch. Int. Hist. Sci.* 22, 51–60) and came out yet more forcefully for him in *Never at rest* (Cambridge University Press, 1980), pp. 387–388, 403.
- 88 It is printed in full by I.B. Cohen in Supplement 1.7 (pp. 297–298) to his 1971 *Introduction to Newton’s ‘Principia’* [note 61].
- 89 On this amanuensis copy, and the augmented autograph version of it (now ULC. Add 3965.1) which I mention in my next sentence, see note 87.
- 90 Paget (at its author’s behest?) took a great deal of trouble to ensure that Flamsteed should read the ‘De motu Corporum in gyrum’. One can hardly say that the latter fell over himself to oblige. In the last days of 1684 he took the ‘hard weather’ then prevailing as his excuse not to travel the half dozen miles up river to the City to see it while it was still in Paget’s custody. A month later, after Paget in response sent it down to Greenwich for him to look over, he pleaded that ‘a benefice ... bestowed upon me in the meane time’ had left him no ‘leasure to peruse it yet’ (from the letters that he wrote to Newton on 5 and 25 January 1684/5; see *Correspondence* vol. 2, pp. 410, 414.) I presume that he thereafter sat on it – however, possibly could he have begun to make sense of its technicalities? – until the *Principia* appeared, and then discarded it. (It was not to be found among his papers when Baily went through them in the early 1830s.)
- 91 Most of the errors in the text (though not the crudities of its badly drawn figures) are made good in the edited version which Rigaud adjoined on pages 1–19 of the Appendix to his 1838 *Historical Essay* [note 1]. The title ‘Isaac Newtoni propositiones de motu’ there given to it is not of course Newton’s own, but derives from the entry in the Society’s Journal Book minuting the meeting on 10 December 1684 where it was ordered that a copy of the original holograph ‘copy’ brought to London by Paget should forthwith be transcribed into the Register Book. Because Rigaud was unaware that Newton’s autograph draft of his tract yet existed among his papers (then still) in the possession of the Portsmouth family, surely no one will quibble at the minimal licence that he took in attaching the title to it that he did.
- The main purpose of transcribing a copy of it, however inferior, in the Society’s Register Book was, as the Journal Book minute here specifies, ‘for the securing his invention to himself’. To that end ‘Mr Halley was desired to put Mr Newton in mind of his promise [to do so] till such time as he could be at leisure to publish it. Mr Paget was desired to join with [him].’
- 92 This posits that ‘Spatium quod corpus urgente quacunq[ue] vi centripeta ipso motus initio describit esse in duplicata ratione temporis’: needlessly so, as it is directly deducible from Theorem 1. The draft also lacks any explicit enunciation of two lemmas to which Newton appeals in the body of his argument. One of these, summing a geometrical progression, will be familiar; but the other adduces a far from immediately evident property of the ellipse (and hyperbola) with a nonchalant ‘Constat ex Conicis’.

- 93 See *The mathematical papers of Isaac Newton* vol. 6, pp. 30–91, where I give an edited version of his draft (ULC. MS Add. 3965.7, 55r–62r/62; this is now reproduced in facsimile as Part 1a of *The preliminary manuscripts for Isaac Newton's 1687 'Principia'* [note 87]), together with my English rendering of its text, additional figures and extensive footnotes.
- 94 My translation of Newton's Latin (which I have quoted in note 92).
- 95 In his Latin: 'Loquor de spatijs BD ... minutissimis inque infinitum diminuendis sic ut pro $\frac{1}{2}$ CF ... scribere liceat circulorum radios SB ...'.
- 96 This, given a complicated demonstration by Apollonius in Proposition 45 of Book 3 of his *Conics* in rider to Proposition 42 on the product of tangential intercepts, directly follows from the constancy of the sum of the two *radii vectores* drawn from any point on the ellipse to its two foci, and was so standardly proved from the late 1630s onwards.
- 97 The basic Apollonian defining 'symptom' of the ellipse with respect to conjugate diameters. (See *Conics*, Book 1, Proposition 21.)
- 98 That this is Proposition 31 of Book 7 of Apollonius's *Conics* was all but unknown in Europe until Borelli published his Latin translation of Books 5–7 (from Arabic; they do not survive in their original Greek) at Florence in 1661. The theorem had, however, been independently derived some dozen years previously by the Belgian geometer Gregory St Vincent in Proposition LXXII of Book IV of his *Opus geometricum* (Antwerp, 1647). But does Newton here refer to either of these?
- We are saved the need to conjecture, for Massimo Galuzzi has recently tracked down his source in a 'Coroll: 6' appended by him in his copy (now Trinity College, Cambridge, NQ.16.203) of van Schooten's second Latin edition of Descartes's 1637 *Geometrie* (*Geometria ... in Latinam linguam versa, & Commentariis illustratâ ... Nunc demum diligenter recognita ... multisque egregiis accessionibus exornata*, 2 vols. (Amsterdam, 1659/1661)) to Theorem XIII of the tract by Jan de Witt on *Elementa curvarum* added to its second volume. There (page 220/foot) he states, virtually in the words of his later 'De motu' Lemma, that 'Parallelogramma omnia circa datam Ellipsin descripta sunt inter se æqualia. Nam' – in de Witt's figure I – '[est] RH. CH :: BH = DA.PH = AS. ergo RH (= AY) × PH = CH × DA per Euclid: lib. 3 prop. 36 coroll' (which demonstrates that the product of the intercepts of two chords in a circle are equal).
- But what credence are we now to place in Whiston's words when (in his 1749 *Memoirs* [note 5], pp. 38–39) he lauded Newton as 'this wonderful Man [who] in Mathematicks, could sometimes see almost by Intuition, even without Demonstration, as was the Case in that famous Proposition in his *Principia*, that All Parallelograms circumscribed about the Conjugate Diameters of an Ellipsis are equal; which he told Mr Cotes he used before it had ever been demonstrated by any one'?
- 99 More precisely, Halley reported back to Newton on 5 April that 'the last part of your divine Treatise ... came to [London] yesterday sennight' (i.e. on 28 March), but he had not then 'received' it, 'having had occasion to be out of Town the last week' (*Correspondence*, vol. 2, p. 473). 'The first part', he added, 'will be finished

- within this three weeks, and considering the shortness of the third over the second, the same press that did the first will get it done so soon as the second can be finished by another press; but I find some difficulty to match the letter [typeface] justly. ... I find I shall not get the whole compleat before [the end of?] Trinity [Easter] term, when I hope to have it published' (ibid., pp. 473–474). Editors are ever optimistic. Only on 5 July 1687 was Halley able to write to Newton that 'I have at length brought your Book to an end ... [Your] last errata came just in time to be inserted' (*Correspondence*, vol. 2, p. 481). 'I will', he went on, 'present from you the books you desire to the R. Society, Mr Boyle, Mr Pagit, Mr Flamsteed and if there be any elce in town that you design to gratifie that way; and I have sent you to bestow on your friends in the University 20 Copies' (ibid.).
- 100 Nor was he on Halley's list (see the previous note) of those 'in town' to be 'gratified' with a gift copy of the *Principia* when it appeared: a most ungenerous act on Newton's part considering the prime role played in its genesis by their correspondence in the early winter of 1679/80.
- 101 See note 90 above.
- 102 Shorn of its concluding folios, what came before (now ULC. Add. 3965.7, 63, 63, 64–70) was afterwards acquired by Newton – in what circumstances I do not know, but suppose that Halley proudly showed him his 'improvement' of the 'De Motu', and then left it with him when he was duly chastened. I likewise am unaware whether it was Newton himself or the 19th-century cataloguers of the Portsmouth papers (see note 2) who set the 'copy' in its present place next to his autograph draft of it.
- 103 Had Hooke possessed a deeper grasp of the requisite mathematics, his could well have been the treatise on the dynamics of motion that Halley brought out in the mid-1680s, and Newton would perhaps have remained forgotten in his out-of-the-way Fenland University. Instead, as Patri J. Pugliese has pointed out in his recent essay on 'Hooke and the dynamics of motion in a curved path' in (ed. M. Hunter & S. Schaffer) *Robert Hooke: new studies* (Woodbridge, The Boydell Press, 1989), pp. 181–205, in an unpublished paper (now Trinity College, Cambridge. MS O.11a.1¹⁶) which he wrote in the late summer of 1685, Hooke was still not *au fait* with the general measure of a central force which Newton had set out in his 'De motu Corporum in gyrum' the previous autumn. Instead, in the single instance of determining a non-circular orbit which he there attempted, that where the central force varies directly as the distance, he could yet only effect an approximate construction of the orbit in the manner Newton had outlined to him in December 1679.
- 104 The three (of its originally some nine) gatherings in eights of the foolscap-size folios of this 'De motu Corporum Liber Primus' which now survive are all in Cambridge University Library: two, namely its ff. 9–16 /17–24, among the superseded draft sheets (now MS Dd.9.46, 24–31 /64–71 respectively) for Book 1 of the *Principia* which Newton presumed to deposit there in the late 1680s as 'lectures' of his in fulfilment of statutory obligation on him as Lucasian Professor to do so, and the

third, its ff. 41–48 (now MS Add. 3965.3, 7–14) in the scientific papers of his which Lord Portsmouth gave to the University in 1888. In their pristine state each of these (they are reproduced in photo-facsimile in *Preliminary manuscripts ...* [note 37], pp. 63–77, 79–93, 217–231) was a fair copy by his secretary Humphrey Newton; what we have are the edited, and on occasion much interlineated and otherwise augmented, revised versions of these made by Newton himself, who has also added the foliations at the top right-hand corner of each recto. I have edited (in *Mathematical papers* vol. 6, pp. 96–187) the greatest part of what yet survives of its text, but it deserves the fuller monograph to itself, which has yet to be. An evident major problem here is to know, at least in outline, what was in its missing portion. I.B. Cohen's spadework in his *Introduction* [note 61], in both his chapter 4.2 (pp. 83–92) and especially his Supplement 4 (pp. 310–321) has here opened up the way, though in one point I would disagree with his analysis. Let me, from his shoulder, just say this in summary.

Folios 1–8 were soon expanded into (the uncorrected text of) the 20 opening ones of the revise of it (see note 106) which Newton likewise deposited in the University Library as 'Lucasian lectures' of his (see Dd.9.46, 4–23, reproduced in *Preliminary manuscripts*, pp. 37–61): what is new there is pretty certainly the elaborate Scholium to the Definitions, most of the Corollaries to the 'Axiomata sive Leges Motûs' and the general Scholium thereto. 'Prop. XXII. Theor. IX' (which is Prop. XXXIII of the *Principia's* first book) breaks off, its proof scarcely begun, at the bottom of f. 31; and the latter part of 'Prop. XXXIV. Theor. XVI' (which is the *Principia's* ensuing Prop. LXIV) 'opens' in mid-sentence on f.41, continuing in sequel through Propositions XXXV–XLII (\approx Props. LXV–LXXIII) till 'Prop. XLIII Theor. XXV' (\approx Prop. LXXIV) terminates in a continuation squiggle at the end of f. 48. Propositions XXIII–XXXII on the missing ff. 33–40 can fairly certainly be identified as the sire of Propositions XXXIV–XXXVII, LVII–LXIII of the *Principia's* first book. Of what came after we know just one thing: Halley (the only one, it would seem, whom Newton allowed to read the 'Liber Primus') refers among other notes that he made on it – presumably at the latter's request – to a 'Prop. LXXII' in it, which must (see Cohen, *Introduction*, p. 317) be in essence Prop. XXII of the *Principia's* second book. We will probably never know for sure what the intervening Propositions XLIV–LXXI were, but it may be that to conjecture that their content was substantially that of Book 1, Props. LXXV–LXXVIII, LXXXIII–XCIII and of Book 2, Props. I–VII, XV–XVII, XIX–XXI in the published *Principia* does not stray too far from the truth.

105 I paraphrase Newton's Latin in preface to the highly technical 'De Mundi Systemate Liber Tertius' with which he came to replace it: 'De hoc argumento composueram Librum .. methodo populari, ut à pluribus legeretur' (*Principia*, 1687, p. 401). Two holograph texts of this original 'Liber Secundus' survive, both in Cambridge University Library: one seemingly complete (now Add. 3990) among the Portsmouth papers, and a fair copy (MS. Dd.4.18) of the first part of it which Newton deposited there, or so its marginal divisions claim, as five of his Lucasian

‘Prælect[iones]’. A year after Newton’s death in 1727, as agreed under the terms by which he came to acquire his papers, his nephew-in-law John Conduitt published the first, fuller version – the manuscript is overwritten with the printer’s pagings – as *De mundi systemate liber Isaaci Newtoni*. (The profits of this, as of all other publications of his papers in Conduitt’s lifetime, went to Newton’s money-grubbing heirs-in-law, namely his mother’s progeny by her second marriage; see *Mathematical papers* vol. 1, xx, note 12.) I.B. Cohen has outlined some of the many problems of textual interpretation and inconsistency between the various printed editions that face the scholar, both in his Introduction (pp. vii–xxii) to the reprint (London, Dawson’s, 1969) of the English translation (perhaps by Andrew Motte) which appeared the same year as the Latin *editio princeps* under the title *A treatise of the system of the world* (London, 1728); also in Chapter 4.6 (+ Supplement VI thereto) of his *Introduction* [note 61], 109–15 (+ 327–335).

The one section of this first ‘Liber Secundus’ that makes mathematical demands upon its reader is that where Newton seeks to lay out a simple and effective way of constructing the orbit of a comet from timed sightings of it (on which see *Mathematical papers*, vol. 6, pp. 482–497). There is no doubt that he was deeply dissatisfied with this: ‘In Autumn last’, he wrote to Halley in his long letter of 20 June 1686, ‘I spent two months in calculations to no purpose for want of a good method’ (*Correspondence*, vol. 2, p. 437). That alone could well have been his reason for discarding this attempt at outlining ‘in a popular way’ the riches that ensue on applying the technical theorems to the System of the [celestial] World’.

106 As with the ‘De Motu Corporum Liber Primus’ (see the previous note) from which it was in part honed, this revised treatise was initially penned in Humphrey Newton’s fair hand, then amended and where necessary further augmented by Newton himself before at length, in April 1686, it was judged ready to be sent to Halley, thereafter to become (after yet further minor changes) Book 1 of the published *Principia*. This final script, carrying the printer’s markings-up, has been in the Royal Society’s possession since at least the time of Newton’s presidency: its *varia* are denoted by *M* in I.B. Cohen’s *Isaac Newton’s ‘Philosophiæ naturalis principia mathematica’: the third edition (1726) with variant readings* (Cambridge University Press, 1972). The working draft has much more interest. Like its parent, it is, other than for its opening 20 folios (tailored to fit ff. 1–8 of the ‘Liber Primus’ which they greatly expand) and for its last four, collected in gatherings of eight folios at a time, these paginated on the top, right corner of the first recto only. Its greatest part, ff. 1–[20], 21–[28], 29–[36], 37–[44], 55(*sic!*)–[62], 63–[70], ... 95–[102] – Book 1 of the *Principia* up to (and including the first half of) Prop. LIV – is again extant as an unintended consequence of Newton lazily electing to deposit it as the purported script of his ‘best Lucasian lectures’ in the University Library, where they are now (recently rebound in this correct sequence) ff. 4–23, 24–31, 64–71, 48–55, 40–47, 88–95, 96–103, 32–39, 80–87, 72–79 of MS Dd.9.46; and its last four folios 127–[130], embracing the latter half of Prop. XCVI and its Scholium together with Props. XCVII, XCVIII and their (and Book 1’s final) Scholium, survive as ff. 425

bis/615–617 of MS Add. 3970 in the 'Portsmouth' papers. (These are reproduced in facsimile on pages 37–93, 110–215, 233–8 of *The preliminary papers ...*)

When Newton revised the initial fair copy of the new 'Liber Primus' he subdivided it into 14 *Articula* (renamed *Sectiones* in the published *Principia*). An edited version of 'Artic.' IV–X (= ff. 38–[44], 55–102) and the latter half of XIV (= ff. 127–130) will be found in *Mathematical papers*, vol. 6, pp. 230–409 and vol. 3, pp. 549–553.

107 'w^{ch} I ought not to diminish now tis yo^{rs}' Newton added in sequel in his letter to Halley on 20 June 1686 (*Correspondence*, vol.2, p.437).

108 Not least by Bernoulli himself, who must have known that his solution to the 'Problème inverse des Forces centripetes', which eventually appeared in the *Memiores de mathématique & de physique ... de l'Academie Royales des Sciences. Année M.DCC.X* (Paris, 1713), stole Newton's thunder. (Compare *Mathematical papers*, vol. 6, pp. 344–359, especially pp. 349–350 of footnote (209).) I appreciate, however, that the garb of geometrical limit-increments in which Newton clothed his reasoning may not at once be seen to be identical with the Leibnizian dress into which Bernoulli retailored it, and which could therefore, not least in its clarity, seem his independent discovery.

To continue note 60, if V be the speed at an 'initial' point (R, O) in its path of an orbiting body, and t be the time of passage thereafter to the general point (r, ϕ) , from $d(v^2)/dr = -2f(r)$ it follows that

$$v^2 = (r \cdot d\phi/dt)^2 + (dr/dt)^2 = V^2 - 2 \int_R^r f(r) \cdot dr.$$

Because $r^2 \cdot d\phi/dt = c$ ($= RV \sin \alpha$ where α is the angle which the orbit makes with the initial radius vector R), there straightforwardly ensues

$$\phi = \int_R^r \frac{c}{r^2 \sqrt{v^2 - c^2/r^2}} \cdot dr \quad \text{and} \quad t = \int_R^r \frac{1}{\sqrt{v^2 - c^2/r^2}} \cdot dr.$$

For an inverse-square law $f(r) = gR^2/r^2$, and consequently $v^2 = V^2 + 2gR^2/r - 2gR$; it follows that the only orbit that can be traversed is the conic

$$1/r = (g/V^2 \sin \alpha) \cdot \{1 - e \cos(\phi - \epsilon)\},$$

of major axis $2R/(2 - V^2/gR)$ and eccentricity $e^2 = 1 - (V^2/gR)(2 - V^2/gR) \sin^2 \alpha$.

But in his Corollary 3, almost as if to show the world what he could do with a careless flexing of his mathematical muscle, Newton himself passed to pose – and in its essence resolve – the entirely novel problem of determining the orbit in an inverse-cube force field, $f(r) = gR^3/r^3$. (Compare pp. 124–6 of my discussion of 'The mathematical principles underlying Newton's *Principia mathematica*' *J. Hist. Astron.* 1, 116–38.)

109 I do not insinuate that Halley had any mercenary purpose in mind when he encouraged Newton to write the *Principia*. No one set on making profit out of the edition would have taken anywhere near the care that he put into checking the script and overseeing it through press. In particular, upon receiving Newton's 'Liber Secundus' early in March 1687, he wrote back on 7 March (see *Correspondence*, vol. 2, p. 472) not only that he had put it out to a second printer who 'promises me

to get it done in 7 weeks' so as to have it 'finished much about the same time' as the first (which had then been in press for almost a year), but he also added that 'if [your third book *de Systemate mundi*] be likewise ready, and not too long to be got printed by the same time, ... I will endeavour by a third hand to get it all done together'. Newton hastened to comply with Halley's offer, for the latter reported back just a month later on 5 April that, even though he himself had been 'out of Town the last week' and not been there to receive it, the 'Liber Tertius' had safely arrived in London 'yesterday sennight' (on 28 March). The following three months during which the three books came sheet by sheet off the presses at separate printers must have been nightmarish. Halley certainly deserved some monetary return for all his effort, and he was too practical a man to be above taking it.

Of the 62 quarto sheets that make up the 1687 *Principia*, we have it from Halley (see *ibid.*) that his first printer – he is named on the *Principia*'s title-page to be Joseph Streater – composed the initial 30, namely (title + *Præfatio* + editorial ode =) A / (pages 1–232 =) B–I, K–T, W–Z, Aa–Ff; his 'second' – who, because of the identity of the type-face and the fluid continuity between sheets Ff and Gg, I feel sure was Streater also – (pages 233–[384→]'400' =) Gg–Ii, Kk–Uu, Ww–Zz, ***; and his last (pages 401–510 + Errata =) Aaa–Iii, Kkk–Ooo. How much he was charged for this (per sheet, then as now) I do not know. Twenty-five years later the cost of setting and correcting the relatively *de luxe* revised edition brought out in 1713 by the new-found Cambridge University Press was assessed by its printers at 10s.6d per sheet, on which they took 50% profit. (See D.F. McKenzie, *The Cambridge University Press 1696–1712. A bibliographical study* (C.U.P., 1966), vol. 1, p. 330) The paper used was of course extra to this.

In comparison with other technical works on mathematics, science and astronomy published in London in the late 17th century, many times more copies of the 1687 *Principia* are still extant. To the 163, scattered worldwide, which Henry Macomber had tracked down by mid-1952 (compare his 'Census ... of copies of the 1687 first edition ...' in *The papers of the Bibliographical Society of America* vol. 47, pp. 269–300 (1953).) I could myself add several tens more. That profusion need mean no more than that virtually every copy of a relatively small edition has survived. Though the known circumstances are not wholly against that having happened, the assumption seems unrealistic. Almost 40 years ago A.N.L. Munby, wise by long experience to the 'so many varying factors' that can determine how many copies of a book do survive, thought to hazard that 'the whole edition cannot have comprised less than three hundred copies, and the figure may well have been a hundred more ...'; see *Notes Rec. R. Soc. Lond.* 10, 28–39 (1952). We now know, however, that in England around the turn of the century runs of 500 and more copies were common for mathematical and science books. (That for the *Principia*'s second edition in 1713 was 700; again see McKenzie's *Cambridge University Press, 1696–1712* vol. 1, p. 330). I therefore see nothing outrageous in supposing that Halley ordered some 500 copies to be printed.

This figure agrees well with the price that Halley proposed to charge for the 1687

Principia. In his letter of 5 July 1687 accompanying the 20 copies of it that (see note 99) he sent Newton 'to bestow on your friends in the University', he added: 'In the same parcell you will receive 40 more, w^{ch}, having no [!] acquaintance in Cambridg, I must entreat you to put into the hands of one or more of your ablest booksellers to dispose of them: I entend the price of them bound in Calves' leather and lettered to be 9 shill here, those I send you I value in quires [sheets] at 6 shill: to take my money as they are sold, or at 5^{sh}. a price certain for ready or elce at some short time; for I am satisfied that there is no dealing in books without interesting the Booksellers, and I am contented to lett them go halves with me, rather than have your excellent work smothered by their combinations' (*Correspondence*, vol. 2, pp. 481–482, minimally corrected by the original in King's College, Cambridge. MS Keynes 97). In the absence of certain knowledge of the book's sale, let me assume that the few copies he himself managed to sell at 5 shillings cash down (I am sure that Newton, if only not to be beholden to him, would have paid him that price on the extra 40 that Halley sent along, whether he sold them or not) roughly made up for the cost of the 20 free copies that he presented to Newton and those (25 or so?) others that he himself gave away. Even by going fifty-fifty with the booksellers at 3 shillings a copy on the edition, he would have made £75 or more. Against this sum must be set Streater's charge for composing and correcting the book's 62 sheets at no more than some 15 shillings each and the cost, say £20(?), of the paper and ink used. I cannot see that he would have been less than £10 in pocket for all his time and trouble.

- 110 That there are two versions of the title-page of the 1687 *Principia* has long been known; see for instance Munby's 'The distribution of the first edition' [note 109], 28, where the variants in type-setting, but not the complete pages, are reproduced. For their greatest part they do not differ in announcing (within double rules) "PHILOSOPHIÆ / NATURALIS / PRINCIPIA / MATHEMATICA / [rule] / Autore IS. NEWTON, Trin. Coll. Cantab. Soc. Matheseos / Professore Lucasiano, & Societatis Regalis Sodali. / [rule] / IMPRIMATUR [!] / S. PEPYS, Reg. Soc. PRÆSES. / Julii 5. 1686'. After a further rule, however, the original title concluded: 'LONDINI, / Jussu Societatis Regiæ ac Typis Josephi Streater. Prostat apud / plures Bibliopolas. Anno MDCLXXXVII'. In perhaps a quarter of the known copies, this original has been cut out in favour of a replacement, pasted on its stub, which ends: 'L[!]ONDINI, / Jussu Societatis Regiæ ac Typis Josephi Streater. Prostant Vena- / les apud Sam. Smith ad insignia Principis Walliæ in Cæmiterio / D. Pauli, aliosq; nonnullos Bibliopolas. Anno MDCLXXXVII'. By fluke, the two appear, one each, in the two facsimiles of the 1687 *Principia* that have been published in recent years: the first in that by Éditions culture et civilisation (Brussels, 1965), and the latter in the one published by Dawson's (London, 1953).