THE NEWTONIAN REVOLUTION – Part One

Philosophy 167: Science Before Newton's *Principia*

Class 14

Newton's De Motu Corporum, Version 3

December 9, 2014

Philosophy 167: Science Before Newton's Principia

Assignment for December 14

Newton's De Motu Corporum, Version 3

Reading:

Newton, Isaac, "The Motion of Spherical Bodies in Fluids," from Herivel, The Background to Newton's Principia, pp. 294-303.

------, "Drafts of Definitions and Laws of Motion," from Herivel, The Background to Newton's Principia, pp. 309-313 and 315-320.

Wilson, "From Kepler's Laws, So-Called, to Universal Gravitation: Empirical Factors," pp. 160-170.

Questions to Focus On:

1. **How do the principles that Newton takes to be axiomatic change from Version 1 of De Motu to Version 3? Why does he make these changes? And why does he switch from calling them 'Hypotheses' to calling them 'Laws'?**

2. **In the paragraph added to the Scholium following Theorem 4, Newton claims that the results in De Motu provide a proof of the Copernican system. What is this proof?**

3. **In this same paragraph Newton concludes that the Sun moves and as a consequence the planetary orbits are not perfectly Keplerian. What causes these effects?**

4. **In this same paragraph Newton remarks, "Unless I am much mistaken, it would exceed the force of human wit to consider so many causes of motion at the same time, and to define the motions by exact laws which would allow of an easy calculation." Why does he think that "it would exceed the force of human wit" to do this?**

5. **The hypothesis of universal gravitation is no more mentioned in Version 3 of De Motu than it is in Version 1. To what extent, if any, is Version 3 more committed to this hypothesis than Version 1 was?**

6. **What objective is Newton pursuing in the "Drafts of Definitions and Laws"? That is, what is prompting him to devote the effort displayed to the definitions and laws?**

Newton's De Motu Corporum, Version 3

I. *De Motu Corporum in Gyrum*, Version 1

A. A Theory of Motion Under Centripetal Forces

1. An initial fragment of a theory of motion under centripetal forces -- i.e. motion in which all changes in motion, all deviations from inertial motion, are directed toward a single point in space
   a. Similar in form to Huygens's theories of pendular motion and centrifugal forces in uniform circular motion
   b. Motion relative to the (unmoved) center that is the point toward which the forces are directed
   c. Emphasis on inverse-square forces, but treating others as well
   d. Such a theory appropriate because motion under centripetal forces turns out to be a distinctive category, viz. the category characterized by the area rule (i.e. constant angular momentum)
2. The conceptual shift from centrifugal to centripetal is widely regarded as the key, for Newton now focusing on physically real force rather than mere *conatus* normal to the curve
   a. A touch of Whig history, for Newton did not abandon centrifugal *conatus*, and centrifugal force real in Huygens's sense, namely as the tension in the string
   b. Shift from centrifugal *conatus* as domain to a subdomain: centrifugal *conatus* balanced by centripetal (impressed) force
   c. Payoff: not so clear how to define the direction of force counterbalancing centrifugal *conatus* at the same time as changing the velocity and curvature for arbitrary curvilinear trajectory, but perfectly clear how to define the direction of centripetal impressed force
3. In a sense a mathematical theory -- 11 'if-then' propositions derived from axioms -- but in another sense not, for axioms make physical claims
   a. Three of the axioms can be viewed as concerning the conceptualization of motion and "forces" altering motion
   b. Specifically, Cartesian “laws” warranting inference to "unseen" external causes, supplemented by Galilean principles concerning the effects of a force and of two forces
   c. The resistance hypothesis makes the most flagrantly physical claim, akin to Galileo's uniform acceleration axiom as the mathematically simplest to consider
4. The propositions show that, given this way of conceptualizing motion, Keplerian elliptical motion is systematically related to centripetal forces of a certain sort
   a. This is the first theory offering a unified account of Kepler's three "laws," plus bi-section of eccentricity and constant inclination too (for it remains constant under centripetal forces)
   b. Moreover, in the process the theory makes Keplerian motion the norm, rather than a special deviation from the norm of uniform circular motion
      (1) Before De Motu: why are the orbits Keplerian, rather than "Platonic"
      (2) After De Motu: why are so many of the orbits so nearly circular

c. Most important, Kepler's third "law" gains generality, for now a consequence of first two, and not dependent on densities of the planets

5. Furthermore, Keplerian motion systematically related to Galilean motion via Problems 4 and 5 -- or perhaps Galilean related to it via Problems 4 through 7
   a. Under the view that Galilean motion an approximation, for Problems 4 and 5 allow for the circularity of the earth and replace uniform acceleration with inverse-square acceleration
   b. Problems 6 and 7 then grant the legitimacy of the approximation and remove Galileo's resistance-free idealization by allowing for resistance proportional to speed
   c. Latter results offer a candidate solution to a long standing problem -- the true trajectory of projectiles
6. Outwardly, De Motu follows a Huygensian approach, for the theory extends a somewhat "Galilean" kernel in the same way that Huygens's theories did, though in this case an extension to the celestial, something Huygens did not try
   a. The theory is thereby tied into the evidence base for Huygens's work, supporting the conceptualization of motion in De Motu, and lending support to Huygens's work
   b. De Motu not just a synthesis of Kepler and Galileo, as customarily noted, but of Huygens and Descartes along with them
   c. Ties together Kepler's rules, vertical fall, projectile motion, uniform circular motion, and what came to be known as the principle of inertia
   d. With button-hooking comet trajectories as a surprising, qualitatively testable consequence

B. The Theory as an Evidential Instrument

1. As presented by Newton, the 11 propositions of De Motu comprise an instrument in evidential reasoning
   a. By tying various things together in 'if-then' propositions, the theory allows reasoning from data on certain things to conclusions about others
   b. This is another respect in which the theory is Huygensian -- though in this case it is an especially good example, for the conclusions are in general about unseen, abstract "forces", and not just about things that are difficult to measure directly
2. The "moon test" is an example of using the theory in evidential reasoning, though one not exploiting the overall theory all that much
   a. From period and distance of Moon and known value of g, use Theorems 2 and 4 to infer inverse-square force and hence Kepler's third "law" around the Earth
   b. But then have a decisive argument against the Tychonic system, for the 3/2 power rule would require the Sun to be less than 400 earth radii away in the Tychonic system
3. There are several other examples, ranging from minor -- inferring the coefficient of resistance -- to major -- a candidate for inferring comet trajectories

a. Results on projectiles (Problems 4-7) provide means for dealing with vagaries in measurements in Newton's proposed experiment to establish the diurnal motion of the earth, if only through giving expected values of the displacement

b. The method for bringing multiple data to bear on determining planetary orbits

c. Better evidence for Galilean through measurement of resistance and capacity to extend to other bodies, yielding implications for pendular motion, etc.

d. Potential inferences from observed non-uniformities in g

4. The theory is most potent as an instrument in evidential reasoning through increasing the utility of discrepancies between observed and (idealized) theoretical values

   a. E.g. in deviations from Kepler's "laws": other forces at work, and hence can turn to search for these other forces in cases like the Moon, opening a path toward the perfectibility of astronomy

      (1) Kepler's approach was to superpose mechanisms to cover the discrepancies

      (2) With De Motu much less ad hoc, for now confined to superposing forces and justifying their existence

   b. Similarly, if the measured coefficient of resistance is not uniform, can conclude that resistance is not simply proportional to v

   c. Of course, increase the utility of observations in the absence of such discrepancies too, for then licensed to reach some strong conclusions -- viz. no other forces at work

5. Finally, should notice that the theory is creating a potential for further evidential inferences once certain measurements become better made

   a. Strength of inverse-square centripetal force "field" is proportional to a^3/P^2, a constant for each force center

   b. With (commensurate) measures of a, can compare simultaneous effects, such as that of the sun and Jupiter on a comet or the sun and earth on the moon

6. Submit that Newton saw the mathematical theory primarily from this point of view

   a. He is not cleverly suppressing a physical theory, but instead is providing a vehicle enabling everyone to draw stronger conclusions from observations, both made and unmade

   b. Looking on the mathematical theory of De Motu in this way will help in understanding the *Principia*

C. The Theory Versus Physical Hypotheses

1. De Motu, of course, contains more than just its mathematical development, for it also includes Scholia in which Newton reaches beyond the mathematical results

   a. Generally exploiting the math as an evidential instrument, especially to draw conclusions about "unseen" external causes underlying planetary (and celestial) motion

   b. Question: how is the theory in De Motu a physics theory too, a piece of natural philosophy, given that all its proved propositions have an “if-then” form?

"Since this doctrine may be judged to be somewhat akin to natural philosophy, in so far as it may be applied to making clear many of the phenomena of natural philosophy, and in order, moreover, that its usefulness may be particularly apparent and the certainty of its principles perhaps confirmed, I shall not be reluctant to illustrate the propositions abundantly from experiments as well, in such a way, however, that this freer method of discussion, disposed in scholia, may not be confused with the former which is treated in Lemmas, propositions and corollaries." De Gravitatione et aequipondio fluidorum

2. An impediment to answering this question: we typically view De Motu from the perspective of universal gravity, suggesting Newton had it in mind as a hypothesis and was building a defense around it
    a. Another example of Whig history, reinforcing the view that the key events in science are the discoveries of "true laws", after which evidence just automatically accrues to them
    b. Here going to proceed under the assumption that Newton had not yet thought of universal gravity; will take both versions of De Motu at face value, in the light of the above quote
3. Like Kepler, Newton trying to infer underlying physics in order to settle questions about Copernicanism, the trajectories around the Sun, and the perfectibility of astronomy
    a. But holding back from proposing a full physical hypothesis, and instead trying to infer decisive features of the physical mechanism from such phenomena as the 3/2 power rule
    b. No mention of vortices, magnetism, or gravity as the general mechanism
    c. Differs from Kepler also in working from a different way of conceptualizing motion, one that has the virtue of making Keplerian motion the norm
    d. Thus promises to yield more decisive evidence -- both positive and negative -- than will Kepler's more ad hoc moves
4. Newton's refusal to put forward any physical hypothesis contrasts him with Descartes, who starts from a comprehensive physical hypothesis
    a. Since inferring features and not proposing explanations, no need for mechanical hypothesis
    b. How inverse-square forces are produced is an entirely open question
    c. Sharp contrast with Descartes: discrepancies have strong implications, largely because of mathematical formulation, in contrast to Descartes' qualitative formulation
5. Contrast with Hooke, as well as Descartes: not exploiting analogy with familiar phenomena on earth
    a. Inverse-square centripetal forces, but without appeals to gravity and magnetism (save for mention that gravity an example)
    b. Requiring mathematical theory and invoked evidence to do all the work, and not jumping ahead
    c. Closer to the way Galileo and Huygens proceeded, but with Newton more preoccupied with getting at features of a unifying underlying physics than they were
6. All of this a reflection of Newton's disdain for proposing hypotheses and adapting them to phenomena

a. A sharp distinction between conjecture and empirically established theory: conjecture kept in its place
b. Looking for ways to answer questions empirically in contrast to looking for ways to support hypotheses (or to generate them)

7. Newton's approach, in contrast to everyone else's, most apparent in the Scholia: use theory and phenomena to infer e.g. inverse-square centripetal forces, at least to a first approximation, and then require the world to force revisions and emendations
   a. To a first (circular) approximation, 3/2 power rule yields inverse-square centripetal accelerations at least toward the sun and Jupiter, and presumably toward Saturn and the earth as well
   b. But then, so long as there is no other bounded trajectory besides the ellipse that is consistent with inverse-square centripetal accelerations, all three of Kepler’s "laws" should also hold at least to high approximation for the bodies in orbit about those central bodies
   c. Even stronger, Kepler’s "laws" should hold exactly unless there are forces acting on the orbiting bodies beyond the inverse-square centripetal forces, thereby authorizing Horrocks's approach
   d. This then becomes a basis for exploring what else is at work, via discrepancies or their absence, using strict logical implications
   e. Successive approximations to the underlying physics, but with steps "forced" by the phenomena, in contrast to Kepler's steps

D. The Evidence for the Physical Hypotheses

1. Thus Hooke's physical hypothesis -- celestial motions from inverse-square centripetal forces resembling terrestrial gravity -- precisely what is emerging from this approach in De Motu
   a. Inverse-square centripetal from Corollary 5 of Theorem 2, just as Hooke and others had concluded
   b. But more robustly, for hypothesis survives the relaxation of the ideal of circular motion to more realistic Keplerian motion
   c. Gravity-like because of the ratio of the moon's acceleration to terrestrial *g* as determined by the "Moon test," which goes unmentioned in the registered version of De Motu
2. Question: why not just come out and make the case in De Motu
   a. Willing to state strong conclusions about "...exactly, unless ...", but not willing to state strong conclusions about Hooke's hypothesis
   b. Must think that evidence still short of warranting such conclusions
   c. Question: what limitations might he see in the evidence?
3. Limitations of "Moon test" as a basis for concluding that centripetal force "field" around earth is inverse-square, and then concluding that it is the same as gravity
   a. A single number, admittedly in reasonably tight agreement, but still open to question, for no mechanism has been specified to yield it, and the number itself is based on the idealization of

uniform circular motion while the moon's motion is known to be neither uniform nor circular, so that the tight agreement may to some extent be illusory

b. Worse, vagaries in lunar orbit not just with respect to Theorem 2, but with respect to Keplerian motion too: Kepler's first two "laws" definitely do not hold for it

c. And there is no independent evidence that terrestrial gravity is inverse-square

4. Limitations of using area rule result to conclude that centripetal forces at work elsewhere

a. No mechanism to explain why always directed toward a single point; contrast with Descartes' vortex theory, which rejects idea that forces always directed toward a single center

b. Concern for imprecisions in area rule, vagaries in motion of the moon, and likely in motion of Jupiter and Saturn as well (based on what Kepler had said in his *Rudolphine Tables*), and the possibility of an epochal parochialism of the sort called attention to by Descartes

c. Worse, De Motu itself entails not one center, but four!

d. Would prefer much tighter evidence on how well planets conform with the area rule, or independent evidence for center-directed forces

5. Limitations of using Theorem 2 coupled with Problems 3 and 4 and Theorem 4 to conclude that inverse-square forces at work elsewhere

a. No independent evidence, as from comets: comets as much a challenge (from Cartesians and others) as an opportunity, and hence a lot hangs on that claim of De Motu

b. Vagaries in planetary motion challenge inference by challenging step from circular to Keplerian

c. "Building inference on inference" when prior inferences not secure because of inexactitudes

6. Note also that deceleration from resistance depends on weight of body, as does the centrifugal tension in a string retaining an object in uniform circular motion; but Corollary 5 of Theorem 2 and Theorem 4 treat centripetal force as independent of weight: what justifies the difference?

7. Upshot: loose-ends and imprecisions in phenomena leave openings in suggested evidential reasoning

a. Problem: at a minimum need grounds for concluding that imprecisions come from secondary effects, and not from falsity of claimed primary

b. Key: characterize discrepancies, separating real from observational, and then showing that they can legitimately be treated as arising from secondary effects

c. Newton's situation is just the opposite of resting self-satisfiedly with De Motu Version 1, for it points the way to a huge amount of additional work

II. De Motu, Version 3 (the "Augmented" Version) -- New Preliminaries

A. The New Title, Versus the Old

1. Version 3 the same as Version 1 in the proofs of the 11 propositions, indicating that Newton is not looking for better proofs (i.e. he is not revising as a mathematician)

a. A new first section, recasting hypotheses and then, in a correction, calling them laws

b. An extended scholium following Theorem 4: "Copernican Scholium"

c. A slightly revised figure to Problem 5 followed by an extended scholium, offering more of a transition to Problems 6 and 7: "Resistance Scholium"

2. The manuscript is in Humphrey Newton's hand, save for small corrections and changes in Newton's
    a. Original manuscript lost, and never any reference to this version in letters etc. by Newton or anyone else
    b. Date uncertain: presumably after Version 1 sent, but surely not much after first letter to Flamsteed at end of December, 1684
    c. Presumption: mid to late November, perhaps in response to second thoughts after forwarding Version 1, or early December, following Halley's second visit
    d. First became public in 1893 (in Rouse Ball's *Essay*)!
3. New title: from *De Motu Corporum in Gyrum* to *De Motu Sphaericorum Corporum in Fluidis* -- "The Motion of Spherical Bodies in Fluids"
    a. Sectioned: preliminaries, followed by "On the motion of Bodies in Non-Resisting Mediums" (Theorem 1 through Scholium preceding Problems 6 and 7), followed by "On the Motion of Bodies in Resisting Mediums"
    b. Still a theory of motion under centripetal forces, but now in two distinct domains
4. Unclear what significance ought to be attached to change in title
    a. Why abandon "in Closed Circuits"?; two likely answers: fails to capture full generality of results, and incompatible with conclusions of Copernican Scholium
    b. But why "in Fluids", for fluid in no way enters first half, unless in the form of aether as discussed in the added Scholium following Problem 5
    c. And why "spherical", for nothing in the math requires strict spheres; maybe to support treating celestial bodies as points
5. Perhaps Newton was struggling to identify the domain the theory in De Motu extends across
    a. Some suggestion of a changing conception of the scope of the theory
    b. Titles of manuscripts keep changing: next, *De Motu Corporum in Mediis Regulariter Cedentibus* -- "in Uniformly Yielding Media"
    c. Lucasian lectures: *De Motu Corporum Liber Primus*
    d. Title of overall work remained *De Motu Corporum* until fairly late in composition, when Newton changes to *Principia Mathematica Philosophiae Naturalis*, remarking to Halley that it will stimulate sales

B. The New "Laws" and Definitions
1. "Preliminaries" extensively recast, though in a way that has virtually no impact on any proofs (and therefore not being done to increase rigor)
    a. Exception: now explicit that force is proportional to change of motion
    b. But otherwise Newton has something else in view

2. Added definition (mistranslated in Hall & Hall): representations of quantities any other quantities proportional to them
    a. E.g. as area represents time, and QR represents both force and displacement
    b. Clarification, but unclear why added since a standard approach
3. Change from "hypotheses" to "laws," presumably with Descartes in mind, though perhaps from fact that material being taken from "Lawes of Motion" paper
    a. In manuscript, 'hypoth' scratched out in 5 places and 'lex' added in Newton's hand, so undoubtedly added upon re-reading
    b. Maybe because of "proof" of Copernican system
4. Newton's second law in basic form it will appear in the *Principia*: force is proportional to change in motion along a specific direction, that of the *impressed* force
    a. From "Lawes of Motion", but implicit in Version 1, where no mention of "*impressed* forces"
    b. Unidirectionality: impressed force a vector quantity
    c. Proportional to displacement from uniform motion in a straight line in a given time: hence neutral between impulse and continuous forces, and fully compatible with $F=ma$
    d. But not rate of change; Newton's focus on proportions allows dimensionality issues to be bypassed (or suppressed)
5. Laws 3 and 4: relative motions same in given space; and inertial motion of center of gravity, sort of from "Lawes of Motion" paper, but also announced for impact generally by Huygens in 1669
    a. Latter explicit In both “Lawes” and Huygens; former, not yet published in Huygens’s full paper, implicit in “lawes” and deducible from velocity of attraction=velocity of repulsion
    b. No derivation of 4 from 3, only a comment that it is derivable; I dispute that the center of gravity principle can be derived from the relativity principle
    c. No relevance to any proofs, only to Copernican Scholium
6. Fairly clear where Laws 3 and 4 came from: concerns over multiple centers raise interaction issue and issue of proper point to which motions should be referred
    a. Newton presumably turns to "Lawes of Motion" for move
    b. But equally could have turned to Huygens, for Law 3 at the base of his work on impact (though proofs unpublished, manuscript in London), and Law 4 published by him in 1669
    c. Law 4 also just a generalization of Law 1 to systems of interacting bodies; this, though not expressly mentioned, doubtlessly behind both Newton and Huygens’s statement of it
    d. In effect, Newton extrapolating from impact, though recognizing the added warrant for doing so
7. Finally, last "law" on resistance clarified by including other factors in the claim
    a. Now more in the form of a law, rather than just a hypothesis whose implications are being explored
    b. But, unlike the others, much more a bare hypothesis

C. The New Lemmas: Deriving Former Hypotheses

1. Two added lemmas, corresponding to Hypothesis 3 and inserted Hypothesis 4 of Version 1
   a. Now deriving from "laws" material previously postulated
   b. Thereby avoiding impact on any proof
2. Lemma 1: the parallelogram rule, derived from Law 2
   a. Crucial need to make directionality in Law 2 explicit
   b. Thus explicit reduction of a feature of the old parallelogram hypothesis to a more fundamental claim; this may be the source of Law 2
3. Lemma 2: old hypothesis 4, now derived from more fundamental considerations
   a. Presupposes mean value theorem, referencing Galileo; unclear whether Newton realized the defects in Galileo's proof (eliminated by Huygens in *Horologium Oscillatorium*)
   b. Let Ab:Ad as AB:AD, with b and d arbitrary, and compare ratios of distances covered (represented by areas) as force changes versus distances covered with uniform forces
   c. ADEG an upper bound: uniform force at end of time AD; ABF a lower bound: uniform force at t=0
   d. Given bounds above and below, now take limits, obtaining result (see Appendix)
4. Comment: key feature in proof is constraint when taking the limits -- i.e. as B, D approach A and e approaches h
   a. Hold the ratios fixed: AB:AD, Ab:Ad, etc.
   b. Conclusion then for general points b and d, via ratios
   c. A feature of the way in which Newton takes geometric limits -- "first and last ratios"
5. Move in "Preliminaries" in part toward more secure foundations, but even more so toward claims that are more truly foundational -- i.e. more appropriate underpinnings of the theory to follow
   a. Toward putatively universal principles that can be said to comprise a simple, straightforward approach to conceptualizing motion
      (1) True even of Laws 3 and 4, for they can be taken to concern the proper point to which to refer motions
      (2) And also of Law 5, for it licenses the treatment of resistance as second-order, induced effects
   b. Making key elements of conceptualization more clear and explicit, as distinct from derived ones
      (1) Parallelogram rule less clearly an element in a way of conceptualizing motion
      (2) Even more so for originally inserted Hypothesis 4
   c. Why doing this unclear, but in spirit of response to Descartes' critique of Galileo's *Two New Sciences*, at least up to a point
   d. Also, though, to some extent being forced to add principles in order to be in position to answer questions, such as questions about true versus relative motions

III. De Motu, Version 3 (the "Augmented" Version) -- Two Extended Scholia

A. The "A Priori Proof" of Copernicanism

1. The "Copernican Scholium," added following Theorem 4, makes a series of claims, falling into three consecutive parts; the first part pivots on the claim that the Copernican system can be proved:

   "Moreover the whole space of the planetary heavens either rests (as is commonly believed) or moves uniformly in a straight line, and hence the communal center of gravity of the planets (by Law 4) either rests or moves along with it. In both cases (by Law 3) the motions of the planets among themselves [*inter* se] are the same, and their common centre of gravity rests in relation to the whole of space, and so can certainly be taken for the still centre of the whole planetary system. Hence truly the Copernican system is proved a priori. For if the common centre of gravity is calculated for any position of the planets it either falls in the body of the Sun or will always be very close to it."

   a. Desire to prove Copernicanism, and not worries about multiple centers, may have prompted Laws 3 and 4 in the first place

   b. Regardless, Laws 3 and 4 entail that the appropriate single point to refer the motion of the planetary system is its "communal centre of gravity"

   c. And calculation shows this point to be very near the sun

2. The standard objection to this line of reasoning, voiced by Wilson (and supported by Whiteside), is that it presupposes key unstated elements of the law of gravity

   The term 'center of gravity' is odd -- 'center of mass' would be preferable -- but it is possible that the "notion of center of gravity played a heuristic role, leading Newton's thought from familiar experiences with connected systems of weights to the idea of the solar system as a group of interconnected bodies. That such mutual interaction actually occurs is so far an unsubstantiated assumption..."

   "The other important assumption in the computation is that the attractions exercised at a given distance from these several bodies are as the masses of the attracting bodies."

   a. In other words, the supposed proof turns on calculating the center of mass of the planetary system, which itself requires first a determination of the masses of the bodies, not to mention the concept of mass itself

   b. But the only way of determining these masses is from the strength of the centripetal force "field" around each body being proportional to its mass, an inference licensed only by the law of gravity

3. There is another way of construing Newton's reasoning here, starting from concerns about multiple centers and the need for a single center to refer the motion to

   a. As indicated in Problem 5, Newton had identified a^3/P^2 as a measure of the accelerative strength of the inverse-square centripetal acceleration field around any body known to have such a field

   b. This quantity is a constant for each such body: C_h, C_j, C_s, C_e, the respective constants for the sun, Jupiter, Saturn, and the earth

4. Consider now just the case of the sun and Jupiter -- a two attractive center problem, with the strength of each attraction given

a. By Law 4 there must be a "center of gravity" around which both are revolving

b. And the only way in which Law 4 cannot be violated is if they revolve in a tandem balance with respect to one another, the sun revolving about this center with a radius r_h, and Jupiter with a radius r_j, where r_h/r_j a constant

5. The question then is, what is the (non-varying) ratio between r_h and r_j -- i.e. what are the comparative radii by which each revolves about the proper center of reference

   a. Assuming circular motion, Jupiter's centripetal acceleration is as r_j/P^2, so that $C_h/(r_{jh})^2$ is as r_j/P^2

   b. Similarly, in the case of the sun: $C_j/(r_{jh})^2$ is as r_h/P^2

   c. But then $C_h/C_j = r_j/r_h$!! -- a conclusion that has been reached without any consideration (or notion) of mass

6. But C_h is known to be much larger than C_j, for the diameter of the orbits of Jupter's satellites in astronomical units are known to sufficient accuracy to assure that C_h/C_j is a very large number

   a. But then r_h must be small compared to r_j; hence proper center of reference is very near the Sun

   b. The argument generalizes to Saturn and earth individually paired with the sun, both of which have smaller a^3/P^2 than that of Jupiter

   c. And the conclusion continues to hold even for a "displacement" of the sun corresponding to 6 times the strongest known planetary attraction, that of Jupiter's

7. Thus indeed a proof, not just that the Tychonic system is false, but that a main claim of the Copernican system is basically true

   a. A much stronger inference is being drawn from Kepler's 3/2 power rule than the one I am proposing that Newton was trying to draw in the original "Moon test" -- an inference made possible by Law 4!!

   b. Of course, there are loose ends in the reasoning, and hence some may say that this is not yet a proof, but it nevertheless is an extremely promising line of reasoning toward a proof

B. The Imperfectibility of Astronomy

1. The second part of the "Copernican Scholium" draws a corollary from the proof, leading to the conclusion that astronomy is imperfectible

   "By reason of this deviation of the Sun from the centre of gravity the centripetal force does not always tend to that immobile center, and hence the planets neither move exactly in ellipses nor revolve twice in the same orbit. So that there are as many orbits to a planet as it has revolutions, as in the motion of the Moon, and the orbit of any one planet depends on the combined motion of all the planets, not to mention the action of all these on each other. But to consider simultaneously all these causes of motion and to define these motions by exact laws allowing of convenient calculation exceeds, unless I am mistaken, the force of any human mind. Ignoring those minutiae, the simple orbit and the mean among all errors will be the ellipse of which I have already treated."

   a. The "proof" of Copernicanism has as a corollary that the movements of the planets are exceedingly complex, and hence orbital astronomy is probably not perfectible

   b. Nothing comparable to the above statement anywhere in the *Principia*!

2. The corollary stems from the fact that the forces on the individual planets are no longer strictly centripetal, thus undercutting the whole structure of the scheme of De Motu Version 1
   a. An ellipse with reference to the sun ceases to be an ellipse with reference to the proper center to which motion is referred if the sun is moving in response to the several planets
   b. This independently of any direct interactions among the planets
   c. Therefore the ellipse and the area rule are just approximations, though ones that might still hold exactly if there were only two bodies, say the sun and Jupiter
3. The comparison of the motion of the planets with that of the moon is instructive, for the moon was known to deviate from Keplerian motion vis-a-vis the earth in ways then beyond any calculation
   a. The suggestion is that perturbed Keplerian motion is the general rule, and not just an occasional exception, though the reason given in the case of the planets doesn't extend to the moon
   b. The claim that no two orbits are ever the same has to be overreaching what was known, and hence calls for qualification
4. The claim that the perturbed motion of the planets is beyond the capacity of the human intellect to calculate is less obvious since at most six planets are involved
   a. But once differences in inclination and period are taken into account, the planets will produce an exceedingly complex motion of the sun
   b. Newton here sounds like Galileo (and Descartes) on resistance -- beyond capacity because too many degrees of freedom
   c. Ironic, for shortly after Newton begins to try to calculate
   d. But nonetheless telling, for Newton here qualifying, if not retracting, a main conclusion of De Motu Version 1, *viz*. that orbital astronomy is perfectible so long as no secondary mechanisms are at work or they can be systematically accounted for via superposition!
5. Finally, the claim that the Keplerian ellipse represents the mean motion (and hence the Horrocksian approach is still authorized) is open to more than one interpretation
   a. If 'mean' is interpreted loosely, then only saying that the actual motion never deviates that far from the ellipse, and the ellipse lies somewhere in the middle
   b. But if 'mean' taken literally, then saying that ellipse approximates the actual motion in the mean, and hence making a strong claim, calling for justification
   c. Some justification can be given, for the sun's deviation from the proper center of reference will presumably result from a series of basically harmonic perturbations
      (1) Deviation in the case of interaction with each planet is harmonic
      (2) Superposition of finite number of harmonic deviations, each with its own fixed period, is a complicated deviation with a long period, rather like the Moon's 18 year Saros cycle
6. Note: Newton is invoking strict inferential reasoning here from the prior "proof", and not from the law of gravity, much less universal gravity

a. Not invoking 3-body effects or interactions with comets

b. And inference that Keplerian motion is a mean has some basis in proof of Copernicanism

7. Note finally: Newton's conclusion about the imperfectibility of astronomy is not coming from unaccounted for observed discrepancies between theory and observation -- as worried Kepler and Horrocks -- but from an underlying physics as inferred from the phenomena! – a physics very different from that on the basis of which Descartes reached his conclusion of imperfectibility

C. Implications for Orbital Astronomy

1. The inferred underlying physics thus entails that there must be discrepancies between Keplerian theory and the actual motion even though they may not yet have been observed

"If anyone tries to determine this ellipse by trigonometrical computation from three observations (as is customary), he will have proceeded without caution. For these observations will share in the very small irregular motions here neglected and so cause the ellipse to deviate somewhat from its actual magnitude and position (which ought to be the mean among all errors), and so there will be as many ellipses differing from each other as there are trios of observations employed. Very many observations must therefore be joined together and assigned to a single operation which mutually moderate each other and display the mean ellipse both as regards position and magnitude."

a. A practical corollary, going with "imperfectibility" corollary

b. And with the warning, a call for a new method of computing orbits, not yet entirely spelled out

2. The basic warning is perfectly clear: do not infer orbital parameters from a minimal or small set of observations, no matter how ideal these observations may be in other ways

a. The orbital parameters are known to be sensitive anyway -- e.g. sensitive to small errors in observation unless highly selective in observations used

b. But now saying that there are always perturbations having the same sort of effect as observational error

c. Consequently, will not get unique or converging values for orbital parameters so long as use small numbers of observations

3. Spirit of the remarks reminiscent of Descartes' view that the actual trajectories are subject to an indefinite number of quasi-random perturbations

a. Also Cartesian in a deeper sense, for analysis and theory are taking precedence over observations -- theory entailing discrepancies beyond any that can as yet be observed

b. Epochs needed to have any hope of using data to clean up data, and hence better off resorting to theory immediately

c. Akin to Kepler's despair in letter to Bernegger -- will not live long enough to have sufficient data to sort out -- but different too, for more basis for settling on Keplerian motion as the underlying mean about which the true motions fluctuate

d. In the process explaining why Kepler might have found the values of some of his elements changing over time

4. Stronger implication: undermine the evidential value of discrepancies between observation and theory, for may be mere transient perturbations!
    a. Version 1: all such discrepancies informative, for provide a basis for identifying and characterizing further forces at work
    b. Version 3: only select discrepancies informative, and burden of argument on anyone drawing inferences from a small discrepancy, for perhaps just do not have mean orbit correct yet
5. Version 3 thus represents a retreat in evidential prospects, and hence standards, to ones more akin to those of Galileo and Huygens
    a. Just as resistance effects may be beyond science, orbital perturbations may be beyond science
    b. Therefore there are limits on empirical evidence -- there will always be room to explain away small discrepancies
    c. Newton in effect being pushed into a position more akin to that of rational mechanics, though what is pushing him into it is a sequence of (theory-mediated) inferences from phenomena
6. The key thing to notice is that all these points in the "Copernican Scholium" are coming from Laws 3 and 4 (and two conclusions of Version 1: multiple centers, and a^3/P^2 as a measure of the underlying attractive strength of each center)
    a. Not from universal gravity, for reasoning neutral with respect to mechanism of centripetal force
    b. Nor from an especially tenuous chain of reasoning; the weakest link in the chain would appear to be Laws 3 and 4 themselves, which is why I say that they are doing the main work

D. Resistance and Planetary Motion

1. The other addition to Version 3, here called the "Resistance Scholium", outwardly appears to be nothing more than a way of smoothing the transition from Problem 5 to Problem 6
    a. First paragraph says that up to this point he has been considering motion in non-resisting media as a basis for drawing conclusions about the motion of celestial bodies in the aether
    b. Aether either non-resisting or insensibly resisting
    c. Notice the Cartesian tie between density and quantity of solid matter: Newton has not yet got his concept of mass!
2. Bottom line of argument of first paragraph: results so far are applicable to planets and comets, for no grounds whatever to conclude that aether offering any resistance
    a. Conclusion stated at beginning of second paragraph: "Motions in the heavens are ruled therefore by the laws demonstrated"
    b. Note: propositions here being termed "laws" in original, not after a revision by Newton
3. Argument: no observed "dissipative" effects, which would have to be present were the medium offering any resistance at all
    a. Argument appealing to claim that resistance proportional to density presupposes that density of aether small -- perhaps justified by negligible effects in measuring densities of bodies

   b. Comet argument amounts to saying no need to invoke resistance at all; notice, however, the increasing burden being put on success with comet trajectories
   c. Persistance of basic orbital motion argument also saying no sign of resistance effects
4. Notice that Newton can no longer argue from the absence of minor discrepancies in Keplerian motion to the conclusion that no resistance forces are at work, for now having to concede that discrepancies that have not been strictly accounted for are present
   a. I.e. cannot reason from preceding 9 propositions, and hence must offer independent reasoning
   b. Perhaps part of the reason for the addition: a further consequence of the Copernican Scholium
5. Reasoning in the first paragraph is largely neutral with respect to a vortex theory of planetary motion versus other mechanisms
   a. Resistance from relative motion, and hence no resistance if planets carried along by vortices
   b. But comments about comets do present a challenge to Descartes' account since comets, especially retrograde comets, not being carried along by vortices

E. Projectile Motion on the Earth
1. Second paragraph of the added "Resistance Scholium" claims that Problems 4 and 5 cover motion "in our air" in the absence of resistance effects
   a. I.e. a correction to Galileo's laws of free fall and projectile motion in the absence of resistance
   b. "Given of course that gravity is inversely proportional to the square of the distance from the centre of the earth"
2. First argument that gravity is an inverse-square centripetal force invokes the "Moon test"
   a. Ratio of moon acceleration to g very nearly inverse-square -- a calculation that anyone could do
   b. (For details of the revised "Moon test" using Picard’s or Cassini’s determinations of the radius of the earth, see the Appendix or Notes for Class 13)
   c. Note Newton's phrasing: he does not expressly conclude that gravity holds the moon in orbit
   d. But it is unclear how the argument is supposed to yield the conclusion without the added claim that the centripetal force on the moon and the centripetal force of gravity are one and the same
3. Second argument invokes Halley's observations at St. Helena (15 deg S latitude)
   a. Newton could be taking Halley's measurements from Hooke's letter, or perhaps from conversations with Halley -- e.g. in the second, November meeting
   b. The argument involves a mistaken explanation of the measurements, a mistake carried over from Hooke's letter and Halley's subsequent report in the 1686/7 *Philosophical Transactions*
      (1) Newton could readily have confirmed that this was a mistake
      (2) For a 2570 ft mountain would have only a 0.12 line effect on the length of a seconds pendulum and a 0.013 percent effect on the period of a pendulum clock (resulting in only a 10.6 sec discrepancy per 24 hours)
   c. (Richer's measurements not known to Newton at the time because Richer’s 1679 report issued

only in French and Cassini's report on the expedition to Cayenne was not published until 1684; Richer's measurements were nevertheless known to those at the Academy and Halley)

4. The basic point of the final paragraph concerns the need to refer projectile motion to a proper frame, and not a frame rotating with the earth (as in Galileo)
   a. (Note the concern with frames of reference here in the light of my proposal that this was the provocation for all of Version 3)
   b. The essence of Newton's "experiment" is the difference between a truly inertial frame and a rotating frame in free fall
   c. The insight underlying Coriolis forces generally, and why they are not real forces
5. Newton finally claims that the experiment works to prove diurnal motion, in effect completing the proof of the Copernican system
   a. No mention made of the vagaries in Hooke's measurements
   b. But Problems 4 through 7 and the following Scholium are just what is needed to address these vagaries, for they allow one to calculate a magnitude of the displacement
   c. That magnitude would have raised worries about Hooke's experimental results

IV. In the Aftermath of De Motu, Version 3

A. The Initial Exchange with Flamsteed

1. Sometime in mid or late December Newton initiates a correspondence with Flamsteed, pursuing reliable astronomical information; it ends abruptly with Flamsteed's letter of January 27
   a. First letter missing, and so exact date and contents unknown
   b. Flamsteed already aware that a document on orbital motion by Newton has been registered at the Royal Society, from the grapevine if not from Newton's letter
2. Based on Flamsteed's December 27 reply, Newton's primary concerns in his initial letter appear to have been with (1) the comet of 1680-81 and (2) whether the satellites of Jupiter (and Saturn) conform with the 3/2 power rule
   a. Strongly affirmative answer to latter from Flamsteed, in no small part because of Roemer's speed of light correction; notice too that Cassini's earlier predicted eclipses wrong by almost an hour (not a speed of light effect, but from vagaries in the motion of Jupiter)
   b. But Flamsteed unable to confirm Cassini's satellites of Saturn
   c. In effect, Flamsteed telling Newton that there is no reason to think that the 3/2 power rule does not hold to full precision for the satellites
   d. Notice also Flamsteed's remark about the complexity of our moon's motion versus the well-behaved motions of Jupiter's satellites!
3. Newton's December 30 reply: "your information about the Satellites of Jupiter gives me very much satisfaction;" shifts then to questions about anomalies in Saturn and Jupiter orbits when they are in conjunction with one another

a. One provocation: Kepler's numbers for Saturn fail to conform with 3/2 power rule to the desired percision

b. Newton has obviously made a calculation of the deviation -- one or two solar radii, greatest the year before conjunction

c. Finally asks for precise radii of Jupiter satellite orbits vis-a-vis Jupiter's diameter, probably because trying to obtain comparative measures of (absolute) centripetal force

4. Flamsteed replies that he has no evidence of such an anomaly in Saturn's orbit, and that he thinks any anomalies in Jupiter's orbit can be eliminated via better orbital parameters than Kepler had

a. Admits that he has yet to be strict enough with Saturn's orbit to "affirme that there is no such exorbitation as you suggest," and promises to look into the matter

b. Expresses some amazement at the very idea of these two planets affecting one another across such a broad distance (from 5.2 AU in the mean for Jupiter to 9.6 AU for Saturn)

c. Gives elongations of the satellites of Jupiter, as requested (clearly using a micrometer)

5. Newton's January 12 reply finds him puzzled by failure to observe an anomaly in the orbit of Saturn, but now indicating that calculations based on Flamsteed's numbers had shown him that Jupiter's "vertue is less than I supposed" (presumably, after conversion to full astronomical units)

"Your information about ye error of Keplers tables for Jupiter and Saturn has eased me of several scruples. I was apt to suspect there might be some cause or other unknown to me, wch might disturb ye sesquialtera proportion. For ye influences of ye Planets one upon another seemed not great enough tho I imagined Jupiter's influence greater than your numbers determin it."

a. Main concern: apparent discrepancy too large to account for via interaction effect, and hence perhaps an indication of some further force at work

b. Letter ends with request for best current orbital parameters "that I may see how the sesquiplicate proportion fills ye heavens together wth another small proportion wch must be allowed for," and says that he will get around to calculating the lines described by the comets of 1664 and 1680

c. The reference to the small proportion is presumably to corrections that must be made to 3/2 power rule from two-body interaction effects: a^3/P^2 varies as $(1+c/C)$ – e.g. as $(1+C_j/C_h)$

d. This small proportion provides basis for concluding that none of the planets for which the value cannot be inferred from satellites have an $[a^3/P^2]$ significantly greater than that of Jupiter, for if they did, the small proportion would be detectable!

6. Newton would appear to be pursuing a new line of evidential reasoning at this point: if can show that no other forces at work besides the inverse-square centripetal forces, then can use deviations from Keplerian motion -- especially deviations from planet interactions -- as evidence!

a. Presumably as evidence in support of mutual interaction, and conclusion that inverse-square acclerations toward Jupiter and Saturn extend indefinitely far into space

b. Thereby substantiating their effects on the sun

c. A potential source of independent evidence, though first must remove the impediment to such evidential reasoning raised in the Copernican Scholium

7. Midway through this letter of 12 January 1685 Newton makes an offhand remark worthy of note: “Now I am upon this subject I would gladly know the bottom of it before I publish my papers.”

B. Drafts of New Definitions: Absolutes

1. The next manuscript we have is entitled *De Motu Corporum in Mediis Regulariter Cedentibus*, that is, "in uniformly yielding media," that consists only of preliminaries to a theory
   a. A series of definitions -- akin to the series opening Part IV of *Horologium Oscillatorium* -- many of them reworked and with some inserts and number changes indicating a subsequent revision
   b. Dates unclear, but sometime in the first half of 1685, likely before April
2. The point of the 18 definitions is made clear at the end:

   "The aim of explaining all these things at length is that the reader may be freed from certain vulgar prejudices and, imbued with the distinct principles of mechanics, may agree in what follows to distinguish carefully from each other quantities which are both absolute and relative, a thing very necessary since all phenomena depend on absolute quantities."

   a. More than just a need for sophistication, for if phenomena depend on absolute quantities, must distinguish them from relative if going to achieve empirical refinements of initial simple theories
   b. As he goes on to point out, the distinction also allows him to duck scriptural arguments (in much the way that Descartes wanted to)
3. Absolute space presented not only as conceptually distinct from relative space, but also as empirically distinguishable
   a. Newton's first argument is conceptual -- i.e. philosophical, for it claims only that we have such a concept and it is fundamentally presupposed by other concepts
   b. Second argument, in Definition 4, is empirical: absolute and relative distinguishable by descent of heavy bodies (Newton's experiment again)
   c. Note the felt need to argue for the definition: arguing for an empirical distinction, not for mere use of words
4. Absolute motion versus relative motion: claim is that they are distinguishable, at least up to a point
   a. Basic argument from *conatus a centro* (as in *De Gravitatione* ...)
   b. Main point: because *conatus a centro* is certain and determinate, there is also some certain and determinate quantity of real motion in individual bodies
   c. Reminiscent of Descartes, argues that forces acting on a body a basis for distinguishing between absolute and relative motion, for former requires force on body in question
   d. To some extent the argument here amounts to saying that the whole way of conceptualizing motion, centered on the principle of inertia, presupposes a distinction between absolute and relative motion, and so the distinction gains warrant from the effectiveness of this conceptualization

e. But then that distinction is going to be drawn in terms of the action of real forces in contrast to the actions of such apparent forces as the Coriolis force Newton has idnetified

5. The definition of quantity of motion is outwardly Cartesian, though presumably the reference to velocity makes it a vector quantity
   a. Quantity of matter -- not solid matter -- here being distinguished from weight, though this is also done by Descartes
   b. It "is to be estimated from the amount [*copia*] of the corporeal matter which is usually proportional to its gravity" -- "usually" leaves open the possibility that not always
   c. (Remarks about pendulum experiments will be commented on below, in conjunction with the further Definitions)
6. Definition of centripetal forces lists gravity ("tending to the centre of the earth"!), magnetic force tending to the center of the magnet (?), and "the celestial force preventing the planets from flying off in the tangents of their orbit"
   a. Note that gravity and magnetism here distinguished from the celestial force
   b. Centripetal force contrasted to innate or internal force – it is an impressed force
   c. No distinction in any of the discussion of forces between impulse-like and continuous forces

C. Drafts of New "Laws": The Third Law

1. The first two laws are in effect "Newton's first two laws", perhaps stated with more concern for clarity than in Version 3 of De Motu
   a. Claim that Galileo employed these two laws to discover the parabolic trajectory of projectiles is, of course, open to question, as is the claim about experiments supporting it
   b. The thing to notice here is that Newton is now feeling the need to offer some sort of explicit defense of the two laws
   c. Question: what is defense supposed to show -- that the laws are true, that they are warranted, or that they are just reasonable?
2. The third is "Newton's third law", to be found earlier in his work on impact in the Waste Book and in the Lucasian lectures on algebra (as in earlier handouts on impact)
   a. The empirical arguments offered in its defense appear to be saying nothing more than that it is compatible with known empirical phenomena
   b. The "derivation" of this law from Definitions 12 and 14, as Newton's explanation of the derivation shows, presupposes the main content of the law -- equal and opposite -- and hence does not add much
3. In fact the third law goes together with the fourth and the fifth -- Laws 3 and 4 of De Motu Version 3; Newton remarks that they "mutually confirm each other", but offers no demonstration
   a. The fifth law does most of the work in De Motu Version 3, but the claim it makes seems least open to any sort of comparatively direct empirical test

   b. Newton may have turned to the third law while searching for a preferred law that would give him the fifth law
   c. The fifth law holds for the interaction of two bodies if and only $\Delta(B_a v_a)+\Delta(B_b v_b)=0$
   d. The obvious virtue of the third law is that it does lend itself to a more direct empirical defense
   e. The fact that all three laws are stated on a par here suggests that Newton is leaving open the choice over which among them is primary
4. The sixth law is simply a more refined version of the claims about resistance offered in the two versions of De Motu
   a. What is striking is the refusal to assert that this law is exact
   b. Questions: (1) in what sense will it suffice for this law to be approximately true?; (2) why the exception here and not above?
   c. Finally, notice the relegation of sphericity to a mere assumption of convenience here
5. Newton's orientation seems to be one of searching for laws that will give him the results of De Motu Version 3 -- in particular, Copernicanism -- in contrast to searching for still further laws to extend these results, or for a minimal set of laws from which to derive these results
   a. The greater preoccupation with offering a defense of the laws here than in De Motu suggests that he is looking for laws that will not raise objections
   b. The question then is why he feels more of a need for a defense here than in the version of De Motu that he sent to London
   c. We should be cautious not to jump to conclusions on the basis of looking at these redrafted laws from the perspective of the *Principia* rather than that of De Motu

D. Further Drafts of Definitions: The Concept of Mass

1. Herivel's second set of draft definitions mistakenly combines an intended insert into the first set with a preliminary draft of definitions for the subsequent Lucasian lectures for 1685
   a. As facsimile in Appendix shows, definitions 6, 7, and 12 intended as revision of first set
   b. Insert later because the mass-weight experiment is here discussed as having been completed
2. Thus, the systematic distinction among three "quantities of centripetal force -- the absolute, the accelerative, and the motive -- comes after the fragment entitled "... in uniformly yeilding media"
   a. This distinction and the clear assertion about weight first appear in the second half of 1685, accompanied by the first uses of *massa* and *inertia* in 1685
   b. "Accelerative force" stems from wanting a term for forces that produce the same acceleration at any point in all bodies
3. The important new feature is the attempt to draw a clearer distinction between weight and quantity of matter, with the latter strictly proportional to the former
   a. "*Pondus*" a word used widely to somewhat the same end in Lucretius's *De Rerum Natura*, though Newton, clearly not happy with the ambiguity of the word, later changes to "*massa*"

b. Appeal to pendulum experiment without much explanation of why it shows what he claims for it

c. Motive force producing motion of the pendulum is gravity, with resistance forces the same when velocity the same, and motion produced is a product of quantity of matter and change in velocity

d. If quantity of matter were not strictly proportional to weight, pendulums of the same weight, but with different materials inside identical bobs should (or might) display different motions

e. (Reasoning should be considered in the light of Descartes' analysis of the relationship between weight and quantity of matter)

4. Question: what considerations are leading him to draw the distinction here, but not in De Motu Versions 1 and 3?; here is one possibility

   a. The pivotal conclusion in the "proof" of Copernicanism was that $C_h/C_j = r_j/r_h$, for this was the basis for reaching a conclusion about r_h

   b. But the point to which these radii are referred is a "center of gravity", so that $W_j{*}r_j = W_h{*}r_h$, where the *W*'s refer to what the Sun and Jupiter would weigh at the surface of the earth

   c. In other words, $C_h/C_j = W_h/W_j$

   d. But their weight at the surface of the earth is a parochial quantity; we want a quantity that they have everywhere, independently of being at the surface of the earth, and that would yield their weight at this surface

5. By the reasoning that distinguishes the three quantities of centripetal force and justifies the claim that the quantity of matter is proportional to the weight, can now relate the absolute force to a non-parochial quantity: $C_h/C_j = M_h/M_j$, where M represents the quantity of matter

   a. But Newton is doing more than just this, for, by virtue of the pendulum experiment, he is also tying this non-parochial quantity to the resistance a body displays to changes of motion

   b. And hence to bulk (*moles*) in impact, "apart from considerations of gravity"

E. The Law of Gravity Emerges

1. With the distinction of what he later came to call *mass* and the relation $C_h/C_j = M_h/M_j$, now have the law of gravity staring us in the face

   a. Centripetal acceleration at any point is proportional to $C/r^2 = M/r^2$

   b. Force is proportional to change in motion and hence to Mm/r^2

   c. Therefore, forces must be equal, via symmetry, yielding the third law as well, this time holding for mutually interactive centripetal forces, and not just forces in impact

2. Alternatively, Newton might have reached law of gravity by first using the second law to conclude that the force on each body is proportional to its mass, the *C* of the other, and inversely proportional to the square of the distance between them

   a. He could then have invoked the third law to equate these two forces, allowing him to conclude that their respective *C*'s are proportional to their masses (as he does in the *Principia*)

b. My guess is that he played different ways off against one another (as in the Appendix)

3. Furthermore, we have before us a line of reasoning that leads to *universal* gravitation:
   a. The absolute quantity of force depends on the quantity of matter in the body at the center, so that if any of that matter were taken away, the absolute force would diminish
   b. Unless the absolute force is a non-extensive, "emergent" property of the total matter forming the body, then each part of the absolute force must be associated with a part of the total matter
   c. I.e. each particle of matter forming the central body must have a centripetal attraction associated with it in such a way that the total centripetal attraction results -- is compounded out of -- from the net effect of all the particles
   d. But, in the absence of having any reason to think that the matter forming the celestial bodies is distinctively different from other matter, can generalize to claim that all matter has such a centripetal attraction to it
4. This line of reasoning from the "proof" of the Copernican system to universal gravity has a number of lacunae in it, especially when considered in the light of its rather extraordinary conclusion
   a. Chief among these is the claim that the attractive characteristics of a celestial body do not arise as an "emergent" property of the matter forming it
   b. The obvious way of responding to this lacuna is to show that the attractive characteristics of the body can arise from the combined effects of the individual particles of matter
   c. Newton shows this in the *Principia*, and in a letter to Halley in 1686 remarks that he did not have a complete argument for universal gravity until he had shown it (in the spring of 1685)
5. The putative line of reasoning extending from the "proof" of Copernicanism to universal gravity yields a conclusion that has a much greater burden of proof on it than Copernicanism has
   a. Not only does action at a distance violate the dictates of the mechanical philosophy, but now endowing matter generally with a new seemingly occult property, *attraction*
   b. Obvious question: what possibilities are there for developing evidence to meet this burden?

F. Toward Universal Gravity: Weight $\propto$ Inertial Mass

1. My proposal for how the law of gravity emerges hinges on an elaborate experiment Newton carried out during the spring of 1685, but it does not explain what prompted him to carry it out
   a. The conclusion drawn from the experiment amounts to saying that, in the absence of other forces, gravity always produces the same change of motions in all bodies, regardless of their weight, shape, and the material composing them
   b. But this was a claim from Galileo and Huygens that was universally accepted
   c. So, why take the trouble to carry out such an experiment
2. Another manuscript gives us what appears to be the best clue, Newton's original version of his "System of the World," entitled *De Motu Corporum, Liber Secundus*

a. Written in what Newton later called "the popular style," namely the same style as Descartes' *Principia*, with numbered article and postils in the margin (see Appendix)
b. Fifty-six page, eighty one Article draft in Humphrey Newton's hand, dating from spring and summer of 1685, heavily re-worked by Newton himself at more than one later stage
c. By virtue of its being complete instead of a mere fragment, its paralleling Book 3 of the *Principia*, and its including so many modifications made by Newton, it is the most informative manuscript we have from while Newton was developing the *Principia* in 1685

3. After concluding that the centripetal forces toward the sun, Jupiter, Saturn, and the earth vary inversely with the squares of the distances from them, Newton turns to the question of other quantities with which these forces vary
   a. Article 18 (see Appendix) concludes that the 3/2 power rule could not hold, nor the regularity of the motions of the satellites of Jupiter, unless the centripetal action of the sun on all these bodies is proportional to the quantity of matter in them
   b. For only then, for example, could Jupiter and its satellites experience essentially the same centripetal acceleration toward the sun
   c. That Newton would worry about this may reflect Flamsteed's question about why the motions of those satellites are so regular, while the motion of our moon is not
4. Notice too that Newton's subsequently deleted all but one of the occurrences of the word *pondus* in Article 18, replacing them not with *massa*, but with different phrases amounting to quantity of matter
   a. Presumably did this because of need for the word to mean *weight* at the one place, and leaving the other occurrences in place produced quite an ambiguity
   b. Nowhere in *Liber Secundus* does *massa* occur, but only later
5. The very next Article (see Appendix) then describes the experiment, in more detail than anywhere else including the *Principia*, showing that *weight* is proportional to *quantity of matter*, with the number of the Proposition that licenses the conclusion to be filled in later
   a. Claim: demonstrated this relationship more precisely than anyone theretofore had
   b. In *Principia* claims to within 1 part in 1000
   c. By the beginning of the 20^{th} century, had been shown to within 5 part in 100 million
   d. Notice too the deletion at the end of this Article, dropping the idea of using *weight* or *heaviness* to denote quantity of matter
6. The suggestion then is that Newton performed this experiment to confirm that some centripetal forces -- in particular, terrestrial gravity -- do act equally on all bodies at any one location
   a. In Einstein's hand this became known as the (weak) equivalence principle, asserting the equivalence of gravitational mass with inertial mass
   b. In Newton's hand, it asserts only that the magnitude of gravitational forces varies with the inertial mass of the bodies on which they act

G. The Law of Gravity: Interaction vs.Force

1. Article 20 (see Appendix) then cites the third law of motion to justify the conclusion,

> And since the action of centripetal force upon the attracted [*attractum*] body, at equal distances, is proportional to the matter in this body, it is also reasonable to grant [*rationi etiam consentaneum est*] that it is proportional as well to the matter in the attracting [*trahene*] body. For the action is mutual, and causes the bodies by a mutual endeavor [*conatu mutuo*] (by law 3) to approach each other, and accordingly the action in one body must necessarily be in conformity with the action in the other.

   a. In other words, the magnitude of the centripetal force in question is proportional not only to the quantity of matter in the body on which it acts, but also to the quantity of matter in the body toward which the other is drawn

   b. Notice here that the mutuality of the action and the applicability of the third law is simply asserted; nothing earlier in *Liber Secundus* has provided warrant for this

2. What is left unsaid but appears to be motivating Article 20 is a somewhat paradoxical aspect of the conclusion that the centripetal forces toward the sun and earth act on bodies in proportion to the quantities of matter in them

   a. According to what later became known as the law of inertia, these forces act as external causes, as a consequence of which their motion becomes curvilinear

   b. But how can the same external cause adjust itself to the body on which it acts at any location?

   c. The proposal in Article 20: "One body can be considered as attracting and the other as attracted, but this distinction is more mathematical than natural"

3. Article 21 (see Appendix) then goes on to elaborate this last idea rather clumsily, with redundancy that suggests Newton is struggling with how to make the point he is trying to make

> And hence it is that the attractive force [*vis attractiva*] is found in both bodies. The sun attracts [*trahit*] Jupiter and the other Planets, Jupiter attracts its Satellites and similarly the Satellites act on one another and on Jupiter, and all the Planets act on one another. And although, in a pair of Planets, the action of each on the other can be distinguished and can be considered as paired actions [*actiones*] by which each attracts [*trahi*] the other, yet inasmuch as these are actions between two bodies, they are not two but a simple operation between two termini.

   a. In other words, the forces in question are not really external causes that adjust themselves to the bodies on which they act, but a single mutual action between the bodies

   b. Notice now how Newton is making a claim for which he has no empirical evidence, but instead making it in response to a conceptual worry tied to the notion of cause

   c. Nothing remotely akin to Article 21 occurs anywhere in any edition of the *Principia*

4. The very next Article attributes our inability to perceive such mutual gravitational interaction to the attractive forces being too small, followed by a long, heavily reworked Article in which Newton tries to justify the claim that such interaction takes place among all terrestrial bodies

a. After inserting six sentences into the middle of Article 23 arguing that the action has to be between bodies, Newton gives up on the entire Article, cancelling it in its entirety

b. Nothing akin to this Article appears anywhere in the first edition of the *Principia*; in the second edition he added the thought experiment about parts of the earth attracting one another

5. These Articles from *Liber Secundus* thus suggest that Newton was led into his universal gravity among all particles of matter -- and the related claim that gravity toward celestial bodies is compounded out of gravity toward their individual parts -- by the question of how motions of the satellites of Jupiter could be so regular when that of our moon is not

a. For, that left him concluding that the action of the sun on Jupiter and its satellites must be virtually the same, and hence independent of the quantities of matter in them

b. And that makes sense as a form of causality only if there is an action between the matter of the sun and the matter in each of them, an likewise between the earth and bodies on and near it

c. And then more generally, only if all matter interacts in accord with the law of gravity

6. Regardless, Newton always claimed that he did not arrive at universal gravitation by leaping to it as a hypothesis, and the line of reasoning I am proposing, starting with the Copernican scholium, does let it emerge in a step-by-step fashion, in response to a sequence of naturally arising questions

a. Universal gravity, far from being a bright idea, involves a sequence of (at the time) ever more implausible claims (see Appendix), including ones not only beyond all available empirical evidence, but as well beyond the bare law

b. And the law itself was reached only under the assertion lifted from the definition of *pondus* that weight varies as the product of *pondus* (=*mass*) and centripetal change of motion

7. Notice how immense the burden of empirical proof has become versus what it was in either version of De Motu (see Appendix); this was the burden to which I claim the *Principia* was a response

V. Toward an Emerging Conception of Science

A. The Conception of Evidence in De Motu Version 1

1. The different versions of De Motu present us with a sequence of contrasting views about the kind of evidence that can be developed out of observations

a. The sort of claim that can receive strong evidential support from observations, and the amount of support it can receive

b. The extent to which conclusions from the evidence have to be qualified because of inexactitudes of observation

c. The conclusions that can be drawn from any discrepancies between theory and observation

2. Version 1 has the promise of supporting extremely strong conclusions about Keplerian motion being exactly true, or exactly true save for various secondary effects

a. Licenses the conclusion that e.g. other trajectories within observational accuracy no longer need be considered as viable alternatives (and other ratios besides exactly 3/2)

b. Licenses the conclusion that Keplerian motion, including the area rule, basic, and that any deviations should be viewed as superposed secondary effects

3. Version 1 allows strong conclusions to be drawn from any systematic discrepancies between observations and theory

a. Prima facie, some force besides the dominant inverse-square centripetal force is at work

b. Can therefore use discrepancies as basis for characterizing these forces, if not for constructing a theory of them -- i.e. discrepancies provide answers to questions about these forces

c. Of course, if no coherent characterization as forces, then in a position to draw one of two other conclusions

(1) A systematic error in observation, again amenable to characterization via the discrepancies

(2) Or a false theory, requiring modification that can be characterized via the discrepancies

d. Notice that such inferences, including the last one, can be drawn from even very small systematic discrepancies; this is what I mean by an improvement in the quality of evidence

4. Can thus think of Version 1 as focused on the issues of the perfectibility of astronomy and the true motions of the five planets around the sun -- the other major issue arising out of the skeptical doubts toward astronomy to which Kepler was replying in his unpublished *Apologia*

a. Newton had never shown much interest in these issues in the 1660's and 1670's, presumably because he was unaware of how much attention was being given to it in some circles

b. Perhaps his first conversation with Halley made him aware of it, and this can then account for his change in attitude about the importance of his 1679 "calculation" of the ellipse

c. Would dovetail with the interest he had previously shown in issues concerning the perfectibility of empirical science generally -- e.g. in his interest in adding resistance effects to projectile motion, not to mention his remark about “science” in his Optical Lectures of the early 1670s

5. Version 1 compatible with two different views of the "perfectibility" of empirical science -- i.e. with two different views about the treatment of secondary effects

a. On the one hand, may just be able to characterize them empirically, thus licensing stronger conclusions about the primary mechanism by permitting empirical corrections of observations

b. Or may be able to develop a theory of secondary mechanisms, thus opening the way to pursuing truly exact science

c. Ambiguity between these two most clear in the proposed treatment of resistance in Version 1

B. The Conception of Evidence in Version 3

1. With Version 3 get a shift from the issues of the perfectibility of orbital astronomy and the true motions of the planets around the sun to the issue of empirically resolving, once and for all, the issue of the world systems

a. Version 3 provides the strongest empirical proof yet for Copernicanism, a concern at most in the background in Version 1

b. But at the price of losing many of the conclusions about the perfectibility of orbital astronomy

2. If concede that calculating the actual orbits is beyond the capacity of the human intellect, then can no longer draw such strong conclusions from any discrepancies between observation and theory
   a. Given a discrepancy, limited basis for concluding that it is from (1) celestial body interactions, (2) some other mechanism, (3) systematic observational error, (4) a false underlying theory
   b. Thus little if any hope of carrying science to a higher level of perfection than that achieved in Version 3, it would seem
3. The conclusions that can be reached about Keplerian motion as the basic ideal are therefore much tenuous, and hence in jeopardy, than with Version 1
   a. More room now for arguing for e.g. other alternative trajectories within observational accuracy, and hence for arguing that Keplerian maybe not the basic ideal
   b. Conclusion about Keplerian as "mean" open to a predominately "instrumentalist" interpretation in the absence of showing that it is strictly speaking the mean
4. Little if any possibility of developing a theory of secondary effects from discrepancies between observations and theory if motions really beyond human capacity to calculate
   a. Can still characterize discrepancies, but only in the form of an approximate empirical characterization
   b. I.e. can characterize them in order to argue that they are attributable to various secondary effects, and hence are compatible with the conclusion that observations are compatible with theory (ignoring certain secondary effects)
   c. Thus in a way can characterize discrepancies to allow corrections of observations, but not precise corrections
5. In other words, the sort of evidential argument that can be run off observations, given Version 3, is fully akin to that which Huygens and rational mechanics was committed
   a. No exact science of secondary effects, hence no exact science
   b. But still can develop empirical arguments that allow one to conclude that observation fully compatible with theory, once theory interpreted as making claims about idealized
   c. And in some cases may be able to characterize or neutralize secondary effects sufficiently to draw some stronger conclusions from discrepancies -- e.g. Huygens and center of oscillation

C. Intimations of a New Conception of Evidence

1. If the view of evidence implicit in Version 3 were the best that we could hope for, then the proposal for universal gravitation would have much the same status as Descartes' vortex theory
   a. A philosophical conjecture that is compatible with observation, at least up to the extent to which we can meaningfully compare observation and theory
   b. But still primarily a philosophical conjecture, different from Kepler's proposed mechanisms mostly in how sweeping it is

2. But if can conclude that no other mechanisms at work in the orbital system, then perhaps can develop stronger evidence for universal gravity by showing that deviations from Keplerian motion are attributable to it -- just the sort of evidence Newton seemed to be pursuing in correspondence with Flamsteed, especially in question of Jupiter's effect on Saturn
    a. I.e. then perhaps a stronger conclusion than just that the actual motions do not confute universal gravity
    b. For evidence that all celestial bodies attract one another in accord with universal gravity
    c. This via an argument that deviations from Keplerian are fully consistent with such gravitational interaction, coupled with argument that no other mechanism producing any deviations
3. Two possible ways in which such a more ambitious line of evidential reasoning can be developed, one of them stronger than the other
    a. Perhaps can only show that universal gravity offers the best explanation of the deviations from Keplerian motion by showing that it explains, at least to a high degree of approximation, the actual deviations
    b. Or can show that universal gravity fully accounts by showing that the actual deviations can be derived from universal gravity to within observational accuracy
4. Either way, back to an at least qualified version of the "perfectibility" of orbital astronomy, for even if cannot achieve an exact science of the motions, can still reach strong conclusion about what mechanisms are at work
    a. I.e. in much the same position with respect to orbital astronomy as projectile motion with resistance, for can at least give approximate characterization of second-order effects
    b. And this sufficient to draw strong conclusions about mechanisms
5. Goal here should be contrasted with that which *Principia* ultimately faces, for an empirical defense of universal gravity is one thing, and an empirical defense of the conception of physics announced in the Preface to the First Edition is quite another
    a. That conception -- all physical processes from a few fundamental forces acting among ultimate particles of matter -- an alternative to the mechanical philosophy
    b. Empirical defense of it calls for truly exact science

D. An Evolution in Conceptions of Evidence

1. Entire course can now be seen as an evolution in conceptions of what can be done with empirical evidence -- an evolution starting with Copernicus and Tycho and still proceeding as Newton starts writing the *Principia*
    a. Copernicus raised the issue and proposed the idea that a better theory would yield better agreement with observation
    b. But Tycho was the first to push the idea that pursuing precise agreement between observation and theory was a means of empirically resolving issues in astronomy

2. Kepler added the idea that some issues in astronomy were going to be resolved only through identifying the physical processes governing planetary motion
    a. And with it added the idea of inferring underlying physical processes from the exact motions, so that pursuit of exactitude became a vehicle for drawing strong conclusions about physics
    b. But insufficient constraints from a general mechanics of motion undercut the strength of his inferences
3. Galileo and Descartes, along with others, began forming such a general mechanics of motion, but under a conception of empirical evidence in which they abandoned all hope of a truly exact science, instead resorting to an exact science of an idealized situation
    a. Principal conclusion from empirical evidence: theory fully compatible with, and hence burden of proof on anyone who wants to propose an alternative theory
    b. But limited empirical grounds for drawing inferences about underlying mechanisms; need either independent philosophic basis, or need to qualify conclusions about underlying mechanisms
4. With Huygens get a more ambitious conception of empirical evidence, for pursuit of precision can yield conclusions about second-order effects, if not second-order mechanisms, in certain suitable cases, as illustrated by his solution for the center of oscillation
    a. Cases in which theory allows experimental paradigms in which such second-order effects as resistance either neutralized or characterizable
    b. Conclusions of the sort that *g* not everywhere the same, but subject to small variations
    c. Much tighter constraint then on Kepler-like reasoning to underlying mechanisms, as illustrated in Huygens's evidential arguments for his theory of gravity
5. Finally with Newton see pursuit of some sort of empirical "proof" of basic claims in physics, pursuit of goal of something stronger than hypothetico-deductive evidence for claims about physics, a conception that he had first announced in his work on optics in the early 1670s
    a. "Proof" that would justify disregarding the possibility of alternative hypotheses equally in conformity with observation
    b. And hence of truly being able to settle issues of underlying physics comparatively definitively on empirical grounds

E. Parallel Evolution in the Science of Orbital Motion

1. Four initial steps that freed orbital astronomy from the millennium and a half Ptolemaic tradition:
    a. Copernicus's challenge that retrograde motion mere appearance
    b. Tycho's body of positional observations of uniform quality
    c. Galileo's telescopic observations, showing among other things how much less was known than had been thought
    d. Kepler's reforms, culminating in the *Rudolphine Tables* and a wholly new preoccupation with being able to calculate locations within observational accuracy

2. Steps before 1650 toward a science of the mechanics of motion of comparable quality to the mechanics of phenomena of static equilibrium
   a. Galileo's formulation of a mathematical science of select motion under uniform gravity along parallel lines, achieved by idealizing the motion to be free of resistance
   b. Descartes' emphasis on the *conatus* to recede from the center in curvilinear motion and the consequent need for an outside impediment or force in all curvilinear motion
3. Huygens's building on the mathematical theory of Galileo and Descartes' *conatus*
   a. Specifically, by extending Galileo's fragment of motion under "Galilean" gravity to include pendular motion of forms determined by evolutes
   b. And developing Descartes' idea into a mathematical theory of uniform circular motion and centrifugal force
4. Prompted by Hooke, Newton's discovery that the mathematical theory of uniform circular motion can be generalized into a mathematical theory of motion under centripetal forces -- i.e. motion in which the area rule holds
   a. In the registered version of De Motu, linking Kepler's three "laws" together through inverse-square centripetal forces toward the Sun, Jupiter, etc.
   b. And then taking the further step in Version 3 of allowing for the interaction between centripetal forces directed toward different bodies and having to face the problematic ramifications of this
5. One can think of the list of ten advances (see Appendix) as milestones toward Newton's *Principia* and the Newtonian Revolution even though a great deal remained to be done between it and them

F. Three 17th Century Revolutions in Evidence

1. Overall course offers grounds for claiming that the scientific revolution reflected in those ten milestones involved a revolution as well in views about what can ultimately be achieved via empirical evidence
   a. A revolution in conception of how to marshal data into evidence, and hence in conception of how to make the most of evidential reasoning
   b. From naive Aristotelian (and Ptolemaic) view of saving the phenomena to a view under which empirical world provides clear answers to questions
2. First revolution was the commitment to the idea that the empirical world ought to be the ultimate arbiter in matters of natural philosophy
   a. The aspect of the scientific revolution that is most widely recognized, in large part because it is the most pervasive, stretching from Copernicus through Newton, and even including Descartes
   b. But not the aspect that is responsible for distinguishing modern science from science before the scientific revolution
3. Second revolution came from the realization that higher quality empirical evidence can be achieved through theory-mediated evidence -- through theory being a vehicle for turning data into evidence

a. First indication of this from Kepler's use of deviations from a first-order theory as evidential basis for second-order refinements and conclusions about underlying mechanisms
b. Second indication from Galileo's realization that a network of theory opens the way to designing more telling experiments and from Descartes' realization that theory is crucial to eliminating the ambiguity of what the world is telling us
c. But fullest realization of the use of theory as an instrument in marshalling evidence comes with Huygens and rational mechanics, both in his measurements of the distance of fall in the first second and in his solution for the center of oscillation

4. Third revolution in conceptions of evidence is the one I prefer to call the "Newtonian revolution", achieved through the *Principia*
   a. Pursuit of exact agreement between theory and observation as a means of reaching very strong conclusions about underlying physical processes and mechanisms
   b. More specifically, the demand that every discrepancy between theory and observation be taken as telling us something that we must pursue until we establish what its source is
   b. First glimmers of this revolution in the various versions of De Motu and the work in their wake
   c. Something Newton discovering in 1684/85, on my view; not something he had formed earlier, his remark about "natural science supported by the greatest evidence" notwithstanding
5. The third of these revolutions is the most important, I claim, for it is what really distinguishes modern advanced science and is responsible for its extraordinary epistemic achievement
   a. It is the least understood of the three, and hence ultimately requires the most discussion -- why we are continuing to a second semester
   b. But the unfolding vision of it, I claim, is what drove Newton on to the full *Principia*, though when this vision became clear, even as a possibility, is unclear
   c. Given the evolution of the *Principia* itself, perhaps not until well into 1685, if not later
6. And what lies behind this vision is his own perception that universal gravitation is not a mere hypothesis, but is instead something he basically established as a "scientific" fact by deducing it from phenomena of motion out of the theory of motion initiated in De Motu

Select Sources

Whiteside, D. T. (ed.), *The Mathematical Papers of Isaac Newton*, vol. 6, Cambridge University Press, 1974.

-------- (ed.), *The Preliminary Manuscripts for Isaac Newton's 1687* Principia*, 1684-1686*, Cambridge University Press, 1989. (Contains facsimiles of the handwritten manuscripts)

--------, "Newton's Early Thoughts on Planetary Motion: A Fresh Look," *British Journal for the History of Science*, vol. 2, 1964, pp. 117-137.

--------, "Before the *Principia*: The Maturing of Newton's Thought on Dynamical Astronomy, 1664-1684," Journal for the History of Astronomy, vol. 1, 1970, pp. 5-19.

--------, "The Prehistory of the *Principia* from 1664 to 1686," *Notes and Records of the Royal Society of London*, vol. 45, 1991, pp. 11-61.

Herivel, John, *The Background to Newton's* Principia: *A Study of Newton's Dynamical Researches in the Years 1664-84*, Oxford at the Clarendon Press, 1965.

The Correspondence of Isaac Newton, vol. 2, ed. H. W. Turnbull, Cambridge University Press, 1960.

Hall, A. Rupert and Hall, Marie Boas (eds.), *Unpublished Scientific Papers of Isaac Newton: A Selection from the Portsmouth Papers in the University Library, Cambridge* , Cambridge University Press, 1962.

Newton, Isaac, *De Motu Corporum, Liber Secundus*, U.L.C. manuscript Add. 3990 (translations by Anne Whitman modified by George E. Smith).

Wilson, Curtis, "The Newtonian Achievement in Astronomy," in *Planetary Astronomy from the Renaissance to the Rise of Astrophysics*: *Part A*: *Tycho Brahe to Newton* ed. René Taton and Curtis Wilson, Cambridge University Press, 1989, pp. 233-274.

--------, "From Kepler's Laws, So-called, to Universal Gravity: Empirical Factors," in *Astronomy from Kepler to Newton: Historical Studies*, Variorum Reprints, 1989; originally in *Archive for History of Exact Sciences*, vol. 6, 1970, pp. 89-170.

Harper, William and Smith, George E., "Newton's New Way of Inquiry," in Jarrett Leplin (ed.), *The Creation of Ideas in Physics: Studies in the Methodology of Theory Construction*, Kluwer, 1995, pp. 113-166.

Smith, George E., "How Did Newton Discover Universal Gravity?," *The St. John's Review*, vol. 45, 1999, pp. 32-63.

--------, "Newton's Evolving Conceptualization of Gravity in 1685," a talk given at the Conference *The Mechanization of Geometry from Antiquity to the Modern Age*, Max Planck Institute for the History of Science, Berlin Germany, 23-25 June 2014. (available in Supplementary Material)

Ball, W. W. Rouse, *An Essay on Newton's* Principia, Macmillan, 1893; reprinted by Johnson Reprint Corporation with an Introduction by I. Bernard Cohen, 1972.

Credits for Appendix

Slides 4, 6, 8, 13, 14, 33-36: Whiteside (1999)

Slides 5, 7, 9, 15, 17: Hall and Hall modified (1962)

Slides 10-12, 29, 42, 43: Smith (1999)

Slides 18-28, 30, 31: Turnbull (1960)

Slides 37-41: Herivel modified (1965)

Slides 44, 49: Newton (U. L. C. Add. 3990)

Slides 45-48: Smith (2014)