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