THE NEWTONIAN REVOLUTION – Part One

Philosophy 167: Science Before Newton's *Principia*

Class 4

Kepler's Planetary System and the *Rudolphine Tables*

September 23, 2014

TABLE OF CONTENTS

I. Kepler's Astronomical Work Following *Astronomia Nova* 1

A. The Orbital Innovations of *Astronomia Nova* 1

B. The Achievement of *Astronomia Nova*: Summary Remarks 2

C. Kepler: The Subsequent Years (1609-1630) 3

D. *Mysterium Cosmographicum* and *Harmonice Mundi* 5

E. Kepler's Discovery of His Third "Law" 6

F. *Epitome Astronomiae Copernicanae* (1618-1621) 7

G. *Tabulae Rudolphinae*: The Culmination (1627) 8

II. Some Philosophic Issues Concerning Kepler's "Laws" 10

A. Kepler's Substantive Legacy: The Generalizations 10

B. Some Advances in Turning Data Into Evidence 11

C. Accidental Versus Nomological Generalizations 13

D. A Complication: Exact Versus Approximate "Laws" 14

E. Further Complications: Range and Qualifications 15

F. Questions to Ask About Kepler's Generalizations 16

III. An Examination of the Evidence for Kepler's "Laws" 17

A. The Precise Statement of Kepler's Generalizations 17

B. The Evidence in 1630 for the Ellipse and Area "Laws" 18

C. The Evidence in 1630 for the 3/2 Power "Law" 19

D. Evidence that the "Laws" are Laws: Kepler's Approach 20

E. Kepler's Approach to the Underlying Physics 21

F. Illustrate Via the "Physics" for Keplerian Motion 22

G. The Empirical Limitations of Kepler's Physics 23

IV. Kepler's Methodological Legacy: Some Final Remarks 24

A. Kepler's Conception of "Scientific" Astronomy 24

B. Some Residual Problems Facing Kepler in 1630 25

C. Issues Raised on His Conception by These Problem 26

Select Sources 28

Credits for Appendix 29

Philosophy 167: Science Before Newton's Principia

Assignment for September 28

Kepler's Planetary System and the Rudolphine Tables

Reading:

Kepler, Johannes. Astronomia Nova. "Introduction to the Work," Translated by William Donahue, pp. 45-69.

----, The Epitome of Copernican Astronomy. Part IV, pp. 5-11, 22-32, 47-48, 52-61, 65-67, 88-89, 93-102. Part V, pp. 124-146.

Questions to Focus On:

1. How did Kepler's subsequent findings on the orbits of Mercury, Venus, Jupiter, and Saturn add to the evidence for his first two "laws" presented in Astronomia Nova?

2. How did the evidence for Kepler's third "law" compare with the evidence for his first two? In particular, was there more or less reason in 1630 to think that the third "law" holds exactly, and not just approximately?

3. The Epitome of Copernican Astronomy presents, for the first time in print, the modern planetary system -- heliocentric, with the planets and their satellites in "Keplerian motion" about their principals. How much was Kepler stretching matters in calling this system "Copernican"? In particular, is this system "simpler" than the Ptolemaic in the way that Copernicus yearned for his system to be?

4. Kepler offers "physical" explanations for various aspects of Keplerian motion, explanations that have long since been discarded. How, if at all, did these false physical explanations affect the evidence for his claims that celestial bodies exhibit Keplerian motion?

5. In his Introduction to Astronomia Nova Kepler argues that, through a combination of mathematical astronomy and physics, evidence can be adduced to choose between the Tychonic and the Copernican systems. To what extent does the evidence presented in the Epitome succeed toward this end?

Class 4: Kepler's Planetary System and the *Rudolphine Tables*

I. Kepler's Astronomical Work Following *Astronomia Nova*

A. The Orbital Innovations of *Astronomia Nova*

1. With *Astronomia Nova* Kepler introduced in sequence and developed evidence for five innovations in planetary theory that are still central to orbital astronomy (see list in Appendix)
   a. The true sun instead of the mean sun
   b. Bisection of eccentricity for the Earth-Sun orbit
   c. The area rule for determining location in time
   d. The elliptical trajectory
   e. The orbit on a plane through true Sun at constant inclination to the plane of the ecliptic
2. Of these five, only the area rule is not clearly formulated in *Astronomia Nova*, and Kepler never quite formulates it in the form, component of v perpendicular to r varies inversely with r
   a. Kepler was using an algorithm equivalent to it, but he did not come to state it as "equal areas in equal times" in non-circular orbits until after the book was published
   b. (Specifically, using equal areas in equal times as a measure of the sum of the arc distances in circle initially, then in circumscribed circle, not remarking that that amounts to the same as the fraction of the total area swept out in the inscribed ellipse)
   c. The first clear published statement of it is in the *Epitome*, where he apologizes for the earlier confusing formulation (V., 4., p. 143)
3. As Kepler fully realized, his orbital innovations can be incorporated directly into the Tychonic system, and even into the Ptolemaic system, to yield comparable accuracy on latitudes and longitudes
   a. More than a little perverse to incorporate them into the Ptolemaic insofar as triangulations that make no sense as evidential reasoning in that system were used to provide key evidence
   b. Nevertheless, because they could be incorporated into all three systems, such refinements in the orbits could not by themselves "prove" the Copernican system
4. This raises the question: what was so unique to Kepler's position that these innovations emerged for the first time here? I think the answer is a combination of five factors
   a. Tycho's data -- their scope, precision, and recognized bounds of observational precision
   b. Kepler's realization of the limitations of relying on acronychal data (yielding both an *experimentum crucis* for the mean vs. true sun and for developing evidence on the trajectory)
   c. A redefinition of the problem of planetary astronomy: find the true trajectory instead of using compounded circular motion to save the salient phenomena
   d. The idea of using theory-mediated triangulations to infer sun-planet or earth-sun distances (legitimated for the first time by Copernicus and carried over by Tycho)
   e. Tycho's theory of the sun, which provided (even before its revision to incorporate bisection of eccentricity) accurate heliocentric longitudes of earth and good earth-sun distances

5. Newton's remark that Kepler merely guessed the ellipse probably reflects his view that Kepler had simply hypothesized his orbital model and then found that it could be made to fit impressively with Tycho's data
   a. Newton was openly contemptuous of such "hypothetico-deductive" reasoning in science, complaining that too many different hypotheses could fit the same data and no empirically based choice could be made among them
   b. The general impression then and now was that this is what Kepler must have done (just as it is that this is what Ptolemy must have done)
   c. This impression stems from not having worked through *Astronomia Nova*
6. Finally, we should note that it is one thing to have evidence for Kepler's conclusions about the trajectory of Mars between 1580 and 1605 and quite another to have evidence for generalizations of these conclusions! -- the central theme of this class
   a. What does the evidence in *Astronomia Nova* say about Mars' orbit in the far past and future?
   b. And what does it say about the orbits of the other planets?
   c. Generalizations beyond Mars in the period covered by Tycho's data involve a huge further leap -- or, to use Nelson Goodman's technical term, a *projection* beyond Tycho's data
      (1) Kepler's claims about the orbit for those years involves a projection from Tycho's data to conclusions that reach well beyond these data
      (2) Still an enormous further projection to this orbit over other times and to the orbits of other planets

B. The Achievement of *Astronomia Nova*: Summary Remarks

1. *Astronomia Nova* did in a sense effect a total reconstruction of mathematical astronomy from the ground up, much as Tycho had hoped for and Kepler had intended
   a. Between one and two orders of magnitude improvement in the accuracy of predicting latitudes and longitudes of the planets over everything that had gone before
   b. Established a new standard for predictive astronomy, replacing a 1400 year old standard -- a new standard that was not itself replaced for the better part of 200 years (telescope notwithstanding)
2. Methodologically, the book also represented a breakthrough of sorts in the problem of turning data into evidence
   a. Showed how to exploit comparatively accurate observational data, with a reasonably well known level of precision, while at the same time making allowances for residual inaccuracies
   b. Turned an age-old question -- what trajectories do the planets actually follow? -- into a question which observations can answer, given some theoretical assumptions, like Tycho's solar theory
   c. That is, Kepler was able to put himself into a position in which a comparatively small range of inexact observations yielded a perhaps qualified, but still unequivocal answer to the question of trajectory (at least up to an appropriate level of approximation) for Mars

d. And in the process put the field in a position where further observations would continue to yield relatively unequivocal answers to other related questions

3. But Kepler was by no means the first to succeed in thus turning questions into empirical questions in the sense just given, for this is precisely what Ptolemy had done too

   a. For example, Ptolemy used observations to generate the bi-section of eccentricity of Mars and Venus, as well as answers to a wide range of specific questions about orbital elements, etc.

   b. Why *Almagest* was so extraordinarily compelling

   c. I say this fully granting that Ptolemy may have played foot-loose-and-fancy-free with observational data, and recognizing that he worked with lower quality data, with less basis for setting bounds on precision; and his circular motion working hypothesis was more confining

4. The point is that Kepler represents a huge step forward because he wanted the "data-determined-answers" to such questions to do more than just be reasonably stable and not totally question-begging

   a. He wanted either to eliminate all further systematic residual discrepancies between observation and theory or to be able to use them as data that could be turned into new, still further evidence for added refinements -- e.g. to refraction corrections

   b. And he wanted to be able to use the "data-determined-answers" as at least an initial evidential basis for answering questions about the physical mechanisms underlying planetary motion

5. Still, do not lose sight of the fact that Kepler started from theories taken from Ptolemy, Copernicus, and Tycho: he can be looked on as the culmination of 1400 or more years of mathematical astronomy

   a. Like them, he fully appreciated that some sort of theoretical assumptions were indispensable to drawing any conclusions from planetary observations

   b. Indeed, he systematically used discrepancies between their theories and Tycho's observations as the evidential basis for his further conclusions

   c. I.e. Kepler's total reformation can equally be viewed as proceeding by successive approximations from already existing theories of a highly advanced science

   d. *Astronomia Nova* written in a way to carry those working in the old astronomy step by step into the new: the new is presented as built on the old, a refinement

6. Finally, keep in mind the extent to which Kepler consistently tried to cross-check each "data-determined answer" -- he fully recognized that observational data can be misleading, whether taken in their own right or in the context of a presupposed initial theory

   a. Cross-check via alternative ways of yielding at least a rough answer to the same question

   b. And cross-checking via considering whether the answer is physically at least reasonable

C. Kepler: The Subsequent Years (1609-1630)

1. In truth, Kepler was quite possibly the only person ever to have been influenced by the evidential argument in *Astronomia Nova*, for he was quite possibly the only person in the era to understand it, and anyway so few copies of the book circulated

a. Only a relatively small number of people had the background needed to read a book with this much original mathematics etc. in it, and few, if any, of them would have had the patience to work through all the details in order to come to grips with the argument
b. Kepler continued to have privileged access to Tycho's data, so few would have been able to assess his reasoning in the light of all the available data
c. And within a few years Kepler and then others provided textbooks on the results, obviating the need to read through the original evidential arguments

2. The evidential argument in *Astronomia Nova* led Kepler to pursue three parallel lines of research through the remainder of his life (1630)
   a. Work out the orbits of all the other planets, and also that of the Moon, yielding a set of tables conforming to the new standard (based on Tycho's data): Venus, 1614; Mercury, 1609, 1614-15, 1616; Jupiter, 1616; Saturn, 1616; Moon 1617-1618, 1619
   b. Use the now better known features of planetary orbits to search for further phenomena in planetary motion, especially phenomena involving comparisons among planets that would shed insight on the system as a whole
   c. Use the now better known features of planetary orbits to theorize about the physical mechanisms underlying planetary motion
3. The pursuit of further phenomena involving comparisons of orbits led to his first major work in astronomy after *Astronomia Nova*, his *Harmonice Mundi* (1618)
   a. Probably his favorite work, though also the one most often used to ridicule him today
   b. This in turn led him to reissue his *Mysterium Cosmographicum* in 1621, with annotations updating his earlier findings
4. At one point he apparently intended to write a comprehensive treatise on the new astronomy, but perhaps because of difficulties with lunar theory, he ended up instead putting out a textbook, *Epitome Astronomiae Copernicanae*, in 3 installments between 1618 and 1621
   a. Became a primary source for his mathematical astronomy, but also for his theories about the underlying physics
   b. Widely read and influential, especially after he died; but did not present the elaborate, intricate evidential reasoning from observations of the sort laid out in *Astronomia Nova*
5. Finally, after many years of effort, including struggles with lunar theory, in 1627 published *Tabulae Rudolphinae*
   a. Tables, plus explanations on their use, for the Sun, the five planets, and the Moon, plus a catalog of stars and a table of logarithms
   b. As the title page indicates, the culmination of the project Tycho started almost 50 years before
   c. A book virtually everyone working in astronomy over the next 100 years referred to in one version or another -- the basic reference work in the field

6. In addition to these major works in the history of astronomy, published a number of other works in science
   a. *De stella nova* (1606), on the nova of 1604
   b. *Dissertatio cum Nuncio siderio* (1610) and *Narratio de Jovis satellitibus* (1611), supporting Galileo's telescopic findings
   c. *Dioptrice* (1611), the first comprehensive treatise on optics, including principles for Keplerian telescope
   d. *Stereometria dolorioum vinariorum* (1615), a precursor to the calculus, describing the small-interval approximation methods used in *Astronomia Nova*
   e. *De cometis libelli tres* (1619), on the comet of 1618, leading to conflict with Galileo
   f. *Somnium seu astronomia lunari* (1634), a fantasy account of a trip to the moon, and how celestial motions would appear from there
   g. Ephemerides on a regular basis, starting in 1610s, that must have impressed astronomers with their accuracy; note especially the ephemerides for 1631

D. *Mysterium Cosmigraphicum* and *Harmonice Mundi*

1. To understand the kind of thinking Kepler engaged in and is taken to task for in *Harmonice Mundi*, need to go back to his first work in astronomy, *Mysterium Cosmographicum* (1596)
   a. Question addressed, presupposing the Copernican system: why should there be exactly six planets?
   b. His answer: They correspond to the five regular solids, nested so that the spheres inscribed in and circumscribing each solid yield the comparative planetary distances from the Sun
   c. He identified two problems with this answer: (1) the dimensions did not exactly conform with Copernicus's orbital radii, raising questions about the accuracy of the latter; (2) why then eccentric circles, with all the added complications of different centers
2. All his life Kepler took any open question about the planetary system as an invitation for theorizing, looking always for a signal insight that would have major ramifications
   a. Looking for a way to gain a key insight into the mind of God
   b. In this respect, rather like Einstein -- as also in his appreciation for the value of theory
   c. But always with an insistence on independent empirical assessment
3. The question about the eccentricities of the nested orbits led him to look for an explanation in terms of some feature of the velocity variations in them, especially the min-to-max velocity ratios
   a. These concerns, along with worries about correct dimensions, led him initially to wanting to have access to Tycho's data
   b. With the findings on Mars, the velocity ratios become systematically tied to the eccentricities
4. *Harmonice Mundi* offers his answer: God deviated from the simple regular solid scheme so that the extremal velocities of the various planets would instantiate the fundamental principles of harmonics

    a. Five books: (1) Geometrical, on constructible figures; (2) Arithmetical, on solid ratios; (3) Musical, on the causes of the harmonies; (4) Astrological, on the causes of Aspects; and (5) Astronomical, on the causes of the periodic motions
    b. Systematically relates planetary periods, dimensions, and eccentricities to one another via the basic rules of harmony
5. Whether you consider this the worst sort of pseudo-science mysticism, or instead an adventurous essay in theorizing that ended up being a dead end, I leave to you
    a. Regardless of Kepler's own view toward it, all his detailed work on planetary orbits and their physical causes is almost entirely independent of it
    b. This in part because of his view that, once God had settled on the scheme, it was implemented and continued via simple physics
    c. Anyway, our concern is not with Kepler himself, but with the public evidence his work produced

E. Kepler's Discovery of His Third "Law" (1618)

1. One important by-product of *Harmonice Mundi* is Kepler's discovery of his third "law"
    a. Had looked for a systematic relation between velocity and the sizes of the orbits since 1596, in large part in keeping with the view that the Sun provided the impetus for planetary motion
    b. Original idea that the mean velocities varied inversely in proportion to distance from the Sun did not conform exactly with the known values
    c. Came upon a relationship that does conform during the last stages of completion of *Harmonice Mundi* (on March 8, 1618, he tells us)
2. "But it is absolutely certain and exact that *the ratio which exists between the periodic times of any two planets is precisely the ratio of the 3/2th power of the mean distances, i.e. of the spheres themselves*" (Book 5, Part 3, p. 180)
    a. In modern form, the ratios of the periods squared = the ratio of the major semi-axes cubed, where the major semi-axis does in fact give the spatial mean distance from the principal at a focus (though not the temporal mean) -- $a^3 \propto P^2$, henceforth the "3/2 power rule"
    b. Found that it applies not only to the planets, but also within a rough approximation (30 percent) to the newly discovered four Galilean satellites of Jupiter, using Marius's elements (Book 4, Part 2, p. 78f)
3. In stating the "law" Kepler did not bother to present the evidence for it, though elsewhere in the *Harmonice Mundi* he does give numbers that allow the reader to verify it (see tables in Appendix)
    a. Not perfectly exact, though discrepancies impressively small with these numbers
    b. Perhaps the most disturbing feature is the relatively large percent discrepancy for Venus
    c. (Data inaccurate for Mercury, because of reliance on correction for refraction, and Tychonic data limited for Saturn because of 29+ year period)

4. Notice that this third "law" differs from the first two: it has much more the character of an inductive generalization from "data"
    a. Of course, the periods, but far more so the semi-major axes, are scarcely data, for they are both being inferred from observations, especially so in the case of the semi-major axes
    b. But the evidence here akin to classical induction from cases, as well as to "curve fitting" -- i.e. formula fitting
5. Kepler's subsequent physical explanation of this law, in the *Epitome*, turned on the claim that the period is proportional to (path length ∗ quantity of matter) / (magnetic strength ∗ volume)
    a. For the amount of matter in the planet provides resistance to continued motion, and the larger the volume (*moles*), the more magnetic effect can be "soaked up"
    b. On Kepler's view the magnetic strength diminishes as $1/r$ (in contrast to the intensity of light, which he had correctly concluded diminishes as $1/r^2$)
    c. Thus, the "law" entailed a potentially testable consequence, viz. the ratios of densities of planets vary as $1/\sqrt{r}$; problem, of course, was to determine densities independently of it

F. *Epitome Astronomiae Copernicanae* (1618-1621)

1. The *Epitome* was published in three separate installments, covering three different subjects:
    a. Books I-III (1618) dealing with (largely conventional) spherical astronomy
    b. Book IV (1620) dealing with theoretical astronomy, including discussions of underlying physics -- "Celestial Physics, i.e. Every Size, Motion, and Proportion in the Heavens Explained by a Cause Either Natural or Archetypal" -- preceding Books V-VII
    c. Books V-VII (1621) dealing with practical geometric problems that arise in the new astronomy; V on orbital geometry, VI on the individual planets, VII a rap-up with comments on Ptolemy etc.
2. With its three opening books on spherical astronomy, it was clearly intended to be a comprehensive text in astronomy for universities
    a. A textbook in Copernican astronomy, more accessible than Copernicus's *De Revolutionibus*, yet presenting not Copernicus's system, but the "Copernican-Keplerian system" -- "the Copernican system as expostulated by Kepler," to quote Newton's statement of the matter
    b. But with a large amount of conjectural physics, from which the motions are derived, and complicated efforts on a number of recalcitrant problems, most notably that of the Moon
    c. Confidently Copernican, and not Tychonic, because so much of this physics turns on the Sun; indeed, offers 18 reasons to reject Tychonic (pp. 71-78), none knockdown
3. Successful as a textbook -- e.g. reissued in 1635 after the initial successes of the *Rudolphine Tables* began securing converts
    a. "For many years it remained one of the few accessible sources for the details of the Copernican system (including, of course, those essential revisions introduced by Kepler)" -- Gingerich, p.75

b. As such, completely replaced *Astronomia Nova* as the fundamental work of Kepler, as well as replacing *De Revolutionibus* (and as Kepler announces, Aristotle's *De Caelo*)

c. Because the reasoning in it proceeds from physics to orbits, the more predominately astronomical reasoning of *Astronomia Nova* -- i.e. reasoning from observations to orbital motions -- becomes lost from view, as does some of the continuity with earlier astronomy

4. Includes a treatment of the moon covering Ptolemy's inequality; the inequality called the "variation," discovered by Tycho; and a new inequality, the annual equation, discovered independently by Tycho and Kepler

   a. All depend on the position of the Sun vis-a-vis the Earth and Moon

   b. "Kepler realized that any physical theory must involve a double interplay of the Earth and Sun." -- i.e. Moon, driven by emanations from both Earth and Sun -- (Gingerich, p. 75)

   c. This was Kepler's second published cinematic model of the moon; an earlier one in the ephimerides of 1617 stayed faithful to the area rule

   d. Whatever else may be said for them, both models fall far short of the level of accuracy in predicting longitude and latitude of the Moon achieved by the model for Mars

5. In a way, a revolution in astronomy textbook writing, for includes physics and mathematical astronomy together, unlike e.g. Ptolemy and Regiomantanus's *Epitome of the Almagest*

   a. As Kepler himself says, "You might doubt whether you were doing a part of physics or astronomy, unless you recognized that speculative astronomy is one whole part of physics." (p. 5)

   b. Even though most key readers ended up discarding the physics early on, they did not discard the need for a physics to the same extent

G. *Tabulae Rudolphinae*: The Culmination (1627)

1. The closest Keplerian counterpart to Ptolemy's *Almagest* insofar as it includes no physics, but only mathematical astronomy; really though, Kepler's counterpart to Ptolemy's *Handy Tables*

   a. 275 pages explaining how to use the Tables (including how to use logarithms) and in some places giving some background on the orbital elements

   b. Followed by 104 pages of tables and then a star catalogue

2. The frontispiece summarizes Kepler's own view of where he fit into the process of reforming mathematical astronomy

   a. Main pillars for Copernicus and Tycho, but pillars to Ptolemy, Hipparchus, etc.

   b. Emphasis on Tycho, Hven, etc. because of the critical role of Tycho's observational efforts, which Kepler never belittled, and the project of the Tables having originated with Tycho

   c. Kepler in basement toiling on calculations by candlelight, with a few coins on the table

3. Kepler's orbits are simpler than they first appear, involving one set of elements pertaining to heliocentric longitude, another pertaining to heliocentric latitude, and the third pertaining to location of the planet at some epochal time

a. The longitude of aphelion, the eccentricity, and the mean distance (or its double, the length of the line of apsides) -- quantities that basically carry over from Ptolemy -- determine the ellipse

b. For, given any "eccentric anomaly," need only simple trigonometry to determine the sun-planet distance and the angular location of P, angle ASP (see figure), leaving only the time at P to be determined, knowing the period and e.g. the time T when at aphelion (see below)

c. Kepler's orbital scheme points to certain preferred observations for determining aphelion, eccentricity, and mean distance for a planet, adding to those from Ptolemy forward

d. As with the preferred observations for determining location of nodes (longitude of the ascending node) and inclination, the two elements dictating latitudes

4. Kepler could not let the area rule determine the shape of the other orbits in the way he had done for Mars, for, given the area rule, Tycho's data could not distinguish their ellipses from eccentric circles

   a. Not able to do a vicarious theory for the inner planets, for they are never in opposition, where heliocentric longitudes match geocentric longitudes

   b. And, as the table in the Appendix shows, the low eccentricity of the orbits of Jupiter and Saturn make their trajectories too close to circles for the area rule to distinguish the ellipse

   c. Indeed, even in the case of Mars, as shown last week, a bisected eccentricity equant in a circular orbit produces heliocentric longitude errors less than 10 min of arc

5. In one critical respect the calculation system required a bothersome form of calculation that contrasted it sharply with all prior systems

   a. Because of the area rule and the ellipse, going from a given time to the determination of heliocentric longitude required an indirect, iterative solution of a transcendental equation, now called Kepler's equation (see figure):

$$n * (t - T) = M = E - e * \sin E$$

   where n is the mean motion, M the "mean anomaly", E the eccentric anomaly locating the projection of the planet on its circumscribed auxiliary circle, and T the time at aphelion

   b. This feature, more than any other, impeded the use of the Tables, leading others to try to by-pass the problem -- e.g. by replacing the area rule -- instead of using the tables Kepler provided giving approximate solutions for his equation

6. By the time the *Rudolphine Tables* were published, Kepler knew better than anyone that they left problems open

   a. For example, in the discussion of the moon, he remarks that the motions of the sun and moon and the diurnal motion of the earth are not equable, but are

   "subject to small intensions and remissions extra ordinem; these perturbations being perhaps due to physical influences from the planets. The physics that Kepler had introduced into the skies thus brought with it some of the complexity of the terrestrial physical realm, where many causes are concurrent in a single event" (Wilson, "Horrocks ...," p. 241) -- see X, p. 44, ln 27-30 and p. 90, ln 7-11

b. And in a letter of 1625 he indicates that Jupiter, Saturn, and Mars are subject to an inequality that will require centuries of observation before it will become amenable to analysis -- a point he alludes to in the Preface to the Tables (Wilson, ibid. p. 240) -- see XVIII, p. 237, (letter to Bernegger) and X, p. 44, ln 21-25, which cites observations by Regiomantanus and Walther

c. His view seems to be one of using the Tables to expose and then characterize these discrepancies in order to refine the theory to handle them, as he had begun to do with the moon; and he recognized that his values for the elements depended on Tycho's parallax corrections for the sun

d. Nevertheless, he had clearly become concerned that astronomy might not be "perfectible"

7. Note that Kepler raised the question, is planetary astronomy perfectible? -- i.e. can the (undisturbed) motion of the planets be predicted into the indefinite future to within observable accuracy?

   a. A question receiving its first reasonably conclusive answer only in the last decade of the twentieth century

   b. For Kepler, not a question about whether e.g. ellipse and area rule exact, but about whether any variation over time in the orbital elements can be specified in a way that makes physical sense

   c. If some alternative to Keplerian trajectories can do this better, then for Kepler it would have had claim to being superior to his

   d. The possibility that no account of the motion could do this he found threatening

8. In addition to such effects that Kepler found beyond his reach, the Tables have shortcomings in orbital elements that they need not have had (see table in Appendix)

   a. Primarily from Tycho's theory of the sun, which had a far too large correction for parallax, because of a far too large horizontal parallax of the Sun -- i.e. Tycho had the sun much too near the earth; this error affects everything else because observing from the earth

   b. This resulted in an excess eccentricity of the earth-sun orbit; this in turn contributed to Kepler's eccentricity for Mars being a little too small (0.09253 versus 0.09304, or 430 parts ingress vs. 433.8) and the aphelion being a little advanced (148° 59′ 54″ versus 148° 41′ 58″)

   c. Kepler reduces Tycho's 3′ to 1′ after concluding parallax of Mars is less than 1¼′, but still does not change earth-sun eccentricity

   d. Nevertheless, as Gauss was to remark in 1809, the problem for post-Keplerian astronomers "was no longer to deduce elements wholly unknown, but only slightly to correct those already known, and to define them within narrower limits." (Wilson, "Derivation", p. 25)

II. Some Philosophic Issues Concerning Kepler's "Laws"

A. Kepler's Substantive Legacy: the Generalizations

1. His most obvious legacy was a comparatively simple, yet extraordinarily accurate version of the Copernican system -- the sort of simplicity and accuracy that Copernicus had yearned for

   a. The Copernican system with Keplerian orbital motion

b. The system as we have known it almost ever since -- at least ever since Newton

2. Wilson says that progress in astronomy over the next century "depended in large measure on the adoption ... of six Keplerian innovations" (p. 161)

   a. The five from *Astronomia Nova* listed in the table

   b. Joined by the 3/2 power rule, relating the different orbits about a single central body

3. The area rule, the ellipse on an inclined plane, and the 3/2 power rule, together characterize what has since become known as “Keplerian motion” (with planar orbit through true sun implicit)

   a. Three (plus one) generalizations about the motions of six planets, extended (with qualifications) to include the moon and the Galilean satellites of Jupiter

   b. Those generalizations the part of the legacy that we will be focusing on for the rest of this class and in subsequent weeks

   c. Came to be called "laws," but only after 1687

4. Kepler also left a large body of mathematical procedures for working with Keplerian orbits that still remain the basis for many astronomical calculations

   a. Seven orbital elements: eccentricity, semi-major axis length, angular position of the line of apsides, angular position of the line of nodes, inclination, the location of the planet at some epochal reference time, and sidereal period or mean (daily) motion

   b. Given these, procedures allow determination of all other "positional" aspects of the Copernican (or, of course, the Tychonic) system

   c. (Subject to second-order effects, causing slow variations in these elements -- see current values from Danby, derived from gravitation theory, not purely from observation)

   d. (Kepler included allowance only for slow precession of equinoxes, aphelia, and line of nodes)

5. Finally, he left not just a large body of largely discarded conjectures about the physics underlying all of this, but also an indication of how the astronomical features listed above could serve as an initial evidential basis for delving into the physics

   a. Not just a legacy of insisting on physics as part of astronomy

   b. But an indication of how astronomical findings might begin yielding conclusions about celestial physics

B. Some Advances in Turning Data into Evidence

1. Even from our limited review of orbital astronomy so far, it should be clear what the basic problem is of turning data into evidence in any new area of scientific research

   a. Need theoretical apparatus to extract evidence from data

   b. In absence of well-substantiated theory, must use working hypotheses of some sort -- hypotheses that cannot really be separately tested because they are needed to draw conclusions from data in the first place

   c. Problem: how to avoid garden-paths ("castles in the sky") owing to "bad" working hypotheses

2. In the case of Ptolemy and Copernicus, we have seen some working hypotheses that paid off to an extent, but then became confining
   a. E.g. epicycles (more precisely, mean retrograde loops) yielded r/R, and hypothesis that five planets orbit the Sun yielded relative r's
   b. But troubles in using discrepancies or other data for refinements past a certain point
   c. Problems in the data themselves, and in determining their level of accuracy, an obstacle
3. One advance, from Tycho: having a body of data of a reasonably well-defined level of accuracy, already corrected for some systematic observational errors (though not independently corroborated)
   a. Could thus begin to separate discrepancies arising because of second-order effects or because of basic theory being wrong from observational errors -- always the key
   b. Extent and uniformity of precision of the body of data help in exposing bad data points and in estimating systematic corrections: play data off against one another, using minimal theory
   c. Thus, for example, Kepler came to realize that Tycho's correction for parallax was almost certainly too large -- i.e. Tycho had the Sun too close to the Earth
   d. Kepler called attention to this source of error, but postponed alterations until parallax better defined and his 0.018 eccentricity for earth could then come not from taking half of Tycho’s value, but more directly from observations
4. A second advance, from Kepler, in working with data: pursue converging evidence, or at least corroborative evidence
   a. E.g. triangulations in support of oval etc., corroboration of area rule using solar theory, etc.
   b. Use (tentative) theory wherever possible to obtain more than one inferential route from data to an evidential conclusion
   c. Also use (tentative) theory to determine when a discrepancy can be from very small errors in data versus when it is indicating an error -- sensitivity analysis
      (1) E.g. contrast between rejection of "vicarious" theory and willingness to proceed with bisected eccentricity
      (2) Assessment of latitude theory, and acceptance of discrepancies in table of 28 observations pending better data (i.e. with better corrections for atmospheric refraction)
   d. But, modulo this recognition of sensitivities, do not ignore discrepancies that may be informative
5. A third advance, from Kepler: in absence of physics, insist on regularities that promise to point to physics, thereby contrasting orbital theories that amount only to what we would call "curve-fits"
   a. E.g. regularities that can plausibly result from a single physical mechanism
   b. Superposed regularities from superposition of physical mechanisms
   c. This in contrast to conclusions that seem hopeless to explain physically
6. But even with these advances, still had serious problems in extracting evidence from Tycho's data
   a. Small residual discrepancies, such as in a^3/P^2 for Venus: what are they indicating?

b. Non-stable values of elements over time indicate either a "secular" or a long-term process: at best many years of data needed, and at worst astronomy ultimately not "perfectible" at all

c. And how to get evidence bearing on the question whether the theory is just describing the way things happen to be (by accident), or the way in which things in some sense have to be

C. Accidental Versus Nomological Generalizations: Projectability

1. Applying the term "laws" to Kepler's generalizations on planetary motion is more than a little anachronistic -- which is why I have been using shudder-quotes around the word throughout

   a. Kepler's three generalizations came to be known as his laws only following Newton's *Principia*, which offered justification for their having such a status

   b. Indeed, the term 'laws' was introduced into astronomy, so far as I can determine, only through the extension of the notion of laws of motion, as in Descartes

   c. First place Kepler's regularities called "laws" appears to be Leibniz's *Tentamen*, which offered a "Keplerian" alternative to Newton's *Principia*

2. Still, the important logical distinction concerning such generalizations -- the distinction that separates laws from other generalizations -- is ancient and universal

   a. Accidental generalizations -- e.g. 'all the coins in my pocket are silver' -- do not support counterfactual conditonals -- 'if that penny were in my pocket, it would be silver'

   b. Nomological (or lawlike, to use Nelson Goodman's term) generalizations -- e.g. 'all mammals have lungs' -- do -- 'if sharks were mammals, they would have lungs'

3. A question about the range of counterfactuals supported still arises even with this distinction

   a. Just as with Ptolemaic theory for Venus through Saturn, Kepler's planetary theory was taken to yield answers to comparative questions that required it to support counterfactuals like, "If the eccentricity of Jupiter were the same as that of Mars, then Jupiter's retrograde loops would …"

   b. In both cases the theory connected the elements of the orbits to further observable features in ways that the evidence gave grounds for such comparative counterfactual claims

   c. But that still leaves open questions about whether the theory supports counterfactuals beyond those concerning the specific planets, like "If there were still another planet, it too would describe an ellipse, sweeping out equal areas in equal times with respect to Sun at its focus"

4. Rather than just ask whether the evidence justified taking Kepler's generalizations as lawlike, better to ask about the range of counterfactual questions over which the evidence supported answers

   a. E.g. over the specific planets, versus over all (possible) bodies orbiting the Sun, versus all (possible) celestial bodies engaged in celestial motions

   b. I.e. over what range does the evidence support the *projection* of Kepler's generalizations beyond the known planets (over the period of observations entering into the *Rudolphine Tables*)

   c. Goodman singled out the *projectability* of lawlike generalizations; all I am adding is an insistence on being attentive to the range of the projections beyond the given cases

5. Upshot, a special evidence problem: given a generalization that holds for certain things, determine whether it is properly construed as nomological, instead of as merely accidental, and then the range over which it should be taken to project beyond those certain things
    a. This the most serious evidence problem facing any scientist who happens upon a regularity -- wants empirical evidence that observed regularity not a mere artifact
    b. General issue: how to bring empirical evidence to bear to show that an observed regularity ought to be taken to be nomological and, if so, the range of its projectability

D. A Complication: Exact Versus Approximate "Laws"

1. Given an observed regularity, the question whether it should be taken to be nomological is confounded by a number of other questions, involving further logical distinctions
2. The most important of these is whether the regularity or generalization holds exactly or only approximately -- i.e to some appropriate standard of approximation
    a. Generalizations that hold only approximately are still of interest in science -- indeed, most "laws" of science are in this category -- for maybe the inexactitude is just from secondary effects of lesser interest
    b. E.g. Boyle's law: pressure ∗ volume = constant: does this hold exactly, or only to a very high approximation?
    c. Here too we need to distinguish between how the generalization is intended or taken, on the one hand, and how it ought to be
3. Thus a further evidence problem: given a generalization, determine whether it holds (or ought to be taken as holding) exactly or only approximately, including what I will below call essentially exactly
    a. This problem is confounded by the fact that measurements themselves are inherently inexact, not only because of observational errors, but also because of systematic errors (biases) intrinsic to the measurement process being employed that have nothing to do with the regularity itself
    b. Thus, for example, Tycho's incorrect value for the Sun-Earth distance introduced a systematic error in his parallax correction for the Sun and hence in the corrected observations Kepler used; this systematic error propagates throughout the *Rudolphine Tables*, producing subtle discrepancies of the order of a few minutes in calculated longitudes
    c. Often a major undertaking to parcel out variance in measurements to decide whether generalization should be taken to be exact, and if not, how inexact: 180 years required in the case of Boyle's law, more than 80 years in the case of Kepler's
4. Generalizations that hold only approximately admit of a further distinction between ones that hold in the mean and ones that do not
    a. Agreement in the mean versus skewed agreement: how are deviations from the generalization distributed with respect to it, via least square error, or in some other, systematically biased way

    b. Agreement in the mean comparatively rare among the approximate laws of the physical sciences -- e.g. Boyle's law

5. One more distinction among generalizations that hold only approximately is between ones that are idealizations and ones that are not
    a. Idealization: a generalization that would hold exactly if certain (secondary) effects were not present ("essentially exact"), or that is in some other way idealized
    b. E.g. Boyle's law would hold exactly if molecules were point masses that did not exert forces on one another at a distance; and laws of linearized elasticity drop all higher order terms
    c. This distinction is orthogonal to the former one, yielding four (or six, if essentially exact is treated as separate) distinct categories: idealization in the mean, etc.
    d. Must also allow for idealizations of another sort: ones that serve only to simplify mathematics, like linear elasticity
6. Still a further evidence problem in the case of any generalization that holds only approximately: does it hold in the mean or not, and is it an idealization or not, and if so, what sort of idealization
    a. First part concerns the nature of the approximation, and therefore is closely related to the question of exact versus approximate (e.g variance from "hidden variables")
    b. But the second part raises important new evidential problems, for close examination of high quality data is not going to tell you much about whether an idealization

E. Further Complications: Range and Qualifications

1. Two further complications, over and above those above, arise, especially with nomological generalizations
    a. Both, because of imprecisions usually inherent in the statement of the generalization
    b. I.e. imprecision or vagueness in what the generalization is asserting
2. First, the range over which the generalization holds or is being taken to hold is not always clear -- universal, but over what class?
    a. E.g. 'all mammals have lungs' is presumably taken to hold over a natural kind, mammals, but which animals fall within this range is not immediately given, nor given a priori
    b. Similarly, Boyle's law is taken to hold (approximately) only over a range of pressure, and not at extreme high pressures
    c. And Boyle's law can be variously stated: pressure is proportional to 1/volume, to density, to mole density, reflecting the range of circumstances in which it is said to hold
3. Second, nomological generalizations almost invariably include a largely tacit "ceteris paribus" clause that, because it is tacit, is not always clear
    a. E.g. a mammal with surgically removed lungs is not taken to be a counterexample to 'all mammals have lungs'
    b. And Boyle's law has the explicit ceteris paribus condition, temperature remaining constant

4. Thus, given any observed regularity, still further evidence problems: to determine the range over which and the ceteris paribus conditions under which the generalization continues to hold
   a. Evidence problems here concern how the generalization ought to be stated -- in contrast to how it is intended
   b. Questions here obviously interrelated with questions about exactness and types of approximation, and hence so too are the evidence problems
   c. And questions here just another way of formulating those about range of projection raised in section before last, though here focused on proper way of formulating the generalization
5. Much of the history of science is concerned with developing empirical answers to questions about the range over which and the ceteris paribus conditions under which observed regularities hold
   a. I.e. to figuring out the precise, "correct"-- i.e. "preferred" -- statement of the generalizations
   b. A mark of the advanced sciences that they can bring empirical evidence to bear on these and the other questions concerning the status of generalizations

F. Questions to Ask About Kepler's Generalizations

1. In discussing Kepler's rules up to this point, have been primarily concerned with whether they ought to have been "accepted"; now see that a whole host of other questions need to be considered in tandem with this one
   a. Questions not only about how Kepler intended his generalizations
   b. But even more so, questions about how they ought to have been taken by him and by others in, say, 1630, at the time of his death, or the decades thereafter
2. Should the three (or if you prefer, the four or even five) generalizations at the heart of the Keplerian system be taken to be nomological, or mere observational artifacts
   a. E.g. artifacts from considering only the six planets -- a rather small data base, to say the least
   b. Or epochal artifacts -- generalizations that are holding to a reasonable approximation now, but will not hold in the future and perhaps did not hold in the past
   c. That the moon does not conform with Keplerian motion lends weight to these questions
3. Do the generalizations hold exactly, or at least essentially exactly, or only approximately, and if the latter, how do they hold -- i.e. in the mean or not, and as idealizations or not
   b. Kepler's suggestion: would be exact were it not for various secondary interactions among the planets
   c. Kepler says that the moon as well would conform were it not for its being physically governed by both the earth and the sun
4. Over what range of objects and values do the generalizations hold, and under what ceteris paribus conditions
   a. Do the generalizations extend to "secondary" planets like the moon and the satellites of Jupiter, not to mention comets, and do they continue to hold regardless of the range of, say, r?

b. Would a comet knocking a planet out of its orbit constitute a counterexample to them?

5. As we have already seen to some extent, Kepler was perfectly aware of questions like these (though not in our jargon for them) and he devoted a great deal of effort toward addressing them
   a. He, and others following him, wanted these questions to be resolved empirically, and not "philosophically" or through "final causes"
   b. And he, and those following him, became acutely aware of the methodological problems in bringing empirical evidence to bear on them
   c. How can such questions be addressed empirically? -- perhaps the most basic issue of this course

III. An Examination of the Evidence for Kepler's "Laws"

A. The Precise Statement of Kepler's Generalizations

1. Goal, then, is to assess the evidence bearing on Kepler's "laws" at the time of his death in the light of these distinctions and complications
   a. With particular emphasis on how he chose to attack the methodological problems arising with the further questions
   b. Best start with concerns about the precise statement of the three generalizations
2. Kepler took the generalizations to apply to the six planets, with some vagueness about their application to "secondary" planets
   a. He expressly remarks that the 3/2 power rule extends to the satellites (his word) of Jupiter, and he applies the other two generalizations to the moon to obtain first approximations
   b. But he is clearly aware that the moon violates his first two generalizations, and therefore knows some sort of qualification is needed in stating them for it
   c. Also, his physical account is geared fundamentally to the sun, so that not entirely clear whether appropriate to include, without further qualifications, bodies not orbiting the sun
3. Kepler does not as such address "ceteris paribus" conditions, but it is clear that he intends that the "laws" be taken to hold, at least to a very high level of approximation, so long as the planets remain undisturbed by physical processes not now at work!
   a. Whatever the physical processes now at work might be, so long as nothing extrinsic to them enters, then generalizations apply
   b. Generalizations viewed as sustained by distinct physical processes, not by the active hand of God, spirits, or minds of any sort!
4. In the *Epitome* he expressly views the first two generalizations as "real world" replacements for an ideal
   a. If the planets themselves were not magnetically sensitive and had started in the plane of the ecliptic, then perfect concentric, uniform circular motion in the plane of the ecliptic
   b. The first two generalizations thus capturing a "second-order" departure from this ideal
5. At the same time he intimates in both the *Epitome* and the *Rudolphine Tables* that the first two laws

themselves are idealizations that would hold precisely were it not for interactions among the planets (primary and secondary)

a. The correlation of the lunar inequalities with sun-moon-earth positions the primary basis for this statement

b. He also allowed (from *Astronomia Nova* on) for slow rotations of lines of apsides and nodes (not so slow in the case of the moon) as another respect in which the "laws" were idealizations

c. And he foresaw the need for long-term data to specify any other variations in orbital elements, attributing any such variations to planetary interactions

6. In sum, a fair amount of uncertainty and vagueness in the statement of at least the first two generalizations, though with comparatively precise versions once restricted to the six planets

B. The Evidence in 1630 for the Ellipse and Area "Laws"

1. As of 1630, the primary evidence for the first two rules was that, taken together along with specific values of the requisite orbital elements, they yielded predictions between one and two orders of magnitude better than anything before, broaching on observational accuracy

a. I.e. accuracy to a level where just as reasonable to question observations or orbital element values as the rules when faced with any discrepancy between prediction and observation!

(1) Both a "resting point" and an impass

(2) Sciences often reach this stage for a while

b. No "deductions" of area rule or ellipse in the case of the other planets; instead, assumed them and determined elements (Venus in 1614; Mercury in 1609, 1614-15, and 1616; Jupiter in 1616, and earlier; and Saturn in 1616 -- Field, p. 191)

2. He, and every other qualified astronomer, was aware that this success left open the possibility that some other trajectories and/or motion rules might achieve a comparable level of success

a. Open in part because of imprecisions in the observations themselves, including recognized possibility of systematic errors -- e.g. from wrong parallax and refraction corrections, or imprecisely measured obliquity of the ecliptic (the reference axis for longitude, latitude)

b. But open also in part because the two rules and the values of the orbital elements could not be independently assessed, thus creating more of a chance for an alternative

c. Underlining this openness was a recognition by all that, given the observational inaccuracies, there was really no separate empirical evidence for the two rules from earth, Venus, Jupiter, and Saturn; Venus could even be done with uniform motion on an eccentric circle

3. Kepler had more responses to this open possibility than just saying, "Okay, you come up with something at least as good"; in defense of the ellipse:

a. In the case of Mars, triangulations show that an oval, and the variation of distance (with eccentric anomaly) in the case of the ellipse is physically reasonable -- $1 + e * \cos E$

b. In the case of Mars, assuming the area rule, an empirical argument specifically to ellipse, and possibility of other curves (e.g. the via buccosa, that is, the locus of points where the diametral distance intersects a radius drawn from the center) meeting the same conditions undercut by above physical argument

4. And in defense of the area rule:
   a. In the case of Mars, determines a specific answer to question of trajectory (i.e. at least an answer meeting the physical requirement)
   b. Concluded to be physically correct at apsides, and elsewhere equivalent to the physically sensible inverse distance rule (once recognized that the velocity in question is that normal to r, i.e. the velocity component driving the planet in its orbit); proof of equivalence with original inverse distance rule in *Epitome* (p. 143) inadequate, for it does not handle adjacent triangles
   c. Finally, alternatives to it, in particular the equant, objectionable (*Epitome*, V, p. 145)
      (1) Loss of accuracy unless equant point has irregular movement
      (2) No physical account of equant, in contrast to Kepler's physical account of area rule
5. All of this said, still some complicating concerns as of 1630
   a. Predictions not altogether within observational accuracy -- e.g. within 2 or even 4 min of arc
   b. Lunar theory fits only by treating higher order inequalities as second-order perturbations on basic rules, and even then doubts about whether have an adequate predictive account of moon
   c. Signs that orbital elements of Saturn and Jupiter (he says Mars too) may vary over time

C. The Evidence in 1630 for the 3/2 Power "Law"
   1. In a very different sort of position with the 3/2 power "law" since evidence for it comes by means of an empirical generalization from cases
      a. An inductive generalization, with residual discrepancies to begin with
      b. Moreover, a generalization not from data, but one involving inferred values of an orbital element (mean distance)
      c. Unclear that Kepler himself put as much stock in this “law,” although he did offer a physical explanation -- something he did not do for other velocity comparisons (the other harmonies)
   2. The physical explanation of the third law, more glaringly than those of the first two, entails further consequences, viz. about planet densities, that are lacking independent evidence
      a. I.e. in some respects a more ad hoc explanation
      b. Though also more open to contrary evidence (from telescopic determinations of planet sizes)
   3. No argument against some other relationship holding instead, given the small discrepancies; and the relatively small number of cases makes this a distinct worry
      a. Mere numerical happenstance (of the sort that subsequently arose with Titius-Bode law)
      b. But, level of agreement a counter to this, as is the proposed extension to the satellites of Jupiter, provided future data remove the discrepancies; (does the physical explanation hold there too?)

4. Since the physical explanation built off the numbers, it yields no argument that the "law" holds exactly, or would hold exactly were it not for secondary effects
   a. Evidence that it may well hold exactly from the level of agreement achieved, with one element (mean distances) subject to variations from observational inaccuracies
   b. But lots of room for its being inexact, especially with physical argument, for even a small departure in density-distance relations would undermine its exactitude
5. One interesting thing to notice here is that the level of agreement in the case of the 3/2 power rule is high enough to give reasons to take it to be exact and use it to correct the inferred mean distances
   a. Periods can be determined more precisely from observations than mean distances can be
   b. Maybe should just take rule to be exact, and use it to obtain better values of this orbital element, in the process narrowing the range of uncertainty in one respect
   c. Can do so non-arbitrarily, for new elements should yield even better predictions than old did
   d. In fact, Kepler's values for mean distances are off by 0.25 percent for Mercury, 0.11 for Venus, 0.01 for Mars, 0.05 for Jupiter, and 0.38 for Saturn (versus values for 1600 implied by Simon Newcomb's tables)

D. Evidence that the "Laws" are Laws: Kepler's Approach

1. Turn now to the evidence that the three "laws" are nomological, and not just some sort of numerical or epochal accident
   a. The fact that the rules are known to apply to so few objects, and then only over a quite limited period of time, underscores the worry here
   b. And Kepler complicates matters by claims that threaten to rule out some counterfactuals -- e.g. the regular solid argument suggests that there could only have been 6 planets, and they had to be situated much as they are, thus barring counterfactual talk of other planets in other positions
2. Kepler did not have much in the way of a model for running evidential arguments to show that generalizations are nomological (or exact)
   a. Mathematical proofs could be used to argue that things had been established in accord with a design -- Neoplatonism
   b. Appeals to reason, in the manner of Aristotle, had clearly failed in astronomy
   c. Kepler was one of the first to try to devise empirical arguments for concluding that observed regularities are (what we now call) nomological
3. Kepler did have some "internal" evidence that his generalizations were not mere artifacts
   a. The level of precision to which they hold, and the way they interlock with one another, tieing parameters to one another; still notice here that a stance is being adopted on the discrepancies that exceeded observational accuracy: they do not amount to counter-evidence
   b. Also, their ability to explain, e.g. Ptolemy's successes, gave grounds for thinking that the planetary system had not changed that much for a long time

c. (Kepler does invoke Ptolemy's successes as evidence for his trajectories, which he takes to be a refinement built off of Ptolemy's first-approximation)

4. But Kepler's preferred strategy for showing that "laws" are nomological, as he makes clear time and again, is to argue that they are manifestations of underlying physical processes and mechanisms
   a. His insistence that any regularity be physically plausible is intended as a safeguard against accidental truth -- this is his way of dealing with the risk of being misled by e.g. numerical agreement, and not just his way of justifying the Copernican system over the Tychonic
   b. His criticisms of his predecessors accepting regularities merely because they work reasonably well -- e.g. comments on the equant in *Epitome*, V, p. 145
   c. His decision in the *Epitome* to present the physics first and then derive the geometric astronomy from it
5. Note here his curious practice of insulating his "efficient" causation arguments from his "final" causation ones
   a. He may feel he has an explanation of why there are 6 planets and why the velocity ratios are as they are, but he rarely permits such explanations to intrude on his physical ones
   b. The "laws" hold not because God chose for them to, but because mechanisms governing planetary motion, once set in place by God, entail that they do

E. Kepler's Approach to the Underlying Physics

1. The trouble Kepler faced, of course, is that he had almost no physics to turn to in forming arguments that the "laws" are manifestations of underlying physics
   a. Only some empirical results of "experiments" and observations on earth, plus analogic reasoning from them
   b. In particular, magnetism, and diminution of driving "force" of a vortex
   c. Simple fact is that Kepler was working in the early stages of the development of the science of motion, and as all scientists have had to do in this situation, he had to try to pull himself up by the bootstraps
2. That is, Kepler turned the situation inside out: he assumed, at least provisionally, that the observed regularities are manifestations of underlying physics, and he then used them to draw conclusions about the physics
   a. If unable to come up with a physics that would yield the observed regularities, then nomological thrown into question
   b. Equally, the more Rube-Goldbergish the physics, the more the worries about nomologicality
3. Kepler should not be criticized for trying to do this, for it is a time-honored procedure that is still being followed today -- e.g. the genesis of the big-bang theory
   a. He is perfectly open about the need for conjecture in physics -- see p. 48 of the Introduction to *Astronomia Nova*

   b. Conjecture constrained by limited knowledge of physical processes on earth and by the need to conform with tentatively accepted, highly accurate astronomical regularities

4. Of course the danger here is circular reasoning, void of content: Kepler is assuming that the regularities are nomological in order to use them to draw conclusions about the underlying physical processes, and he is then using these conclusions as grounds for arguing that the regularities are nomological
   a. On his view, can't get anywhere without conjectures about physics
   b. Problem then is to make sure the conjectures are not question-begging
   c. Strategy sure to leave a large promissory note outstanding
5. He tried to counteract this danger by minimizing the number of basic physical assumptions and by insisting that the regularities then be strictly (and exactly) derivable from the physics
   a. I.e. exactly derivable under appropriate ideal conditions, such as no interaction with third bodies
   b. Exactitude, at least under ideal conditions, a key constraint here; reasoning loses much of its force if regularities hold only very roughly!

F. Illustrate Via the "Physics" for Keplerian Motion

1. Kepler's basic physical model separates two aspects of planetary motion, attributing each to a different mechanism
   a. The basic motion of planets revolving around the Sun (and satellites around their principals)
   b. The "libration" in the distance from the planet to the Sun that causes a non-circular trajectory
2. Planets revolve around the Sun because of a magnetic or magnetic-like vortex given off by the rotating sun
   a. Rotation of sun postulated before Galileo observed it
   b. Strength of the vortex -- i.e. the push of the vortex -- diminishes with distance
   c. Different planets have different periods because of their "inertias" (Kepler invents the term) -- their differing tendencies to resist motion, either initial or continuing, altogether
3. Planets have a non-circular trajectory (which lies outside the plane of the ecliptic) and hence variable velocity because they contain magnetic fibers themselves that cause them to be attracted to and repelled from the Sun
   a. Magnetic fibers (ideally) always pointed in the same direction -- perpendicular to the line of apsides, so that at perihelion and aphelion, no attraction or repulsion
   b. Orientation in one half of orbit, vis-a-vis the Sun, then attractive, and in the other half repulsive
   c. Attraction reaches a maximum when pointed directly to the Sun
4. From these two together can derive elliptical orbit and area rule exactly with minimal additional assumptions
   a. Impetus from spinning Sun always drives planet in direction normal to radius vector -- the impetus that would yield a perfect (circumscribed) circle if planets were magnetically neutral

b. The attraction and repulsion, occurring along the line of the radius vector, yield the relation, SM=$r(1+e*\cos E)$, that is, the diametral distance rule

c. But the area rule is tantamount to the delay per equal arc varying directly, and hence the velocity varying inversely, with distance (see *Epitome*, p. 143), and the ellipse then results from SM radius vectors being laid out properly (p. 133ff)

d. The obliquity of the orbital plane also from the magnetic fibers

5. This physical account of ellipse and area rule end up entailing a lesser status for the 3/2 power rule

   a. The 3/2 power rule is no longer strictly nomological as it stands, for it reflects a choice by God of planet densities making it hold

   b. But it is nomological when re-expressed as a relation between period, mean distance, and density

G. The Empirical Limitations of Kepler's Physics

1. I have been more sketchy in describing Kepler's physics than it merits in large part because (i) it does not work and (ii) it itself had relatively little influence

   a. Stephenson's book lays the physics out in far more detail, bringing out the logical integrity of his physical reasoning

   b. The physics is wrong because he has the elementary physics of motion wrong, but the reasoning is neither mystical nor crazy

2. Having said this, however, we should pause to be clear about the evidential shortcomings of the physical reasoning -- shortcomings that can be detected without having to know the right answer!

   a. The basic problem is that the evidential arguments never close the loop -- i.e. the physics never entails much of anything in the astronomical realm that was not built into it in the first place!

   b. I.e. the physics remains ad hoc, with little or no independent evidence for it -- something that was quite clear at the time

3. In truth, this is an exaggeration, for Kepler tries to get the inequalities of the motion of the moon out of the very same physics

   a. By using the magnetic properties of the bodies needed to account for their "two-body" motion, but now with a "three-body" interaction

   b. Likely the part of the *Epitome* of which he was most proud

   c. Did not assign it because difficult, and ultimately again ad hoc, for he was unable to get the inequalities out "for free," much less to within observational accuracy

4. Kepler did see ways in which "the loop" might be closed -- i.e. ways in which his reasoning from the assumed nomologicality of astronomical regularities to an underlying physics back to the nomologicality of the regularities might not beg questions

   a. First, by getting several regularities which are astronomically independent of one another out of the same physics -- the area rule and the ellipse together, in particular (cf. pp. 143ff)

    b. Second, by having the very same physics then cover systematic discrepancies from the initial regularities
5. Kepler succeeded only partially in the first respect, and even less in the second
    a. But he had no way of knowing that in the long run converging evidence would not develop for some version of his physics
    b. And in this regard he is to be criticized no more than others who have offered conjectural theories that did not pan out even though they were carefully crafted from observed regularities
    c. Science really is difficult, especially in the early stages of theory construction

IV. Kepler's Methodological Legacy: Some Final Remarks

A. Kepler's Conception of "Scientific" Astronomy

1. In one respect Kepler was the culmination of a 2000 year tradition of mathematical astronomy, stretching back through Ptolemy and Apollonius; but in another respect, he was the initiator of a quite new science of astronomy
    a. Physical astronomy, in contrast to just mathematical astronomy -- a branch of "physics", not of mathematics, as it had been for centuries
    b. Needed because of the crisis posed by the three systems -- i.e. because it seemed hopeless to settle the dispute among the three systems unless astronomy became a branch of "physics"
2. One way in which this shows up is in Kepler's attention to the specific physical trajectory of planets, in contrast to that of his predecessors on the geometric constituents needed to synthesize a trajectory that gives an account of the salient phenomena
    a. The actual trajectory is a physical fact, the geometric constituents are part of geometry, and different geometric constituents may yield the same net result
    b. The issue is whether that net result is correct, to at least a very high level of precision
3. It also shows up in the insistence not merely that claims about the trajectory allow for a physical -- mechanical -- explanation, but also that astronomical regularities be derivable from physics
    a. Kepler akin to Darwin in a way: he (ultimately) exorcized the need for "mind" in astronomy, insisting that all regularities be purely mechanical
    b. Note the passages in the *Epitome* that argue this point; he keeps pointing out that Copernican astronomy allows an end to a certain kind of nonphysical explanation
4. He further puts forward a conception of how to go about marshalling evidence in physical astronomy, namely by using astronomical regularities to infer some physics, then deriving the regularities from the physics
    a. Multiple, astronomically independent regularities and "laws" from the same physics (as much as possible)
    b. Derivations to yield the exact "laws" under idealized assumptions, which in turn makes a tight relationship between the "laws" and observations more important (the tighter the better)

c. Finally, physics must cover any systematic deviations from the "laws" (with minimal additional apparatus)

5. Not just a new "science", but a new scientific methodology, placing much greater emphasis on theorizing, not merely as an end, but as part of the process of developing evidence
   a. Also greater emphasis on exactness, for one of the key ideas is to use systematic discrepancies between observation and theory as new evidence
   b. Discrepancies not being swept under the rug, but looked to as providing information about what is going on, with the corollary of attaching much greater importance to the data themselves being very precise
   c. In particular, discrepancies that can be characterized as ones that would disappear were it not for certain second-order effects
   d. Consequently, a science that proceeds via successive approximations, playing off two levels of theory against one another and against observations
6. Even while granting that Kepler was the culmination of a 2000 year tradition, I nevertheless want to insist that his efforts illustrate the early stages of theory construction
   a. Two tenets of that tradition, trajectories compounded from circles and equiangular motion about some point, had provided not just constraints in theorizing, but principles entering into evidential reasoning from observations
   b. Once those tenets were abandoned, theorizing ceased to be constrained, and novel principles were needed for reasoning from observations, while still granting and hence needing to explain the successes of the past
   c. In particular, Kepler's appeals to physics and his use of triangulation under the assumption that Mars orbits the sun replaced them
7. An historical parallel to the situation in which Kepler found himself occurred in the first decade of the 20th century when the constraint that *energy* is a continuous variable was dropped, and a couple of decades of effort was then needed to figure out how to constrain theorizing
   a. Initiated by Planck's law for black-body radiation, under Einstein's 1907 interpretation of that law, and Einstein's 1907 proposal for the specific heats of solids
   b. The first Solvay Conference of 1911 called to address the question of how to incorporate quanta into physics while still granting the successes of classical physics
   c. Fifteen years then before Heisenberg's matrix mechanics and Schrödinger's equation emerged

B. Some Residual Problems Facing Kepler in 1630

1. For all his achievements, Kepler could not help but be aware of certain difficulties in his account at the time he took sick and died in 1630
   a. He more than anyone would have been aware of these, though others saw them over the next 10-15 years

b. (Indeed, he calls attention to some of them explicitly in the *Rudolphine Tables* and implicitly in subsequent Ephemerides, though without challenging claims about underlying physics)

2. One concern was whether he had optimal values for the elements of the various orbits

   a. He knew perfectly well that the calculated positions were not always within observational accuracy, though he was probably unsure how much of this should be attributed to faults in observation and how much to the elements

   b. He openly questioned the solar parallax, and hence by implication openly questioned the corrected "data" he worked from in obtaining the values of the elements

   c. The small residual discrepancies in the 3/2 power rule also raised questions (though less for him, given his physical account)

3. Another concern was the apparently slowly changing values of the elements of Saturn and Jupiter (and perhaps Mars)

   a. By 1625 was confident that not just a data problem, but a real variation extending over a long time

   b. Speculated that from planetary interactions, but no way of beginning to argue for this until the variations were characterized

4. Final concern was the Moon, for which he had managed to devise a better predictive account than anyone before him, but still had not come close to achieving observational accuracy

   a. "The problem of the moon" -- just to give an astronomically accurate system for predicting its observed positions

   b. Further inaccuracies had yet to be characterized systematically

   c. This in turn raised questions about the adequacy of the physics invoked in support of the model

5. Finally, his physics was clearly ad hoc and largely conjectural, with a need for much more independent, converging evidence

   a. His physics logically akin to Ptolemy's astronomy -- not as unified as one would like

   b. With implications remaining to be tested -- e.g. density implications

6. A worry in the background that some others made increasingly explicit around the time of his death and after: can claims about underlying physics be anything more than mere conjectures?

   a. Kepler's physics scarcely gave reasons for thinking that the underlying physics could be settled once and for all

   b. Maybe best one should hope for is accurate prediction of phenomena

C. Issues Raised on His Conception by These Problems

1. These residual difficulties had to raise some fundamental questions in the mind of anyone with Kepler's conception of "scientific" astronomy

   a. Questions that would presumably have preoccupied him had he not died at the age of 58

   b. Questions that came to preoccupy others over the next 50 years

2. Do the three "laws" hold exactly -- or exactly were it not for certain second-order physical effects -- and if not, is this reason for worrying about the possibility of alternatives to them
   a. Maybe they just happen to approximate some "true laws" which, if discovered, would remove residual discrepancies and yield a better physics
   b. In particular, maybe some alternative would allow further lunar inequalities to be characterized
3. To put the point differently, the question, given his conception of science, is not whether the three "laws" hold to a very high degree of approximation -- for they do -- but whether they may nevertheless be systematically misleading
   a. Misleading with regard to whether deviations from them can be systematically characterized, and hence astronomy be "perfected"
   b. Misleading with regard to physical processes underlying the regularities "laws" are capturing
4. The interesting issue facing anyone who saw things in this way was whether it was appropriate to accept Keplerian theory, at least provisionally, or instead to look aggressively for alternatives to it
   a. One can always construct alternative hypotheses, at least up to a point
   b. Which promised the greater likelihood of long run success, to build on Keplerian theory or to hold it in abeyance and look for alternatives to it?
5. Kepler himself probably felt that he had reached somewhat of a dead-end -- i.e. had gotten as much out of Tycho's data as it was possible to get
   a. Needed at least further observations, made specifically in the light of his theory, or else still more accurate observations
   b. Perhaps explains why he had done almost nothing new in astronomy since 1625 at the time he died
   c. In effect, he had reached the same sort of point that Ptolemy had reached 15 centuries earlier: could see no way to extract further evidence from the data available at that time
6. Regardless, we can be confident that by the time he died Kepler had come to appreciate the magnitude of the problem of establishing physically correct trajectories for the planets, for he had come to recognize the challenge posed by the issues listed in the table at the end of the Appendix
   a. Residual systematic error in data
   b. Risk of garden-path from theory-mediated evidence
   c. Limitations of astronomical data in selecting among alternative trajectories at same level of accuracy
   d. Threat of circularity in appealing to physics to select trajectory and trajectory as evidence for physics
   e. Questions about what to make of residual discrepancies
   f. Projection, in time and to other orbiting bodies

Select Sources

Kepler, Johannes, *Mysterium Cosmographicum*, Vol. 8, *Johannes Kepler Gesammelte Werke*, Max Caspar (ed.), C.H. Beck'sche Verlagsbuchhandlung, 1963.

--------, *Astronomia Nova*, Vol. 3, *Johannes Kepler Gesammelte Werke*, *ibid*., 1937.

--------, *Epitome Astronomiae Copernicanae*, Vol. 7, *ibid*., 1953.

--------, *Tabulae Rudolphinae*, Vol. 10, *ibid*., 1969.

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

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

--------, *The Harmony of the World*, tr. E.J. Aiton, A. M. Duncan, and J.V. Field, *Memoirs of the American Philosophical Society*, Vol. 209, 1997.

--------, *Epitome of Copernican Astronomy* (Books 4 and 5) and *Harmonies of the World* (Book 5), tr. Charles Glenn Wallis, Prometheus Books, 1995.

Wilson, Curtis, "The Error in Kepler's Acronychal Data for Mars," in *Astronomy from Kepler to Newton: Historical Studies*, Variorum Reprints, 1989; originally in *Centaurus*, vol. 13, 1969, pp. 263-268.

--------, "The Inner Planets and the Keplerian Revolution," in *ibid*.; originally in *Centaurus*, vol. 17, 1973, pp. 205-248.

--------, "Newton and Some Philosophers on Kepler's 'Laws'," in *ibid*.; originally in *Journal for the History of Ideas*, vol. 35, 1974, pp. 231-258.

--------, "Horrocks, Harmonies, and the Exactitude of Kepler's Third Law," in *ibid*.; originally in *Science and History: Essays in Honor of Edward Rosen*, *Studia Copernicana*, vol. 16, 1978, pp. 235-259.

--------, "Predictive Astronomy in the Century after 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.

Stephenson, Bruce, *Kepler's Physical Astronomy*, Springer-Verlag, 1987.

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.

--------, "Kepler, Johannes," in *Dictionary of Scientific Biography*, Vol. 7, Scribner's, 1973

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.

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

Caspar, Max, *Kepler*, tr. C. Doris Hellman, Dover 1993.

Koyré, Alexandre, *The Astronomical Revolution: Copernicus-Kepler-Borelli*, tr. R. E. W. Maddison, Dover Publications, 1973.

Morin, Jean Baptiste, *Tabulae Rudolphinae, or, The Rudolphine Tables Supputated to the Meridian of Uraniburge / First, by John Kepler, from the Observations of the Tre Noble Ticho Brahe; Afterwards Digested into a Most Accurate, and Easie Compendium, 1650 (1675)*, Early English Books Online.

Field, J. V., *Kepler's Geometrical Cosmology*, The University of Chicago Press, 1988.

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

Swerdlow, N. M., "Kepler's Iterative Solution to Kepler's Equation," *Journal for the History of Astronomy*, Vol. 31, 2000, pp. 339-341.

Danby, J. M. A., *Fundamentals of Celestial Mechanics*, 2nd ed., Willmann-Bell, 1988.

Colwell, Peter, *Solving Kepler's Equation, over Three Centuries*, Willmann-Bell, 1993.

Credits for Appendix

Slide 2: Voelkel and Gingerich (2001)

Slide 3: Kepler (1937)

Slides 7, 8: Voelkel (1999)

Slide 10: Gingerich (1992)

Slide 11: Kepler (1963)

Slide 12: Kepler (1997)

Slides 17-21: Kepler (1995)

Slides 24, 25: Kepler (1969)

Slide 28: Wilson (1992)

Slide 29: Danby (1998)

Slide 31: Swerdlow (2000)