THE NEWTONIAN REVOLUTION – Part One

Philosophy 167: Science Before Newton's *Principia*

Class 13

Newton's *De Motu Corporum in Gyrum* (i.e. Version 1)

December 2, 2014

Philosophy 167: Science Before Newton's PRINCIPIA

Assignment for December 2

Newton's De Motu Corporum in Gyrum, Version 1

Reading:

Correspondence between Hooke and Newton, 1679/80, from The Correspondence of Isaac Newton, Volume II, 1676-1687, ed. H. W. Turnbull, especially pp. 309-313.

Newton, Isaac, "The Motion of Revolving Bodies," from Herivel, The Background to Newton's Principia, pp. 277-292.

Wilson, "From Kepler's Laws, So-Called, to Universal Gravitation: Empirical Factors," pp. 147-160.

Questions to Focus On:

1. What question did Hooke ask Newton to address with his "excellent method"? To what extent does Newton answer this question in De Motu? Does he use his "excellent method"?

2. The version of the tract De Motu that was sent to the Royal Society proceeds from four announced hypotheses. What is the basis or source of each of these hypotheses?

3. Precisely what conclusion or conclusions can be drawn from De Motu about Kepler's area rule? Do Newton's results support Kepler's suspicion that the area rule and the ellipse go together?

4. Precisely what conclusion or conclusions can be drawn from De Motu about Kepler's claim that the planets and their satellites move in elliptical orbits?

5. Precisely what conclusion or conclusions can be drawn from De Motu about the claim that the periods of the planets and their satellites about their respective principals vary as the 3/2 power of their mean distance from their principals?

6. The last three Problems of De Motu concern projectile motion and its constituents under various assumptions contrasting with those of Galileo. How do these three problems fit into the rest of De Motu -- e.g. do they even belong in it?

7. What claims, if any, does De Motu make about celestial physics -- i.e. about the physics underlying planetary orbits?

Newton's *De Motu Corporum In Gyrum* (i.e. Version 1)

I. Background To *De Motu Corporum in Gyrum*

A. Newton's Work in Mechanics Before De Motu

1. Newton's unpublished work in mechanics of interest to us not as a contribution to the growing field, but because it reveals influences on him -- in particular, ones that did not affect Huygens comparably
2. Much of Newton's work before 1673 superficially parallels Huygens's -- a theory of impact, a theory of uniform circular motion, and derivations of the key properties of cycloidal pendulums
    a. In contrast to Huygens, however, no growing theoretical network at all, and no pursuit of increasingly high quality evidence
    b. Newton, by comparison, is merely dabbling in mechanics, in response to some glaring deficiencies in Descartes' *Principia*, and, even more so, to problems associated with Copernicanism called attention to in Galileo's *Dialogue*
3. Newton was conceptualizing motion along the lines of Descartes, in which changes in direction or speed call for an explanation -- i.e. for external intervention of some sort
    a. Uniform motion in a straight line maintained by an internal *vis*
    b. Changes in motion always involve an external cause, sometimes termed an impressed force
        (1) Huygens thought of forces as static -- the force in the string or pressing on a wall
        (2) Newton closer to Descartes in this regard
    c. No less than Descartes, Newton saw this as implying a *conatus a centro* in all curvilinear motion, a *conatus* that had to be resisted by some external force
    d. This is not a Galilean way of conceptualizing motion
    e. But with one notable exception -- the emphasis on impressed forces -- it is the way Huygens was conceptualizing motion
4. Newton had adopted some of Descartes' laws and principles, often with refinements, but he had rejected others, including conservation of motion, and added some of his own
    a. In addition to inertia, he had come to identify impressed force with change in motion, but along a single direction, thus in effect introducing a vectorial conception of force and motion
    b. Like Huygens, he had shown that the principle of conservation of total motion does not hold under impact of two objects
    c. He had asserted the principle that the center of gravity of a set of interacting bodies does not change motion as a consequence of their interactions -- a principle Huygens announced in 1669
5. Newton's interest in uniform circular motion was clearly prompted by his interest in "proving" Copernicanism -- something Huygens showed no interest in during this period
    a. His calculation of the ratio of the *conatus a centro* of the moon in its orbit to the force of gravity on the surface of the earth may have been intended to test the hypothesis that terrestrial gravity holds the moon in orbit

   b. Though it might have been intended to serve other purposes -- e.g. to establish Kepler's 3/2 power rule for bodies orbiting the earth, allowing then for a decisive argument against Tycho
6. Newton's treatment of the cycloidal pendulum is the one place in which he shows an appreciation for the way in which Huygens had extended Galileo's theory of free fall, in the process developing approaches that were yielding much higher quality evidence
   a. A full appreciation of Huygens's achievement may have come only upon reading the *Horologium Oscillatorium* in 1673/74
   b. But, given Newton's general insistence on maintaining a sharp distinction between conjectural hypotheses and experimental certainty, he surely would have noted the evidentiary implications of Huygens's work

B. The Correspondence with Hooke in Late 1679

1. The six-week correspondence, which Hooke initiated at the end of 1679, came at a time when Newton had little reason to be interested in mechanics
   a. He had been concentrating on chemistry and alchemy for the prior few years, after "renouncing" philosophy to Oldenburg
   b. And he had just returned to Cambridge following the death of his mother in June and the settling of her estate
2. Hooke's initial letter was part of an effort he was undertaking, as the new Secretary of the Royal Society, to solicit interesting papers for the society -- a task which Oldenburg had performed very well from the outset of *Phil. Trans*., which at that time had ceased publication
   a. Included in the list of topics that Newton might want to address was Hooke's hypothesis "of compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards the centrall body" -- NB: two *motions*, not tendencies
   b. Other topics included what we now call Hooke's law and Picard's method for determining precise longitude differences
3. Newton's almost immediate reply instead focused on another topic -- a novel experiment to demonstrate the motion of the earth (Copernicanism again) and with it a conjectured solution to an old problem
   a. An object falling from a height should show an eastward displacement (owing to what became known in the 19th century as Coriolis effects)
   b. The path such a falling body would follow, if it continued on toward the center of the earth, would be a spiral of the sort shown in the figure (see Appendix)
4. Hooke's Dec 9 reply reports the lively interest shown in the idea of the experiment at the Dec 4 meeting of the Society, and then objects to the spiral, claiming instead that the path would resemble an ellipse
   a. A spiral only if medium resistance effects disturbed it from its roughly elliptical path

    b. And adds, correctly, that a southward as well as an eastward displacement will occur if Newton's experiment conducted at the latitude of London
5. Newton's almost immediate reply concedes that the path will not be a spiral to the center, but -- supposing gravity to be uniform -- it will "circulate with an alternate ascent & descent made by its vis centrifugal & gravity alternately overballancing one another"
    a. Newton had clearly put some more careful thought into the solution than he had in writing the initial letter, but the trajectory is still not quite the one he derives rigorously in the *Principia*
    b. The idea of alternately overbalancing one another, in a kind of dynamic exchange, undoubtedly comes from a model proposed by Borelli in 1666 to account for any non-circular motion of Jupiter's satellites (Whiteside, p. 1; also in Notes for Class 11)
    c. The multi-lobe figure Newton draws (see Appendix for drawing) is striking first because Newton appears to have drawn the first segment, down to a point where nearest the center, then inverted the sketch and traced it to get the second segment
    d. Michael Nauenberg has argued that he most likely used a curvature method to draw the first segment, with the retracing to get the second producing a somewhat excessive angle (vs. the correct 207.8 deg for projectile motion under a constant centripetal force in the *Principia*)
6. Hooke's reply of Jan 6 grants Newton's solution, but rejects the uniform gravity assumption on which it is based, instead pressing an inverse-square gravity assumption (invoking Kepler as grounds)
    a. Rather than asserting a roughly elliptical shape again, Hooke argues that the exact determination of the curve "will shew the error of those many lame shifts made use of by astronomers to approach the true motions of the planets with their tables."
    b. Though he concedes both that gravity will not hold as the inverse square all the way to the center of the earth, and he remarks on the idealization of treating bodies as points
    c. (Hooke may well have performed a graphical or numerical solution for the trajectory under inverse-square, and found it to resemble an ellipse, but was unable to demonstrate the exact curve)
7. The reply of Jan 6 also announces successful results for Newton's experiment performed out of doors -- three trials, with different eastward displacements, all no less than 1/4 inch
    a. Encouraging, but scarcely compelling results, as Hooke notes
    b. (With great care the experiment would provide evidence for the motion of the earth: in the absence of air resistance, fall from a height of 50 m at latitude 45 deg will yield a quarter of an inch eastward displacement; Hooke's irregular results from far lesser height are excessive)
8. The reply of Jan 6 ends with a discussion of the possibility that gravity may vary with altitude, as suggested by Halley's findings on St. Helena, and, if so, Huygens's proposed universal standard of length will be "spoiled"
9. Hooke's final brief letter of Jan 17, after not hearing further from Newton, reports additional successes with Newton's proposed experiment indoors, and once again implores Newton to address

the problem of the path under an inverse-square center-directed force:

"It now remaines to know the proprietys of a curve Line (not circular nor concentricall) made by a centrall attractive power which makes the velocitys of Descent from the tangent Line or equall straight motion at all Distances in a Duplicate proportion to the Distances Reciprocally taken. I doubt not but that by your excellent method you will easily find out what that Curve must be, and its proprietys, and suggest a physicall Reason of this proportion."

a. Method to which Hooke refers surely the calculus -- that is, Newton's method of fluxions

b. Well known in science circles that Newton had made a breakthrough in mathematics

C. The Comet(s) of 1680-81

1. Exactly what Newton discovered in 1679-80 in conjunction with this correspondence with Hooke we do not know, for no document remains

   a. Some have speculated that a paper he wrote for Locke after the *Principia* appeared that gives a more simplistic approach to ellipses and the inverse-square represents what he found earlier

   b. But there is no evidence for this, and Newton himself never gave any indication after the dispute with Hooke over credit for the inverse-square emerged in 1686

2. Newton's sole interest in mechanics in the period from 1679 to 1684 for which we have any documents involves an intense episode starting in mid-December 1681 and extending at least to April 1682 in which he was trying to work out the trajectory of the comet (or comets) observed in November 1680 and then December through February 1681

   a. Flamsteed, Newton learned, was proposing, on the basis of symmetry, that this seemingly pair of comets was one comet, seen initially moving from north to south and then from south to north

   b. This proposal initially included a claim that magnetic repulsion as it had approached the sun had caused the comet to be deflected through a large hairpin angle

   c. Included in Newton's letters to Flamsteed in response to this proposal is a denial that (1) that the magnetism of the sun could do this (heat cancels magnetic effects) and (2) that such a hairpin angle could happen through repulsion

   d. In its place Newton offers Flamsteed the alternative that the comet button-hooked around the sun, with the attraction of the latter producing the hairpin angle

3. This supporting proposal notwithstanding, Newton's own efforts on the comet of December centered on a straight line trajectory at uniform speed, with at most slight curvature either from the sun or the "vortex" around it

   a. In particular, the late James Ruffner's (2013) detailed analysis of Newton's recorded observations and calculations from 1681 show him struggling, with less than reliable data, to fit a straight line trajectory of the sort Wren had proposed in 1664 (see Notes, Class 11)

   b. So, while Newton offered suggestions to help Flamsteed with his proposal, he was throughout their correspondence at this time working on an approach expressly denying one instead of two comets

c. So, whatever Newton had concluded about planetary motion in conjunction with his 1679-80 correspondence with Hooke, it did not include universal gravity, or even solar attraction acting on comets in the same way as on planets

d. And he was still clearly wedded to the presence of an aetherial vortex around the sun

4. Newton breaks off the correspondence with Flamsteed insisting on two separate comets, partly because he had concluded from the observations available to him, after much effort, that the requisite high curvature is not consistent with them

a. Ruffner has shown that during this period Newton initiated a review of comets, turning first to Riccioli's *New Almagest* and then to Hevelius's review from the 1660s

b. This review led to his concluding,"this sways most with me that to make ye Comets of November and December but one is to make that one paradoxical. Did it go in such a bent line other comets would do ye like & yet no such thing was ever observed in them but rather the contrary"

c. In short, therefore, his position on comets in the 1679-1684 period seems close to Hooke's and not at all what subsequently emerged

5. There is one further single sheet document that on comets from somewhere between 1681 and 1685 in which Newton returns to the comet(s) of 1680-81

a. Ruffner (2000) originally dated this before Halley's visit in 1684, but subsequently (Ruffner, 2013) backed off this, allowing it to post-date Halley's visit

b. This document (ULC Add. 3965,14, fol. 613r and 613v) still has a fluid vortex around the sun, but allows for trajectories "oval" and "nearly hyperbola" in response to inverse-square gravitation toward the sun and each of the planets (see Ruffner, 2000 for details)

D. The Renewal of Interest in 1684 -- Halley

1. Newton's life forever changes as a consequence of a visit by Halley in August 1684 (see Halley letter in Appendix; but Newton years later said earlier) in which he informed Newton of the intense interest in London in inverse-square center-directed forces and the path a body will follow under them

a. Halley had himself noticed the inverse-square force implied by Kepler's 3/2 power "law" and Huygens's centrifugal force, and had entered into intense, somewhat contentious discussions with Wren and Hooke

b. Newton said the path is an ellipse, and when Halley asked how he knew, he said he had calculated it

c. He looked for the proof, said he was unable to find it, promised to send it to Halley forthwith

2. (Though Halley was still in his late 20s at this time, he may well have had a more complete overview of what was and was not known about orbital motion at the time than anyone else in England

a. He had himself worked in astronomy ever since his student days at Oxford, including efforts assisting Flamsteed from time to time

   b. Halley had carried out observations with Hevelius in Danzig during a visit in 1679, and maintained correspondence with him thereafter
   c. While in Paris in 1680-81he had assisted Cassini in observations of the comet of 1680-81, including an observation of the tail that would have had an impact on Newton's view of that comet had he known of it at the time
   d. While in Paris at that time he had contact with Academicians other that Cassini, and among other things seemed to have learned results from Richer's expedition to Cayenne, including the need to shorten the length of the seconds-pendulum)
3. Paget delivered *De Motu Corporum in Gyrum* to Halley in November, prompting a second trip to Cambridge in which Halley found Newton at work expanding the tract
   a. The trip was presumably prompted by the fact that the tract far exceeded Halley's expectations, for he clearly appreciated the deep implications it had for astronomy, probably more so than Newton himself may have appreciated them at first
   b. He gained permission to announce Newton's research at the December 10 meeting of the Society, officially registering De Motu with the Society, "securing his invention to himself till such time as he could be at leisure to publish it"
4. The version copied into the Register of the Royal Society is just like the one assigned, except that the Theorems and Problems are listed right after the Hypotheses, which is also the case with the one Herivel lables as "Version 2," which lacks the last two propositions
   a. The Fellows of the Society had access to this registered version from the time it was entered into the Register, presumably not long after the December 10 meeting
   b. Copies of this version presumably circulated, for some survive to this day, including the one in which the last two problems on air resistance are absent (though still listed)
   c. Herivel's "Version 3," *De Motu Sphaericorum Corporum in fluidis*, which by contrast makes three notable changes, remained in Newton's possession; we shall discuss it in the next class
5. *De Motu Corporum in Gyrum* is, of course, just the embryo out of which the *Principia* grew, but word of it and its forthcoming revise spread
   a. Flamsteed, for example complained in a letter to Newton at the end of December 1684 that he had not had access to it, and Halley noted the propositions on resisted motion to Wallis
   b. Leibniz is reputed to have rushed some work on the calculus into print, apparently fearing that the revise would include Newton's work on the subject
   c. And Hooke, who had boasted that he had solved the trajectory problem, was put in the awkward position of having to show his solution or withdraw his boast

E. The Conceptual Shift to Centripetal Forces

1. Hooke began intimating that Newton was plagiarizing his discovery of inverse-square gravitation, a suggestion that provoked intense anger from Newton when he learned of it in 1686

"For as Kepler knew ye Orb to be not circular but oval & guest it to be Elliptical, so Mr Hook without knowing what I have found out since his letters to me, can know no more but that ye proportion was duplicate quam proxime at great distances from ye center, & only guest it to be so accurately & guest amiss in extending yt proportion down to ye very center"

a. Newton ended up being forever ungenerous -- to say the least -- in giving Hooke credit for anything concerning the *Principia*

b. This raises the question, what exactly did Hooke contribute

2. The now-standard answer among Newton scholars is that Hooke produced a "conceptual shift" in the way Newton was viewing orbital motion -- a "conceptual shift" of the sort Kuhn emphasizes

   a. Before Hooke's letters, was thinking in terms of a centrifugal tendency to recede, counter-balanced by mechanisms that were a matter of conjecture -- e.g. pressure in Descartes' vortices

      (1) Newton had the differential geometry of curves in 1670s, and comments about the force associated with the curvature of an ellipse in the Waste Book (Slide 29, Class 12)

      (2) Could determine radius and center of curvature and hence so generalize centrifugal *conatus* normal to the curve (by means of progressing from one osculating circle to the next, as shown in the figure in the Appendix)

      (3) Three changing variables governing *conatus*: radius, direction to center, speed

      (4) Approach proved at time intractable: too many unknowns, in effect both the v^2/r force normal to the curve and a force tangential to it, leading from one circle to the next

   b. After the letters, was thinking in terms of motion governed by forces always directed toward some single point -- "centripetal forces" as he came to call them, honoring Huygens

   c. Quit thinking about what mechanism was balancing the centrifugal *conatus*, and instead think about what force was drawing it off a straight line: motion compounded from a rectilinear component and a centripetal (accelerative) component, just as Hooke had proposed

   d. But in general not orthogonal to one another, as they are in circular motion

   e. Conceptual shift simplifies matters in the case of non-circular trajectories, as we will see below

      (1) Removes a degree of freedom in the sequence of circles tangent to the trajectory

      (2) Making problem far more tractable

3. Presumably what Newton did in 1679 was to come up with a derivation tying the inverse-square center-directed force hypothesis to an exact ellipse

   a. Some suspect the original derivation contained a fallacy, delaying him in submitting it to Halley

   b. But regardless, Newton thought that he had the solution to Hooke's problem in 1679

4. The question then is why Newton did not immediately attach the significance to this result that it deserved, as shown subsequently by Halley's reaction

   a. Wilson's suggestion: the result was based on the hypothesis of inverse-square, centrally directed forces, a hypothesis for which Newton at the time could not adduce adequate evidence

b. This would explain his interest in the comet of 1680-1, if only because it raised a question about whether comets move in the manner of planets

c. Another possibility: contrary to suggestion in Hooke's letter, planets trajectories turn out to be the very one astronomers were generally employing, namely the ellipse, and hence nothing new

d. Only with Halley's visit did he come to appreciate how much of a question there was about whether the ellipse was merely an approximation, and perhaps too learned enough more about comets to abandon his 1680-81 view of them

5. Regardless of why not earlier, by late 1684 Newton saw, perhaps with Halley's help, that the calculation of the ellipse represented a breakthrough

a. A cornerstone of this breakthrough was the discovery of using areas swept out by radii as measures of time

b. Newton, alone among those thinking about inverse-square center-directed forces at the time, had tied them to Kepler's area rule, which he knew of from Mercator's 1670 paper in *Phil. Trans*., and from Mercator's 1676 book as well

c. Perhaps too Halley, who had not long before spent a good deal of time observing comets with Cassini and discussing them with Hevelius, also changed Newton's mind about comets

6. Newton clearly did not get the idea of an inverse-square relationship from Hooke, since he had entertained it a decade earlier; and there is no textual or manuscript evidence that he had even thought of the hypothesis of *universal* gravity at the time he wrote De Motu, Version 1

a. Perhaps he was entertaining something akin to it, as Wilson says, but was unable to see any way of bringing decisive evidence in favor of it over various vortex-theory hypotheses that would explain the inverse-square proportion

b. Or perhaps he was still only at the level of thinking in terms of a hypothesis involving gravity-like inverse-square forces directed from celestial orbiting bodies toward their principals

c. An inverse-square relationship would not have seemed at all strange or unnatural to him since he had read Kepler's *Optics*, in which light intensity is shown to be inverse-square

II. Three Fundamental Initial Discoveries

A. Preliminaries: Definitions and Hypotheses

1. *De Motu Corporum in Gyrum* -- literally "On the Motion of a Body in a Closed Circuit" -- opens with three definitions

a. The first adds forever a new term to the technical vocabulary -- '*vis centripeta*' or 'centripetal force', named after and in direct contrast with Huygens's '*vis centrifuga*'

b. The second adopts a Cartesian conception of the force internal to a moving body by virtue of which it persists in its motion in a straight line

c. The third, on 'resistance', fails to point out that Newton is conceptualizing this as a force always in a direction contrary to that of the body's motion

2. These definitions are followed by four hypotheses -- not laws -- the first three of which require little comment
    a. The first is simply a stipulation: resistance effects are ignored in all but the last two propositions
    b. The second is a version of the so-called principle of inertia: its *vis insita* maintains a body moving uniformly in a straight line, unless it is impeded by something external
    c. The third gives a version of the parallelogram rule, this time for the distances covered as a consequence of two forces -- in contrast to two motions, as in Galileo and Huygens
3. The fourth hypothesis, absent from Newton's original manuscript, defines the effect a centripetal force has on motion: the space which a body, urged by any centripetal force, describes at the very beginning of its motion is as the square of the time
    a. I.e. over a brief time it is uniformly accelerated, regardless of trajectory, just as it is in free fall and in a circle
    b. In effect, hypothesis licenses approximating each infinitesimal arc of the trajectory by a parabola
    c. This is what Huygens had done in his original derivation of the law of the cycloidal pendulum
    d. Newton will continue to examine the basis for this claim as he proceeds beyond this tract
4. Not included here is the second law of motion as it is given in the *Principia*: the effect of a force is a change in motion, proportional to the magnitude of the force and in its direction
    a. At least at one point Newton will invoke discontinuous impact type forces that produce a net change of motion
    b. But he shows no sign here that doing so requires another hypothesis, or that there is some significant distinction between the continuous forces invoked in the fourth hypothesis and the discontinuous ones invoked in e.g. Theorem 1
    c. Comparing Newton's hypotheses here with Huygens's at the beginning of *Horologium Oscillatorium* shows much in parallel (see Appendix)
5. Version 2 includes two inserted lemmas used subsequently, both without proof, but treated as established by others
    a. If A:(A-B) = B:(B-C) = C:(C-D) = ..., then A:B = B:C = C:D = ...; this is relevant only to the results involving resistance
    b. All parallelograms described about a given ellipse are equal (in area) to one another – understood to be tangent to the end points of a pair of conjugate diameters; Newton's attribution of this is unclear, but it follows from generating ellipses by projecting circles
6. An important thing to notice is the absence of any mention of *mass* or of force in relation to mass
    a. Force throughout the work taken to be proportional to change of motion -- i.e. change of velocity for a single body
    b. I.e., in modern parlance this is an exercise in kinematics, talk of "force" notwithstanding, for forces until the last two problems amount only to acceleration and deceleration

B. Theorem 1: Kepler's Area Rule -- The First Key

1. The first key to the other results is the realization that a body orbiting in a plane under the influence of a centripetal force automatically satisfies Kepler's area rule
   a. I.e. the bodies describe, by radii drawn to the center in question, areas proportional to the times
   b. So that these areas can be used in what follows to represent times (geometrically)!
   c. Answers the question, which, if any, measure of time in uniform circular motion carries over to centripetally governed motion generally? Answer: not arc length or angle, but area
2. The proof proceeds in two steps, the first of which replaces the action of the continuous centripetal force by a series of discrete force-impulses
   a. By the parallelogram rule, such a force impulse at B will produce a motion BC compounded from Bc and Cc, the latter parallel to BS since the force is acting in the direction of BS
   b. But then triangles SCB and SBA are equal in area, since SCB is equal in area to ScB, and the latter is equal in area to SBA (same base, same heights)
3. The second step then says that the proposition follows from taking the triangles to be infinitely small and infinite in number, so that the force-impulses approach the action of a continuous force
   a. Whiteside has argued that this proof holds rigorously only so long as arc BF is infinitesimal, which in fact is the only application of the theorem in De Motu; Pourciau has singled out the missing assumption, which can be proved by modern methods: "Every motion generated by a centripetal force acting uninterruptedly for a given time is the limit of motions generated by centripetal impulses." (*Archive for History of Exact Sciences*, vol. 58, no. 4, 2004, p. 320)
   b. Newton and others generally took the proof to have shown that Kepler's area rule holds automatically under centripetal forces, which in fact is true
   c. Undoubtedly, neither Newton nor anyone else realized that the proof may be problematic -- that the action of a continuous force perhaps cannot be represented over non-infinitesimal arcs by the action of an infinity of force-impulses
4. The main thing to notice is how easy the first part of the proof is -- raising questions about why no one had noticed it before
   a. Only suggestion I can offer is that no one had ever put the two ideas -- centrally directed force and Kepler's area rule -- together
   b. Streete and Wing did not adopt Kepler's area rule, so maybe it never occurred to Hooke or even to Wren to put the two together, and Newton had no earlier reason to do so since he was thinking in terms of *conatus a centro* and not centripetal *vim*
   c. Newton probably did so precisely because he was looking for a suitable geometric measure of time in order to determine the exact trajectory in the problem Hooke had set him (see Appendix on question of how to generalize from uniform circular motion)
      (1) Need some geometric representation of time, and cannot use arc length or angle

(2) Natural, given Kepler Horrocks,and Mercator to look to see if area will work

5. The result, taken at face value, is of course of profound significance
   a. No physical mystery to Kepler's area rule, for it holds under a comparatively weak condition: all forces -- all departures from uniform motion in a straight line -- directed toward a common point throughout the trajectory, with the magnitude of the force irrelevant
   b. Though the converse not proved here, the result does suggest that the area rule holds exactly only if this condition is satisfied, for forces acting in other directions will clearly alter the areas
   c. A further implication: if area rule holds regardless of the force rule, then the latter must determine the particular trajectory -- i.e. given the trajectory, infer the force rule

C. Theorem 2: Forces in Uniform Circular Motion

1. This theorem simply gives Newton's earlier "*conatus a centro*" result for uniform circular motion, but now in a slightly different form: centripetal force varies as arc length^2/r = $(\theta*r)^2/r$ in a given time
2. Of course, in the case of a circle obeying the area rule, the force is constant -- i.e. the force does not vary around the circumference
3. The theorem gives a determination of the relative magnitude of the force for different uniform motions in different circles, not of the variation of force along a single trajectory
4. The proof is along the same lines as that given in the ca. 1669 tract, though rephrased for an arc of the circle, comparing two circles in the manner of Huygens
   a. Given two circles with arcs BD and bd in the same time, then by their own internal force would instead describe straight lines BC = arc BD and bc = arc bd in this time
   b. Then, for this time the centripetal forces are as CD:cd
   c. (Note peculiar orientations of CD and cd – see DTW note, p. 38, comparing Newton's choice of orientation of CD and cd with Huygens's at the time unpublished choice)
   d. From Euclid, Proposition 36, Book 3, latter ratio is equal to $BC^2/CF : bc^2/cf = BD^2/CF/2 : bd^2/cf/2$
   e. Result then follows when time taken to be infinitesimal
5. Five corollaries are then given, the first four of which are essentially the same as results announced, without proof, at the end of Huygens's *Horologium Oscillatorium*, while the fifth uses these results to tie Kepler's 3/2 power rule applied to uniform circular motion to inverse-square centripetal forces
   a. E.g. force as v^2/r, as r/P^2 etc.
   b. Corollary 5 now states that inverse-square force is both a necessary and sufficient condition for the 3/2 power rule to hold for uniform circular motion
6. In the Scholium that follows Newton simply asserts that Corollary 5 holds for the planets and for the satellites of Jupiter and Saturn
   a. He subsequently withdraws the claim about Saturn when Flamsteed expresses an inability to detect the two additional satellites that Cassini had recently announced

b. And notice that he does not assert it for the moon, for the same reason that he subsequently withdraws it for Saturn: need multiple orbiting bodies to establish it empirically

c. Of course, in stating it here, he is treating the orbits as circular, and hence he must mean that it holds to the extent that their orbits approximate circles and their motions uniformity

d. While the trajectories do closely approximate circles, the motions are far from uniform: ratio of max v to min v = $(1+e)/(1-e)$, which amounts to 1.2/0.8 for Mercury and nearly 1.1/0.9 for Mars

7. No applications of the Theorem here of the sort in the earlier "Moon test": just a bare statement of a result that he knew from Halley had become widely recognized, even though no proof had been published for the v^2/r result Huygens had announced

D. Theorem 3: Force Variation along Arbitrary Trajectories: The Fundamental Enabling Result

1. Issue: what is the force rule, given a body moving along a curvilinear trajectory, with the force always acting in the direction of a single point S

   a. In contrast to theorem 2, which compared forces in two separate uniform circular motions, Theorem 3 concerns how the centripetal force varies in magnitude along a single trajectory

   b. In effect, then, the areal velocity is being treated as a given in this theorem

   c. Force here entirely in terms of change in velocity -- a kinematic result, in the manner of Galileo and Huygens, without reference to mass or bulk

2. The theorem states that the force varies as $1/SP^2 * \lim(QR/QT^2)$ as Q approaches P

   a. This theorem thus gives a geometric representation of the magnitude of the centripetal force, just as Theorem 1 gives a geometrical representation of time

   b. This for a single body moving along a curvilinear path under a centripetal force

3. The proof uses Theorem 1 and Hypothesis 4, with the former taken only over an infinitesimal arc:

   a. Given the time -- i.e. for a fixed time interval -- QR varies as the magnitude of the force (displacement over a given time is proportional to the magnitude of the force, just as in Huygens's solution for uniform circular motion)

   b. Given the force -- that is, for a given value of the force -- QR varies as time squared, via Hypothesis 4

   c. Therefore QR varies as force$*$time^2

   d. But time^2 as (area QPS)^2, which approaches $(QT*SP/2)^2$

   e. Therefore force varies as $1/SP^2 * \lim (QR/QT^2)$

4. A non-geometric formulation of this result was developed by Johann Bernoulli after 1710, using what we now call Taylor's Series (which was already known to Newton at the time of De Motu)

   a. Assume polar coordinates: r,θ

   b. Then $\lim(QR/QT^2) = 1/2 * (1/r + d^2/d\theta^2(1/r))$

   c. But then the force varies as $1/r^2$ times this limiting value

d. More precisely the force = $(k/r)^2$ times this, where k is Kepler's constant ($r^2 * d\theta/dt$), the areal velocity

5. The question this theorem answers is how the force -- i.e. the change of motion -- varies along non-circular trajectories dictated by centripetal forces -- i.e. when all changes of motion are directed toward a single point
   a. In the case of a circle so dictated, the force does not vary since equal areas entail uniform motion
   b. The key to obtaining it is the realization that time can be represented by the area swept out
   c. This is why I suspect that the question answered in Theorem 3 was posed first, leading to the question answered in Theorem 1; which then unlocked the door to everything else
6. Theorem 3 should be seen as a generalization of Theorem 2, employing essentially the same geometric construction to infer a measure of force from departures from inertial motion
   a. Old approach, using curvatures and Theorem 2 directly, had not yielded a well-behaved measure of force: two unknowns, the normal "force "and the tangential "force"
   b. Added constraint of directed toward a center reduces the number of separate unknowns
7. The corollary to Theorem 3 simply states that the result gives a means for determining the rule of force for any point along a given trajectory governed by centripetal forces
8. A subtle, but radical step has been taken with Theorem 3, for force is now being treated in the abstract, as a mere magnitude, divorced from any question of mechanism!
   a. I.e. in contrast to Huygens's treatment of the static centrifugal force in a string or on a wall
   b. Now just a mathematically characterizable force taken to be acting on the moving object
   c. In other words, Newton's talk of forces in the abstract here parallels his talk of rays in the abstract in his earlier work on optics (which people had objected to)

E. Problems 1 & 2: Two Applications of Theorem 3

1. The precise reason unclear why Newton included Problems 1 and 2, both of which involve direct applications of Theorem 3, but to questions that there is no immediately obvious reason to be asking
   a. At first glance mere mathematical curiosities, serving to illustrate the thrust of Theorem 3 before turning to its key application (but only at first glance)
   b. Notice in manuscript here and elsewhere, "*gravitas*" was replaced by "*vis centripeta*"
2. Problem 1: the rule of force for a body going through a circular arc under centripetal forces directed to a point on that arc
   a. Akin to Galileo's rejected claim in *Two New Sciences* that a circular arc all the way to the center of the earth
   b. Symbolically, this case has $1/r = 1/D * \sec(\theta)$
   c. Of course, a singularity at the center of force, so solution only up to that point
3. Solution: force varies as $1/r^5$, where r is distance from the force center

a. Because triangles ZQR, ZTP, and SPA are similar, $RP^2/QT^2 = SA^2/SP^2$, where $RP^2 = QR*LR$ (by Euclid's Prop. 36 again)

b. Applying Theorem 3, $\lim(QT^2/QR) = SP^3/SA^2$

c. Hence force varies as $1/SP^5$

4. (My guess is that Newton had first solved for the Ptolemaic eccentric circle, discovering that force varies as the product of two varying geometric magnitudes and then decided to present this result which follows from that one when the eccentricity becomes 1; see Appendix)

5. Problem 2: the rule of force for a body in an ellipse under centripetal forces directed to the center of the ellipse, not a focus

6. Solution: force varies with distance from center -- a linear relationship

a. Since an ellipse, $PV*VG/QV^2 = PC^2/CD^2$ (the counterpart to Euclid's Book 3 Prop. 36 for ellipses) and $QV^2/Qt^2 = PC^2/PF^2$ -- facts about conjugate diameters from Apollonius (derived in the Appendix)

b. Therefore, $PV*VG/Qt^2 = (PC^2/CD^2)*(PC^2/PF^2)$

c. But QR=PV and BC*CA = CD*PF, and in the limit 2PC = VG

d. Thus, $Qt^2*PC^2/QR = 2*BC^2 * CA^2/PC$ -- q.e.d.

III. The Results on Keplerian Elliptical Orbits

A. Problem 3: The Inverse-Square Rule of Force

1. Suppose now that the trajectory is an ellipse governed by centripetal forces aimed at a focus -- i.e. the converse of the problem Hooke posed in his letter

a. Solution turns on proving that $\lim(QT^2/QR)$ for any such ellipse varies simply as the latus rectum of the ellipse, and hence a constant

b. But then the force varies as $1/SP^2$ -- i.e. as $1/r^2$, or more fully as $1/(L*r^2)$

2. Newton's proof is comparatively intricate, turning on a number of properties of ellipses

a. EP = AC, the semi-major axis (something Newton "discovered" in solving this problem)

b. By a complex chain of ratios,

$$L*QR/QT^2 = (2*PC/GV)*(M/N), \text{ where } M/N = QV^2/QX^2$$

(1) Using $(GV*VP/QV^2) = CP^2/CD^2$ -- as above

(2) And using lemma ii to obtain 4*CD*PF = 4*CB*CA

c. But as Q approaches P, (2*PC/GV) and (M/N) approach 1, so that $(SP^2 * QT^2)/QR = L * SP^2$

3. Newton's solution is more general and perhaps easier in analytical form, as developed by Johann Bernoulli around 1710

a. Equation for any conic: $1/r = A + B*\cos(\theta)$, where $e = B/A$, $B = CS/CB^2$

b. An ellipse so long as $0 < B/A < 1$; a parabola when A=B; and a hyperbola when $B/A > 1$

c. $\lim(QR/QT^2) = 1/2[(A+B*\cos(\theta)) + d^2/d\theta^2 (A+B\cos(\theta))] = A/2$

e. But the latus rectum is just 2/A

f. Therefore, for any conic section trajectory governed by centripetal forces aimed at the focus, the force along the trajectory varies as $1/r^2$

4. The Scholium immediately following Problem 3 has mystified people ever since -- vide Wilson

"The major planets orbit, therefore [sic!], in ellipses having a focus at the centre of the Sun, and with their radii drawn to the Sun describe areas proportional to the times, exactly as Kepler supposed"

a. To infer that the area rule holds exactly, need to establish that the only forces acting on the planets are aimed to the center of the sun

b. To infer that the orbits are exactly elliptical, then need the converse of Problem 3 -- i.e. the problem Hooke originally wanted solved, and Newton told Halley he had solved: if a body is governed by inverse-square centripetal forces, then its only closed circuit trajectory is an ellipse

c. And even then need the claim that the centripetal force is inverse-square, something that has so far been shown for the planets only under the assumption of uniform motion in perfectly circular orbits

d. The seeming logical lacuna persists right through the first edition of the *Principia*, to be filled in the second edition following some pointed comments by Johann Bernoulli

5. My suspicion is that Newton was here engaging in a type of evidential reasoning (hence appropriate to a Scholium) that he employs widely (provoking much controversy) in Book III of the *Principia*

a. From the rough phenomena alone -- e.g. circular orbit -- can conclude from Theorem 1 and 2 that, at least to a first approximation, an inverse-square centripetal force aimed at the sun is the dominant factor keeping the planets in their orbits

b. Problem 3 shows that the next level of refinement of the phenomena, to a roughly elliptical orbit sweeping equal areas vis-a-vis the focus, requires no revision of that conclusion

c. Licenses the inference that, at the second level of approximation, the planets obey Kepler's first two rules exactly, for nothing beyond forces inferred from the first approximation is required for them to do so

6. Whether such evidential reasoning is legitimate -- or more to the point, exactly what conclusion is to be drawn from it -- I leave as an open question for now

a. But the interpretation I offer at least has the virtue of not saddling Newton with a glaring blunder in logic

b. I find the idea that Newton would fall trap to such a blunder in simple 'if-then' reasoning beyond belief

B. Theorem 4: The Keplerian 3/2 Power Rule

1. Theorem 4 provides a generalization of one half of Corollary 5 of Theorem 2: Kepler's 3/2 power rule holds for bodies in elliptical orbits governed by centripetal forces aimed at (a common) focus when those forces vary as $1/r^2$ -- i.e. P^2 varies as a^3

a. Contrast the inverse-square claim here with the one in Problem 3, where the claim applied only to variations along a single trajectory
b. Here the inverse-square claim is holding throughout the space with the focus at its center
2. Geometric proof achieved by piggy-backing the case of an ellipse on that of a circle, and then using Corollary 5 of Theorem 2
a. Consider the special case in which P is at the end of the minor axis, and compare with the circle of radius SP in which P is governed by the same inverse-square force
b. In the limit $L*QR = QT^2$ and $2*SP*MN = MV^2$
c. Since forces are the same, QR = MN, $QT/MV = \sqrt{(L/2*SP)} = PD/2*SP$ since AB=2∗SP and $L=PD^2/AB$
d. But then the area SPQ/area SPM = QT/MV = PD/2∗SP = $\frac{1}{4}*\pi*AB*PD/\pi*SP^2$ = area of ellipse/area of circle
3. But then by the area rule the time of PQ/period of ellipse = the area SPQ/the area of the ellipse, and analogously for the time PM in the circle
a. Hence, since the time of PQ = the time of PM, the total areas of the circle and the ellipse will be completed in the same times
b. The Theorem then follows from Corollary 5 of Theorem 2
4. In sum, if bodies moving in elliptical orbits are governed as required, then Kepler's 3/2 power rule holds exactly
a. Thus offers at least a qualified answer to the question whether Kepler's third "law" holds exactly, as Streete, following Horrocks, claimed, or only approximately
b. Any deviation from Kepler's rule indicates that the elliptical orbits are not governed exactly as the Theorem supposes
c. Licenses the inference that, at the second level of approximation, the planets (can) obey Keplerian motion exactly, for nothing beyond the forces inferred from the first (circular) approximation is required for them to do so
5. Theorem 4 also provides a basis for concluding that the 3/2 power rule is nomological -- i.e. it holds in all orbital systems governed by inverse-square centripetal forces
a. Recall that Kepler's account made the 3/2 power rule parochially contingent on specific planet densities, and hence an accidental generalization
b. Newton by contrast removes the main source of the parochialism, thereby opening the way for the 3/2 power rule to extend to bodies orbiting the earth
c. If this extension can be confirmed by showing that the centripetal force toward the earth is inverse-square, then have a decisive argument against the Tychonic system of the sort I suggested Newton was pursuing in the late 1660s

C. Scholium: Determining the Orbits of Planets

1. The Scholium following Theorem 4 in effect proceeds under the assumption that the planetary orbits are perfectly Keplerian in all three respects
    a. It does not as such presuppose that the planets are governed solely by an inverse-square centripetal force aimed at the center of the sun
    b. But the clear suggestion is that proceeding under strict Keplerian assumptions has gained added warrant from the preceding propositions and attendant scholia
2. The problem is to determine the elliptical orbits from observations, given the periods and the length of one semi-major axis -- e.g. the earth's
    a. One solution for the outer planets and one for the inner
    b. Both of which adopt Horrocks's and Streete's approach of first inferring the length Q of the major axis from the periods, taking Kepler's third "law" to be exact
3. In the case of the outer planets, given observations at A, B, C, etc., draw arcs of radius Q-AS around A, Q-BS around B etc., and their intersection point will determine the other focus
    a. This has the advantage of allowing a large number of observations to be taken into account, taking the mean value for the focus, thereby allowing inaccuracies in individual observations to tend to cancel one another
    b. Distances AS, BS, etc. via a method Newton attributes to Halley that presupposes the orbit of the earth (in fact, a method from Kepler's *Astronomia Nova*, showing that Newton had not read it, but unless Halley came up with the method independently, he may may well been familiar with the Kepler book); for Halley reference see *Phil*. *Trans*., 25 Sep 1676 or Rigaud
    c. Iteration, using an orbit of a planet to correct the orbit of the earth by this method, and vice versa, until achieve good agreement on the focus will help eliminate residual errors
4. In the case of the inner planets, same general idea, but using observations at maximum elongation to define tangents to the orbit of the planet about the sun
    a. Drop a perpendicular from Sun to such a tangent, then draw a circular arc of radius ½*Q about the point where the perpendicular meets the tangent, and the center of the ellipse lies on this arc
    b. Has the same advantage of allowing a large number of observations to be taken into account, with iteration as needed on the earth's orbit
5. An example of the way in which Newton, the mathematician, is not at all reluctant to think like an experimental scientist, looking for ways to exploit redundant data
    a. Methods are not historically significant in their own right -- not a major advance -- and Newton himself will shortly be concluding that the orbits are not perfectly Keplerian
    b. But still indicative of his thinking
    c. Jed Buchwald has concluded that this is one of the first places in history where averaging of

redundant data has been used as preferable to selecting the "best" observation, which was (with one exception) the practice followed in astronomy by Tycho and others following him

D. Problem 4: Determining Inverse-Square Trajectories

1. Finally, offers a geometrical solution to the problem of determining an elliptical orbit, given a known inverse-square force and an initial velocity
   a. A geometrical solution in which the magnitude of the inverse-square force is represented by a perfectly circular orbit of given radius and period
   b. And the magnitude of the velocity is represented by the length of the tangent PR, where PR:ρπ is taken in the ratio of the given velocity to the uniform velocity in the circle
2. Under these specifications, the latus rectum L of the ellipse is determined, making use of results in Problem 3
   a. But, given a focus and the tangent at P, know the direction of line PH on which the other focus H lies, and know the perpendicular distance to this line from S, SK
   b. A complicated series of algebra steps, exploiting properties of ellipses, then allows the length of PH to be determined, so that the other focus is established
   c. As is the transverse axis, for it is SP+PH
3. Newton here adds, without proof, that the construction will yield an ellipse so long as the initial velocity is not excessive, but it will yield a parabola when L= 2∗(SP+KP) and a hyperbola when the initial velocity is still greater -- i.e. when (SP+PH)/PH < 1
   a. In short, he fully realized that inverse-square centripetal forces yield different conic sections depending on the initial velocity, with $\lim(QT^2/QR)$ always L
   b. He may have even verified that the conic is uniquely determined -- no other curve can result
   c. Thus, the literal text of De Motu notwithstanding, he may have already solved Hooke's original problem, at least to his satisfaction, though we have no record of it now
4. As the attendant Scholium makes clear, the whole point of this problem is to allow the calculation of comet orbits under the assumption that comets are acted on by an inverse-square force directed toward the sun
   a. Can use planetary orbits to fix the strength of the inverse-square force "field" around the Sun (i.e. the strength of the inverse-square centripetal accelerative tendency around the Sun!): a^3/P^2
   b. And can obtain an estimate of the initial velocity from a sequence of observations curve-fitted to a straight line, though will have to iterate since do not know distance to the comet
   c. Specifically, take four observations to define a straight line, obtain velocity, now let inverse-square effect produce deviation from the straight line, then use comparison between observed and predicted longitude and latitudes to generate four new points, and repeat as needed
   d. Then use an approximate solution to Kepler's equation to determine the areal velocity

5. This method of determining comet orbits Newton soon discovers is inadequate, so that within weeks he has switched to another method, and even in the middle of 1686 he is still seeking an iterative method that will converge on a solution
   a. Notice how important he is taking the problem of comets to be and how resourceful he is in concocting approximate, iterative methods when faced with such a problem
   b. Notice also that Newton is openly speaking of the return of comets, via highly eccentric elliptical orbits -- something that may well have been fairly, if not totally, novel at the time
   c. Halley was the first to confirm this when in 1705 he used modified Newtonian methods to calculate the orbit of the comet of 1682, identifying it with the comet of 1531 and 1607 and predicting its return in 1758 (actually returned in 1759, after he died)
6. A further subtle, yet radical step has been taken in Theorem 4 and Problem 4 by tacitly concluding that any body at any point in space about a "force center" must experience an accelerative tendency toward this center of magnitude proportional to $[a^3/P^2]/r^2$
   a. What we now call an inverse-square centripetal acceleration field, with field strength (a^3/P^2)
   b. And no variation in either orthogonal angular direction
   c. De Motu now far removed from Huygens's restricted talk of force in his paper on uniform circular motion, though Huygensian in appealing to comets as a surprising, testable consequence
7. De Motu now also far removed from Newton’s view of comets expressed to Flamsteed in 1681!

IV. Some Extended Results on Galilean Motion

A. Problem 5: Vertical Fall Under Inverse-Square Gravity

1. One virtue of the tenuous solution to the problem of determining comet orbits is that it points to a means of addressing another problem -- free-fall under inverse-square acceleration
   a. I.e. instead of uniform acceleration (along parallel lines), treat the case of inverse-square acceleration toward a center, with the goal of determining *s* versus *t*
   b. In effect, addressing the problem of fall to the center of the earth, including the special case of direct vertical fall, on the terms Hooke posed
2. Newton ends up providing a fairly simple geometrical solution to a rather nasty nonlinear differential equation: ($d^2r/dt^2 = -k/r^2$)
   a. Suppose body has a small initial velocity perpendicular to the vertical, so that (just as Hooke said), its trajectory will be an ellipse APB with focus at S, the center of the inverse-square centripetal force
   b. Circumscribe the circle ADB about this ellipse; then the time of fall is proportional to the area ASP, and hence to the area ASD
   c. As the initial perpendicular velocity approaches 0, ASD will continue to remain as the time, but in the process the orbit APB will approach AB, B will approach S, and area ABD, now = ASD, will be proportional to the time

d. Therefore the distance AC for a given time will be defined by taking the area ASD=ABD to be proportional to the time, thus determining C

3. Solution sufficient to allow evaluation of the difference between uniform acceleration and inverse-square acceleration in free-fall, given the distance of fall in the first second at the surface of the Earth

   a. By stipulating a distance of fall -- e.g. 64 feet or 256 feet -- and comparing the ratios of the time between the first 16 feet and the total with uniform acceleration (2 sec and 4 sec) with the ratios of the corresponding areas within the circle ASD

   b. Difference in the ratios of the order of 10^{-6} over 64 feet and 10^{-5} over 256 feet

   c. In other words, the difference over testable distances of free fall lies beyond the precision with which times could be measured, and hence no *experimentum crucis* at the time

4. Scholium indicates that Problems 4 and 5 together provide solutions to projectile motion and vertical fall in the absence of air resistance, but under inverse-square centripetal rather than the uniform acceleration along parallel lines presupposed by Galileo

   a. Projectile in general not a parabola, but in the cases of interest to Galileo, an ellipse that approximates a parabola over the relevant distance when AC small compared to AS

   b. And vertical fall not uniformly accelerated, although when AC small compared to AS, it is approximately so

5. Thus, if we are willing to accept the claim that gravity is an inverse-square centripetal force, Problems 4 and 5 are telling us that Galileo's laws of free-fall and parabolic projection are merely approximations, indeed ones that do not hold even in the mean

   a. I.e. neither holds exactly in the absence of air resistance, nor would they hold exactly in the absence of other secondary effects, but they hold approximately to the extent that the inverse-square change in g remains negligible and the radius of the Earth is large

   b. Furthermore, the solutions to the two problems provide us means, as above, for calculating the magnitude of the error in the approximation for various cases!

6. In other words, Newton here managing to tie the theory into that of "natural" motion near surface of the earth in just the manner Huygens managed to tie his work on centrifugal force, impact, and the pendulum to this theory

   a. Newton is continuing an approach that Huygens has shown is very fruitful in yielding evidence

   b. Notice that he did not have to do so -- he could have stopped with Problem 4 without mentioning projectile motion and vertical fall under terrestrial gravity

B. The Moon Test Repeated in 1684: Confirmation

1. Notice that the Scholium ends with the assertion that "gravity is one species of centripetal force"

   a. The claim that gravity is a centripetal force is unproblematic

   b. But the same cannot be said of the claim that it is an inverse-square centripetal force -- what is the evidence for that

2. Although Newton provides no evidence for this claim about gravity in De Motu, it would not be presumptuous to think he had repeated the "Moon test" of the late 1660s, but now using current values
    a. No unqualified documentation that he had done so for a few more weeks, where the result appears in a manuscript that is an immediate successor to *De Motu Corporum in Gyrum* (i.e. *De Motu Sphaericorum Corporum in fluidis* -- Version 3, called by DTW the "augmented" version)
    b. But every reason to think that he would have done so in the fall of 1684, if he had not done so in 1680 (as Westfall says)
3. By 1684 Newton had various choices for the number of Paris feet in a degree of longitude -- e.g. Picard's value of 342,360, which Newton subsequently used, and Cassini's value of 342,366
    a. With Picard's value, the circumference of the earth, assuming a sphere, is 123,249,600 Paris ft, giving a radius of 19,615,783
    b. The period of the moon 27d 7h 43m, or 2.36058e6 sec, so that its angular velocity is 2.6617e-6 rad/sec
    c. Taking the distance to the moon to be 60 earth radii, then the acceleration of the Moon, $r*\omega^2$, is 8.33833e-3 ft/sec/sec -- i.e. the Moon falls 4.16916e-3 ft in 1 sec
    d. Dividing this number into Huygens's value for the fall in 1 sec at the surface of the earth -- 15.0833 ft, we get 3617.9 -- only 0.5% off a perfect inverse-square, well within known accuracy of the lunar horizontal parallax
4. So, a repeat of the "Moon test" with Picard's values for the radius of the earth rather than Galileo's would have been a great success
    a. From 4375 -- a 21.5% discrepancy -- in the late 1660s to 3617.9 -- a 0.5% discrepancy -- in 1684, clearly within the accuracy of the value for the mean distance of the moon
    b. Whatever Newton was looking for in the 1660s, he had surely found it by the end of 1684, though his doing so in no way depended on any of the new results in De Motu
5. Notice carefully, however, what I am taking the successful result to have shown
    a. Not just that the moon is held in place by terrestrial gravity
    b. But more so that terrestrial gravity varies inversely with the square of the distance from the center of the earth at least to the moon
    c. Since the moon is the sole body orbiting the earth, there was no basis at this juncture to draw the conclusion that the inverse-square forces governed its motion without the "Moon test"
    d. And there was no other basis for concluding that terrestrial gravity diminishes in accord with the inverse-square rule

C. Problems 6 and 7: Resistance and Galilean Motion

1. Resuming with the text of De Motu, Newton next turns to the problems of uniform and orthogonally uniformly accelerated motion, but allowing for air resistance proportional to velocity

a. Commentators have had some difficulty explaining why Newton thought these two problems were germane to the rest of De Motu, especially since gravity not treated as centripetal in them

b. Thus Whiteside, for example, suggests Newton was just putting on record a solution to a problem he had addressed unsuccessfully in 1674

c. (Huygens had solved the problem including the projectile problem with resistance proportional to velocity in late 1660's, but had withheld the result when experiments disagreed with it, indicating that resistance varies more closely as v^2 than as v)

2. The solutions themselves are at the cutting-edge of mathematics at the time, in effect yielding fully correct geometrical solutions to two differential equations, with g and k constant

$$d^2x/dt^2 = -k*dx/dt \quad \text{and} \quad d^2y/dt^2 = g - k*dy/dt$$

a. The modern solution to both has an exponential form, employing Euler's constant e (Euler was decades from being born)

b. The main difficulty Newton faced, once he had formulated the problems, was to find a geometrical relationship that could represent such an exponential relation between the independent variable t and the dependent variable s

c. I.e., in Newton's words, to find a geometrical relationship that can represent one variable's increasing (or decreasing) in geometrical proportion as the other increases in arithmetic proportion

d. For, as Newton says, the consecutive decrements in velocity must be proportional to the velocities, so that Lemma 1 applies, and this Lemma entails that $V_0:V_2$ as $[V_0/(V_0-V_1)]^2$, $V_0:V_3$ as $[V_0/(V_0-V_1)]^3$ etc.

3. Newton's approach is to use hyperbolas with rectangular asymptotes -- i.e. curves of the type $u*w$ = constant -- which define areas that increase in an arithmetic progression for a geometric progression along the abscissa, as was known from Napier, Huygens, and Mercator

a. Appropriate since integral is $u = c*\log w$ if $u = c/w$

b. The key then to understanding Newton's solution to these two problems is just to understand how the different variables are being represented in the geometrical constructions (see Turnbull, II, p. 460f, in the Appendix)

4. In the solution to Problem 6 time is being represented by the increasing area BADG, distance traveled by the increasing length AD, and velocity by the decreasing length DC

a. Newton's solution thus gives exactly the modern solution

$$x = (u/k)(1 - e^{-kt}) \qquad dx/dt = ue^{-kt}$$

b. For his curve represents $w*(a-x)=ab$, where AC=a, AB=b so that, taking A as origin and the initial velocity $u=a*k$, we obtain $t = 1/k*\log(a/a-x)$ and w [= $ab/(dx/dt)$] is proportional to the reciprocal of the velocity

5. In the solution to Problem 7, for the case of descent, the distance fallen in time represented by the area AB^2G^2D is represented by the area B^2E^2G, and in the further time represented by area $^2D^2G^2g^2d$, the distance represented by the area $^2G^2E^2e^2g$; and the velocity of descent by 0 at the outset, by the areas AB^2ED after the first time and AB^2e^2d after the second, with the area ABCH representing the terminal velocity
   a. Newton's solution thus again gives the modern solution in descent

$$y = (g/k)t - (g/k^2)(1 - e^{-kt}) \qquad dy/dt = (g/k)(1 - e^{-kt})$$

   b. For his curve represents $w*(c-v)=c$, where $c=g/k$ and, taking A as origin, w $[=g/(d^2y/dt^2)]$ is proportional to the reciprocal of the acceleration; but then the two parts of the area under the curve represent y and $k*y$ respectively, and their sum represents $g*t$

D. Scholium: Resistance and Projectile Motion

1. In the Scholium following Problem 7, Newton compounds the solutions for Problems 6 and 7 to give the solution for projectile motion in the case of uniform vertical acceleration and resistance proportional to velocity
   a. Newton's solution has the projectile reaching r along the trajectory DarFK in the time represented by DRTBG, and the speed at r represented by the tangent to the curve at r, rL
   b. For, with c=1/k and a=(u/k)∗cos(α), Newton's solution for the trajectory and time is simply:

$$y = g/k*\{x - c*\log(a/a\text{-}x)\} \text{ and } t = c*\log(a/a\text{-}x)$$

2. The trajectory with resistance proportional to velocity is thus a skewed parabola, with the amount of skewing dependent on k
   a. In qualitative agreement with what almost everyone had been saying from Galileo on
   b. But now a precise quantitative solution, given k, u, and g
3. Newton gives an empirical method for determining $k*u/g$ from observations of the initial angle of ascent and the final angle of descent (for a given spherical surface area and weight)
   a. Deceleration in resistance for a given medium and speed here assumed to be proportional to surface area and inversely proportional to weight
   b. Can then obtain u from a measure of range, and finally infer k, so that henceforth k can be taken as that value multiplied by (1) the ratio of the surface area of the projectile to that of the reference projectile and (2) the ratio of the weight of the reference projectile to the weight of the projectile
   c. So, the solution is as formally complete as Galileo's
   d. Newton's use of *weight* here, and not *mass*, is evidence that he had yet to distinguish the two!
   e. And his use of surface area means that he is viewing resistance as fluid clinging to the body
4. Still a calibrated solution to the projectile problem insofar as a coefficient of resistance has to be measured, but not calibrated in a way to absorb unaccounted for effects

a. In Galileo's case the Tables are in effect being recalibrated for each different velocity and projectile size and shape

b. In Newton's case, by contrast, the only calibration involves a presumable constant

5. The trouble, of course, comes when multiple measurements for this coefficient fail to yield (remotely) consistent values over a range of velocities and projectiles, which is in effect what Huygens had discovered years earlier.

A. Newton is here assuming that resistance effects are proportional to velocity, while the truth is more accurately represented as if by two distinct effects, one roughly proportional to velocity and another, usually dominant, roughly proportional to velocity squared

b. The *Principia* will offer such a further refinement

V. The Significance of *De Motu Corporum in Gyrum*

A. Advances Made by Version 1 of De Motu

1. Even though *De Motu Corporum in Gyrum* was not published, it gained enough circulation through the Royal Society that it is appropriate to ask, what exactly did it, by itself, contribute

a. Or, maybe more appropriately, what exactly would it have contributed if it had not been followed up two and a half years later by the *Principia*

b. A question that is undoubtedly best answered from the perspective of Halley, though also from that of Newton, since he was evidently not satisfied with the contribution as it stood

c. In other words, instead of reading the tract through the lens of the subsequent *Principia*, we should try to read it through the lens of the state of science as of late 1684

2. One thing that this tract did not contribute in its own right is universal gravity, for the tract as such does not even require mutual attraction between celestial bodies, much less between every two particles of matter

a. I see nothing in this version of De Motu that gives any reason to think that Newton had yet even thought of universal gravity among particles

b. No reason to think that Newton had yet even reached a clear concept of mass, as distinct from weight, for the one place where he needs *mass* in the final scholium he uses *weight*

3. In the version sent to the Royal Society, the one key place in which gravity is referred to is at the end of the Scholium to Problem 5, where Newton speaks of the "hypothesis" of gravity being an inverse-square force

a. But the manuscript shows him to have been using "*gravitas*" in Problems 1, 2, 3, and 5, only then to delete it, usually replacing it with "*vis centripeta*"

b. What he had in mind when he first used it and why he then replaced it with a more abstract locution is unclear; but no one looking at the public version of De Motu saw any sign of this

c. The effect is clear, however: the text presents an abstract theory of motion under centripetal forces until after Problem 5, where it turns to motion under hypothetical terrestrial gravity

4. Something the registered version nonetheless did achieve was to tie Kepler's three "laws" together for the first time as manifestations of a specific mathematically describable situation -- inverse-square departures from uniform motion in a straight line toward a single point in space
   a. Keplerian motion no longer a deviant form of uniform circular motion involving a second mechanism; instead uniform circular motion now a special case in which velocity has a peculiar (vector) value for which the eccentricity of the elliptical trajectory becomes 0
   b. Tying the three "laws" together within such a Galilean (or, more properly, Huygensian) mathematical theory also greatly increases the grounds for taking them to be nomological, and provides added reason for thinking that they might be exact
      (1) The former because of the added grounds for holding that the three are manifestations of a single underlying physical mechanism -- indeed, one not peculiar to orbits around the Sun
      (2) The latter because have now identified a comparatively simple circumstance in which the three would hold exactly, viz. if the only force acting on the orbiting bodies is an inverse-square centripetal force
   c. Notice here that a correlative effect De Motu has on the status of the three "laws" is the enormous increase in importance of any evidence of even minor deviations from them
      (1) Major deviations, of course, would tend to undercut the physical applicability of the overall theory
      (2) But minor deviations may provide critical information about celestial physics -- e.g. some forces other than just inverse-square centripetal forces are present
5. Further, the tract puts the area rule on a totally different footing, not only tieing it to centripetal accelerations, but also clarifying Kepler's $r*v_{\perp}$ invariance
   a. Puts burden of proof on alternatives to the area rule, requiring similar simple kinematic underlying principle
   b. Eliminates any remaining worries about whether any possible physical basis for the area rule
6. Tract shows Galileo's "laws" of free fall and parabolic trajectory probably hold only approximately, even in absence of air resistance
   a. That is, under the assumption that acceleration of terrestrial gravity is inverse-square, as supported by the "Moon test," which of course is not mentioned in the tract
   b. Still, both hold to high approximation near surface of Earth, for parabola closely approximates end of high eccentricity ellipse
7. Results on motion under resistance offer promise of being able to test Galileo's "laws," confirming that they would hold to high approximation in absence of resistance
   a. Even if resistance not exactly proportional to *v*, may be near enough over limited distances to allow corrections to Galileo's to be determined

b. Offering a prospect of solving artillery problem

c. And also substantiating Galileo's separation of free fall and resistance, with former independent of weight (and shape), but not the latter

8. Results lend added support for Descartes' and Huygens's idea of contraposing principle of inertia, inferring forces from deviations from uniform motion in a straight line

   a. Simple rules of force for two different ellipses promising in just the way a simple rule for force in uniform circular motion was obtained from contraposed inertia

   b. Now have reason to adopt this approach as a general strategy

9. Finally, Problem solutions offer promise of advancing state of art in calculating celestial trajectories

   a. License for Horrocks-Streete use of 3/2 power rule to infer mean distance (i.e. a) from P

   b. An iterative method for determining specific ellipses

   c. Promise for determining comet trajectories

B. A "Galilean-Huygensian" Theory of Orbital Motion

1. From a broader perspective, De Motu presents a mathematical theory, a la Galileo and Huygens, in which uniform acceleration along parallel lines toward the surface of the earth is replaced by acceleration, especially inverse-square acceleration, toward a point in space

   a. In modern sense, a kinematic theory, for talk of force notwithstanding, only accelerations in the manner of Galileo within the theory until resistance is added

   b. Following Galileo, resistance is treated as a separate mechanism, and no cross-talk between components of motion assumed in parallelogram rule

   c. Following Huygens, deviation from uniform straight line motion, taken as proportional to t^2 over first small increment of time, gives measure of acceleration and force

2. Key enabling theorem a direct generalization of Huygens on uniform circular motion and centrifugal force, enabling inference of centripetal force from geometry and speed

   a. Key step for this: geometric representation of time by area for centripetally governed motion

   b. Again, a direct generalization of uniform circular motion, but with centripetality the key factor

3. As solved Problems show, a rich mathematical theory from limited initial assumptions, yielding question-answering power

   a. Key assumption: all deviations from uniform motion in a straight line are directed toward a single point in space!

   b. Different motions -- defined by trajectory and "force center" -- yield different rules of centripetal acceleration

   c. Inverse-square case most richly developed, yielding solution to initial value problem, with different conics from different initial situations

4. In the style of Huygens, takes trouble to tie results for more general curvilinear motions to Galilean free-fall and parabolic trajectory, enhancing opportunities for evidence

a. By providing a contrast between Galilean and inverse-square free fall and projection

b. By proposing solutions to Galilean motion in a resisting medium that have at least some promise of separating resistance effects from motion without them

5. Note too that theory has a Huygensian testable surprising consequence that, if verified, will provide strong evidence of an inverse-square centripetal acceleration "field" around the sun, namely comet trajectories of a sort that were novel to propose at the time

6. Finally, approach to resistance "Galilean" in adopting simplest assumption -- linear in velocity

a. Same approach Huygens took in the 1660s, undoubtedly for the same reason

b. Hoping to yield good enough approximation to allow corrections for confounding effects of resistance in experiments

C. Departures from Galilean-Huygensian Science

1. Given the extent to which De Motu follows the existing tradition and the strength of interest in inverse-square forces after publication of *Horologium Oscillatorium* in 1673, why didn't someone else come up with Newton's main results earlier

a. The mathematics was not out of the reach of Wren or Halley, for no calculus was involved

b. Huygens took himself to task for not having done so when he saw these results in the *Principia*

2. Part of the answer to this question lies in the several respects -- some obvious, some subtle -- in which De Motu reaches beyond Galilean and Huygensian science

a. Obviously, the mathematics of inverse-square accelerations and elliptical trajectories is more complicated, even than that of cycloidal trajectories

b. And not confined to local motion near surface of earth, but extending to celestial motions in a way that both Galileo and Huygens never attempt

3. Still more subtly, even though theory is strictly speaking kinematical, talk of forces is not gratuitous, for dealing with departures from uniform motion in a straight line

a. This talk reaches beyond Galileo in just the way Descartes' treatment of motion reaches beyond geometry in appealing to forces

b. Also, with talk of impressed forces, external to moving body, question of cause cannot help but arise in a way that it doesn't in Galileo's work

4. A further departure: treating forces mathematically, in the abstract, and not tieing them to static forces in string or on wall in the way Huygens did

a. Breaking away from statics, with 'force' being used in a somewhat new way, raising questions about legitimacy

b. No longer a means for confirming measures assigned to forces through appeals to statics

5. This departure is amplified by Newton's step to what we now call fields -- i.e. to idea that every point in space surrounding a "force center" has associated with it a distinctive accelerative tendency toward the center -- $[a^3/P^2]/r^2$

a. In the process, center endowed with a new property, $[a^3/P^2]$ -- at least whenever a body orbits it
b. Save perhaps for glimmers of this idea in work on magnetism, nothing like this in prior science save for the incorrectly proposed universality of the distance of fall in the first second

6. Finally, question of mechanism producing inverse-square centripetal accelerations is left entirely open in De Motu, as if not worth raising
   a. This might or might not have bothered Galileo, but it not only would have, but did bother Huygens (and it appears to have bothered Newton)
   b. What is sneaking into the Galilean-Huygensian tradition here is the English tradition in magnetical philosophy, where appeals to analogy with magnetism were permitted
   c. Would have been more apparent if Newton's had not replaced 'gravitas' in several places

D. Loose Ends in Version 1 of De Motu

1. The absence of mechanism for inverse-square centripetal forces is not the only loose end in version 1 of De Motu
2. Legitimacy of the reasoning in the Scholia is unclear: infer inverse-square from uniform circular and 3/2 power rule, then infer exact Keplerian ellipse from no need for anything beyond inverse-square
   a. Planets are not in uniform circular motion: 40 percent variation in (angular) speed from min to max in case of Mars and 100 percent variation in case of Mercury
   b. Have not explicitly eliminated other bounded trajectories besides ellipse
3. No independent evidence that inferred inverse-square force in case of planets and their satellites is real, *unless comet trajectories turn out to be calculable from inverse-square centripetal accelerations*
   a. A lot seems to be riding on the motions of a handful of bodies, motions that are known at most to approximate Keplerian
   b. Moon provides a clear counterexample to Keplerian motion, and orbits of Jupiter's satellites known from Cassini not to be stationary
4. No evidence for inferred inverse-square variation of terrestrial gravity, save for unstated "Moon test"
   a. A lot seems to be riding on a single number that may be a mere happenstance
   b. Worse, that number predicated on false assumption of uniform circular motion
   c. Would like to have other evidence, such as the claimed variation in surface gravity with altitude observed by Halley at St. Helena and mentioned by Hooke in one of his letters to Newton
5. No evidence that resistance varies linearly with velocity, even to reasonable approximation
   a. Newton provides a way of measuring the constant of proportionality for any one body
   b. But until measurements show that this "constant" is a constant, an open question
6. Orbital theory in De Motu refers all motions to a single point in space, but there are at least four "centers of force," three of which are orbiting the sun
   a. I.e. Saturn, Jupiter, and earth are not stationary centers in space
   b. This raises the question, to what center should orbital motions be referred

c. Because this was the very issue raised by the Tychonic versus Copernican controversy, those at the time would have been quick to notice this

7. If there are inverse-square acceleration "fields" around the earth, Jupiter and Saturn, then not only comets should experience them, but the sun as well
   a. I.e., the results on their face imply an interaction between the sun and e.g. Jupiter
   b. Is this real, and if so, does it nullify the results derived mathematically in Version 1 of De Motu

E. Version 1 of De Motu Versus the *Principia*

1. While the main body of De Motu offers a purely mathematical theory, and not as such a physical theory (save for the way of conceptualizing motion), the theory and the Scholia clearly point to a physical hypothesis
   a. The physical hypothesis: the curvilinear motion of every celestial body, especially bodies orbiting about a principal, is governed by gravity-like, inverse-square centripetal forces
   b. Not universal gravity, but a hypothesis closely akin to one Hooke and Wren were entertaining since mid-1670's, and Newton appears to have been entertaining as well when drafting De Motu
   c. This may have been one reason why Halley was so excited with the tract
2. The standard picture, both then and now, is that Newton's response to the results of Version 1 of De Motu was to leap to a much bolder conjectural hypothesis: every particle of matter gravitates toward every other particle of matter in accord with the inverse-square rule
   a. I.e. a leap of genius to a bold explanatory hypothesis that reaches far beyond any of the results in De Motu and any phenomena then known
   b. An explanatory hypothesis that turned out, over the next century, to explain all sorts of other things
   c. A picture under which De Motu served only to stimulate a hypothesis, and subsequent evidence for this hypothesis was all hypothetico-deductive
3. A consequence of this picture now is the standard answer to the question of this course, how did we ever come to have high quality evidence in any science
   a. Answer: Newton happened upon an extraordinary hypothesis that turned out to be (nearly) true, and the evidence then fell into place
   b. Corresponding picture of science: carry out limited investigations until in a position to leap to general explanatory hypothesis, after which test this hypothesis, replacing it as need be
4. A consequence of this picture then: substantial reluctance in many circles to accept Newton's universal gravitation precisely because it did leap so far beyond De Motu
   a. E.g. Huygens accepted without reservation Version 1 of De Motu plus the "Moon test" and the conclusion from it that the moon is retained in orbit by inverse-square terrestrial gravity, and the planets by inverse-square gravity one in kind with terrestrial gravity, but regarded everything beyond it as unwarranted conjecture -- a view that upset Newton

   b. Not just a complaint about lack of mechanism -- and hence the implication of action at a distance -- but also about lack of evidence for interactive celestial gravity, much less universal gravity
   c. Universal gravity ultimately came to be accepted as its consequences fell into place between 1740 and 1790, so that its historical acceptance actually did conform largely with a hypothetico-deductive picture
5. This standard picture of what Newton did, once he had De Motu, is in fact not what he did, nor what he said he did
   a. Clearest evidence for this: if he had leapt directly to universal gravity, he would have turned right away to question whether an inverse-square acceleration toward the sun can be composed out of inverse-square accelerations toward each particle of matter comprising it
   b. But did not turn to this question until March or April 1685, after several intervening steps
   c. Further evidence: anger Newton consistently expressed toward those who took him to have leapt to the hypothesis of universal gravity
6. What Newton did instead, once he had De Motu, was to begin focusing on its loose ends, proceeding step by step to a sequence of further conclusions
   a. Initially to a "proof" of Copernicanism
   b. Then to the law of gravity
   c. Then to a new conception of how to do science, which he much later came to call the new "experimental philosophy"
7. Will start this sequence next week, but need to read entire *Principia* to see clearly how the science in it departed from all science before the *Principia*
   a. *De Motu Corporum in Gyrum* derives eleven propositions in nine handwritten pages; *Principia* derives more than 180 propositions in some 500 printed pages
   b. When Newton first discovered the elongation of the image cast by the prism, he conducted a large battery of further experiments to explore what conclusions are properly to be drawn from his initial discovery
   c. One way to think of the transition from the registered version of De Motu to the *Principia* was that he did something analogous to that from November 1684 to the first three months of 1687, adding a large battery of further propositions that allowed him, in his mind, to draw secure conclusions about celestial motions fom his initial findings

<u>Select Sources</u>

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Credits for Appendix

Slides 2, 4, 6-17, 42, 43: Turnbull (1960)

Slide 3: Brackenridge (2000)

Slides 22, 24, 34, 36: Smith (1999)

Slide 23: Huygens (1986)

Slide 26: Smith (2002)

Slides 28, 29: Whewell (1838)

Slides 31-33, 39: Whiteside (1989)