    b. Claimed a speed of light corresponding to 11 min from the Sun to the Earth (versus our mean value of 8 min 19 sec), though it turns out his data would have supported a conclusion of 9 min
    c. No attempt at a precise value in terms of an earthly measure because he knew that the solar parallax was still at issue
4. Notice here how the marked increase in precision in orbital astronomy led not only to a fundamental discovery in physics, but to a measurement that could not at the time be done on the ground
    a. Galileo's protegés at his center for experimental science in Florence had tried and failed to measure the speed of light, as noted by Descartes in his letter criticizing Galileo
    b. Roemer one of the first examples clear to everyone at the time that planetary astronomy had progressed to a point where it could provide "experimental" measurements of a much higher quality than comparable experiments on earth
    c. Roemer's measurement presupposed not just Cassini's accurate tables for Io, but also an extremely precise value of Jupiter's heliocentric longitude, as well as precise values for the Earth's longitude and Jupiter-Earth distances
5. Roemer's result, which was accepted right away by Huygens, Flamsteed, and Newton -- and more generally as it continued to account for the variation in the eclipses -- had important implications
    a. In astronomy it entails a new round of corrections of observations of planetary position -- e.g. observed oppositions of Mars as great as 7 minutes (in time) off the true opposition
    b. In physics it strikes at the heart of Descartes' theory of light in just the way that Huygens describes, opening the way to alternative accounts of the physics of light, including both Newton's particle theory and Huygens's wave theory
6. By 1684, enough had been learned about how to correct planetary observations -- for atmospheric refraction, parallax, and the speed of light -- to permit a substantial improvement over Tycho's level of accuracy
    a. Still limited by the "movement" of the North Star, so that full advantage of the telescope could not be taken until after the 1740's
    b. But major anomalies leading to potentially conflicting results and hence confusion had been eliminated -- in no small part in the way Kepler had envisaged, by drawing conclusions from discrepancies between observation and increasingly refined theory

V. Astronomy 70 Years After Kepler's *Astronomia Nova*

A. Mathematical Astronomy at the End of the 1670s

1. By the end of the 1670s seven distinct approaches to calculating the motions of the planets were known to be of the same general level of accuracy, all of them mathematically neutral between Copernicus and Tycho, all of them known to Newton in 1684
    a. Kepler's original approach, and Horrocks's variant in which 3/2 power rule used to determine mean distance; of the seven, only Horrocks's did not include full tables for all of the orbits

   b. Boulliau's approach of 1657, after rejecting his initial approach and the equant
   c. Streete's approach, combining Boulliau's method for location vs. time and Horrocks's approach for mean distance
   d. Wing's approaches, with either his oscillating equant or his subsequent geometric device
   e. Mercator’s approach of 1664 and 1676

2. The one thing all these approaches had in common was that their calculated planetary trajectories were ellipses with the central body at a focus
   a. The ellipses were more or less the same, though specific values of the elements differed and there was some dispute over whether aphelia were precessing, with Streete saying no
   b. Triangulations supported the oval shapes and (to reasonable accuracy) the eccentricities
   c. Evidence insufficient to claim that they were exact ellipses, especially given uncertainties about changing elements of Jupiter and Saturn
3. The primary difference among the different approaches was the method used to locate the planet versus time, making the "true" rule for that the largest open question at the time
   a. Mercator had shown that, whatever approach is used, it will yield excessive errors for an elliptical Mars trajectory unless it closely approximates the area rule
   b. The alternatives to the area rule had the virtue of being computationally simpler, but they did have somewhat the character of computational constructs, at least as seen by some
   c. The area rule alone offered a promise of a physical account through the invariance of $rv_{\perp}$, though defenses for the other approaches were offered, as illustrated by Mercator’s
   d. Question left open by the alternatives to area rule was whether some other no less accurate alternative might offer both a geometrical solution and have no less promise of a physical account
4. The question whether the 3/2 power rule should take precedence over more direct ways of determining mean distance gained importance from Streete's success and the publication of Horrocks's papers
   a. Does 3/2 power rule hold only in high approximation for the solar system or does it hold exactly
   b. Main argument for latter is Horrocks’s demonstrated increase in predictive accuracy for Venus and Streete’s high accuracy generally when rule used to determine mean distance
   c. Does the 3/2 power rule hold universally -- e.g. for satellites as well
   d. Question not openly addressed in the literature; Cassini's 1669 tables for satellites of Jupiter gave support, but Roemer's findings had put these tables under some question
5. Moreover, mathematical astronomy had yet to achieve an adequate lunar theory
   a. Kepler's and Boulliau's complex lunar models fell well short of observational accuracy
   b. Publication of Horrocks's papers had made public a more accurate model, but still inadequate
   c. Raising questions about the contrast between the Moon’s motion and the comparatively well-behaved motions of the planets

6. A no less perplexing contrast between the prominent inequalities in the Moon's motion and their seeming absence from the motions of the satellites of Jupiter and Saturn
    a. Borelli's 1666 book inadvertently underscored this question when it used our Moon as the basis for a physics applying to the satellites of Jupiter, contending that the same complexities had to be present with the latter even though they had not been observed
    b. Cassini's 1668 tables, by contrast, had yielded very simple orbits, though the inequality he introduced rather than accepting the finite speed of light had complicated the orbits a little
    c. Cassini also had their lines of nodes precessing, just as our Moon's nodes precess
    d. The point is that the question was open about the relationship between our Moon's motion and that of the satellites of Jupiter and Saturn
7. And finally agreement had yet to be reached on the question of the trajectories of comets
    a. Hooke's lectures on comets had them describing a linear or near linear path, following Descartes
    b. Hevelius's *Prodromus cometicus* (1665) and *Cometographia* (1668) also offered physical causes to bend Kepler's rectilinear paths; latter book includes a catalogue of known comets to antiquity
    c. In 1681 Flamsteed proposes from symmetry of trajectory that what appeared to be two separate comets were in fact one
        (1) Comet had approached close to the Sun and then away from it
        (2) First time anyone had suggested anything like this
    d. Newton was at first excited by Flamsteed's proposal, but on reflection decided it was mistaken; three years later he changed his mind

B. The Underdetermination of Theory by Data

1. So many different orbital theories agreeing comparably with Tycho's data is an example of what philosophers have come to call the underdetermination of theory by data
    a. Philosophers now talk of the underdetermination of theory by all possible evidence, but in practice the situation that arises is underdetermination by available evidence
    b. We had earlier seen the underdetermination of the choice between Copernican and Tychonic theory by data consisting of geocentric longitudes and latitudes
    c. Now raising the possibility that those same data cannot select among competing orbital theories
    d. (As we can see retrospectively, the data were also underdetermining Galilean-Huygensian theory at least to the extent that they were compatible as well with inverse-square vertical acceleration)
2. In fact, while all the approaches were achieving more or less the same level of agreement with Tycho's data, none of them was falling entirely within what was thought to be the accuracy of those reduced observations
    a. (See graphs from Wilson in Appendix giving a modern assessment of typical inaccuracies)
    b. Unclear what to make of this so long as questions about the obliquity of the ecliptic and corrections for parallax and atmospheric refraction remained unsettled

        c. Finite speed of light and the seeming motion of the North Star observed by Picard at Uraniborg and Hooke's similar claim about a different star called attention to the potential need for still further corrections to observations
    3. The long-term project of reconstructing observational astronomy telescopically from the ground up by the French Academy and Observatory offered some prospect of clarifying just how inaccurate the existing tables were
        a. But even then the question would remain whether the inaccuracies were coming from inadequate orbital elements or from the fundamental calculation rules used
        b. And, given the open questions about the motions other than those of the planets, the question of which calculational system had claim to truth might still remain open
    4. Kepler had originally proposed that the underlying physics inferred from precise orbital motions would resolve the Copernican vs. Tychonic issue
        a. The multiplicity of calculational systems was now making it look like the physics underlying orbital motions might be needed to resolve questions about the precise orbital motions!
        b. Even the ellipse was open to question insofar as the most that had been shown was that it holds only to high approximation, with vagaries in the motions of Jupiter and Saturn raising questions
        c. And indeed a few years later Cassini proposed his "Cassinoid" as an alternative to it as part of his continuing goal of restoring the equant
    5. The challenge was to establish the relevant physics without in the process having to beg the question of the actual motions
        a. Otherwise might end up with different mathematical systems with different underlying physics, all agreeing with the same body of celestial data
        b. In other words, underdetermination of theory by data all over again

C. Emerging Interest in Inverse-Square Forces
    1. One final development in regard to underlying physics that had a major impact on planetary astronomy, as well as on Newton's *Principia*, requires mention
    2. In 1673 Huygens published his masterpiece *Horologium Oscillatorium*, to which he appended without proof the theorems on centrifugal force
        a. They formed the theoretical foundation for his conical pendulum clock, though he apparently felt that the proofs were too far afield from the main subject
        b. Once published, the theorems were granted by many even though no one but Newton appears to have proved them before the *Principia*
    3. One key theorem says that the centrifugal force in uniform circular motion -- as evidenced by the tension in the string holding the object -- is proportional to the weight of the object, its linear velocity squared, and inversely proportional to the radius

   a. I.e force varies with v^2/r, equivalent to $r*\omega^2$, and hence to r/P^2, where ω is angular velocity and P is the period of rotation
   b. Introduction now of Kepler's third "law" yields by trivial substitution that the centrifugal force involved in uniform circular motion conforming to Kepler's third "law" varies inversely with the square of the radius
   c. (Huygens made no mention of this result, but Newton called attention to a topic related to it in a letter to Oldenburg in 1673 forwarding thanks to Huygens for his complimentary copy of the *Horologium Oscillatorium*; Oldenburg did not forward that letter to Huygens)

4. In England, where Streete's influence (i.e. Horrocks's) had produced stronger interest in Kepler's third "law," many people besides Newton noticed this result
   a. Most notably Wren, who saw it as a basis for proposing that the 'magnetic' gravitational forces preventing the planets from going off in a straight line vary inversely with distance squared
   b. Hooke had put forward a detailed set of hypotheses on 'magnetic' gravitation and celestial motion at the end of his monograph in 1674 announcing stellar motion (see Appendix)
   c. By 1679 he was in a position to add an inverse-square variation
5. Hooke's gravitational hypothesis therefore now took the form that the planets are deflected from straight line motion by an inverse-square gravity-like force aimed toward a central body
   a. The question Hooke then posed is, what trajectory will a body follow if it enters into the gravity-like influence of some other body with a given velocity and direction
   b. His conjecture was that it is some curve approximating an ellipse, but different -- "an Elleptueid" -- though still with a circle as a limiting case
6. Interest in this hypothesis by Wren and Hooke stimulated a good deal of discussion in London from the mid 1670's on
   a. Wren had a discussion with Newton on forces governing curvilinear planetary motion during a visit to Cambridge in 1677
   b. Hooke put his problem to Newton in a famous series of letters at the end of 1679, contending that Newton's superior skill in mathematics would enable him to determine the answer
   c. These letters initiated Newton's work that led to his *Principia*

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