d. The discovery that the $1/r$ distance rule can be reconciled with the area rule by having v be the component of velocity perpendicular to the radius vector is nowhere stated clearly in *Astronomia Nova* or quite anywhere else by Kepler, but only subsequently

e. The orbit is an exact ellipse (or, as he adds in a letter to Fabricius, differs insensibly from such an ellipse), for "here the consequences derived from physical principles come into agreement with the results of observation and the vicarious hypothesis." (Wilson, p. 21)

7. Upshot: ellipse and diametral distance rule hold as perturbations of a circular motion produced by a second physical action superposed on the basic action of the Sun, with the second action varying in strength from planet to planet

IV. Kepler's Original Justification for the Two "Laws"

A. The Context of Discovery versus the Context of Justification

1. Now want to turn to the question, what exactly did Kepler's evidence show about the orbit of Mars?
   a. Need to be careful here because the evidence for a scientific claim typically changes over time, sometimes quite rapidly
   b. Want to determine the best evidential argument as of 1609 for the following claim: the orbit of Mars is an ellipse (with minor axis 0.429 percent shorter than major), along which Mars sweeps out equal areas in equal times vis-a-vis the Sun; furthermore, the plane of this ellipse, which passes through the Sun, is tilted 1 deg 50 min relative to the plane of the Sun-Earth orbit
   c. Then want to decide what his best evidential argument in 1609 actually shows about this claim
2. Here doing something a little controversial, for I am implicitly invoking Reichenbach's distinction between the context of discovery and the context of justification
   a. I.e. separating the steps leading to discovery from the steps justifying the conclusion
   b. Former, of course, need not be rational or logical at all
   c. But latter, on the view that the two are distinct, must be rational and logical for justification
   d. To put the point differently, justification will in general require a rational reconstruction of the discovery process, eliminating elements of irrationality and leaving just an evidential argument
3. Kuhn, in particular, has attacked this distinction as one of the primary ways in which philosophers distort science and its history
   a. He argues that acceptance of a new result within the scientific community is often influenced by aspects of the process of discovery that are not amenable to rational reconstruction
   b. These arguments are a central part of his attack on the idea that (rational) evidential considerations drive science
4. Kepler's *Astronomia Nova* would appear to be fertile ground for Kuhn, if only because the book itself argues for its conclusions about Mars by “recounting” the process by which they were arrived at
   a. In truth, Voelkel has shown that Kepler himself engaged in some (presumably) rational reconstruction of the discovery process in *Astronomia Nova*

b. Voelkel himself calls the shifts from the actual discovery process rhetorical steps
c. But still the argument is presented in the spirit that anyone who will follow him along the presented steps of the discovery process ought to reach the same conclusions that he did
d. And, of course, with the advantage of hindsight we know that much of the physical reasoning Kepler invoked along the way will not stand up under scrutiny

5. So, in isolating Kepler's evidential arguments and subjecting them to a critique, we will be able to examine Kuhn's claim that the distinction between the context of discovery and the context of justification yields a distorted picture of science
   a. Did the evidential arguments provide those at the time with compelling reasons for accepting Kepler's conclusions about Mars?
   b. If not, then is this indicative that science is less rational than philosophers would have it be?
6. In addressing these questions, need to keep three distinctions in mind:
   a. Kepler's conclusions holding ***exactly***, versus holding "***essentially exactly***" (i.e. would hold exactly save for some external perturbing factors) versus ***merely approximately*** (i.e. not precluding alternatives to them equally, if not more, supported by the evidence)
   b. Between assessing the specific form of Kepler's conclusions, with their particular values for all orbital parameters, and assessing his generic conclusions -- e.g. Mars describes *an* ellipse
   c. Between Kepler's conclusions holding and their being (provisionally) ***taken*** to hold for purposes of ongoing research

B. The Evidential Argument for the Latitudes of Mars
1. Not going to go into detail here on argument for true sun and bisected eccentricity of earth-sun orbit
   a. True sun: *experimentum crucis* on three systems, plus triangulated distance confirmation and support from solution for latitudes
   b. Bisection: idealization of an approximate result, supported by large reduction in worst discrepancies with observation
2. The claim about the latitudes of Mars really consists of four separate claims
   a. The trajectory of Mars lies in a single plane
   b. That plane passes through the true sun
   c. That plane is inclined at a constant angle with respect to the plane defined by the earth and sun
   d. The angle of inclination (in final theory) is 1 deg 50 min 30 sec
3. The evidence for the claim about the specific value of inclination turns on sets of privileged observations, like the ones in Wilson's Figure 4
   a. But the reasoning from the observations to the value is thoroughly "theory-dependent": e.g. not only does the interpretation of the observations presuppose the other claims forming the account of latitudes, but also the position of the line of nodes and the claim that the angle MES is 90 deg -- a claim that depends on the account of longitudes

b. The other privileged observations also presuppose the other claims, but they do not rely on entirely the same aspects of the other parts of the account of Mars
c. Furthermore, the different observations, with their different theory-dependencies, yield almost the same values (within observational accuracy) thus indicating that measurements are not radically begging the question -- i.e. the measured inclination is robust

4. Furthermore, the overall account of the latitudes yields reasonably good agreement between predicted and observed latitudes
   a. Discrepancies extraordinarily small compared to all earlier accounts, though still a little larger than desired -- i.e. not strictly within observational accuracy
   b. Discrepancies are perhaps attributable to refraction and parallax effects
   c. And the discrepancies exhibit no systematic pattern, especially none of the sort requiring either a second inclination or varying inclination of the sort proposed by Ptolemy and Copernicus
   d. In other words, the discrepancies are more likely than not a consequence of imprecision in measurement
5. Ultimately, a simplicity argument can also be invoked here, though with caution
   a. Reasonable agreement with observation from a model that is physically simple -- i.e. that places almost no demands on an account of underlying physics
   b. Furthermore, orbital inclination at least tied to a physical object, the sun
   c. In the absence of a pattern in the discrepancies from observation, no grounds for further complications (as of 1609)
6. The evidential argument for the account of latitudes clearly does not close the door completely to alternative accounts, but it places a burden of proof on anyone who is going to offer such an account
   a. Must agree at least as well with observation
   b. To the extent that less simple physically, must include some grounds for thinking that physically plausible
   c. And should provide an explanation for the success -- i.e. the enormous level of improvement -- achieved by Kepler's account

C. The Evidential Argument for the Ellipse
1. Strictly speaking, the claim here concerns only the relationship between angular position, say vis-a-vis the Sun, and distance: form an ellipse
   a. No claim that sun at focus (a term Kepler coined), though eccentricity in terms of sun, perihelion, aphelion etc.
   b. No claim about time at which planet at each position, only about the locus of points
2. Triangulation off of observations gives compelling evidence that an oval of some sort, narrower than circle at quadrants and symmetric about line of apsides through true sun
   a. Theory dependent -- vicarious theory and (upgraded) solar theory

b. Sensitive to observational error and to errors in these theories

c. But sensitivity analysis showed that oval conclusion supported regardless of these errors -- i.e. the conclusion is robust

3. An argument that, given area rule, must be an ellipse, 429/100,000 narrower at quadrant

a. For must be midway between circle of Phase 4 and auxiliary ellipse of Phase 5 since discrepancies at octants in these are equal and opposite in sign, while discrepancies at apsides and quadrants negligible

b. And distance-angle relationship of ellipse physically plausible since distance variation of a sort that is physically plausible on two counts

(1) Sinusoidal variation of a natural sort

(2) (normal v) varies as 1/r -- something that a continuous physical mechanism might yield

c. And finally the "diametral" distance rule together with accurate heliocentric longitudes yields roughly this ellipse, and with area rule taken as exact, this ellipse precisely

4. Three key premises in this argument, each open to contention

a. Octant error exactly equal and opposite -- an idealization, consistent with observational error, but without separate justification except via the distance rule argument

b. Vicarious theory yields heliocentric longitudes (vs. time) within observational accuracy -- confirmed by 8 "observations" in opposition

c. Area rule -- without it, no argument to ellipse rather than oval, for "midway" argument turns on considering three orbits satisfying the area rule, and so does distance rule argument

5. A further argument: ellipse plus area rule together yield good predictions of observations from earth

a. Enormous improvement, but still with some predictions apparently lying outside observational accuracy

b. Enough discrepancy to allow for possibility of some other oval, though admittedly no pattern to discrepancies to provide clear evidence of it

6. Upshot: the argument that an oval compelling, but the argument that an ellipse, and hence the argument for the specific ellipse, rests on a collection of assumptions, including most notably the area rule

a. Nevertheless, the ellipse was accepted long before the area rule was

b. As above, this is in part an indication that nobody bothered with Kepler's reasoning in *Astronomia Nova*

D. The Evidential Argument for the Area Rule

1. Area rule makes a claim about motion that cannot be "tested" independently of a trajectory

a. I.e. to define area, need trajectory as well as distance

b. Hence, so long as the trajectory is entirely unspecified, no possible way to bring “direct” evidence to bear on area rule

2. To a large extent the evidential arguments for it in *Astronomia Nova,* where it is never stated as clearly as it subsequently came to be, are arguments countering objections to it
    a. A calculationally superior approximation to a physically plausible rule (inverse distance rule) that holds exactly at apsides for planets in Ptolemaic, Copernican, and Tychonic theory (once solar theory suitably refined and correct relation between area rule and 1/r rule made clear)
    b. Yields a physically reasonable invariance around orbit (component of v perpendicular to radius vector SM varies as 1/SM) -- which Kepler made somewhat clear only later
    c. Level of approximation to the other rule within observational accuracy for case of earth-sun
    d. (Though this other rule does not yield as unequivocal an answer about Mars trajectory in the way it does)
3. Together with ellipse, area rule yields reasonably good predictions of observations from earth
    a. But, as remarked above, not strictly within observational accuracy
    b. Still, accurate enough to put burden of proof on anyone who is going to offer an alternative to these two
4. There are two other arguments in support of the area rule, both beginning, "If you accept it, then ...."
    a. "... with a slight idealization of some numbers it will determine an unequivocal answer to the question of the trajectory"
    b. "... once distance and velocity properly understood, can be reconciled with inverse distance rule and allows for a physically plausible answer to why an ellipse rather than an eccentric circle" -- viz. superposition of two independent effects
5. Upshot: the least evidence for the area rule, for it was introduced as a mere convenient approximation to begin with, and most of the evidence accruing to it derived from its yielding the ellipse
    a. Danger of a question-begging line of argument here: ellipse presupposes area rule, and area rule justified because it yields ellipse
    b. Still, a clear burden of proof: any alternative to area rule must, together with trajectory, yield at least as good overall agreement with observations and not be far more physically implausible
    c. And ellipse and diametral distance rule, taken as exact, entail area rule

E. Some Standard Objections to the Arguments
1. Issue: what do the evidential arguments really show -- i.e. what conclusion is warranted
    a. Newton later remarks that Kepler only guessed that the orbit was an ellipse, while he proved it
    b. Newton almost certainly was not familiar with the argument as laid out in *Astronomia Nova*
    c. Unclear whether much of anyone other than Kepler understood the argument in *Astronomia Nova*, for that argument supplanted by other arguments within a few years, so that few ever worked their way through the original argument
2. One obvious response to the evidential argument is that Newton is correct, for Kepler pulls the area rule out of the air with no supporting physical evidence

a. Idealization of "data" in the crucial argument is bad enough
b. But, granting it, all that the evidential argument really shows is that, if the area rule is correct, then the orbit is the ellipse Kepler says it is
c. And once idealization of the "data" is taken into consideration, all that the argument really shows is that, if the area rule is correct, then the orbit differs from an ellipse imperceptibly

3. The obvious reply to this argument: but the area rule works!
   a. Kepler's orbit for Mars yields predictions enormously better than any earlier accounts -- enormously more accurate (virtually within observational limits) and enormously simpler
   b. The area rule leads to a single definite answer, namely the ellipse, arrived at in two ways, with the second yielding an argument for physical plausibility
   c. Thus the area rule does just what we want it to: puts us in a position where data more or less forces a single answer on us, and that answer stands up when checked in other ways!
4. But this reply will not be enough, for someone arguing as above will respond:
   a. The second point, about yielding a definite answer, is question-begging, for the issue is whether an ellipse, and once we allow that it is not an ellipse, then the area rule no longer "works" -- indeed, the area rule is then in trouble
   b. The first point at most shows that the true orbit approximates Kepler's, for errors above observational limits are still present
   c. Thus the evidential argument at most shows that orbit an oval approximating an ellipse that conforms to high approximation to the area rule, but other possibilities remain open
5. Kuhn would jump into such an exchange at this juncture to point out that Kepler's efforts on Mars are a wonderful example of the way in which science overreaches the available evidence
   a. Kepler is being persuaded by the combination of a purely conjectural physics and the extra-ordinary way in which his numbers happen to come together
   b. Any others are being persuaded just because of the order of magnitude improvement (after 14 centuries); they are not pausing to realize how many other possibilities remain open
6. The issue is whether it was appropriate, as of 1609, for Kepler or anyone else to accept his conclusions about Mars on the basis of the evidential arguments presented in *Astronomia Nova*
   a. One option is to accept the conclusions and proceed with them
   b. The other is to consider the conclusions one more competing alternative on planetary trajectories, and leave the issue of the "true" trajectory open
   c. In other words, what if anything did Kepler's arguments settle?

F. A Contrasting View of Scientific Evidence

1. Some sciences have much higher quality evidence than others do
   a. I.e. they are much more able to turn data into evidence
   b. Physics able to get much more compelling evidence from data than, say, political science is

2. *Prima facie*, the sciences that are more successful have a good deal more established theory with which to interpret and marshal data -- i.e. the more advanced the theory available in the background, the higher quality evidence that can be derived from data
    a. I am here saying only that this in fact appears to be the case
    b. Leaving open questions about whether it has to be the case, or even whether it truly is the case
3. The obvious problem then is to get the whole process off the ground
    a. If need theory to get quality evidence -- i.e. to turn data into evidence -- then how does one get reasonable quality empirical evidence for theories in the first place?
    b. The problem in the early stages of sciences -- in neuropsychology today, in physics in 1600
    c. A chicken-and-egg problem to which there are some standard answers
        (1) Get lucky: just happen onto a basically correct theory
        (2) Kuhn: overreach, but then learn to live with the residue of a totally arbitrary element in the theory, leading to subsequent scientific revolutions
4. On my view, the proper approach to this problem is to adopt working hypotheses instead of theories
    a. Working hypotheses: hypotheses that enter constitutively into evidential reasoning, yet at the time cannot be tested or verified in any non-question-begging way
    b. A good working hypothesis: one that (i) can be fruitful and (ii) can be accepted safely
        (1) I.e. one that yields higher quality evidence than can be achieved without it, leading to developments that will ultimately allow it -- the hypothesis itself -- to be empirically evaluated and, if need be, refined or discarded
        (2) One for which there are safeguards against its leading down a long, illusory garden path, so that when hypothesis finally brought into empirical scrutiny, will not lose everything predicated on it if forced to abandon or modify it
        (3) Note: issue of truth of the hypothesis not as such crucial
    c. The reasons for accepting such a working hypothesis will differ from the reasons for accepting conclusions evidentially predicated on it
5. Empirical evidence can be brought to bear on whether a proposed working hypothesis ought to be accepted
    a. Does it yield higher quality evidence than is attainable without it?
    b. If two competing working hypotheses both yielding higher quality evidence, then need empirical arguments supporting one over the other, or should pursue separate lines of research predicated on each, and let things come out in the wash -- just as Kepler did with area rule and 1/r rule
    c. If hypothesis does not -- or ceases to -- yield higher quality evidence, then it becomes suspect, for then reasons to worry about whether all is going to come out in the wash
    d. If someone has identified a possible way for the hypothesis to lead down a garden path -- e.g. some other hypothesis is true and it entails that the working hypothesis is systematically mis-

leading, giving only the illusion of quality evidence -- then must eliminate this risk, usually by showing that the alternative not true

G. A Defense of the Evidential Arguments for Mars

1. Claim: Kepler's evidential argument a paradigm of science at its best in the early stages of theory development
   a. A paradigm of how to evaluate and use working hypotheses
   b. A type of evidential argument that we will find again, much refined, in Newton's *Principia*
2. The area rule, in particular, is a classic example of a working hypothesis
   a. Evidence that it is true was beside the point -- indeed, no independent evidence for its truth possible at the time Kepler began using it
   b. Instead, he adduced evidence that it is plausible and that it is unlikely to lead down a garden path (given its relation to a physically plausible alternative to it, as well as to the equant)
3. Empirical evidence then emerged that it is a good working hypothesis, for on two counts it led to higher quality evidence
   a. With it, data can be marshaled to determine an unequivocal answer about the trajectory
      (1) In the process getting around difficulties with inaccuracies in the observations
      (2) And hence getting much more evidence out of the data than can get without it
   b. Leads to an order of magnitude improvement in predictive accuracy for Mars and earth orbits, thereby putting everyone in a position to use residual small discrepancies for further refinements
   c. Provides at least an initial basis for examining underlying physics, for constrains physics, but is not physically paradoxical or perplexing in a way that would shut the door to physical theorizing on the basis of it
4. I therefore claim that provisional acceptance of Kepler's Mars orbit was in fact warranted on the basis of his 1609 evidential argument
   a. Provisional because working hypothesis, now extended to include ellipse, must continue to yield higher quality evidence
   b. Provisional because of the possibility that some competing working hypothesis will prove equally effective
   c. Provisional because have much less evidence for its truth than expect will accrue to it in the future, if it is really true
5. In saying that Kepler and others ought to have accepted his conclusions about Mars on the basis of his evidential arguments, instead of remaining neutral and open-minded, I am saying that there were real advantages to doing so
   a. Could simply adopt area rule and ellipse for each of the other planets, bypassing the need to extract the orbit from the data; evidence would begin accruing to the first two "laws" if the results of doing so were satisfactory

b. Could begin examining any small discrepancies outside observational accuracy for implications about (i) the need to refine orbital elements and (ii) the presence of second-order effects
   (1) Looking for systematic discrepancies that would be informative
   (2) If do not accept Kepler's conclusions, then still at square one, asking what the trajectories are, and hence not looking at higher order, small discrepancies
c. For example, apply area rule and ellipse to earth-sun orbit to see whether reduce or increase agreement with observation for either it or Mars, especially before turning to other planets
d. Provides promising evidential basis for conjecturing about underlying physics -- i.e. for developing a much richer, more complete theory -- not just by imposing some distinctive constraints, e.g. via ellipse rather than other oval and via area rule, but also because trajectory composable out of no more than two superposed physical mechanisms, with no need for additional special mechanisms

6. In fact, in 1609 and the decade following no one seems to have been especially convinced by all this but Kepler himself, for no one but Kepler adopted both the ellipse and the area rule, even as a working hypothesis
   a. Surely no one in those years worked their way through *Astronomia Nova*, and, even if they had, Kepler's argument requires huge effort to extract it in above form and to appreciate how many cross-checks he provided along the way, plus of course access to Tycho's observations
   b. Magini's 1614 tables came the nearest to following Kepler, for they incorporated all his innovations except the area rule; but that shortcoming is evident in their discrepancies versus those Kepler subsequently achieved in his *Rudolphine Tables*
   c. Not to mention the enormous departure in the mathematics for determining Keplerian orbits and planet positions, at least compared with the old compound circles of Ptolemy and Copernicus
   d. Historically then, in claiming that the argument in *Astronomia Nova* provides adequate grounds for (provisionally) accepting Kepler's first two rules, I am making a claim about the rationality of only Kepler himself
7. The moral I suggest you take away from Kepler's *Astronomia Nova* is that it falls very far short of establishing once and for all the truth about the orbit of Mars, yet it is nevertheless a model of evidential reasoning in science at its very best in the early stages of theory construction
   a. This sounds paradoxical only if one is inclined to think that evidential reasoning in science at its best must establish truth once and for all
   b. Asking evidential reasoning to do this in the early stages of theory construction, I submit, is asking for the impossible
   c. And once Kepler became prepared to drop the Ptolemaic working hypothesis of at least equi-angular, if not uniform, circular motion, he was in the early stages of theory construction even though he was part of a tradition that was a millennium and a half old

Select Sources

Kepler, Johannes, *Astronomia Nova*, Vol. 3, *Johannes Kepler Gesammelte Werke*, Max Caspar (ed.), C.H. Beck'sche Verlagsbuchhandlung, 1937.

--------, *New Astronomy*, tr. William H. Donahue, Cambridge University Press, 1992.

--------, *Apologia pro Tychone contras Ursum*, in N. Jardin, *The Birth of History and Philosophy of Science, Kepler's* ***A Defense of Tycho against Ursus*** *with essays on its provenance and significance*, Cambridge University Press, 1984.

--------, *Optics Paralipomena to Witelo & Optical Part of Astronomy*, tr. William H. Donahue, Green Lion Press, 2000.

Caspar, Max, *Kepler*, tr. and ed. C. Doris Hellman, Dover, 1993 (reprint of 1959 edition).

Jardine, N., *The Birth of History and Philosophy of Science: Kepler's* ***A defense of Tycho against Ursus****, with essays on its provenance and significance*, Cambridge University Press, 1984.

Wilson, Curtis, "How Did Kepler Discover His First Two Laws?," in *Astronomy from Kepler to Newton: Historical Studies*, Variorum Reprints, 1989; originally in *Scientific American*, vol. 226, 1972, pp. 93-106.

--------, "Kepler's Derivation of the Elliptical Path," in *ibid*.; originally in *Isis*, vol. 59, 1968, pp. 5-25.

Stephenson, Bruce, *Kepler's Physical Astronomy*, Springer-Verlag, 1987. (The best source for laying out the many aspects of Kepler's evidential reasoning throughout the course of *Astronomia Nova*)

Voelkel, James R., *The Composition of Kepler's* ***Astronomia nova***, Princeton University Press, 2001.

--------, *Johannes Kepler and the New Astronomy*, Oxford University Press, 1999.

Voelkel, James R. and Gingerich, Owen, "Giovanni Antonio Magini's "Keplerian" Tables of 1614 and Their Implications for the Reception of Keplerian Astronomy in the Seventeenth Century," *Journal for the History of Astronomy*, vol. 32, 2001, pp. 237-262.

Gingerich, Owen, "Johannes Kepler," in René Taton and Curtis Wilson (ed.), *Planetary Astronomy from the Renaissance to the Rise of Astrophysics, Part A: Tycho Brahe to Newton*, Cambridge University Press, 1992.

Smart, Robert, *An Account of the Astronomical Discoveries of Kepler*, London, 1804; reprint, University of Wisconsin Pres, 1963.

Beer, Arthur and Beer, Peter, *Kepler: Four Hundred Years*, vol. 18 in *Vistas in Astronomy*, Pergamon, 1975.

Gilbert, William, *De Magnete*, tr. P. Fleury Mottelay, Dover, 1958.

Whiteside, D. T., "Keplerian Planetary Eggs, Laid and Unlaid, 1600-1605," *Journal for the History of Astronomy*, vol. 5, 1974, pp. 1-21.

Neugebauer, Otto, *Astronomy and History: Select Essays*, Springer, 1983, especially the essay on Kepler.

Donahue, William, "Kepler's Fabricated Figures: Covering Up the Mess in the *New Astronomy*," *Journal for the History of Astronomy*, vol. 19, 1988, pp. 217-237.

--------, "Kepler's First Thoughts on Oval Orbits: Text, Translation, and Commentary," *Journal for the History of Astronomy*, vol. 24, 1993, pp. 71-100.

Martens, Rhonda, *Kepler's Philosophy and the New Astronomy*, Princeton University Press, 2000.

Pafko, Wayne, "Visualizing Tycho Brahe's Mars Data," http://www.pafko.com/tycho/observe.html, 2000.

Wesley, Walter G., "The Accuracy of Tycho Brahe's Instruments, *Journal for the History of Astronomy*, vol. 9, 1978, pp. 42-53.

Chapman, Allan, *Astronomical Instruments and Their Users: Tycho Brahe to William Lassell*, Variorum, 1996, especially Chapters 1-3.

Grafton, Anthony, *Michael Maestlin's Account of Copernican Planetary Theory*, *Proceedings of the American Philosophical Society*, vol. 117, no. 6, *Symposium on Copernicus*, 1973, pp. 523-550. (This includes a translation with some commentary of Maestlin's exceptionally clear account of Copernican orbits, "On the Dimensions of the Heavenly Circles and Spheres, according to the *Prutenic Tables*, after the Theory of Nicolaus Copernicus," which appeared as an Appendix in Kepler's *Mysterium Cosmographicum* of 1596.)

Moore, Patrick, *The Great Astronomical Revolution: 1534-1687 and the Space Age Epilogue*, Albion Publishing, 1994 (reprint of *Watchers of the Stars*, 1973).

Koestler, Arthur, *The Sleepwalkers: A History of Man's Changing Vision of the Universe*, Penguin Books, 1964.

Credits for Appendix

Slide 1: Moore (1994)

Slides 3, 22, 36: Voelkel and Gingerich (2001)

Slides 6, 7, 9-15, 18-21, 23, 27-32, 34, 35: Kepler (1992)

Slides 8, 17, 18, 23, 32, 33: Kepler (1937)

Slide 16: Stephenson (1987)

Slides 24, 26: Wilson (1972)

Slide 25: Gingerich (1992)