The Physical Astronomy of Levi ben Gerson

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Levi ben Gerson (1288–1344) was a medieval astronomer who responded in an unusual way to the Ptolemaic tradition. He significantly modified Ptolemy's lunar and planetary theories, in part by appealing to physical reasoning. Moreover, he depended on his own observations, with instruments he invented, rather than on observations he found in literary sources. As a result of his close attention to the variation in apparent planetary sizes, a subject entirely absent from the Almagest, *he discovered a new phenomenon of Mars and noticed a serious flaw in Ptolemy's treatment of the Moon.*

Levi ben Gerson (1288–1344), also known as Gersonides, lived in Orange, a town in southern France not far from Avignon, which was then the papal residence, and is best known for his Biblical commentaries and his philosophical work, *The Wars of the Lord* (see Touati 1973). But his contributions to science and mathematics were considerable, and his approach to astronomy was outstanding in the context of medieval scholarship in this discipline (see Freudenthal 1992). His *Astronomy* constitutes book 5, part 1, of his *Wars of the Lord*, but it is as long as the rest of the treatise altogether and is preserved in separate manuscripts (Goldstein 1974, pp. 74ff; see also Appendix). This astronomical text was translated from Hebrew into Latin by Peter of Alexandria (a town in the north of Italy), who also collaborated with Levi—nothing is known about this Peter except that he was a member of the Order of Hermit Brothers of St. Augustine, but it can be said on the basis of the

I have greatly benefited from discussions with J. L. Mancha concerning the *Astronomy* of Levi ben Gerson, and thank him for comparing some of the passages in the Hebrew version with the Latin manuscripts.

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translation that he had a clear understanding of Levi's *Astronomy* (see Mancha 1992*b*).

Let me begin with an evaluation of Levi (here called by his Latin name, Leo) by the fifteenth-century humanist George of Trebizond:

> In each discipline there is one ancient scholar of distinction whom we should follow as, for example, Aristotle for laying bare the secrets of nature, Euclid for the elements of geometry, Homer for Greek poetry, Virgil for Latin poetry, Demosthenes and Cicero for oratory, and surely one person for astronomy, namely Ptolemy. Now among the later commentators, some have made [new] instruments for themselves, as we learn from their writings, that have led them—and those who follow them—into great errors. Indeed, a certain Jew, Leo, describes the positions of both the planets and fixed stars by means of his own instruments. . . . But Leo and others descended to inept demonstrations trying to save the appearances . . . seeking glory most basely by false and deceitful detraction of divine men. (Goldstein 1985, p. 11)

It is striking that Levi is given such prominence here, but the point is that Levi departed from Ptolemy, insisting that he could do better than Ptolemy, and, from a humanist perspective, this was blameworthy. For George, as for most other scholars in the premodern age, the greater part of truth was already known and available in books venerated by tradition. The first response is to say that George was right—Levi did think he had done better than Ptolemy (and in many ways he had). But Levi's attitude to Ptolemy was not as simple as George imagined, and it is this attitude that I wish to explore.

What distinguishes Levi from the vast majority of medieval mathematical astronomers is his insistence that mathematical astronomy cannot be treated apart from physical considerations. In chapter 1 he writes:

> We found that [previous] investigators, namely some of the mathematicians, decided it would be sufficient to determine a model from which there would follow that which is in close agreement with what is perceived by the senses, but they did not attempt to explain the model according to true principles. Indeed the model they produced contains so many doubtful matters that it is altogether impossible that it be as they assumed. . . . In its perfection this investigation belongs to [several] sciences: to mathematics because of the geometric proofs, and to physics and metaphysics because of the physical and philosophical proofs. (Goldstein 1985, pp. 22–23, 304–305)

In chapter 2, he adds:

> [Astronomy] is of value for itself . . . for the subject of this inquiry, celestial body, is the most noble of all natural bodies . . . and it is most instructive for the other sciences, particularly physics and metaphysics. . . . [I]f we could perfect this investigation fully without leaving any remaining doubt or confusion, it would be infinitely precious and noble, and therefore we ought to elaborate on it, perfecting it in every way possible. (Goldstein 1985, pp. 24–26)

As part of this project, Levi devised a new astronomical instrument, later known as the *cross-staff*, that was easy to construct and that produced reliable and accurate observations. Between 1321 and 1340, he observed ten eclipses, solar and lunar positions at other times, and a considerable number of planetary positions (Goldstein 1979, 1988). This is a substantial list and sets Levi apart from other medieval astronomers who recorded few observations of their own. Moreover, Levi constructed a new lunar theory based on his own observations of lunar positions and claimed that it produced closer agreement with his observations than did Ptolemy's theory (Goldstein 1974, pp. 53–74). While this may seem like a "normal" thing for a scientist to do, in a medieval context, this was by no means the case.

Levi's emphasis on physical considerations also affected his attitude toward the ancients. As he writes in chapter 46 of his *Astronomy:*

> In this chapter we direct the community of scholars not to hasten to dissent from the views of the ancients except after much investigation and careful scrutiny. . . . We first tried to solve some of the difficulties raised against [Ptolemy] by our predecessors with respect to his postulates concerning eccentric spheres and epicycles, seeking to find observational evidence to establish his hypotheses. Indeed, the reason for which we invented the aforementioned instrument [the cross-staff] was to determine the amount of the eccentricity. When we investigated this matter for the Moon and found that its model could not possibly be as Ptolemy postulated, we took pains to investigate alternative possibilities for the models of the celestial bodies until we discovered [a model] according to which the motions [of these bodies] conform to observational evidence. (Goldstein 1988, p. 385)

Of special significance in this passage is that Levi appealed to the phenomena as the ultimate criteria for determining the truth of a theory and that he depended primarily on the observational evidence that he had gathered for himself rather than on the observational evidence preserved in texts. As we shall see, the phenomena that Levi considered were not restricted to positional data. This direct appeal to phenomena is surprisingly rare in the Middle Ages and virtually without parallel among Jewish, Christian, and Muslim scholars. As Kepler remarks in his *Astronomia Nova* (1609, chapter 14), "Copernicus, ignorant of his own riches, ever took it upon himself to express Ptolemy, not the nature of things, to which, nonetheless, he of all men came closest" (Donahue 1992, p. 232). What Kepler says about Copernicus, that he was responding to Ptolemy rather than to nature, can be said of virtually all medieval astronomers.

Let us now turn to some details of Levi's contributions to astronomy. It is immediately striking that he does not follow the standard arrangement of material in a medieval book on astronomy, and this reflects his different motivation and methodology. After a preliminary discussion of trigonometry, mainly of the construction of a table for the sine function, Levi turns to observational instruments, breaking with the standard order of subjects in a medieval astronomical treatise. We are not given any precise clues for this break with tradition, and it is worth pausing to understand it. Levi says repeatedly that astronomical theory must be based on observations—so far nothing unusual—but he is concerned about observations of two kinds: (1) the positions of the planets (including the Sun and the Moon), which he shared with other medieval astronomers, and (2) the physical attributes of the planets, which were given much less consideration by other medieval astronomers. His models were intended to satisfy observations of both kinds, and in this he parted company with his contemporaries.

The underlying issue relates to the interpretation of the Ptolemaic planetary models and the criticisms of them by astronomers before Levi, notably by al-Biṭrūjī (ca. 1200). In fact, after Ptolemy, the most prominent name in Levi's *Astronomy* is that of al-Biṭrūjī, despite the fact that Levi argues vigorously against his views. Al- Biṭrūjī revived the notion of homocentric planetary models and tried to harmonize that notion with the models of Ptolemy by envisaging Ptolemy's models on the surface of a sphere near the pole of the sphere with some way for the planet near the ecliptic to be guided by the motion of its pole. Levi was aware of many technical inadequacies of al-Biṭrūjī's scheme, but there was another matter that needed to be investigated (Goldstein 1971, 1:40–43). Al-Biṭrūjī claimed that Venus always maintained the same size (i.e., brightness) and that it did not display phases as would be required if its distance varied and if it received its light from the Sun:

> Those whose pretext for opposing the ancients' opinion is that they have not seen Venus or Mercury obscure the Sun under any circumstances, as the Moon does during solar eclipses, would have a sound reason if these two planets were illuminated by another body, as the Moon is illuminated by the Sun. The proof that they [Venus and Mercury] are not illuminated by the Sun and do not receive light from any other source is that their luminosity seems the same whether they are close to the Sun or far from it. But if their light came from the Sun, like the Moon's, Mercury would always appear as a crescent, because it is never very distant from the Sun; and similarly for Venus most of the time. If someone were to say that the distance between them in height makes the surface facing us always appear luminous, it still remains that some of the surface would be without light, and [the planet] would appear elongated.
>
> Moreover, if the Sun were below both of them and they were receiving their light from it, then the higher would receive light from the lower, so that the lower would then be more perfect. This is a repugnant notion and remote from the principles by which things exist. Since they do not obscure the light of the Sun, despite their lying below it and above us, either the rays of the Sun pass through them on account of their translucence, or their light replaces the part of the Sun which they obscure. Since this is the case, their argument is not sound. (Goldstein 1971, 1:125)

This argument was used as evidence to support the claim that the planets stayed at the same distance from Earth and that neither the epicyclic model nor the eccentric model was appropriate. For us, it is noteworthy that this argument depends on physical considerations rather than on positional data. Levi's response was to say that the epicyclic model is indeed inappropriate (but for a different reason) and that the eccentric model conforms to the observational facts, for, he argues in chapter 44 of his *Astronomy,*

> It is clear from the observational data with respect to the Sun and Venus that they are sometimes seen greater and sometimes smaller . . . from which it follows that there are eccentric orbs, and this contradicts what [al-Biṭrūjī] assumed, for he tried to invent models without eccentric orbs. (MS P 84a:20ff)[1]

This follows from one of Levi's methodological principles, stated in chapter 42:

1. Sigla for MSS are identified at the beginning of the appendix.

> [E]ccentric spheres are derived directly from observations about which there is no doubt. Thus, they cannot be refuted by philosophical argument. True beliefs must correspond to reality; reality, however, need not correspond to what one prefers to believe. (MS P 75a)

So, in a fundamental way, Levi's arguments depended strongly on finding a variation in planetary sizes and taking that variation to be significant. To be sure, Ptolemy had assigned apparent sizes to the planets in his *Planetary Hypotheses* (Goldstein 1967, p. 8), but he did not discuss the variation in their apparent sizes, nor did any of these data serve as the basis for his models. One might say that, for Ptolemy, planetary sizes were an afterthought, a consequence of his theory, but not part of the data for its construction. On the other hand, for Levi, planetary sizes are equally to be considered as part of the data that an astronomical theory is to explain. Levi is much more precise:

> It also became clear to us on the basis of observation that the apparent size of the diameter of Venus is greater at greatest elongation from the Sun than at 0° or 180° of anomaly [superior and inferior conjunction]; on the other hand, we did not observe it to be greater at 180° of anomaly than at 0° of anomaly. All this is at variance with what follows from Ptolemy's model, for according to it the diameter of Venus should appear to be greater at 180° of anomaly than at 0° of anomaly by more than 6 times. We also observed diligently seeking to find the apparent size of Venus at each time relative to the apparent size of the fixed stars of first or second magnitude, and in general to determine the variations in the apparent sizes of the planets. For Venus we could determine this by its appearance with the Sun during the day, because when Venus is at its greatest elongation from the Sun, you can see it in the afternoon sunlight. But when it is closer than 20°, it cannot be seen in sunlight. You can also verify this by observing the size of the diameter of Venus in these two places and by observing it at 0° and 180° of anomaly. Another way to verify this is by noting its rays that enter the window of the instrument that we described earlier [i.e., the camera obscura], and this should be done when the light of the Moon is not shining and it is pitch dark. (Levi, *Astronomy,* chapter 17; Goldstein 1985, p. 105; cf. Goldstein 1996*b*)

In other words, Levi failed to find any difference in the apparent size of Venus near inferior and superior conjunctions (180° and 0° of anomaly), while claiming that its greatest apparent size was at greatest elongation (about 47° from the Sun). According to modern theory, maximum brightness occurs about thirty-five days after inferior conjunction at an elongation of about 39° and greatest elongation about thirty-five days after that (give or take a few days). So, one might argue that Levi is just as far off in the other direction as some medieval astronomers (e.g., Bernard of Verdun, ca. 1300; see Goldstein 1996*b*, p. 5) who took maximum size to take place at inferior conjunction. Given an epicyclic model, Venus would be much closer to Earth at 180° of anomaly, and hence one would expect it to appear larger in size. But, in fact, the phases of Venus very nearly counteract the variation in distance, and the phases of Venus were not observed until Galileo's observations with a telescope in the early seventeenth century. Thus, those who claimed maximum size took place at inferior conjunction seem to have depended on Ptolemaic theory, recast as an observation, whereas Levi gives the impression of reporting what he had observed. There was no previous significance to an elongation of about 39°, and he was equally unaware of any, but greatest elongation was a known phenomenon of Venus, and it would have been natural to observe Venus in that circumstance.

Levi also measured the apparent sizes of the Sun at apogee and perigee with a camera obscura on specified dates in 1334, and his results were that the apparent diameter of the Sun varied from 0;27,51° to 0;30°, from which he derived a solar eccentricity of 2;14, where the radius of the deferent is 60 (Levi, *Astronomy,* chapter 56; Mancha 1992*a*, pp. 292ff). His derivation of the solar eccentricity from observations of the apparent solar diameter (rather than from the variation in the length of the seasons, on which Ptolemy had based his argument) convinced Levi of the reality of the Sun's variation in distance from Earth. Note that Ptolemy's value for the solar eccentricity was 2;30, which Levi reduced to 2;14.

So what gave Levi such confidence in his observations of planetary sizes? For this purpose, he appeals to an observational instrument, the camera obscura, that was unavailable to Ptolemy and that was described in detail by Ibn al-Haytham (d. 1039), a Muslim scholar noted for his contributions to astronomy and optics. It is not clear how Levi learned of this instrument—the text of Ibn al-Haytham was not translated into Hebrew, as far as I know. It is quite possible that Levi learned of it from Latin sources, for it was discussed by a number of scholars in the thirteenth century, notably Roger Bacon and William of St. Cloud (Mancha 1992*a*). As usual, Levi did not cite his sources, but his analysis of the way this instrument worked far surpassed that of any medieval Latin author, and it was not until Kepler that a Christian author

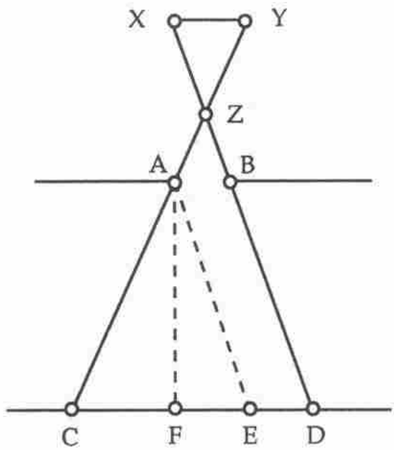

Figure 1. Illustration of the reason for subtracting the diameter of the aperture of the camera obscura from the size of the image (see figs. 2 and 3). The luminous object is XY, the aperture is AB, and the image on the screen is CD. The angular size of XY is angle XZY which is equal to angle CZD, and in turn equal to angle CAE. But tan (CAF) = CF/AF, where CF is half of CE, and CE = CD − ED. Since XY is assumed to be very far away, the rays from any point on it arrive at AB along parallel lines. Hence AB = ED, and the angular size of XY is twice angle CAF.

reached his level of understanding. Levi first describes this instrument in chapter 5 of his *Astronomy,* assuming that the aperture has the size of a point, but in chapter 9 he indicates how to compensate for an aperture of finite size—the size of the aperture is to be subtracted from the size of the image (Goldstein 1985, pp. 140–43) (fig. 1). Without this correction, the values produced by observations with this instrument are not valid quantitatively and even lead to qualitative problems that bothered astronomers as late as the time of Tycho Brahe at the end of the sixteenth century. The camera obscura is useful for observing solar eclipses, but without this correction the size of the Moon seems to be much smaller than it should be, based on observing it at times other than eclipses. As Tycho wrote in 1598, "Truly it must be acknowledged that the Moon during a solar eclipse does not appear to be the same size as it appears at other times during full moons when it is equally far away; but it appears as if it were constricted by about ⅕th, by causes to be disclosed elsewhere" (Straker 1981, p. 278). The correct analysis of the camera obscura is one of the main themes of Kepler's

Paralipomena ad Vitellionem of 1604; Kepler knew about some of Levi's work but not his discussion of this instrument.

Levi's interest in planetary sizes led him to notice a phenomenon of Mars that had previously escaped attention, namely, that Mars varies in brightness at successive oppositions to the Sun. Levi's account is brief, and he seems to have been surprised by his observations, judging by the ad hoc explanations he offers:

> When Mars was retrograde in Leo, we found its size perceptibly greater than that of Saturn, and similarly in Capricorn, but there its size appeared greater than it was in Leo. However, when it was retrograde in Scorpio its size did not seem greater than that of Saturn. . . . You should know that we ascribed what we first discovered concerning Mars in Leo to thin clouds through which it was seen at that time. We did this because during its retrogradation in Scorpio its size was not found to be augmented, and because we did not see this increment in the proper order in which it should take place were it due to its closeness to us. You cannot argue that after a short time it [Mars] can appear to be smaller in size than what follows from the appropriate ratio, for we observed its size while retrograde in Capricorn where we found it somewhat augmented as compared to the size that we found it in Leo. We also ascribed the absence of an increment in the size of Mars in Scorpio to the thickness of vapors through which it was seen at that time. We then understood that this phenomenon took place because of the comet that continued to appear for more than 3 months; that vapor came into being under Scorpio and it was drawn from there to somewhat below the north pole: there it burst into flame and it perished in Scorpio. (Levi, *Astronomy,* chapter 17; Goldstein 1985, p. 106; cf. Goldstein 1996*a*)

The comet in question can be dated to 1337 on the basis of observations in China and in Europe, and this is consistent with dating the opposition of Mars in Scorpio to April/May 1337 (see also Mancha 1992*b*, p. 32). (Note that Mars is in opposition to the Sun every two years or so.) I take Levi's appeal to comet formation, clouds, and the thickness of vapors to be part of his explanation for anomalous phenomena rather than as part of the observational reports. The theory of comet formation to which Levi alludes was commonplace in the Middle Ages, but to appeal to it in an astronomical context is unprecedented. According to modern theory, the values for the brightness of Mars, M, measured in stellar magnitude (the higher the negative value, the brighter the object), at its closest approach to Earth near these three oppositions were: February 8, 1333 (Mars in Leo), $M = -1.3$; May 3, 1337 (Mars in Scorpio), $M = -2.0$; and July 1, 1339 (Mars in Capricorn), $M = -2.8$. In more familiar terms, Mars was four times brighter in 1339 than in 1333. No other dates during Levi's lifetime are possible for oppositions of Mars in these zodiacal signs. This variation in brightness is not surprising, given the modern view that the brightness of Mars at opposition depends on its position relative to its perihelion (where Mars is closest to the Sun). When an opposition takes place in February, Mars is near aphelion (farthest from the Sun) and hence dimmest, whereas when an opposition takes place in August it is near perihelion and hence brightest. Levi's report that Mars at opposition in 1337 was not as large (i.e., bright) as Saturn is a puzzle for which I have no explanation. I also have no way to explain how Levi could remember the brightness of Mars in 1333 in order to compare it with its brightness in 1339, but he correctly reports the facts and he was not guided by a commonly accepted theory.

Let us now consider the case of the Moon. In chapter 71 of his *Astronomy,* Levi presented a new model for the Moon that was to account for positions of the Moon when it was in conjunction with the Sun or at a distance of 90°, 180°, and 270° from the Sun (these distances are called "elongations"), and in these cases his data were not much different from those of Ptolemy. But at multiples of 45° of elongation (known as the "octant points"), he found Ptolemy's theory to be deficient in accounting for the lunar positions, whereas his new theory was much more successful in accounting for them. Rather than analyzing his new model, let us pay close attention to what Levi calls his "additional proofs" in favor of his new model, all of which involve physical considerations: (1) the variation in lunar parallax; (2) the "spot" on the Moon; (3) the variation in the size of the image of the Moon cast on a screen; and (4) measurements of the apparent diameter of the Moon. His conclusion is that "the Moon has no epicycle or eccentric orb in the way that Ptolemy assumed, as will become clear and, once that has been clarified, it will be clear from the force of our previous remarks that the motion of the Moon can only take place in the way we have set it" (Levi, *Astronomy,* chapter 72; see Appendix).

The argument from parallax depends on the fact that parallax is a measure of the distance from the Moon to Earth—the greater the angle of parallax, the closer the Moon is to Earth. Ptolemy allows the Moon to vary in distance such that the maximum is about double its minimum distance, which implies that its apparent size varies in the same ratio, i.e., at closest approach the Moon's apparent diameter should appear to be twice as great as at its farthest distance from Earth, quite contrary to the appearances. Yet, no one before Levi called attention to this discrepancy between the phenomena and Ptolemaic lunar theory. Levi's lunar theory drastically reduces the difference in lunar parallax at quadrature and syzygy (conjunction and opposition) and hence the lunar distances from Earth under these conditions (see Appendix; Levi's *Astronomy,* chapter 73). After him, Ibn al-Shāṭir (Damascus, d. ca. 1375) and Regiomontanus (d. 1476) both mentioned this flaw in Ptolemy's lunar theory, and Ibn al-Shāṭir produced a new theory in which this flaw was eliminated. It has now been shown that Copernicus' lunar theory in the sixteenth century is virtually identical to that of his predecessor, Ibn al-Shāṭir (see Goldstein 1972, p. 46; Swerdlow 1973, p. 456; 1990, pp. 174, 190).

The "spot" on the Moon is usually known as the "man in the Moon." Levi's argument is that, if the Moon had an epicycle, we would see both sides of it, but, in fact, we always see the same side. Levi insists that the "man in the Moon" is a reality and not an appearance or an optical illusion. This passage is essential for Levi's argument against the epicyclic model—given the principle of the uniformity of nature, if an epicyclic model is inappropriate for the Moon, it is inappropriate for any planet—but there is no comparable argument against the eccentric model. Consider the following figure (fig. 2)—the eccentric and epicyclic models are geometrically equivalent, but they have different physical consequences.

Physical reasoning also appears in Levi's account of cosmic distances. He seems to have taken a hint from Maimonides (*Guide* ii.24) concerning an interplanetary celestial body, but he may have depended directly on Ptolemy's *Planetary Hypotheses* (book ii) (for Levi's ownership of a copy, see Weil 1991, p. 108), where there is a layer of unspecified thickness between adjacent planetary spheres. Maimonides reports the following from a lost book by Thābit ibn Qurra (d. 901):

> [I]t follows necessarily that when the higher [planetary] sphere is in motion it must move the sphere beneath it with the same motion and around its own center. Now we do not find that this is so. . . . Hence necessity requires the belief that between every two [planetary] spheres there are bodies other than those of the spheres. . . . Thābit has explained this in a treatise of his and has

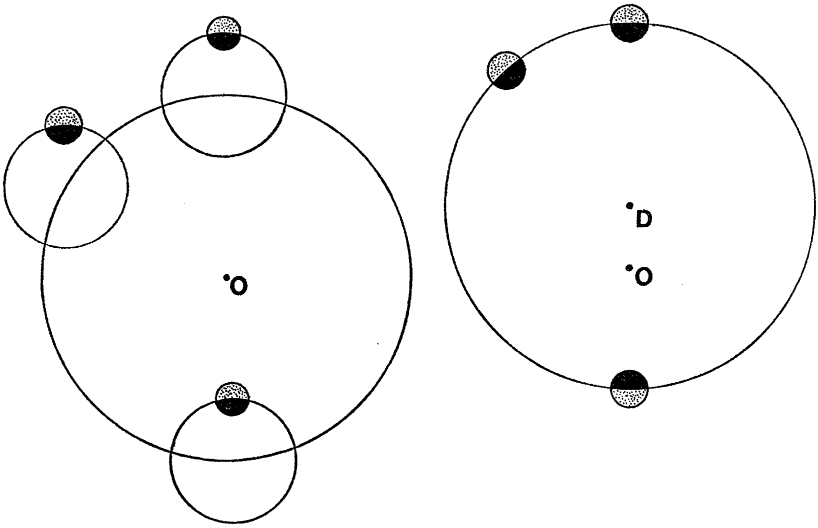

Figure 2. Illustration of the apparent disk of the Moon according to the epicyclic and eccentric models under the assumption that the Moon is fixed to its orb. The observer is at O, and the center of the eccentric circle is D. Note that the Sun may shine on both the solid and the shaded portions of the Moon. Ptolemy proved that these two models are geometrically equivalent. Levi argues, however, that they are not physically equivalent: in the epicyclic model the observer would see both sides of the Moon, but in the eccentric model this would not happen.

> demonstrated what we said, namely, that there must be the body of a sphere between every two spheres." (Pines 1963, pp. 324–25)

Levi modifies Ptolemy's nesting hypothesis by introducing a fluid between adjacent planetary spheres, and he then goes on to specify the thickness of those interplanetary layers. For Levi this fluid, called "the body that does not retain its shape," is left over from the creation of the world. Each planetary sphere (or spherical shell bounded by two concentric spherical surfaces) is composed of a set of orbs. The lowest orb moves with the daily rotation and the highest part of the sphere moves with a velocity equal to the algebraic sum of 360° to the west, and the planet's minimum daily velocity to the east. Between each pair of adjacent planetary spheres, the interplanetary fluid makes sure that none of the motion of one planet interferes with that of another: there are no empty spaces in the cosmos. This fluid obeys certain laws of mechanics that allow Levi to compute its thickness: there is no relative motion at the convex (or concave) surface of the planet's sphere, i.e., the fluid there moves at the same rate as the spherical surface. But as the radial distance from that surface increases, the motion of the fluid decreases uniformly until it reaches zero at a certain distance from the highest (or lowest) orb of the planet (fig. 3). (Although Levi does not mention it, this case is the reverse of water flowing in a river: there is no relative motion of the water at the banks of the river; the motion increases with the distance from the banks, and the swiftest part of the river is in the middle.) For Levi, the layer of zero motion is assumed to have zero thickness, but this takes place with respect to the superior concave surface and the inferior convex surface; hence, the motion of one planet has no effect on the adjacent planet. I omit the details of the computations, but the results are extraordinary. For Ptolemy, the fixed stars are at a distance of 20,000 terrestrial radii from Earth, whereas Levi considers them (under one set of assumptions) to be at a distance of $157 \cdot 10^{12}$ terrestrial radii, a truly astronomical distance and well beyond anything proposed in a serious medieval astronomical work (Goldstein 1986*a*).

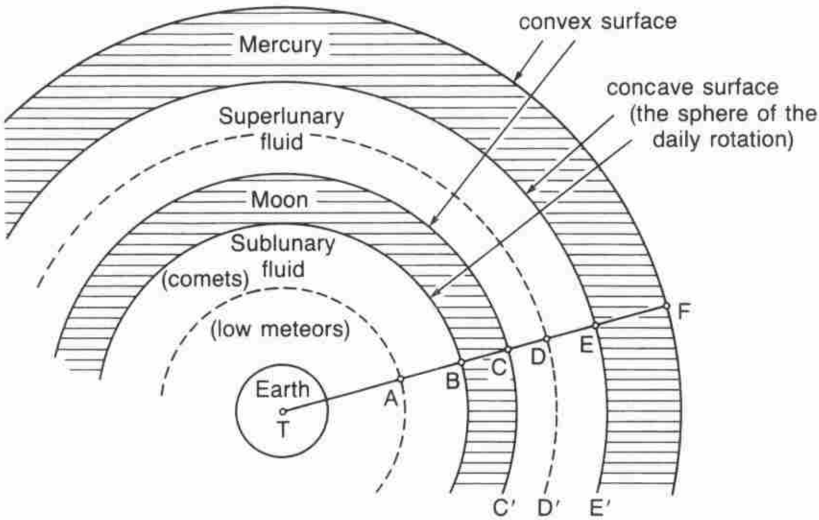

Figure 3. Illustration of the inner part of Levi's cosmos. The superlunary fluid layer is shown between the convex surface of the lunar orb, C′C, and the concave surface of Mercury's orb, E′E. The layer in which no motion takes place is labeled D′D.

The essence of this derivation of planetary distances depends on Levi's analysis of the motion of the celestial fluid by means of terrestrial analogies. As he puts it:

> [T]he strength of the impulse depends upon the strength of the motion. This is clarified from the throwing of stones, for the greater the force, the greater is the amount of the medium that receives the form of the impulse. But when the [force] is weak, its amount is diminished until it happens, due to the weakness of the impulse, that [the stone] will fall immediately upon its separation from the hand of the thrower. From this it is clear that the daily orbs move the celestial fluids below them with differing amounts. (Levi, *Astronomy,* chapter 130; Goldstein 1986*a*, pp. 286–87)

Levi's analysis of fluid motion is quite reasonable, from the point of view of fluid dynamics, and unprecedented as far as I know, but his application of it to a celestial fluid is truly remarkable, for he is failing to adhere to a rigid distinction between the celestial and terrestrial realms that was virtually unchallenged by medieval philosophers. The reason Levi can appeal to this analogy is that he believes there are universal physical principles that apply to both realms and that they can be discovered experimentally. To be sure, Levi recognizes differences between these realms, e.g., light and heavy do not apply to the celestial realm (see Glasner 1996). In chapter 130 of his *Astronomy,* Levi tried to determine the parallax of a comet that ought to have displayed some parallax because it was assumed to be closer to Earth than the Moon, and the Moon has a measurable parallax. The closer an object is to Earth, the greater should be its parallax. Levi interpreted his failure to find any parallax for the comet as an indication that the celestial fluid has different properties from the fluid below the Moon, but he did not take this to mean that the sublunary fluid had no properties in common with the celestial fluid.

The metaphysical or theological warrant for this use of physical reasoning is stated in Levi's *Wars of the Lord* (vi.2.8): "The Torah gave us a great benefit when it made known to us that there is a unity of a sort of the lower matter and the heavenly body. The reason for this is that if there were not a unity between them, it would necessarily follow that there would be two divinities" (Levi ben Gerson 1866, p. 430; Staub 1982, pp. 263–64). This underlying unity of the sub- and superlunary realms is in opposition to (among others) Maimonides (*Guide,* ii.26): "the matter of the heavens is altogether different from that of the earth" (Pines 1963, p. 331), following the standard Aristotelian distinction. But Levi understands the discussion of the separation of the "waters" in Genesis, chapter 1, as support for his views about the celestial and sublunary fluids (cf. Freudenthal 1986).

On a methodological level, Levi accepts the hierarchy of disciplines—astronomy, physics, metaphysics—that was a common feature of Aristotelian science. Metaphysics is the source for the principles that govern physics, and physics is the source for the principles that govern astronomy. This is what Levi calls the "a priori" method. But he also accepts the "a posteriori" method, according to which the data at one level of the hierarchy can disconfirm the principles at a higher level. In chapter 19 of his *Astronomy*, he says, "We decided to introduce these results based on our observations and those of others here so that we may use them as assumptions in our proofs, for in this science you have to rely on sensory perception to find the true models for the celestial orbs, and in this way it is similar to the Science of Physics in which proofs are taken a posteriori" (Goldstein 1985, pp. 113, 230). What is of particular interest is that Levi's new view of astronomy depended on a (partial) breakdown of the distinction between the sub- and superlunary realms, much less thorough in the case of Levi than in the case of Kepler, for Levi still accepted the physical reality of celestial orbs. The conventional account of the breakdown of this distinction places greatest emphasis on Brahe's observations of the comet of 1577, leading him to conclude that there are no hard impenetrable planetary spheres (Goldstein and Barker 1995). That story is not so simple, but for Kepler there are certainly no longer any such spheres in the heavens. Kepler's goal was to find a causal account of planetary motion taking place in a celestial fluid, based on physical laws that conformed to his theological commitments (see, e.g., Stephenson 1987). Levi, to be sure, did not see his task in the same way, but for him the principal issue was also theological: the demands of monotheism led him to question the astronomical tradition and gave physical arguments legitimacy in astronomy. He then applied physical arguments to the lunar and planetary models as well as to the celestial fluid.

Levi's insights into the relationship between astronomy and physics were not appreciated by his contemporaries or immediate successors, even by those who were aware of his contributions to mathematical astronomy, but they show that a medieval scholar could well transcend the legacy of antiquity. Levi was confident that he had made significant improvements on Ptolemy's astronomical theories and that he could account for the positional data as well as the physical phenomena of the planets.

Appendix

The *Astronomy* of Levi ben Gerson

The Hebrew text of the *Astronomy* of Levi ben Gerson consists of 136 chapters. I have previously published the Hebrew text of chapters 1–20 (Goldstein 1985) as well as the following translations: chapters 1–20 (Goldstein 1985); chapter 29 (Goldstein 1986*b*); chapters 46, 109, 113, 117, and 122 (Goldstein 1988); chapter 61 (Goldstein 1975); chapter 71, in part (Goldstein 1974); chapters 80 and 100 (Goldstein 1979); and chapters 130 and 131 (Goldstein 1986*b*). The translation of chapters 72–75 that follows in section I is based on MSS P (ff. 143b:3 to 146b:21), and Q (ff. 111a:−5 to 114a:3), where P is Paris, Bibliothèque nationale de France, heb. 724; and Q is Paris, Bibliothèque nationale de France, heb. 725 (negative line numbers are counted from the bottom of the page). Sentence numbers, in square brackets, and paragraphing have been added to the translation for ease of reading. The commentary on these chapters is in section II.

I. Translation: Chapters 72–75

Chapter 72

[1] Now that we have established this model with respect to the Moon's apparent motion, we shall bring additional proofs for its truth with respect to other considerations, and there are 4 of them.

[2] The first is with respect to the apparent lunar parallax at quadratures and oppositions, at 0° in anomaly and 180° in anomaly. [3] We have found that its amount at oppositions is about 0;55° [with Q; P reads: 0;5°] and at quadratures it is about 0;57°, to a close approximation. [4] We have repeated this observation many times, only to find that the parallax of the Moon was greater at quadratures. [5] If someone were to say that this [increment at quadratures] may be due to approximation in the observations, [we would point out that] it is curious that this approximation is always on the incremental side. [6] But this shows, without doubt, that the Moon is closer to us at quadratures than at oppositions. [7] We have not seen, up to this time, any difference in the amount of parallax when the Moon is at 0° in anomaly as compared with its parallax at 180° in anomaly. [8] We should investigate this matter further because, from it, we may determine whether the eccenters of the motion in anomaly are different or whether they

are one and the same, as we have argued [*lit.* agreed]; and if they are different, what the distance is between them.

[9] The second is with respect to the spot [Heb.: *ṣel;* Lat.: *macula*] seen on the Moon which is a reality (*amitut 'inyan*) rather than an appearance (*re'iyya*), as we explained above.

[10] The third is with respect to the ray of the apparent Moon [that passes] through the window of the instrument of the Staff at quadratures and oppositions, and at diverse places in anomaly.

[11] The fourth is with respect to the apparent size of the diameter of the Moon at quadratures, oppositions, and at diverse places in anomaly, for it does not appear to vary noticeably. [12] We will determine the truth of this with the instrument that we invented for this investigation, i.e., the Staff.

[13] All these imply that the Moon has no epicycle or eccentric orb in the way that Ptolemy assumed, as will become clear and, once that has been clarified, it will be clear from the force of our previous remarks that the motion of the Moon can only take place in the way we have set it. This explanation [shall be the subject of] the following chapters. [End chapter 72]

Chapter 73

[1] Here we present the proof taken from parallax, and it is as follows. [2] If the Moon were closer to us at quadrature than at opposition in the way that follows from Ptolemy's model, the apparent parallax at quadrature would be much greater than at opposition. [3] But, to repeat, we found that the parallax at quadrature was only slightly greater than at opposition, and at its maximum it is greater by only about 0;2°. [4] It follows that the Moon is only a little closer at quadrature than at opposition. [5] By this very argument, it is evident that the Moon is not sensibly closer to us at 180° of anomaly than at 0° in anomaly. [6] You ought to know that if the matter were in accordance with Ptolemy's model, the apparent parallax at quadrature and 180° of anomaly would be nearly twice that [amount] at opposition and 0° of anomaly, and this is clear to anyone who examines his model.

[7] I will explain that the parallax at quadrature is only slightly greater than at opposition, and that it is not seen to be greater at 180° of lunar anomaly than elsewhere. [8] Let us observe the Moon with a fixed star close to the ecliptic and 20° [with Q; P reads 2°] or more away from it [i.e., the Moon] with the instrument that we invented to determine accurately the position [of the Moon relative to] the star by observation, and we should determine as accurately as possible the time of the observation. [9] Then about 4 or 5 hours later we again

observe the Moon with the same fixed star as before, and from this we can determine the apparent motion of the Moon between the two observations and the time interval between them with the greatest possible accuracy. [10] Then we compute the true lunar motion between the two observations according to either Ptolemy's model or our own model, for there will be no sensible difference between them in such a short time interval. [11] The difference between the true motion of the Moon and its apparent motion in this interval is the excess of the parallax at one observation over the parallax at the other observation, if both of them were to be added to, or subtracted from, the true position of the Moon. [12] But [take] the sum of the two parallaxes if at the first observation the parallax was additive, and at the second observation it was subtractive. [13] We examined this problem when the Moon was at quadrature [and when it was at opposition]: we computed the parallax according to its amount found at the times of eclipses when its maximum is about 0;55°, and we found that what is between the two motions agrees closely with this computation. [14] But the additional amount of parallax at quadrature over its amount at opposition was only 0;2°, i.e., its maximum was 0;57°. [15] Thus our results are quite different from what follows from Ptolemy's model, and we repeated this observation many times. [16] We have computed the parallax in all our lunar observations in this way. [17] The best results are found when there is a large excess in the parallax of one observation over the parallax of the other, or when the sum of the parallaxes is large in those cases where one is additive and the other substractive.

[18] To illustrate this: consider the Moon at Cancer 0°; the first observation takes place 2 equal hours before it crosses the meridian, and the second observation 3 equal hours after it crosses the meridian, for then the diminution of the Moon's apparent motion in these 5 hours would be about 0;45° at the horizon of Orange if we set the maximum parallax equal to 0;55° [i.e., at opposition]. [19] Then it is quite clear that the lunar motion in anomaly and longitude is not according to Ptolemy's model with an eccentric and an epicycle, for in that case the excess of the parallax in this example at quadrature would be much greater than at opposition, and likewise greater at 180° of anomaly than at 0° of anomaly, and analogously for other parallaxes, whether all of them or some of them, when the Moon is at mean distances in these respects. [20] The excess between these two [extremes under the condition specified for] the observation that we mentioned should be quite sensible.

[21] Similarly, consider the Moon at Cancer 0° in quadrature; the first observation takes place on the meridian and the second before or

after it by 4 or more hours, according to what is possible, for when it is so, one should see a diminution in the motion of the Moon according to our assumptions of 0;32°, but according to Ptolemy it would exceed 0;55°—setting the Moon at 180° of anomaly. [22] When we examined this in this way, we did find not the parallax seen in the observation of the Moon at quadrature greater than the amount that follows from assuming the Moon at greatest distance except by the amount that we mentioned, namely a maximum of 0;2°.

[23] We determined this from our observation of the Moon on 20 June of the aforementioned year [1333][2] 8;13 hours after mean noon, and we found the Moon at Libra 2;8,35°, as we have already mentioned. [24] Then 2;19 equal hours later, we again observed the Moon with the same star as in the first observation. [25] The apparent motion of the Moon between these two observations was shorter than the true motion by the appropriate amount according to what we mentioned. [26] In the same way we again observed the Moon on the night of 21 June, and 2;44 hours passed between the two observations. [27] We found the matter in very close [agreement], and we were as careful as possible, for otherwise we would have had no way to determine the true position of the Moon from the observation. [28] You may verify this by observing the Moon with the observational instrument that I invented for you in accordance with my directions. [end chapter 73]

Chapter 74

[1] A doubter may ask, how is it possible for the truth to contradict itself? [2] For it seems that Ptolemy explained on the basis of perception, as is mentioned in the *Almagest*, that lunar parallax conforms with what follows from his model. [3] In the *Almagest*, v.13, he records that he observed in the 16th [sic] year of Hadrian, on the 13th day of the Coptic month, Athyr, that the apparent altitude of the Moon on the meridian was 39;5°, and its true position was Cap 3°, and its latitude was to the north at its maximum less 0;1°. [4] According to Ptolemy, the true lunar altitude at that time in Alexandria was 40;12°, since the latitude of the Moon was 4;59°. [5] Therefore, he concluded that the lunar parallax at that time was 1;7°, but according to our explanations the parallax at that time should only have been 0;42°. [6] The way to resolve this doubt is not difficult, for [Ptolemy's] proof is based on a maximum lunar latitude of 5°, but this is not right, for the maximum

2. Cf. chap. 71, P 141a:−5.

lunar latitude is only 4½°, as we will explain. [7] Moreover, al-Battānī agreed that the maximum lunar latitude is 4½°. [8] When we subtract these 30 minutes from this parallax found by observation, the observed parallax that remains is only 0;37° which is even less than the amount arrived at by our reckoning by 0;5°. [9] This discrepancy may be due to approximation either in the observation or in the instrument with which the observation was taken, or it is possible that the altitude of the pole there was greater by 0;5° than the amount assumed by Ptolemy. [10] Be that as it may, it is clear that this observation does not contradict our claim that parallax does not agree with what follows from Ptolemy's model, but that it is evidence and proof in favor of our remarks. [11] It is a property of the truth that it agrees with itself in every respect.

[12] To demonstrate that the maximum lunar latitude is not sensibly greater than 4½° we depend partly on the observations of Ptolemy, partly on the observations of his predecessors that he reported, and partly on our observations of solar and lunar eclipses. [13] Moreover, we depended on our observation of the Moon on the meridian when it was very nearly at its maximum northern latitude from which we verified that its latitude was 4;30° very nearly, and on other observations as well. [14] Let us take one example from the observations of Ptolemy and those that he recorded by his predecessors, namely, the observation that we just cited, that he used to derive his view for the distances from the Moon to Earth, as evidence for showing that the lunar latitude only reaches 4½°.

[15] Moreover, in the *Almagest*, v.14, Ptolemy claimed that when the Moon is 7;48° from the ascending or descending node, half of the Moon's diameter enters the shadow; but when it is 9;20° from these points, only a quarter of its diameter enters the shadow. [16] Therefore, the excess in the latitudes corresponds to about a quarter of the lunar diameter, 0;6,52°, according to our determination of the lunar diameter. [17] For the lunar diameter, as determined by means of the ray entering through the window of the instrument, is 0;27,51° of the circle on which it travels. [18] It follows from this that its true amount on the circle on which it travels about Earth is less than this amount by 0;0,24°, as we will explain later, God willing. [19] Therefore, it is clear that the excess in latitude of the Moon at 9;20° over its latitude at 7;48° is 0;6,52°, and from this we derive a maximum lunar latitude of about 4;30°. [20] This is easily explained by means of the table that Ptolemy arranged for finding the lunar latitude, for the excess in the latitude corresponding to 9;20° over the latitude corresponding to 7;48° is

0;7,55°[3] and the arc corresponding to this excess is 0;7,34°,[4] very nearly, assuming a maximum lunar latitude of 5°. [21] But when we seek a number whose ratio to 5° is equal to the ratio of 0;6,52° to 0;7,34°, that number is about 4;32,15°; indeed, the ratio of 4;32,15° to 5° is equal to the ratio of 0;6,52° to 0;7,34°. [22] This shows that the [maximum] latitude of the Moon is 4½°, very nearly, for we cannot verify the excess of the 0;2,15° over the 4½°, because the determination of the digits of eclipse is difficult and we cannot be certain that there is no approximation in those observations to that extent. [23] This is because these 0;2,15° will affect this excess by only 0;0,3½°, and they will affect the digits of eclipse by only 1 part in 40 of a digit.

[24] It is clear that the latitude of the Moon is about 4;30°, very nearly, because in the year 1334 according to the Christian reckoning, on 7 May, we observed the altitude of the Moon on the meridian at 3 hours after mean noon, very nearly. [25] We found its altitude in Orange to be 72;53° when the longitude of the mean [with Q; P omits] Moon was in Cancer 4;8,19°, and its motion in anomaly was 274;25,48°, and its distance from its apogee was 81;50,47°. [26] Therefore, reckoning with our model, the Moon was at Cancer 11;19,19°, whereas according to Ptolemy's model it was at Cancer 9;41,38°. [27] Nevertheless, the difference between these two reckonings does not matter in this place because of the smallness of the excess and diminution of these inclinations in this place, namely, the inclination [i.e., the declination] of the degree [on the ecliptic], and the inclination of the Moon. [28] The distance from the Moon to the ascending node according to the reckoning with our model is 62;36,48°; the altitude of the degree of the Moon [on the ecliptic] at this horizon is 69;4,2°, and the inclination of the Moon according to our reckoning is 3;59,40°. [29] When we add this to the altitude of the degree [on the ecliptic], the resulting true altitude of the Moon is 73;3,42°, which means that the parallax is 0;11°, very nearly. [30] This is less than what follows from our reckoning by 0;4°, and this [level of] approximation can be ascribed to the approximation in the observation in taking the altitude of the Moon. [31] But when the maximum latitude of the Moon is taken to be 5°, the inclination of the Moon in this place is 4;26,18°, from which it ought to follow that the true altitude of the Moon at this horizon is 73;30,20°, and the parallax would then be more than 0;37°. [32] But this is impossible, for

3. With al-Bāttanī's tables (Nallino 1903–07, 2:78), β(9;20°) = 0;48,35°, and β(7;48°) = 0;40,40°; hence the difference is 0;7,55°.

4. Note that Sin (0;7,34°) = 0;7,55.

even according to Ptolemy's reckoning the parallax at this place would only be about 0;20°. [33] Therefore, it is clear that this observation is evidence that the inclination of the Moon does not exceed 4½°.

[34] According to Ptolemy's reckoning, the altitude of the degree [on the ecliptic] is 69;11,47°, and according to our reckoning the latitude of the Moon is 3;56,7°. [35] When we add them together, the true altitude of the Moon on the meridian is 73;7,56°, and this agrees with what is appropriate for the parallax at this place according to our reckoning. [36] But if the maximum latitude of the Moon is taken to be 5°, the resulting parallax would be more than 0;41° [with P; Q reads 0;45°]. [37] For, according to this reckoning, the lunar latitude was 4;22,25°, and when added to [the altitude of the degree on the ecliptic], the resulting true altitude of the Moon would be 73;34,7°; this is impossible for, even according to his reckoning, [the parallax] should only be 0;20°.

[38] We also determined this from another observation in that aforementioned year [1334] on the 24th of June, after sunrise: when the altitude of the Sun was 9;10°, we found the altitude of the Moon on the meridian to be 43;16°, very nearly. [39] At that time the Moon was very nearly 324;42° from the ascending node, and it was at Aries 1;20°. [40] When we computed the latitude of the Moon with a maximum of 4½°, the true altitude of the Moon at that time is 43;56° [with Q; P reads 40;56°]. [41] Therefore its apparent parallax at that place is 0;40°, and this should be its amount according to our assumptions. [42] This observation, in addition to providing evidence that the amount of the inclination of the Moon is as we set it, is also evidence that the parallax is as we set it, very nearly. [43] For, according to Ptolemy's view for the latitude of the Moon, the altitude of the Moon at the time of that observation should have been much less than the amount we mentioned for these two reasons together: because at that time the Moon was at its perigee and very nearly at 180° in its motion of anomaly as well, and the inclination of the Moon was to the south. [44] Its altitude, according to Ptolemy's reckoning, ought to have been less than what we found by observation by about ⅚ of a degree. [45] Al-Battānī emphasized that the latitude of the Moon does not reach the entire amount that Ptolemy assumed, and agreed that the maximum latitude of the Moon is 4½°. [46] This adds clarity and perfection to what we explained about it, and this will be further verified when we mention the eclipses that we observed. [end chapter 74]

Chapter 75

[1] The proof taken from the spot seen on the Moon is as follows. [2] If the Moon had an epicycle in the way that Ptolemy assumed, the

portion of the Moon's body seen when it is at the highest part of the epicycle would be different from the portion of its body seen by us when it is at the lowest part of its epicycle, for then we would see the convex surface of the epicycle, but at the highest part of the epicycle we would see the concave surface of the epicycle. [3] At middle places we should see a composite of these two portions of the Moon's body, i.e., a part of what is seen of it is what is seen at the lowest part of the epicycle, and a part of it is what is seen of it when it is at the highest point of the epicycle. [4] The spot seen on the Moon is necessarily something in the body of the Moon at a definite place on it, for it is a reality rather than an appearance, as we mentioned before; therefore, it clearly follows that the spot would sometimes be seen, sometimes not seen, and sometimes only part of it seen and part of it hidden. [5] Also, when it is seen, it ought to be seen in diverse conditions with respect to the luminous [part] of the Moon, and this is self-evident to the careful reader of this book. [6] But, we repeat that this spot is always seen in the same condition with respect to the luminous [part] of the Moon when we examine this with respect to the ecliptic. [7] The contrary of what was assumed previously follows, namely, that the Moon has no epicycle. [8] We stipulated that this should be examined with respect to the ecliptic because, if it is examined with respect to the horizon, it may be imagined that this spot is seen in diverse conditions, for the condition of the ecliptic [relative to] the horizon varies according to the changes in the [point of the ecliptic] that is on the horizon, and all this is clear to the careful reader of this book.

[9] The proof taken from the ray of the Moon that enters through the window of the Staff is as follows. [10] If the Moon had an epicycle and an eccentric orb in the way that Ptolemy assumed, it would follow that the diameter of the image of the Moon would be much greater at quadrature than its size when seen at opposition, and that it would be much greater when seen at 180° of its motion in anomaly than when it is seen at 0° of its motion in anomaly. [11] But we find that the size of the diameter of the image at quadrature is not perceptibly greater than its apparent size at opposition by much. [12] Similarly, we find no perceptible increment in its size at 180° of its motion in anomaly. [13] We repeated this observation, and it yielded the contrary of what was previously assumed, namely, that the Moon has no epicycle and no eccentric orb in the way that Ptolemy assumed. [14] We determined from this observation the apparent size of the lunar diameter on the circle on which it travels when it is at apogee, for it was necessary to investigate this on account of eclipses, but we have not yet repeated this observation of the size of the lunar diameter at quadrature in such

a way that we could derive from it the amount of eccentricity of the orb of the apogee, as we mentioned previously.

[15] The proof taken from the apparent size of the Moon with the Staff is like the previous proof except that this proof is taken by observing the body of the Moon [directly] whereas the previous proof was taken by observing the size of its image. [16] The size of the lunar diameter can be observed in many ways with this Staff. One way is for there to be a square missing in the middle of the plate in the upper direction whose side is less than 1 part in 120 of the length of the Staff. [17] We bring the plate closer to, or farther from, the eye until the edges of the hole appear to touch the ends of the Moon's apparent diameter at that time perfectly at right angles, for this will lead us to the apparent size of the Moon at that time.

[18] The second way is for us to observe the Moon with a star on a straight line passing completely through its [i.e., the Moon's] apparent diameter. [19] We observe the Moon with that star [using this instrument] when the entire diameter of the Moon is included in the distance, and we observe the Moon and that star with [the instrument] again when none of the diameter of the Moon is included in the distance. [20] The difference between these two distances is the apparent size of the Moon at that time. [21] When we observed this in this way, we did not find until now that the Moon was perceptibly greater in size at quadrature than at opposition by much, and we did not find that it was greater at 180° of its motion in anomaly than at 0° of its motion in anomaly. [22] From this we conclude that without doubt the Moon has no epicycle and no eccentric orb as Ptolemy assumed. [23] Moreover, it follows from the force of our previous remarks that it is impossible to consider the model for the orbs of the Moon in a way other than the one that we have mentioned except for slight changes in the parameters that may be reached by our senses by repeating this observation. [24] You, O careful reader, see how noble is the truth, and how it produces evidence for itself in every respect. [end chapter 75]

II. Commentary

Chapter 72

In chapter 71, Levi established his lunar model from observations of the positions of the Moon; he now wishes to establish his model on physical grounds. He claims to have four arguments: (1) his determination of lunar parallax supports his model against that of Ptolemy; (2) the fact that we always see the same side of the Moon is inconsistent with the epicyclic model; (3) the size of the image of the Moon in a camera obscura is consistent with Levi's model but not with Ptolemy's model; and (4) direct measurement of the apparent lunar diameter is also consistent with Levi's model but not with Ptolemy's. The first proof, based on parallax, is the subject of chapters 73 and 74, and the other proofs are treated in chapter 75.

Chapter 73

Levi claims repeatedly that the lunar anomaly has no effect on parallax and that the parallax at quadrature is 0;57°, while at opposition it is 0;55°. This is a small variation in contrast to the large variation in parallax that follows from Ptolemy's lunar model. Levi describes some observational methods to determine the lunar parallax and ends the chapter with references to two dated observations, June 20, 1333, and June 21, 1333, but no details are given here.

In [8] I prefer the reading 20°, based on a parallel passage in Levi's *Astronomy,* chapter 11 (Goldstein 1985, p. 80).

Chapter 74

In this chapter, Levi discusses Ptolemy's determinations of lunar parallax, the inclination of the lunar orb, and the apparent size of the lunar diameter. Levi first turns his attention to *Almagest* v.13, which is concerned with the determination of lunar parallax. There is a problem with his citation of the date of Ptolemy's observation: Levi says it took place in the sixteenth year of Hadrian, whereas the Greek text has the twentieth year (cf. Toomer 1984, p. 247). Levi argues that Ptolemy's determination of parallax at the time of that observation is incorrect. Instead of 1;7° it should have been 0;42°. Levi ascribes Ptolemy's error to an erroneous value for the maximum lunar latitude: according to Ptolemy, it is 5;0°, but, according to Levi, it is only 4;30°. It is interesting that the modern determination of parallax under those circumstances yields 0;44° (cf. Pedersen 1974, p. 206), but the error in Ptolemy's determination is not associated with his value for the maximum lunar latitude; rather, it is associated with errors in his values for the latitude of Alexandria and for the obliquity of the ecliptic. Levi claims that the value 4;30° for the maximum lunar latitude was accepted by al-Battānī, but there does not seem to be any other evidence in favor of this claim. Indeed, the tables of al-Battānī use 5;0° as the maximum lunar latitude (see Nallino 1903–07, 2:80). On the other hand, the tables of al-Khwārizmī use a maximum lunar latitude of 4;30° (see Suter 1914, p. 134), and Levi may have confused these two sources.

Levi next turns to a passage in *Almagest* v.14 in order to undermine Ptolemy's value for the maximum lunar latitude. The data there are that, namely, that when the argument of lunar latitude is 9;20° only ¼ of the lunar diameter is eclipsed, whereas when the argument of lunar latitude is 7;48° half of the lunar diameter is eclipsed. Ptolemy, using a maximum lunar latitude of 5;0°, shows that the apparent lunar diameter is 0;31,20°. Levi tries to show that, starting with an apparent lunar diameter of 0;27,27°, the maximum lunar latitude is 4;30°. Levi's argument in [20ff] is faulty, but a proper argument can replace it. The correct formula relating the apparent lunar diameter and the maximum lunar latitude is the following:

$$i \sin(9;20) - i \sin(7;48) = d/4,$$

where i is the maximum lunar latitude and d is the apparent lunar diameter.
Then

$$i = \frac{d/4}{\sin(9;20°) - \sin(7;48°)}.$$

Given modern sine tables, if i = 5;0°, d = 0;31,45° (Ptolemy computes 0;31,20°); whereas if i = 4;30°, d = 0;28,35°. Moreover, if d = 0;31,20°, i = 4;56° (rather than Ptolemy's 5;0°), and if d = 0;27,27°, i = 4;19° (rather than Levi's value of 4;30°). Levi's point is that for him the determination of the apparent lunar diameter is secure and based on direct observations, and he is therefore willing to use this value for the determination of the maximum lunar latitude. On the other hand, Ptolemy did not think that direct observations of the Sun or Moon could yield reliable values for their apparent diameters (cf. *Almagest* v.14).

Levi then confirms his value for the maximum lunar latitude and for lunar parallax by appealing to observations on May 7, 1334, and June 24, 1334. In both cases the observed parallax agreed with his model (and his value for lunar latitude) and not with Ptolemy's model (or with Ptolemy's maximum lunar latitude). Levi assumes that the lunar latitude lies on an altitude circle, but this is only approximately true when the Moon is on the meridian (it is precisely true only when the Moon is at the nonagesimal, the point on the ecliptic 90° from its rising point). For Levi the importance of his values for lunar parallax is that they indicate that Ptolemy's values for the distances from Earth to the Moon are erroneous and that in fact the Moon varies relatively little in its distance from Earth.

Levi's value for the lunar diameter, 0;27,51° (in [17]), is the same as the value he found for the apparent solar diameter at apogee in chapter 56 (see Mancha 1992*a*, p. 292). The computation of the correction, 0;0,24° (in [18]), is given in chapter 92 (see Mancha 1993, especially pp.

111, 119, and 125). The reason for the correction is that the value for the apparent lunar diameter, 0;27,51°, was observed on the surface of Earth, but Levi wishes to determine the value for the lunar diameter as it would be seen from the center of Earth. In chapter 92, values are given for the apparent radius of the Moon under these two circumstances: 0;13,55°, from the surface of Earth, and 0;13,43°, from the center of Earth (both values have been rounded), whose difference is 0;0,12°. Hence the difference in the apparent diameters is 0;0,24°. (I am grateful to J. L. Mancha for calling my attention to the relevant passage in chapter 92.)

Chapter 75

In the first paragraph, Levi argues that we always see the same side of the Moon, i.e., that it always has the same markings even though it is not always entirely illuminated by the Sun. Levi argues that the spot on the Moon is a physical property of it and not an optical illusion. The term "shadow" (*ṣel*) used in the text for the spot on the Moon is rather unfortunate; the Latin translator understood the sense of the passage as is clear from his term, *macula*. If the Moon were attached to an epicycle, we should sometimes see one side and sometimes the other side, but this is not so. Hence, the Moon is not attached to an epicycle (cf. Goldstein 1974, pp. 25–26). Levi is here appealing to an Aristotelian argument that the Moon has no motion of its own; rather, its motion is that of the orb to which it is attached (cf. Goldstein 1985, pp. 114, 230). The claim that we only see one side of the Moon goes back to antiquity (see Aristotle, *De caelo*, ii.8: 290a; Guthrie 1960, p. 189), and the difficulty it poses for the epicyclic model was noted by some Latin authors of the thirteenth and fourteenth centuries (cf. Duhem 1954, p. 437; Grant 1987, pp. 203f; Gabbey 1991, pp. 115ff) but, as far as I know, Levi was the first to mention it in a work on mathematical astronomy. In [8], Levi emphasizes that the markings on the Moon are fixed in position with respect to the ecliptic and that comparison with other reference systems (e.g., the horizon) may be misleading. Buridan (fourteenth century) put this argument somewhat differently:

> Then I argue that it [an epicycle] ought not to be assumed for the Moon, because then it would follow that in that spot (*macula*) of the Moon which appears as if it were an image of a man whose feet always appear to be below [or toward the bottom], the feet would sometimes appear above [in the upper part of the Moon]. But we experience that this consequence is false, since this image always appears situated in the same way with respect to us, on

> the assumption that it appears to us in the same part of the sky, for example, on the meridan. But I prove the principal consequent, because if the feet appear to us below when the Moon is in the *aux* [that is, apogee] of the epicycle, it should follow that when it reaches the opposite of the *aux* [that is, perigee] the feet should appear above, since by the motion of the epicycle the Moon has traversed [a distance such that] the part of the Moon which previously was above is now below. (Grant 1974, p. 525)

According to Buridan, if the epicyclic model was a true representation of reality, the "man in the Moon" would sometimes appear upside down and sometimes rightside up, whereas Levi argued that, if the epicyclic model was a true representation of reality, we would sometimes see the "spot" and sometimes it would not be visible. It would seem that Levi understood the Aristotelian argument better than Buridan.

The camera obscura combined with the Staff allows for a measurement of the image of the Moon on a screen. Levi was well aware of the need to subtract the size of the hole from that of the image (see Goldstein 1985, 141ff). In chapter 56 of his *Astronomy,* Levi gives many more details of his observations for finding the apparent diameter of the Sun than he does here for the Moon.

Finally, Levi claims that direct measurement of the lunar diameter with the Staff leads to the same conclusion that he reached using the other methods, but again details of his observations are not given.

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