Springer

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Source: *Archive for History of Exact Sciences*, Vol. 66, No. 3 (May 2012), pp. 241-264

Published by: Springer

Stable URL: https://www.jstor.org/stable/41472232

Arch. Hist. Exact Sci. (2012) 66:241–264
DOI 10.1007/s00407-012-0093-x

Newton's *De gravitatione*: a review and reassessment

J. A. Ruffner

Received: 9 January 2012 / Published online: 23 March 2012

Abstract The widely accepted supposition that Newton's *De gravitatione* was written in 1684/5 just before composing the *Principia* is examined. The basis for this determination has serious difficulties starting with the failure to examine the numerical estimates for the resistance of aether. The estimated range is not nearly nil as claimed but comparable with air at or near the earth's surface. Moreover, the evidence provided most likely stems from experiments by Boyle, Hooke, and others in the 1660s and does not use evidence available in the late 1684. The document supports Newton's contention that the aether medium incorporates very large voids thereby proving that body and space differ but does by no means completely reject its corporeal nature or eliminate its resistance. Newton's use of the term *inertia* provides no conclusive evidence for a late date as often claimed and his definition of *gravitas* is difficult to reconcile with a late one.

1 Introduction

Henry (2011) has brought the debate about the date of Newton's untitled manuscript familiarly known as *De gravitatione* full circle. Its date for a long time was believed by most scholars to be the late 1660s or the early 1670s. Works since the 1980s culminating in Dobbs (1991) built a case for its date to be from around January 1684/5 during the early stages of formulating the *Principia*. Many scholars follow her lead, placing its date more vaguely around the early or mid-1680s, although presumably still in association with Newton's masterpiece. Henry shows such circumstances to be

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unlikely, favoring a date around 1668 as originally offered by Hall and Hall (1962). This study supports no particular date but strengthens the case for a date prior to Halley's epoch-making visit of August 1684. Some background is needed.

The opening sentence states *De Gravitatione et aequipondio fluidorum et solidorum in fluidis scientiam duplici methodo tradere convenit* (It is appropriate to relate the knowledge of the heaviness and equilibrium of fluids and of solid bodies in fluids by a twofold method). The document starts and ends as an unfinished tract on hydrostatics.[1] After an introduction and four definitions, on page 2 Newton launches a 29-page anti-Cartesian metaphysical digression before returning inconclusively to the topic for 10 more pages. Gravitation, in accordance with the subsequent definition of *gravitas*, is used in the sense of weight or endeavor to press down by a body's "imparted" force (*vis corpori indita*). The *vis indita* of a fluid presses in any direction not just down toward the center of the earth. The document provides the first known uses of the term *inertia* to denote the natural endeavor of a body to maintain its state of motion or rest as well as the term *quantitates materiae* in relation to density. The twofold method frames abstract propositions said to be sufficiently well known to the student with freer discussion of natural phenomena and experiments so that their usefulness may be made apparent. The promised Scholia with abundant illustrative experiments to confirm the propositions are not realized. It does, however, include a key section at the end of the digression that alludes to various conditions and experiments which I dub the "resistance Scholium."

Hall and Hall (1962, p. 90) note the striking connections with the *Principia* (1687) and later documents but date it in the 1660s or 1672 at the latest based on immaturity of some thoughts, the tightness of the handwriting, and other features.[2] Koyré (1965, p. 83), Herivel (1965, pp. 92–93), and Whiteside (1970, p. 12) also cite the handwriting, the immaturity of some thought, or other evidence of youth in dating it between 1668 and 1673. Westfall (1971b, p. 403, n. 26) agrees with the handwriting assessment and later (1975, pp. 217–218) argues that the emphasis on active principles within passive matter and the corresponding treatment of forces matches other work in the late 1660s, which was laid aside until the *De motu* tracts of late 1684. The Halls consider the unfinished treatment of hydrostatics worthless but Shapiro (1974, pp. 241–242)

[1] Cambridge University Library (CUL) Ms. Add. 4003. The document was first published with translation and commentary by Hall and Hall (1962) followed by extracts and further comments from Herivel (Newton 1965a). A partial revision of the Hall translation lacking the final section on non-elastic fluids has been published by Janiak (2004). A facsimile of the manuscript with German translation has been published by Böhme (1988). The Latin text posted on line by the Newton Project (THEM00093) includes both "diplomatic" style with all of Newton's deletions and other changes and its final "normalized" state. Biarnais (1985) provides a French translation and commentary.

[2] Hall and Hall also note Newton's habit of using notebooks for his early work and loose sheets for later work. Given the jumble of heavily overwritten loose sheets associated with other work in the winter of 1684/5, it is difficult to imagine him sitting down to draft, or better to compile, a tract in a notebook without the assistance of Humphrey Newton, his amanuensis from about 1684 to 1689. Nevertheless, many of those loose sheets are from what might be termed Newton's "waste basket" because they represent work in progress that contain errors or need further development while notebooks generally contain work to be preserved. A theological notebook (Newton Project THEM00002) with scattered entries by Humphrey that may begin in 1684 and certainly before 1689 blunts the above argument. Stronger evidence from the text is needed.

finds it being unsurpassed for at least the next 50 years. While he accepts a date around 1668, Shapiro considers the treatment in the *Principia* to be simply a reworking of *De gravitatione.* Shapiro argues that the key hydrostatic concepts took shape before 1672 converging with Newton's work on optics. Gabbey (1971, pp. 11, 33, n. 94) in a review of seventeenth-century concepts of force and inertia accepts without comment a date of about 1669.[3] Pride of place, however, is given by most scholars to the long metaphysical digression and its discussion of the relationship between God, space, body, and motion against the contrary views of Descartes. Stein (1967, pp. 185–186; 1970, pp. 274–276) considers this treatment to be of the highest order and the analysis "deep and trenchant." While considering an early date as plausible, he hints at reasons to consider a rather late date. Palter (1987, p. 387) questions the early date in the light of the maturity of the metaphysical concepts and how closely they resemble treatments in the *Principia* and later, but accepts a date in the late 1660s or the early 1670s for at least some of the ideas.

On the contrary, Biarnais (1985, p. 182) extols *De gravitatione* not so much for its metaphysics as for its foundational concepts which despite their immaturity provide the basis for measurement and experimentation developed much more fully in *Principia* (1687). According to Biarnais (1985, pp. 11–13), *De gravitatione* must be dated between 1662 and 1665 because of the close connections found in Newton's student notebooks, coupled with the lack of ideas developed in 1666 when Newton was able to relate the weight of terrestrial bodies to the force that retains the moon in its orbit about the earth.[4] A large part of the subsequent analysis (Biarnais, 1985, pp. 87–181) concerns differences between key definitions in *De gravitatione* and *Principia* (1687). Similar ideas are found in Steinle (1991, pp. 124–125) who also favors an early date and suggests that a long ripening process of mechanical philosophy was required between *De gravitatione* and documents in 1684, especially in relation to the third law of motion, the concept of conatus, and the intension and extension of quantities.[5]

Despite occasional expressions of doubt or caution, a composition date of 1668–1673 was widely accepted, often without comment, until the work of Dobbs in the 1680s. Dobbs (1982, p. 552, n. 43) argues that Newton's proof in *De gravitatione* that vacuous spaces exist in aether is contrary to the plenist views of his youth and must have been dated from the 1680s. Finally, Dobbs (1988; 1991, pp. 139–145) summarizes the close relations with the *Principia* noted by other scholars and claims that when they are combined with internal empirical evidence (found in the "resistance Scholium"), they support a date of late 1684 or early 1684/5 just before composing the *Principia*. She finds the handwriting indistinguishable from other documents known

[3] At this stage, Gabbey as well as Dobbs (1991, p. 144) follow Herivel's misreading of *indita* as *insita*, the significance of which is shown later.

[4] While a date before 1666 might apply to some parts, perhaps in the definitions, it cannot be true for the entire text. The digression, before returning to the definitions, ends with a reference to a letter found in Descartes (1668). The source of the Descartes reference is mentioned in Whiteside (1970, p. 12) and Shapiro (1974, p. 267, n. 70) but stated most clearly in Cohen (1980, pp. 189, 332–333, n. 21).

[5] I owe the reference to Henry (2011).

to be written in that period.[6] Her argument has acquired considerable traction since 1991.

Cohen (Newton, 1999, 58, 94 *passim.*) supports Dobbs' analysis and goes on to extol Newton's introduction of the term *inertia* just then on the eve of the *Principia* and not decades earlier, leaving a 15-year gap in its use. This point is reinforced by Feingold (2004, pp. 25–26) who thinks that the document originated in 1671 in association with a previously unknown course of lectures delivered by Newton against Descartes' mechanics and Henry More's hydrostatics but took on its present form only after the invocation of the term inertia in the mid-1680s. McGuire (2000, p. 217) after a long time accepting a date not later than 1673 also finds Dobbs' claim to be persuasive and proceeds to strengthen it by illustrating an unbroken development of Newton's theological world system from the first rough drafts of *Theologiae gentilis origines philosophicae* (Philosophical origins of gentile theology) in the early 1683/4[7] on through *De gravitatione, Principia* (1687), documents in the 1690s including abortive attempts to prepare a revised edition, and the General Scholium of the second edition as realized in 1713. A process that stretched over 30 years, however, could just as well have begun 15 years earlier.

The *Cambridge Companion to Newton* includes a matter-of-fact acceptance by Gabbey (2002, p. 331) of a date in the mid-1680s shortly before the *Principia*, revising his position above.[8] In a companion essay Stein (2002, pp. 302–303, n. 39) continues to express well-founded caution. He thinks that the evidence for an early date is plausible but inconclusive and decides that after all evidence based on handwriting may be conclusive. Hall (2002, pp. 409, 417) cautions the use of handwriting. Acknowledging that we do not know when it was composed, Hall offers continued support for a date before 1672. The Newton Project accepts, without comment, a date of the mid-1680s.[9] More recently, Henry (2011, pp. 25–26) finds Dobbs' date to be in the winter of 1684/5 after the first set of *De motu* documents virtually turned to be untenable because ideas developed around 1679 are missing, while ideas outmoded by then are included. Henry favors the original date of about 1668 given by the Halls. Somewhat similar ideas are advanced by Kollerstrom (1999) who also finds Newton's concept of *gravitas* in the document to be incompatible with any date after the mid-1670s.

This study adds strong proof that *De gravitatione* was composed before Halley's visit in August 1684 and was not part of the process in the winter of 1684/5 leading to the *Principia*. Less robust considerations rule out any date in 1684. The slim possibility for a date around winter of 1683/4 is not necessarily ruled out. This conclusion follows the same path adopted by Dobbs and Henry, but calls for a different reading

[6] The script in *De gravitatione* is slightly tighter than in documents known to be from the 1680s but variation in quill points, pace of writing, and other factors make it a close call. Similar concerns about dating by handwriting are raised by McGuire and Tamny (1983, p. 8) and Hall (2002).

[7] Yahuda Newton Ms 17.2. Newton Project THEM00060, "Notes and drafts related to *Theologiae gentilis origines philosophicae*." Seminal studies are in Schaeffer (1993) and Iliffe (1995).

[8] In a forthcoming paper on Newton and *materia*, , Gabbey chooses the secure ground of a date of 1668 or later based on Newton's reference to a "letter 96" from Descartes to Mersenne originally found under that designation only in Descartes (1668).

[9] Rob Iliffe (2007, p. 64), director of the Newton project, offers a date to be in the late 1670s or more likely the early 1680s but cites no particular evidence.

of the "resistance Scholium," a proper accounting of the "new" pendulum experiment, Newton's definition of *gravitas* , and his use of the concepts of *inertia* and quantity of material.

2 Dating the empirical content

The fate of a late date hinges on how well the definitions and empirical claims in *De gravitatione* advance or conflict with comparable material in the series of documents under the general title of *De motu*—On motion). These documents date from November 1684 and stem from a meeting with Halley's meeting with Newton at Cambridge in August 1684. Dobbs argues *De gravitatione* follows the "new" pendulum experiment and belongs precisely to a brief period between two specific documents around January 1684/5.

The story is well known (Westfall, 1980, pp. 402–406). Hooke for some time had claimed that he was able to derive Kepler's laws of planetary motion by supposing an attraction to the sun followed an inverse-square relation but could not or would not produce adequate proof. Despite an inducement in January with a significant prize and subsequent fame, he still meant to keep it secret until others had tried and failed. When Halley put the same question to Newton seven months later, Newton is said to have responded immediately that the orbit would be an ellipse as he had found subsequent to his correspondence with Hooke during 1679/80. Not finding the proof among his papers, Newton promised to renew it and send it to Halley.

On the contrary, Kollerstrom (1999, p. 332) accepts the contention of Lohne (1960, p. 35) that Newton developed the orbital proofs from scratch in 1684. Kollerstrom (p. 345) suggests "the problem of elliptical orbits caught Newton's interest once he realized that a comet trajectory could have this form." He also suggests (p. 354) "the truth of the theory of gravity ... may have dawned silently as [Newton] pondered over the path of "Halley's comet" of 1682 .... The parabola of its trajectory passed close to the ecliptic, but in the reverse direction to the planetary orbits." [10] This "new testimony of the heavens" negated the idea of aether resistance and led Newton to abandon the vortex theory of gravitation, thereby tacitly eliminating a late date for *De gravitatione*. Kollerstrom (p. 333) dates *De gravitatione* to be around 1668 because a "rather Aristotelian downward impulsion is compared with the *conatus recedendi a centro* of the (Cartesian) solar vortex." This last point is treated in Sect. 6.

Newton made four relatively crude observations of the comet of 1682 (Correspondence, 2, pp. 380–381). The apparent motion of the comet was direct according to the order of the zodiacal signs and the motion of planets. Some analysis was needed to determine the true motion. There is no record to show that Newton reduced his observations to standard coordinates, let alone determined a retrograde parabolic path. The task was left to Halley working with Flamsteed's observations (Ruffner 2010, p. 445; Newton 1999, pp. 934–935). Nevertheless, Newton might have realized quite early that

[10] Kollerstrom notes that the parameters of its orbit were radically different from the comet of 1680 (as if Newton had any notion of these parameters in 1684, those cited by Kollerstrom were determined much later by Newton or Halley).

the motion was actually retrograde. Approximate positions noted on a celestial globe (with allowance for precession) if transferred to a Copernican plot would have revealed the motion among the planets to have been retrograde under almost any hypothesis.[11] Better observations were not available to Newton until 1685. In any case, the comet of 1682 was not the first retrograde comet known to him. Newton's work in 1680/1 revealed the comet of 1664 to be retrograde and Newton knew of other examples from sources such as Hevelius (1668). The realization that such motion had implications for the properties of space and a theory of gravity, of course, might have occurred to Newton only after the appearances of 1682, as a trigger. Yet, Kollerstrom, claiming to adhere to the historical record, cites no documents in support. There are documents that suggest another course of development in which comets played a significant role.

Comets very likely were a matter of discussion at the August meeting.[12] The issue of comets had been revived earlier in 1684 as Newton pondered over the degradation of knowledge in antiquity when working on early drafts of "Philosophical Origins."[13] Among the lost ancient truths that needed to be demonstrated anew were the principles of planetary motion and the ancient concept that comets are a kind of planet which require the heavens to be fluid, and that more generally celestial motions take place in "very free spaces" (*spatijs liberrimis*) Ruffner (2010, pp. 27–28). The heavens were certainly free of solid spheres, but beyond that discovery further proof was needed.

Nothing was heard for about four months until November when Edward Paget delivered a nine-page tract to Halley with the title *De motu corporum in gyrum.* A transcription and translation of the original copy retained at Cambridge is available in Newton (1965b) and (Math. Papers, 6, pp. 30–70). The copy delivered to Halley is lost, although it provided the basis for the copy deposited at the Royal Society (Ball 1893, pp. 35–51). The requested proof is in inverted form showing the inverse-square relationship can be derived from an ellipse using a series of definitions and hypotheses, then generalized to other conics to eliminate other possibilities. Restoring the proof had not been easy, involving false starts and some difficulty in casting the definitions and hypotheses from which the proof followed. Such details may support Kollerstrom's thesis about the origins of the Kepler-motion proofs but not the trigger-

[11] Newton's study of comets during 1680/1 included work using celestial globes (CUL Ms Add. 4004: f. 101r.) Later, in the late summer or the early fall in 1685, Newton devised a method to demonstrate that the time between the estimated date the comet of 1680 entered the earth's orbit to the estimated date of its exits was consistent with a parabolic orbit that passed very close to the Sun (Math. Papers, 6; pp. 481–487). This account by Whiteside omits the cruder (and seriously flawed) method that Newton applied to four other comets, using coordinates obtained from Hevelius and others (Newton, 1960, pp. 618–619). Three of these comets, including the comet of 1682, were retrograde. Newton made no comment on this fact, although by then, a retrograde orbit was no longer an issue.

[12] A note added to Newton's compilation of data for the comet of 1680 includes information given to him by Halley about an observation on December 8, 1680 while traveling to Paris. CUL Ms. Add. 4004, f. 101r. Additional information is squeezed in at f. 99r. Newton had wanted this information in April 1681 and likely took the first opportunity to press Halley for details. See *Correspondence*, 2, p. 361. The note also includes an unrelated item on Jean Richer's pendulum experiments at Cayenne, which reveal an interest in gravitation. There is no evidence for a prior meeting in 1682 as Westfall and others suppose.

[13] Yahuda Newton Ms. 17.2, f. 18v-19r; Newton Project THEM00060. Seminal studies are in Schaeffer (1993) and Iliffe (1995).

ing role of the comet of 1682.[14] Newton's proofs specifically invoke zero resistance as an assumption. Still assuming zero resistance Newton outlines a method to determine whether a comet returns again and again in the same orbit thereby resolving an age old question freshly revived in the Philosophical Origins documents. The final section, added as an afterthought, treats bodies moved by their *vis insita* or inherent force alone or in conjunction with the force of *gravitas* through a uniformly resisting medium which would also include an all-pervasive aether.[15] Resistance is to be determined experimentally using small spheres and scaled up by assuming (in a medium of the same density) it is proportional jointly to speed and the surface area of the sphere. Unlike *De gravitatione*, the proposed determination continues the assumption the resistance of aether is zero because no allowance is made for the additional resistance due to the vastly increased surface area as it passes through the internal pore spaces. The subsequent version adds the necessary proof.

An augmented version *De motu sphaericorum corporum in fluidis* (Newton, 1962e, Math Papers, 6, pp. 74–81, excerpts) provides sections on orbital perturbations and proof that the resistance of celestial aether is nil or too small to be detected based on comets, both direct and retrograde, and the age-long constancy of planetary orbital periods. There is no mention of any specific retrograde comet. The text, predominately in the hand of his amanuensis, has insertions in Newton's hand, which include changing the term hypothesis to law. This version can be dated around December 1684 (Westfall 1971a, pp. 195–196, n. 21).

There is another evidence that comets were under active consideration in fall 1684 alongside the work on planetary orbits. Newton had not completed the task of extracting data from his micrometer measurements needed to reduce his observations made in February and March 1680/1.[16] Half way through the process, Newton discovered inconsistencies and turned to Flamsteed for the coordinates of the basic reference stars in the first of two requests delivered in mid-December via Paget. Newton's second request sought data related to Kepler's laws, the satellites of Saturn, and the problem of perturbations.[17] Both topics were central to Newton's work off and on for many months.

A manuscript from about January 1684/5, *De motu corporum in medijs regulariter cedentibus* (On the motion of bodies in uniformly yielding media), offers definitions of absolute time and absolute space and further laws of motion (Newton 1965c; corrected in Math Papers, 6, pp. 188–193, Latin only).[18] None of the *De motu* documents

[14] Kollerstrom (1999, p. 345) following Iliffe (1994, pp. 68–95) notes Newton was deeply immersed in discussions with Henry More about the Apocalypse and Catholic idolatry beginning about January 1679/80. Kollerstrom suggests the complexity of the orbital problem would have precluded the solution in the period during which Newton also never strayed far from alchemical studies and his "Chymical furnace."

[15] *Gravitas* is restricted to the terrestrial realm and by the inverse square rule to the moon. This stylistic move avoided unnecessary controversy over the causal mechanism of celestial motions; a move not found in *De gravitatione*.

[16] An arcane process to be detailed in another study was used to convert the readings into useable data.

[17] *Correspondence*, 2, pp. 403–415. Flamsteed's reply is dated 27 December 1684 with exchanges taking place up to 27 January 1684/5.

[18] Necessary corrections were indicated by Cohen (1971, pp. 92–96) in which he refers to *De motu corporum in medijs regulariter cedentibus* (Newton 1965c) as *De motu corporum*. Cohen refers to document with the actual title of *De motu corporum* (Newton (1962d)) as *De motu corporum: definitions*.

so far use the term *inertia.* According to Dobbs (1991, pp. 138–141), Newton found a narrow window at this point to draft *De gravitatione* which allegedly introduces the term *inertia*, rejects the corporeal nature of aether utterly and completely and eliminates it as a mechanical cause of gravity. The newly invoked term carries over to subsequent manuscripts starting with *De motu corporum* (On the motion of bodies) (Newton 1962d; Wilson 1969, pp. 250–258; Math Papers, 6, pp. 92–97).[19] This short series of definitions is part of the increasingly expanded manuscripts in the winter of 1684/5 related to the *Principia* (Cohen 1971, pp. 82–92, 310–321). A closer look is needed.

3 *De gravitatione's* telltale numbers

The purpose of the digression in *De gravitatione* is to dispose of Descartes' fictions about space and motion expressed in his *Principia Philosophiae* and *Epistolae.* [20] It includes a lengthy telltale argument on the nature of body intended to demonstrate, in accordance with Descartes' first law of nature, that the essence is not extension alone as claimed but includes capacities to resist change of state and stimulate perceptions. Newton's argument culminates with the "resistance Scholium" in which he adduces proof that aether is a corporeal fluid with those fundamental capacities but which must contain large vacuous spaces because of its small yet detectable ability to offer resistance. The focus is on actual scattered corpuscles of aether rather than a simplified ideal continuous fluid discussed later in the document.

I will pick up the argument in Newton's section on "space" as distinct from "body." For some reason, Newton has stopped making reference to the particular Cartesian articles which are clearly implied. Newton argues, "...in space there is no force of any kind that might impede, assist, or in any way change the motion of bodies. And hence projectiles describe straight lines with a uniform motion unless they meet with an impediment from some other source" (Newton 1962b, p. 137; 2004, p. 26).[21] This point is a gloss on *Principia Philosophiae* (Principles), Part 2, article 37 containing Descartes' first law of nature that each and every thing, in so far as it can, always remains in the same state except as the result of external causes, (article 38) the

[19] Further evidence of Newton's deep involvement with comets in late 1684 and early 1685 is found on the other half of this document (CUL Ms. Add. 3965.5, f. 21). A much emended final draft of part of Definition 3 and an unnumbered but clean final draft of Definition 4 seem to be squeezed in below an unused heading for star longitudes and latitudes where presumably empty space had been left earlier for listing data. Immediately below the revisions of the definitions and scattered around the edges are calculations to determine the position of the sun using Flamsteed's solar theory for the times the comet of 1680 had been observed at Canterbury by Thomas Hill and at Rome by Pontheo. There is no evidence Newton had access to Pontheo's data in 1681 when he was using Wing's solar theory, as will be shown in a forthcoming paper. Cohen (1971, pp. 92–93) omits notice of this solar data.

[20] Newton would have used Renati Descartes *Principia Philosophiae* (Amsterdam, 1656) and Renati Descartes, *Epistolae* (London, or Amsterdam, 1668). I briefly consulted the London imprint along with Descartes (1985–1991) , Miller and Miller (1991) and Graukroger (2002).

[21] *... spatio non inest vis aliqua impediendi aut promovendi vel qualibet ratione mutandi motus corporum. Et hinc corpora projectilia lineas rectas uniformi motu describunt si non aliunde occurrant impedimenta* Newton (1962b, p. 104).

confirmation of this first law by the everyday experience of projectiles, and (article 39) Descartes' second law of nature that all motion is in itself rectilinear. Newton's argument continues with a critique of Descartes' contention that the nature of body is simply extension and not its being something which is hard, heavy, or colored, or which affects the senses in any way (Principles, Part 2, article 2). Bodies are defined, in so far as possible, as determined quantities of extension endowed by omnipresent God with certain conditions, namely, mobility; impenetrability, and the ability to arouse mental perceptions (Newton 1962b, p. 140; 2004, p. 28). Bodies must be impenetrable lest two occupy the same place at the same time. As such they obstruct each other's motion and are reflected, most notably, according to certain laws which tacitly include the first law of nature and the endeavor to resist change of state that Newton began to articulate in the mid-1660s.

In essence, Newton's view depicts a "billiard ball" physics in which inert bodies have been endowed only with a principle of perseverance and act only through laws of impact and reflection. Henry (2011, 23–24) notes there is no hint of active principles (other than "inertial" endeavor) or inter-particulate forces of attraction and repulsion that begin to appear in writings after around 1670 and that the sparseness of properties suggest an earlier date. The date is ambiguous. Newton declares at the beginning of the essay that he will postulate only the properties required for local motion. Whenever written, Newton's argument can be seen as narrowly constructed simply to refute Descartes' proposition. Moreover, whatever may be true at the micro-level is not necessarily true at the macro-level. Consider magnetic forces of attraction and repulsion which are not essential for all kinds of bodies. The important point is a body has the essential ability to arouse mental perceptions. You cannot have one without the other. Newton argues whatever reality that we attribute to bodies arises from their phenomena and sensible qualities. Removing the ability from bodies would also remove the other endowed ability to resist change of state and transfer mutual actions from one to another, thereby absurdly reducing body to empty space (Newton, 1962b, p. 146). The task is to use the testimony of the phenomena to demonstrate the contradiction in Descartes' argument since there is no difference between the extension of space and the extension of a body, and therefore, empty spaces cannot exist between bodies (Principles, Part 2, article 16, etc.).

For this demonstration, Newton reverses his earlier gloss. Invoking an oft-repeated principle he argues, since resistance to motion decreases from mercury to water to air to aether, to further eliminate all such resistance would utterly eliminate the corporeal nature of the medium by which actions are transferred from one to another. Therefore,

> … if there were any aerial or aetherial space of such a kind that it yielded without any resistance to the motion of comets or any other projectiles, I should believe that it was utterly empty. For it is impossible that a corporeal fluid should not impede the motion of bodies passing through it, assuming that (as I supposed before) it is not disposed to move at the same speed as [them] (Part II, Epistle 96 to Mersenne) (Newton 1962b, p. 146; 2004, p. 34).[22]

[22] *… si spatium aëreum vel aethereum ejusmodi esset Cometarum vel corporum quorumlibet projecilium motibus sine aliqua resistentia cederet crederem esse penitus inane. Nam impossibile est ut fluidum*

Notice that planets are not mentioned as test probes. It is Newton's concept following the contention of Descartes, Wing, and others that they are carried by the solar vortex. The idea is adapted for a theory of lunar motion in a note added (c. 1669) to the end papers of Newton's copy of Wing's *Astronomia Britannica* (Whiteside 1964, pp. 124–127). Newton (1962b, p. 124; 2004, p. 15) also uses the idea "as supposed before" when countering Descartes' absurd notion about philosophical motion. The absurdity is posed against their common belief that the planets ride in a vortex. As such there is no reason to invoke the age long stability of the planetary periods which the force of aether would otherwise erode.[23] Palter (1987, pp. 410–411) contends the references to vortices have only a rhetorical role in the *reductio* arguments and do not commit Newton to a belief in their reality. The test is what Newton meant by the phrase "as I supposed before" and the *conatus* of aether "gyrating about the sun" in his subsequent definition of *gravitas*.

The notion that comets, like other projectiles, cut through their fluid surroundings is contrary to Descartes' claim cited earlier in the document that comets (come to) ride at rest with the solar vortex near its outer edge (p. 127).[24] Given the formulation of the argument it is puzzling no evidence is provided about the effect, if any, that aether has on comets, being content a little further on to offer evidence based on terrestrial phenomena. The retardation of projectiles by air infused with aether would have been obvious and needed no further exposition. Was the same true of comets hurling through pure aether? Or did Newton omit celestial evidence based on the phenomena of comets because he lacked a clear demonstration and deferred to better established or less contentious evidence? In any case, since *De motu corporum in fluidis* addresses the problem directly for both comets and planets, *De gravitatione* must have been written earlier.

Throughout the 1660s and 1670s, Newton most likely adhered to the view offered by Streete (1661, p. 15) that comets travel among the planetary orbits but unlike planets they are "incompact and dissolvable" and "their motions are (as Kepler defines them) in or near to right lines." Newton obtained a few observations in 1664 and 1665 but made no known attempt to determine an actual trajectory. If *De gravitatione* was drafted in this period, then the idea could be seen as a prospective test, in which case Newton would have deferred to the best available terrestrial evidence, as here.

Newton's initial work on the comet of 1680 was framed by what might be called Platonic archetypes of straight lines and their great circle equivalents on the celestial sphere. For his part Newton found difficulty with every suggestion from Flamsteed that the sun attracted or influenced the motion of the comet in some other way.

Footnote 22 continued
corporeum non obstet motibus trajectorum, puta si non disponitur ad motum juxta cum eorum motu velocem (Part 2 Epist 96 *ad Mersennum*) (Newton 1962b, pp. 112–113). The translation has modified slightly in the forthcoming paper by Gabbey. See note 8. A translation of the original Mersenne letter is in Descartes (1985–1991), 3, pp. 131–133, To Mersenne, 9 January 1639.

23 Westfall (1971b, p. 350) notes that Newton realized quite early that stability, albeit in a circular orbit, requires the interactions to be perfectly elastic.

24 It is noteworthy that Newton's interest in comets had anti-Cartesian twist since his student days and that at least twice, in 1680/1 and 1685, he gathered evidence of the kind long available by naked eye observation that proved Descartes to be wrong. See Ruffner (2010, pp. 426, 432–433).

The famous outline of such a mechanism by which the comet turned about the sun with an imbalance between a continuous solar attraction and the comet's *vis centrifuga* was drafted so that Flamsteed could build a plausible theory. The suggestion was abandoned because Newton's own work showed it to be false. A significant part of Newton's evidence stemmed from a determination of the comet's ascending node based on the idea the apparent path of a comet follows a great circle arc in its initial appearances with later departures due to the perspectives of the earth's annual motion.[25] Newton's solution of the actual path was based on uniform rectilinear motion with only slight curvature in the vicinity of the sun. Such a finding was compatible with his view at the time that the vortex could deflect the passage of bodies cutting across it. The finding, however, was inconsistent in failing to allow for resistance to forward motion except perhaps in the backward bending of the tail, where the effect of resistance would be most evident.[26] The failure to cite this evidence for the existence of a corporeal aether may be taken as an indication that *De gravitatione* was not composed between 1680 and 1683, or until he began to doubt his views on comets held in those years, especially with regard to resistance.

As noted above, around the beginning of 1684 Newton began to entertain the ancient idea that comets are a kind of planet traveling in highly eccentric orbits in "very free spaces." Tycho's comet observations had already proven the heavens to be free of solid spheres. What needed to be proven anew was that comets were a kind of planet and the heavens were free of resistance. If Newton wrote *De gravitatione* after the Philosophical Origins document of winter 1683/4 and was uncertain or lacked proper demonstrations, once again he might defer to terrestrial evidence. It is not clear, however, that Newton would have linked comets with projectiles in this period. Depending on how one reads the evidence cited, this section of *De gravitatione* might be dated before 1680 or between the winters of 1683 and 1684 and the composition of *De motu corporum in fluidis* where the first proof is offered.

Newton continues the argument by noting Descartes' logical error in begging the question when he asserts the force of resistance to the passage of bodies can be removed from space without having first proved that space and body do not differ. Then to remove any doubt, Newton cites the best possible terrestrial evidence that there are empty spaces in the natural world proving that body is not simply extension. He posits another oft-repeated principle that ordinary fluids impact only the external surfaces of ordinary (porous) bodies while aether flows through all their pores spaces drastically increasing the exposed surface area and the resistance of each aetherial particle. These particles must be relatively far apart in relation to their diameter with large empty spaces at the micro level. Otherwise, aether would yield to bodies passing through it with much greater *inertia* and offer more resistance than the particles of an ordinary fluid which do not penetrate the interior parts. Finally, a quantitative estimate follows that indicates a considerable degree of resistance:

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25 Two observations fix the position the node by calculation or by extrapolation along their apparent great circle arc with the aid of a globe. Newton did not realize or chose to ignore the variation that results from the choice of dates. This variation was demonstrated by Flamsteed at the very same time (Forbes 1975, pp. 28, 109).

26 These ideas are touched lightly in Ruffner (2010, pp. 426–427), a more detailed study is in progress.

> Since the resistance of the aether is on the contrary so small when compared with the resistance of quicksilver as to be over ten or a hundred thousand times less, there is all the more reason for thinking that by far the largest part of the aetherial space is empty, scattered between the aetherial particles (Newton 1962b, p. 147; 2004, p. 35).[27]

Putting the claim in context, experiments by Boyle (1662, pp. 148–155) and Hooke (1665, p. 27) indicate that the density of air at the surface (and hence its resistance which is assumed to be proportional to density) is about 1/13,000 or 1/14,000 that of quicksilver. Thus, with a density of aether somewhat over 10,000 times less than quicksilver its resistance would be of the same order of magnitude as air at the surface. This result is consistent with the "old" pendulum experiment of Boyle (1662, pp. 103–105) that found little difference between the duration in air assumed to be infused with aether and an evacuated glass sphere presumably filled only with aether.[28] Even a density ratio of 1/100,000 would yield resistance comparable to air only a few miles above the surface of the earth based on the decrease of air pressure with altitude.[29] Did Newton attempt to quantify the results of Boyle and Hooke, make a mistake in some experimental setup of his own, or (especially in the case of 1/100,000) rhetorically toss off numbers intended to evoke an image of trivial resistance? Newton provides no idea, here or indeed in the *Principia*, of how the density of aether may vary with distance from a central body.[30] The passage proves only that there is no plenum and that widely scattered aether corpuscles exert detectable resistance. Space per se may be completely free of resistance but not the fluid mediums in it.

This outcome can be compared with the results of Newton's "new" pendulum experiment which Dobbs claims was accomplished before *De gravitatione*. As reported (from memory) in the *Principia* (1667, p. 353; 1999, pp. 722–723), the resistance of pure aether is over 5,000 times smaller than the combined resistance of air and aether. This result makes the resistance more than 60–70,000,000 times smaller than quicksilver, a value much less than reported here and one that at last truly supports a conclusion that the resistance of aether is either nil or imperceptible. Manifestly that pendulum experiment is not the basis of the result in *De gravitatione* as commonly assumed.

In further support of his contention that aether is composed of scattered particles of corporeal matter and hence capable of some degree of resistance Newton's argument

27 *Sed cum aetheris e contra tam parva est resistentia ut ad resisentiam argenti vivi collata videatur esse plusquam decies vel centies mille vicibus minor: sane spatij aetherei pars longe maxima pro vacuo inter aetherea corpuscula disseminato haberi debet* (Newton 1962b, p. 113).

28 Newton refers to this experiment in *De aere et aethere* (Newton 1962c, pp. 220, 227–228).

29 See for example Hooke (1665, pp. 227–228). Hooke did not sum the result of the problem set up divided into1000 layers. With or without the actual result Newton would have realized the lower density would be found not far above the earth's surface.

30 By way of contrast, as an afterthought in his letter to Boyle, dated 28 February 1678/9, Newton conjectures that the particles of aether vary in a continuum from fine to gross and that terrestrial gravitation results from aether in which the gross proportion increases with distance about the earth's surface. He offers no speculation about celestial aether. *Correspondence*, ii: 295. And in Query 21 (originally 1717) the density of aether, as a cause of gravity, grows perpetual denser with distance from the bodies of the sun, stars, planets, and comets (Newton 1952, p. 350).

continues with a passage that uses the term "quantity of matter" rather than earlier terms such as "the bulk of body" or simply "body" (see Sect. 5):

> The same may also be conjectured from the various gravities of these fluids, for the descent of heavy bodies and the oscillations of pendulums show that these are in proportion to their densities, or as the quantities of matter contained in equal spaces. But this is not the place to go into this (Newton 1962b, p. 147; 2004, p. 35).[31]

Again these notions about bodies falling in various fluids are not based on experiments reported in the *Principia* (1999, pp. 758–761). Those experiments are restricted to air and water mediums and postdate the first edition.[32] It is more likely Newton had heard about the experiments with falling bodies fostered by the Royal Society in the 1660 (Gunther 1967–1968, 1, p. 248). This information could possibly have been obtained from John Collins in November 1669 during a trip to London, if not before.[33] Be that as it may, implicit recognition of the principle is found in a tract of about 1666 on the laws of motion (Newton 1962a, p. 163). Newton notes that the motions of ordinary aggregate bodies are continually impeded by the mediums in which they move, implying the resistance increases as the density of the aggregated parts increases. It is also possible he freely extrapolated from items such as Descartes' account of projectile motion, in which the resistance in any other fluid is even more obvious than in the case of air (Principles, Part 2, article 38). Similar recognition also underlies the treatment of violent motion in one of Newton's student notebooks (McGuire and Tamny 1983, pp. 406–409).

Newton's pendulum experiments using boxes filled with equal weights of diverse materials such as water, sand, lead, and gold do not seem to be at issue here.[34] The reference to pendulum experiments in different fluid mediums is more difficult to pin down. The *Principia* reports experiments of pendulums oscillating in water and air indicating resistances on the order of 800 or 900 (later 850) to 1, or nearly in proportion to their densities. In quicksilver and water the results are on the order of 13 or 14 to 1, again in proportion to their densities (*Principia*, 1687, pp. 347–350; 1999, pp. 719–21). Aether is not an issue, however, and again information about experiments ordered by the Royal Society in the 1660s may have been the source. Greater attention to the sources of the empirical assertions in *De gravitatione* is needed.

Newton's corresponding argument in *De motu corporum in fluidis* is also based on the regression of resistance from mercury to water to air and the great increase in surface area with which aether collides. The much stronger claim is that the aether

[31] *Quod idem praeterea ex diversa gravitate horum fluidorum conjicere liceat, quam esse ut eorum densitates sive ut quantitates materiae in aequalibus spatijs contentae monstrat tum gravium tum descensus undulationes pendulorum. Sed his enucleandis jam non est locus* (Newton 1962b, p. 113). Note what is evidently the first use of "*quantitates materiae*" rather than simply "bulk" or "body."

[32] For an extended discussion see Smith (2000, pp. 105–136).

[33] There is no detailed account of what was discussed. See *Correspondence*, 1, pp. 53–54.

[34] These experiments were first mentioned in *De motu corporum in medijs regulariter cendentibus* (circa January 1684/5) and were restricted to oscillations in air. See Herivel (1965, pp. 316–317, 319) and Wilson (1969, pp. 162–164). See also Sect. 5 below.

flows freely over all the individual parts yet offers no detectable resistance which is either non-existent or extremely meager. Proof includes the observation that the tails and comas (or atmospheres) of comets are not torn off despite being carried at immense speed in all directions through the heavens. Additional proof is the constancy of the mean periods of the planets over thousands of years (Newton 1962b, pp. 285–286). There is no mention of the "new" pendulum experiment that proves the same point. As Cohen (1980, p. 314) notes, if the experiment was conducted before drafting *De motu corporum in fluidis* he would have mentioned it. Indeed, it is possible the experiment was conducted later in winter 1684/5 looking for direct confirmation. It is almost beyond belief that *De gravitatione* was composed after these first two *De motu* tracts unless it incorporates old notes he carelessly neglected to bring up to date.

The treatment of aether in *De gravitatione* manifestly does not remove its corporeal nature utterly and completely. Dobbs misses the point while Henry (2011, p. 25) grasps it without realizing the significance of the numbers. The small but detectable and hence quantifiable resistance of "pure" aether is the culmination of his evidence against the Cartesian principle that the nature of body consists not in weight, hardness, color, or the like, but simply in extension. The Cartesian argument is unsound because no vortices or other parts of a system of the world can be fashioned from extension without also invoking the corporeal nature of God's "determined quantities" and their *inherent laws* of impact, reflection, and perseverance (Newton 1965b, pp. 147–148).

4 Definitions and the questions of *inertia and gravitas*

Returning to the main theme of gravitation or heaviness and equilibrium of fluids and of solids in fluids, *De gravitatione* continues the series of definitions interrupted by the digression and finishes inconclusively with the elements of fluid science. First I will take a close look at the definitions and various translations of *vis*, *inertia*, and *gravitas* including the use of *indita* here and elsewhere in the text.

> Definition 5. Force is the causal principle of motion and rest. And it is either an external one that generates or destroys or otherwise changes impressed motion in some body; or it is an internal principle by which existing motion or rest is conserved in a body, and by which any being endeavours to continue in its state and opposes resistance (Newton 1962b; 2004, p. 36).[35]

The German translation (Newton 1988, p. 77) similarly indicates the state of motion or rest is something a body has (die ein Körper hat). These translations do not properly account for the Latin "*indita*." A better translation of the phrase *quo motus vel quies corpori indita conservatur* would be "by which the motion or rest *imparted* to a body is conserved." This rendering is supported by the French translation which reads "par

[35] *Vis est motus et quietis causale principium. Estque vel externum quod in aliquod corpus impressum motum ejus vel generat vel destruit, vel aliquo saltem modo mutat, vel est internum principium quo motus vel quies corpori indita conservatur, et quodlibet ens in suo statu perseverare conatur & impeditum reluctatur* (Newton 1962b, p. 114).

lequel le movement ou le repose attaché au corps est conservé." The significance of "*indita*" as something imparted or attached to a body is developed below.

> Definition 7. *Inertia* is the inner force of a body, lest its state should be easily changed by an external exciting force, *ibid.*[36]

These ideas are distilled from Descartes (Principles, Part 2, articles 37–44) and sketched in Newton's early dynamical writings in the Waste Book which date from about 1664 or 1665. Force as a cause of change stems from Axiom 3 (Herivel 1965, p. 141), "There is exactly required so much and noe more force to reduce a body to rest as there was to put it upon motion:*et e contra.*" The concept of force as an internal principle stems from Axiom 100 (p. 153) which states in part, "Every thing doth naturally perseveres in that state in which it is unless it bee interrupted by some external cause." As for the new definition, can Newton be saying the inner principle adjusts its endeavor to oppose resistance and conserve a body's the state of motion in accordance with whatever change is imparted?

The term *inertia* is not found in the Waste Book entries. Cohen (Newton 1999, pp. 100–101) thinks its invocation here as an active principle, allegedly on the eve of the *Principia*, is intended as an anti-Cartesian move contrary to usage in the correspondence between Descartes and Mersenne.[37] Newton, however, had many anti-Cartesian moments and its adoption could have occurred any time after the publication of the Latin edition of letters in 1668. By any account of the date, early or late, the definition of *inertia* provides the first known use of the term for the concept of perseverance.

Cohen (Newton 1999, p. 58) also believes Newton equated this inner or inherent perseverance principle with *vis indita*. That is doubtful. Newton makes several others uses of the verb *indere* which nearly all the various translations render as something imparted or applied to the object. Newton (1962b, p. 96) mentions a "*vis caelis indita,*" in which "*indita*" is rendered variously as "impressed" (Newton 1962b, p. 128), "imparted" (Newton 2004, p. 18), "appliquée" (Newton 1985, p. 28), and "eingeprägte" (Newton 1988, p. 27). In another passage, Newton (1962b, p. 100), the phrase "*color si inditus esset*" is rendered similarly as "if color were introduced" (Newton 1962b, p. 133; 2004, p. 23); "la couleur introduite" (Newton 1985, p. 38); "die Farbe beigibt" (Newton 1988, p. 41). In another instance, Newton (1962b, pp. 107, 140; 2004, p. 29) refers to a body's "impressed form" (*inditam formam*). In a departure from the idea of something that has been added, Newton (1985, p. 52) renders the phrase "la forme qui lui est *inherente.*" However, Newton (1988, p. 58), in basic agreement with the English translations, renders the phrase "ihr verliehenen Form (its conferred form)."

Finally, and most importantly, there is the definition of *gravitas* (Newton 1962b, p. 114) the first line of which reads, "*Gravitas est vis corpori indita ad descendendum incitans.*" The English and French translations are in basic agreement: "Gravity is a force *in* a body compelling it to descend" (Newton 1962b, p. 148); "Gravity is a force

[36] *Inertia est vis interna corporis ne status ejus externa vi illata facile mutetur, ibid.*

[37] See Descartes *Epistolae* (1668), Part II: 110 *passim* (Letters 25, 34, 96). For corresponding English translations (in a different sequence) see Descartes (1985–1991, 3, pp. 135, 141, 131). For details see Cohen (1980, pp. 189, 332–333, n. 21)

within a body impelling it to descend" (Newton 2004, p. 36); "La gravité est la force qui incite un corps a descendre." (Newton 1985, p. 68). Only Newton (1988, p. 77) provides the sense that gravity is provided (damit versehen) to a body: "Schwere ist die Kraft, die in der Körper, der damit versehen ist, antriebt herabzufallen." Newton has defined two different concepts of force: an external one causing change of state, and an internal one actively preserving the status quo and resisting change. Does Newton's definition of *gravitas* provide a different internal force that actively generates motion to descend? The concept makes more sense if, as in the German, the gravitational force is provided or imparted to a body by an external cause consistent with a variety of hypothetical mechanisms, or God's direct action.

In sum, *vis indita* is not innate but something has been added to body or put there whether by of divine will, mechanical impact, or other action. It is not a term denoting the inherent or innate principle of *inertia*. It is noteworthy, after adopting *vis insita* (inherent force) as the term for the inertial principle the concept is successively enhanced, as if a matter of self-correction, to denote a force that is inherent and innate (*vis insita et innata*) and then one that is inherent, innate and essential (*vis insita, innata et essentalis*).

Let us assume, however, Cohen correctly identifies *inertia* with a *vis indita* in *De gravitatione*. He is concerned that an early date creates a gap of about fifteen year in the use of the term. *De motu in gyrum*, *De motu corporum in fluidis*, and *De motu corporum in medijs regulariter cedentibus* use the current term *vis insita* for the innate principle and make no mention of *inertia*. Dobbs' placement of *De gravitatione* after this point allows *inertia* to appear as the term for the innate principle with an anti-Cartesian thrust. Allegedly *inertia* is first equated with *vis insita* in the fragmentary *De motu corporum*, followed by drafts of Lucasian lectures and finally Definition 3 in the *Principia* (1687). This sequence leaves no gap in the use of the term but, after already using *vis insita* for the inherent principle, a problem arises if *inertia* is then equated with *vis indita* in *De gravitatione*. If so, there is also a problem in that the *vis corpori indita* of *gravitas* should also be considered an inherent rather than an imparted force which drives a body downward. But if, as suggested, *vis indita* has nothing to do with the innate principle, then neither problem arises.

By itself, the use of *inertia* in *De gravitatione* creates no problem for a late introduction. Setting the composition date after any of the early *De motu* tracts, however, requires an accounting for the quantitative measure of aether resistance and other points noted here that have greater empirical significance. Despite the concern over the date for the invocation of the term *inertia* culminating with use in Definition 3, *Principia* (1687), it is noteworthy that the working term throughout the rest of the first edition remains the *vis insita* just as in the early *De motu* tracts (Cohen 1980, pp. 315–326; Newton 1999, p. 98, n. 22).

The early *De motu* documents address certain questions about the motion of planets and comets in specific response to Halley's query for which the current term *vis insita* denoting an innate or natural power would have been appropriate. A gap in the use of the term *inertia* in a novel sense should be of little concern. If a target was intended, it would have been Hooke, not Descartes. These documents answer a question that Hooke could not solve and were not initially intended as preliminary to a large systematic work devoted to philosophical principles. Moreover, the section on comets is not set up to challenge Descartes directly but to resolve an age-old problem. It is not known what scale of treatment Newton contemplated in December 1684 and January 1685, but he expected to complete it within a few weeks. By late February 1685, Newton was lamenting the failure to finish his intended work because the examination of several things took more time than expected with much of it to no purpose (*Correspondence*, 2, p. 415). The failed efforts surely included very demanding but unfinished work on perturbation theory and comet orbits. Does *De gravitatione* fit in as one of the several things?

5 The issue of quantity of matter

As noted in Sect. 3, *De gravitatione* makes use of the concept of quantity of matter wherein Newton speaks of "densities, or ... the quantity of matter contained in equal spaces." The concept shows up again in *De motu corporum*, Definition 1, "the *quantity of matte*r is that which arises from its density and bulk conjointly. ... This quantity I designate under the name of body or mass" (Newton 1962d, p. 241).[38] The usage in *De motu corporum* follows exactly the place Dobbs contends *De gravitatione* was written. Newton makes many early uses of terms such as bulk, body, and density. If there were no uses of "quantity of matter" between *De gravitatione*, given an early date, and *De motu corporum* the problem could be more serious than a gap an early date is said to leave in the use of the term i*nerti*a. Several possibilities exist.

De motu corporum in gyrum introduces density as part of Hypothesis 1, "Resistance ... is as the speed of the body and the density of the medium conjointly."[39] Since it was directed at Halley and his circle no further explanation of density may have been need. The final Scholium in the text treats the motion of projectiles in air which is assumed to be homogenous and gravity acts uniformly in parallel straight lines. The resistance of the air is to be determined using a small test ball and scaled to any other desired size under the assumption that resistance is proportional jointly to the speed and surface area of the sphere. Since only one homogeneous medium is involved, again there is no need to consider variation in density or quantity of matter. The more general point is stated in Law 5 (originally Hypothesis 5) of the emended version, *De motu corporum in fluidis*, "The resistance of a medium is as the density of the medium and as the spherical surface of the moving body and its velocity conjointly."[40] Density is not further defined. Furthermore, Newton argues "Quicksilver resists strongly, water far less, and air still less. These mediums resist according to their density, which is almost proportional to their weights and hence (I may almost say) according to the quantity of their solid matter." (Newton 1962e, p. 286). [41] The term *materiae crassae* translated as

[38] *Quantitas materiae est quae oritur ex ipsius densitate et magnitudine conjunctim....Hanc quantitatem per nomen corporis vel massae designo* (Newton 1962d, p. 239).

[39] *Resistentiam ... esse ut corporis celeritas et medij densitas conjunctim* (Newton 1965b, p. 257).

[40] *Resistentiam medii esse ut medii illius densitas et corporis moti spherica surperficies et velocitas conjunctim* (Newton 1962e, p. 243).

[41] *Valide resistit argentum vivum, longe minus aqua, aero vero longe adhuc minus. Pro densitate sua quae ponderi fere proportionalis est atque adeo (poene dixerim) pro quantitate materiae suae crasse resistunt haec media* (*ibid*, p. 261).

"solid matter" might well be thought of in its literal sense as "thick matter" in contrast to thin or tenuous aether, the resistance of which by this time Newton has proven to be non-existent or extremely small.

The idea of quantity of matter persists in *De motu corporum in medijs regulariter cendentibus* including some definitions once considered part of *De motu corporum* [42] Definition 6 indicates, "The density of a body is the quantity or bulk of matter compared with the quantity of space occupied."[43] Definition 7 continues the use of "quantity or bulk of matter" and goes on to indicate, "when the oscillations of two equal pendulums with bodies of the same weight are counted, the bulk of matter in both will be reciprocal to the number of oscillations made in the same time [in the same medium]."[44] This result is said to be confirmed by experiments with a variety of materials, such as gold, silver, lead, glass, sand, common salt, water, lead, and wheat. If an early date is accepted for *De gravitatione*, then it cannot be said that Newton totally neglected the idea of quantity of matter before it reappeared in *De motu corporum,* Definition 1.

6 A further look at *gravitas*

The definition of *gravitas* in my revised translation reads as follows:

> Definition 10. Gravity is a force imparted to a body impelling it to descend. Here, however, by descent is not only meant a motion towards the center of the earth, but also towards any point or region, or even from any point. In this way if the *conatus* of the aether gyrating about the sun to recede from its center be taken for gravity, in receding from the sun the aether could be said to descend. And so by analogy, that plane should be called horizontal that is directed opposed to the direction of gravity or *conatus*.[45]

The rest of the definition indicates that *gravitas* and other force related concepts can be reckoned according either to intension or extension.

According to Definition 9, pressure is transmitted to the remotest parts of any body, whether hard, soft, or fluid, only by contiguous parts acting upon one another at a point or surface of contact (Newton 1962b, p. 148; 2004, p. 36). Definition 10 treats heaviness or gravity in a fluid medium in which, according to the hydrostatic principle, pressure acts in any direction so that concepts of up and down have no particular significance. The definition has the sense of universality in that the terrestrial

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[42] CUL Ms. Add. 3965.5, f. 25v, see Herivel (1965, pp. 316, 319); Cohen (1971, p. 93).

[43] *Densitas corporis est quantitas seu copia materiae collata cum quantitate occupati spatij* (Math Papers, 6, p. 189).

[44] *Pendulis aequalibus numerentur oscillationes corporum duorum ejusdem ponderis ex copia materiae in utroqz erit reciprocè ut numerus oscillationum eodem tempore factarum* (*ibid*, 6, p. 190).

[45] *Gravitas est vis corpori indita ad descendendum incitans. Hic autem per descensum non tantum intellige motu versus centrum terrae sed et versus aluid quodvis punctum plagamve, aut etiam a puncto aliquo peractum. Quemadmodum si aetheris circa Solem gyrantis conatus recedendi a centro eius pro gravitate habeatur, descendere dicetur aether qui a Sole recedit. Et sic analogiam observando, planum dicetur horizontale quod gravitatis sive conatus determinationi directe opponitur* (Newton 1962b, p. 114).

meaning of *gravitas* is generalized to *gravitas* pressing in every direction to and from points and regions. In a fluid there is no problem in imagining action from one point to another, but what does Newton mean by region? Perhaps he is thinking abstractly where a region represents a "determined quantities of extension" such as a planet or an individual star. Although Newton is silent on the matter, according to this definition, planets that stay in orbits must have a force imparted to them which balances the endeavor of aetherial particles gyrating about the sun to recede from its center and which also constantly deflects the endeavor of a planet to proceed tangentially in a straight line. Whatever the case, *gravitas* is depicted as a property of gyrating celestial matter as well as a property of terrestrial bodies, a further indication that Newton has moved on from Descartes' concept but not reduced aether almost to nothingness as Dobbs suggests.

Böhme (Newton 1988, pp. 99–100) thinks the definition of *gravitas* could be connected with an intrinsic "gravitation upward," a phrase found in More (1676: Preface: f. 1 *passim.*) when commenting on the idea as found in Hale (1673, pp. 10–11). While More (Preface: ff. 10–2) finds the term "is not destitute of all good ground," he considers his view of a "Spirit of Nature" as the agent "far less obnoxious." But the underlying mechanics of *De gravitatione* is connected to laws of impact, reflection, and perseverance. Even if Newton considers the force that drives a body downward as put there by God it would be by direct action rather than an intermediary.[46]

Henry (2011, p. 24) contrasts the pressure by contact concept in *De gravitatione* with the much richer concept in Newton's tract *De aere et aethere* (Newton 1962c, pp. 223–224) which he believes was written around 1679. That document links pressure to actions at a distance and the agitation of particles by heat, making it highly probable that *De gravitatione* was written first. A strong point, although one could counter that for different purposes Newton might well have chosen ideal models of gases and fluids that happen to be incommensurable.

Less successful is Henry's argument (2011, p. 24) that Newton's definitions of hard and fluid bodies do not invoke concepts of atoms and inter-particulate forces which characterize much of the writings of the 1670s. The argument is suggestive but not conclusive. These definitions relate to abstract constructs. Newton (1962b, pp. 151–152) states clearly the definitions were crafted after the manner of geometers even though nothing actually exists that is absolutely or uniformly fluid or solid. Moreover, the discussions of fluid resistance in the early *De motu* documents make no allowance for inter-particulate forces.

Henry (2011, p. 24) goes on to suggest the definition of *gravitas* is compatible with if not actually derived from Descartes' account and is out of keeping with Newton's views developed after the controversy with Hooke in 1679. Kollerstrom (1999, p. 333) makes much the same point with further connections to the Trinity notebook. Clear roots are found in Descartes' Principles, Part 4, articles 20–27, according to which gravity or heaviness is limited to terrestrial bodies. The parts of terrestrial matter do not naturally cohere but are pushed together by surrounding celestial matter acting equally on all sides. As the gyrating particles of celestial matter recede from

[46] McGuire (1968, p. 156) notes Newton prefers direct action by God over action by a world soul.

the center they stream around the particles of terrestrial matter, displacing them and driving them downward.[47] Newton's account slides over the exact mechanism of displacement making it difficult to connect with ideas expressed at different times. But Newton has eliminated the difference between up and down giving new meaning to down in a fluid medium. More significantly, Newton has a much more general notion than Descartes who finds the cause of gravity in celestial matter but limits the effect to terrestrial matter.

Kollerstrom (1999, pp. 333–337) emphasizes the difference between this account of *gravitas* and passages in Newton's Hypothesis of Light according to which celestial matter has a tenacious and gummy nature which may cause not only the descent of terrestrial matter but also may keep the planet from receding further from the sun (Newton 1958, p. 181). Kollerstrom make a significant point but one which neglects Newton's discussion of celestial matter in the early 1680s that do not posit such properties.

Henry (2011, p. 24) notes the much more explicit treatment *De motu corporum in gyrum* where Newton identifies gravitas as one kind of centripetal force. Its use is restricted to uniform gravity acting on projectiles conjoined with progressive horizontal motion or with an inverse square relation force extending to the moon. References to *gravitas* are deleted in other instances and replaced by terms such as *vis centripae* and *vis centripetae circumsolaris*. More directly, Newton indicates planets are retained in their orbits only by some kind of centripetal force, "whatever it might be." Although Newton undoubtedly believed that force to be *gravitas*, this rhetoric avoids unnecessary controversy. If *De gravitatione* were written later, then its treatment of *gravitas* that shows only the influence of Descartes would be out of place. Nevertheless, aetherial mechanisms, rotating celestial matter, and endeavors to recede lurked in Newton's mind for a long time.

Newton may not have totally discarded the notion of a mechanical aether until at least mid-1685. His propositions on comets include rotating celestial matter along with a proposition indicating the sun is nearly in the focus of a comet's trajectory, a possible reference to perturbation that may post date *De motu corporum in fluidis*.[48] Furthermore, an early draft of a portion of *De motu corporum liber secundus*, *Principia's* original book 2, intended as a popular treatise on the "System of the World," still refers to the solar vortex, although perhaps as a dig at Descartes' theory of comets, only to be replaced in another unused passage by circumsolar force (CUL Ms. Add. 3965.11, ff. 175r–176r). Moreover, the endeavor of planets or other bodies to recede from the center of their orbits is found in the *Principia* (1687, p. 8; 1999, p. 411) and in an augmented discussion of centripetal force first inserted much later in Newton's interleaved copy of the first edition (Newton 1972, 1, p. 42; *Principia* 1999, p. 405). And when confronted with certain problems such as the ascent of comet tails he invoked an explanation involving celestial aetherial matter that becomes entangled

[47] See Graukroger (2002, pp. 165–156).

[48] See Ruffner (2000, pp. 259–277) revised in Ruffner (2010, p. 442). Whiteside and I discussed this document at length when I first retrieved the document in 1963. At the point of time, we both thought the propositions might have been Newton's musings after the close of correspondence with Flamsteed in April 1681, more or less as Newton was said to have done with the orbital propositions following his correspondence with Hooke a year earlier. Whiteside (1970, pp. 14, 18, n. 42; 1991, pp. 17, 45, n. 43). Although I now find indications of a much later date, as here, the matter is still open for others to examine in detail.

with the particles which compose a tail and carry them away (*Principia*, 1687, p. 505; 1999, p. 925). Nevertheless the use of endeavor to recede instead of centripetal force in *De gravitatione* is the strongest argument offered by Henry and Kollerstrom for an early date, or at least one before the *De motu* tracts. The case can be strengthened by invoking the numerical measure of resistance, the basis of other experiments considered here, and fresh considerations of key definitions.

7 Conclusion

The narrative that led to the wide scale acceptance of a late date has serious problems, starting with failure to examine the numbers. It seems fairly obvious that by comparing their contents, *De gravitatione* was drafted before the "new" pendulum experiment, the *De motu* tracts, and most likely Halley's visit. Newton had considerable difficulty recreating the basic Kepler-motion solutions followed quickly by concern about the effects of perturbation that show the limitations of those solutions. It is also clear he had become fully engaged in a revival of his work on the comet of 1680. These were incredibly difficult issues that Newton was determined to get to the bottom of. It does not seem likely that he would also rush to prepare what was originally intended as an elementary treatise on hydrostatics. If I am right, the chronologies and document sequences of Dobbs or others who place it as part of the process of composing the *Principia* fall apart. Placing it earlier in 1684 as the aftermath to thoughts on the fluidity of the heavens in work on *Philosophicae origines* is also problematic. The main difficulty is Newton presumably would have been seeking to prove that celestial aether has little or no resistance, a task not adequately treated in *De gravitatione* and only by way of a digression. Moreover, we would expect to find Humphrey Newton's handwriting in such a relatively polished effort. Indeed would he have taken time to bring together various rougher drafts as Dobbs and others suggest? Perhaps there is a slim possibility for composition before Humphrey's arrival about the time he started work on reviving the ancient truths, but some documentation would be needed.

The inconvenient truth about Newton's manuscripts is that given a nearly lifelong interest in certain major themes they can be arbitrarily sorted in different sequences and seemingly plausible stories invented, without strong hooks to datable material. Perhaps independent reasons can be developed to establish a late date for the invocation of the term *inertia.* Lacking such reasons, Newton's lectures on hydrostatics in 1671 noted by Feingold (2004) offer one of the most plausible early dates and purposes. Unfortunately, the only evidence we have for these lectures is a mere mention in a document discovered by Feingold (personal communication, Bodleian Library Ms Smith 8, f. 147). Henry (2011) also offers an attractive new beginning, with further arguments yet to offer. Elements of Dobbs' (1991) analysis also remain in play. Different times and circumstances are not ruled out, however. Sources sufficient for the composition of *De gravitatione* were available by 1668 or 1669. The particular sources Newton used and when he focused on various items is another matter that needs renewed scrutiny. And if the document is a compilation, then there needs to be explicit distinction between older and newer materials. The fate of the date remains open.

Acknowledgments This article draws from a lecture based on an earlier version of the paper presented on April 1, 2011 for a seminar at the Department of Philosophy, the University of Western Ontario. I am grateful to Dr. William Harper for the opportunity provided and to the warm reception from those in attendance. I dedicate this work to Rupert Hall who was my earliest mentor and with whom I was able to discuss some of these ideas shortly before his death. Niccolo Guicciardini provided useful comments.

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