EMPIRICAL EQUIVALENCE AND APPROXIMATIVE METHODS IN THE *NEW ASTRONOMY*: A DEFENCE OF KEPLER AGAINST THE CHARGE OF FRAUD

WILLIAM L. VANDERBURGH, University of Western Ontario

1. *The Case Against Kepler*

On 25 January 1990, *The Times* of London published a short, provocative article under the headline, "Planet fakery exposed".[1] The caption, "Falsified data: Johannes Kepler", appears under a rather sinister-looking portrait of the famous astronomer. Kepler, to the apparent delight of the reporter, stands accused of scientific fraud.

The *Times* article was inspired by William Donahue's (1988) "Kepler's fabricated figures: Covering up the mess in the *New astronomy*".[2] In that article, Donahue — who in 1992 published the first complete English edition of Kepler's *New astronomy*[3] — begins from the interesting and surprising revelation that a large table of positions and distances of Mars given at the end of Chapter 53 of the *New astronomy* (p. 537 in Donahue's translation) could not have been calculated by the method described in that chapter for calculating the Mars–Sun distance, the method by which Kepler originally claims to have derived the values in the table.[4] Donahue argues that the numbers in the table can be obtained *only* by using Kepler's area law for the ellipse. The trouble with this is that Kepler does not propose the ellipse as a model of Mars's orbit until several chapters later. Donahue surmises that the version of Chapter 53 that appears in the completed *New astronomy* is a (poorly) revised version of a chapter first written before Kepler realized that Mars's orbit is elliptical; after discovering the ellipse, Kepler must have gone back and revised the chapter, using the ellipse to calculate the table.[5] According to Donahue, Kepler's scientific fraud consists in the fact that he portrayed the source of the table as other than it was, with the goal of making the elliptical hypothesis look as if it had a greater degree of empirical support than it actually had.

The evidence for Donahue's reconstruction of the writing of Chapter 53 of the *New astronomy* is very strong: Donahue himself recalculates the Mars–Sun distances for the observations listed in the table by using the method described in Chapter 53, and he finds that the method cannot yield the results that are listed in the table. It is clear that if anything can be conclusively established about Chapter 53, it is this: *the table at the end of the chapter was not calculated using the method described in that chapter*. This is, indeed, mysterious. But does this mean that Kepler is really guilty of *fraud*? As is perhaps obvious from the fact that my title is modelled after the *Defence of Tycho against Ursus* (which Kepler wrote as a defence of

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Tycho Brahe from false claims about the status of his world system),[6] my answer is "no". As I will argue, there is a better way to view what has happened in Chapter 53.

Note that Donahue's claims about the fraud are more modest than those reported in the *Times*, though still very serious, and they are backed up by detailed mathematical arguments. As the present paper unfolds, it should become clear that I disagree with very few of Donahue's conclusions. Indeed, I am convinced that Donahue is right about the facts of the case (that is, about what Kepler did); my concern is simply with the interpretation that Donahue has imposed on those facts (namely, that what Kepler did amounts to fraud).

It would not be unfair to ask why we should care about the charge of fraud now, several years after Donahue's article appeared. Setting the historical record straight and rescuing Kepler's honour are reasons that are likely to appeal to historians. But a more compelling reason to examine the charge of fraud is that defending Kepler from it leads to a deeper understanding of his philosophy of science; *why* the case of Chapter 53 should not be construed as fraud is, in important respects, more interesting than the mere fact that the case should not be construed as fraud.

The structure of the argument in defence of Kepler is as follows. Section 2 describes the place and purpose of Chapter 53 in Kepler's *New astronomy*. This shows why the fraud with which Donahue charges Kepler would have been pointless had Kepler perpetrated it — Kepler could not have furthered any sensible end by doing what Donahue claims he did. This fact alone should make us wary of believing that Kepler committed the fraud in question. Section 3 shows that, given the margin of error in the astronomical observations available to Kepler, Donahue's recalculation of the table of Chapter 53 using the method described in that chapter gives Mars–Sun distances that are *empirically indistinguishable* from the values in the published table. That is to say, Donahue's claim that the values in the published table are *different from* the values one gets using the method of Chapter 53[7] — the claim upon which the charge of fraud is founded — is unjustified. Section 4 gathers evidence from other parts of the *New astronomy* to argue that what happened with the table of Chapter 53 can be interpreted as an example of Kepler's use of approximative methods; furthermore, the way Kepler uses approximative methods makes it clear that he has a sophisticated understanding of the limits imposed on theory construction and theory confirmation by the inescapable fact of observational error. It follows that what Kepler did with the table of Chapter 53 should not be construed as fraud, although admittedly the case is not unproblematic: he should have been more clear about how the table was ultimately calculated and about why that procedure was justified.

2. *Chapter 53 in the Argument of the* New Astronomy

Kepler is a realist about astronomical theories. That is, he believes (in contrast to Osiander and Ursus, for example) that astronomical theories are not merely contrivances for calculating the geocentric positions of the planets.[8] Kepler's goal in the *New astronomy*, therefore, is to give a semi-historical account of the long and arduous journey he took in search of a theory of Mars's motion, one that gives both the planet's *actual path* through space (not just its geocentric positions), and a plausible account of the *physical cause* of that motion. In fact, Kepler gives more weight to physical considerations than any previous astronomer; in the end, his choice of the elliptical orbit crucially depends on physical arguments. This is so because empirical considerations by themselves are (in principle, not just in fact) insufficient to determine a unique (i.e., *true*) theory of Mars's motion. Since Kepler is a realist, he is forced to consider more than a theory's ability to save the phenomena when he is trying to construct an account of Mars's motion.

Kepler's "War on Mars" begins as a comparison of how well each of the Ptolemaic, Copernican and Tychonic world systems (corrected and updated using Brahe's observations) predicts Mars's apparent positions. Kepler at first presents his calculations in all three forms despite the fact that he thinks the Copernican hypothesis is the correct one. He is able to rule out the Ptolemaic hypothesis as a result of his examination of the eccentricity of the Earth's orbit in Chapter 26. It takes physical arguments, however, finally to decide against the Tychonic system and in favour of a version of the Copernican hypothesis (this is perhaps not decisive until as late as Chapter 57). The relative positions and motions are the same in the Tychonic and Copernican hypotheses, so Kepler does not need to give two versions of every calculation, especially in the second half of the book where he is mostly concerned with the theory of Mars itself (in both systems, of course, Mars orbits the Sun). As Kepler's investigation proceeds, it soon becomes clear that even the assumption that Mars's orbit is circular will have to be abandoned. The inquiry into what sort of non-circular orbit Mars has begins in Chapter 41 and continues until the ellipse is proposed in Chapter 57. The key to this is Kepler's investigation of the distance of Mars from the Sun. In Chapter 44, he compares the Mars–Sun distances predicted by the modified Copernican hypothesis against distances calculated trigonometrically from observations. The result is that the circular hypothesis makes Mars's distance from the Sun too great at the quadratures. Kepler therefore knows that Mars's orbit is some oval-shaped path. In Chapter 45 Kepler suggests a particular oval and begins testing it. By Chapter 51, he shows that this oval also gives incorrect distances, but this time the theory predicts distances that are too short compared to what is observed. So the Copernican circle is too wide, and the first oval too narrow: they make (nearly equal) opposite errors in calculating the Mars–Sun distance. As Kepler puts it, "they have truth in the middle" (*New astronomy*, 99).

By the end of Chapter 51, then, Kepler knows that the orbit of Mars is some oval, and he knows that that oval falls about halfway between the circle of Chapter 44 and the oval of Chapter 45. It might be asked at this point: if Kepler can determine Mars's distances from trigonometric calculations from observations, why doesn't he use this technique to determine the entire orbit? This is an issue that Curtis Wilson[9] discusses in detail; the main reason that the suggested "point-plotting" technique will not succeed is that, because of the fact of observational error, there are many (actually, an infinity of) slightly different oval paths that are consistent with the observations.[10] The fact of observational error means that it is impossible to determine the unique path that Mars takes through space purely from observations of its geocentric positions. This is why even the very precise techniques discussed in Chapters 51 and 53 for determining the Mars–Sun distance from observations do not lead directly to the final theory of Mars's orbit. Given that observations *cannot* by themselves support choosing the ellipse over other nearby ovals (that is, ovals falling within the band of possible paths defined by the margin of error in the observations), other reasons — Kepler prefers *physical* reasons — need to be appealed to in order to allow us to choose the ellipse.[11] Chapter 53 does not — and cannot — offer decisive support for the final elliptical hypothesis.

Given all of this, what purpose do the distance determinations of Chapter 53 serve? They are an aside in the main argument, a double-check on the conclusion reached by Chapter 51 — namely, that the observed distances of Mars from the Sun fall about halfway between the distances predicted by the circular and oval hypotheses. The heading Kepler gives to Chapter 53 shows that he views its role in this way, too: he calls it, "*Another* method for exploring the distances of Mars from the Sun ..." (*New astronomy*, 529; italics added). Why does Kepler want to give a double-check? Because he is incredibly thorough; in the *New astronomy* he rarely lets a conclusion rest on a single calculation, and whenever possible proves the same thing by several independent methods. This serves the purpose of verifying that his mathematical techniques are not somehow flawed, and at the same time it is rhetorically powerful: those who would disagree with Kepler's conclusions (perhaps because of dogmatic adherence to a particular astronomical system) must overcome several arguments instead of just one.

The calculation described in Chapter 53 is an ingenious way of obtaining accurate Mars–Sun distances when Mars is near opposition to the Sun. Ordinary triangulation is very unreliable in such circumstances owing to the fact that slight errors in measuring the angle at the Earth (angle SEM in Figure 1) produce large errors in the triangulated Mars–Sun distance. Kepler develops the method of Chapter 53 to overcome this, and thereby makes observations of Mars near opposition useful for calculating the Mars–Sun distance. The method involves taking pairs of observations of Mars near opposition, where one of the observations is before the opposition and the other an approximately equal time after. The total geocentric angles between Mars and the Sun (angles SEM and $SE'M'$ in Figure 1) are known from observation; the Earth–Sun distances SE and SE' are known from Kepler's theory of the Earth; likewise, the angle ESE' can be calculated from the elapsed time and the theory of the Earth. The method of Chapter 53 essentially involves adjusting the Mars–Sun distances SM and SM' until the following equation is satisfied:

$$\angle ESM + \angle MSM' + \angle M'SE' = \angle ESE'.$$

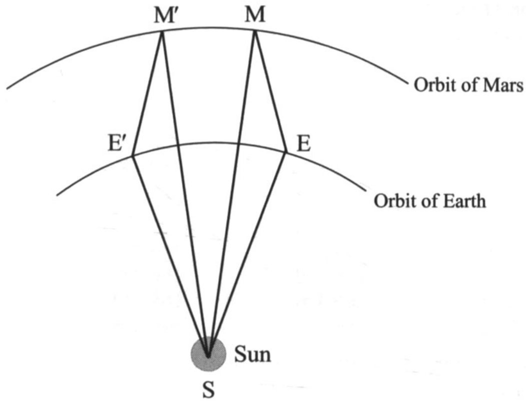

FIG. 1

The calculation is in fact quite difficult, not least because Kepler gives us insufficient information to follow him through every step and, as Donahue discusses, Kepler sometimes gives conflicting or incorrect information — fortunately, we do not need to be able to sort out the details of the calculation in order to be able to see that the charge of fraud is unwarranted. Readers interested in details of the calculation are referred to Donahue's article, where he gives a reconstruction of the method of Chapter 53 that is as clear and complete as it is possible to achieve, given the available information.

After describing this new method for determining Mars–Sun distances, Kepler returns to the main flow of the argument. Chapter 55 takes up where Chapter 45 left off, and summarizes the evidence that shows that Mars's true path lies halfway between the circle and the oval. Chapter 57 provides the main physical arguments to show that a magnetic-like force emanating from the Sun would push Mars in an elliptical path of the correct size. The remainder of the book (to Chapter 70) is devoted to specifying the parameters of this elliptical orbit.

The important thing to notice is that Chapter 53 is designed to be a redundant check on a distance that had already been calculated elsewhere with sufficient accuracy to prove that Mars's orbit is not circular. The most that Chapter 53 can do, then, is to provide *confirmation* of this result. When we combine this with Wilson's point that Kepler's determination of the shape of Mars's orbit is *not* an exercise in point-plotting, we can easily see that there is nothing that the alleged fraud could achieve. One way to summarize this point would be to say that Chapter 53 is not (and cannot be) part of the argument *to the ellipse*; it is, rather, part of the argument *away from a circular orbit*. As Wilson says, "The role of the distance determinations is mainly admonitory and negative: seldom do they provide the numbers required for the construction of an exact theory".[12] Another way to put this would be to say that empirical considerations define a class of possible theories that fit the data, but

TABLE 1. Comparison of the Mars–Sun distances given by Kepler (*New astronomy*, 537) with those obtained by Donahue (*op. cit.* (ref. 2), 226). (The data for 26 and 30 Dec 1582 are ignored in what follows; see text.)

Date	Chapter 53	Donahue	Difference
24 Nov 1582	158 852	158 792	+60
[26 Dec 1582]	[162 104]	[160 859]	[+1245]
[30 Dec 1582]	[162 443]	[161 195]	[+1248]
26 Jan 1583	164 421	164 364	+57
21 Dec 1584	164 907	164 934	−27
24 Jan 1585	166 210	166 226	−16
04 Feb 1585	166 400	166 412	−12
12 Mar 1585	166 170	166 206	−36

Kepler defines the mean radius of the Earth's orbit to be 100 000 units.
Average Mars–Sun distance in Donahue's data: 164 489 units.
Average Mars–Sun distance in Kepler's data: 164 493 units.
Average of the absolute value of the differences: 34.667 units.

they do not permit us to say *which* of the competitor theories is the correct one. So even if the elliptical hypothesis did have empirical support from the table of Chapter 53, that would still not be enough to establish the hypothesis. Kepler knows this, which is why his argument to the ellipse, in preference to other equally empirically adequate ovals, has a physical foundation.[13]

3. *Donahue, Kepler and the Problem of Observational Error*

So far we have discussed the structure of the *New astronomy* in sufficient detail to show that the fraud with which Donahue charges Kepler could not have achieved its supposed goal. It remains to show that, even if Donahue is right that the table of Chapter 53 was calculated using the final ellipse, Kepler is still not guilty of scientific fraud. To see how this can be, let us now take a closer look at the results of Donahue's calculations. Table 1 compares the Mars–Sun distances Donahue obtains, using what he considers to be his best attempt at the method described in Chapter 53, with the distances that Kepler gives in the table of that chapter.

We have there eight of the twenty-eight observations that Kepler gives in the table of Chapter 53; these are, unfortunately, the only dates for which Donahue reports the results of his recalculation of the distances. (Presumably Donahue did not base his allegations on a mere two-sevenths of the table, since he does report his recalculated *longitudes* for all 28 dates; see Section 5, below.) The observations for 26 and 30 December 1582, however, are problematic. There is either a systematic observing error, or the dates are the wrong ones for the observed positions.[14] Following Donahue, these two observations will be excluded from consideration in the discussion that follows.[15]

What the above table makes clear is the fact that the distances in Kepler's table were not calculated by the method of Chapter 53. Donahue remarks, "[A]s you can see, the numbers from [the table of Chapter 53] are *all different* from the [distances which Donahue calculated using the method described in Chapter 53]. Clearly, something is going on here about which Kepler is not being entirely candid".[16] After going on to examine the heliocentric longitudes of Mars that are also listed in the table, Donahue concludes that the table must have been calculated using the using the area law in the ellipse, not the method described in Chapter 53.

Donahue is probably right in his speculation that Kepler hastily revised Chapter 53 after having discovered the ellipse, and about the fact that Kepler used the elliptical theory to calculate the table. But this is no surprise, since Kepler tells us so himself in Chapter 56. Kepler writes, "In the observations of Chapter 53, there is no need to [repeat the calculation he has just described]. For I previously used this same method of reciprocation to find out the distances of Mars from the Sun which I presented in order to compute Mars's apparent positions" (*New astronomy*, 546). The so-called "method of reciprocation" presented in Chapter 56 is shown in Chapters 57 through 59 to have a physical foundation if the orbit is really elliptical. Given this admission, which Donahue also notes,[17] it is difficult to understand why Donahue persists in charging Kepler with fraud.[18]

The important point, however, even if Kepler did use the ellipse to calculate the Mars–Sun distances in the table, is that this certainly does not amount to fraud: the two methods in question are *empirically equivalent* with regard to the Mars–Sun distances. That is, the two methods give distances that are the *same*, to within the bounds allowed by the margin of error in the data. Kepler would be justified in using *any* alternative to the method of Chapter 53, for the purpose of calculating the table, so long as its Mars–Sun distance results were empirically indistinguishable from the results one would obtain using the method of Chapter 53.[19] As will shortly be illustrated in more detail, Donahue's own calculations show that the distances in Kepler's table *are* empirically indistinguishable from what one obtains with the method of Chapter 53.

Donahue does not note the *amount* by which his distance results differ from the distances given in Kepler's table. The differences are rather small: as the table above shows, where we adopt Kepler's convention of taking the mean Earth–Sun distance to be 100 000 units (and ignoring the two anomalous observations of December 1582), the average of the absolute value of the differences between Donahue's and Kepler's Mars–Sun distances is just 34 parts.[20] The largest difference is 60 parts; the smallest is 12 parts.

These differences between Donahue's results and the distances reported in the table of Chapter 53 are so tiny that they could not have been discerned on the basis of Tycho Brahe's data, as Kepler was well aware. The margin of error in Brahe's data is around two arcminutes (*New astronomy*, 276) — for the sake of comparison, note that Ptolemy's and Copernicus's observations were accurate only to within about ten arcminutes (*New astronomy*, 286). It is difficult to say precisely how the margins of error in observations of Mars's geocentric position translate into margins of error for calculated Mars–Sun distances. In other contexts, Kepler claims that Brahe's observations allow Mars–Sun distances to be determined to within 100 or 200 parts.[21] For example, Kepler notes that for the distance calculations of Chapter 51, he will "rejoice if [he is] able to come within an uncertainty of 100 units everywhere" (*New astronomy*, 513). Indeed, Kepler knows his data so well that the margin of error he allows *varies* depending on the observational situation. For some of the planetary configurations considered in Chapter 51, Kepler grants that it is necessary to allow an uncertainty of as much as 300 parts (*New astronomy*, 521). One way to determine how observational error translates into error in the distances calculated by the method of Chapter 53 would be to replicate Donahue's distance calculations using geocentric positions that differ from the reported ones by two arcminutes. The difference between Donahue's results and the new results would then tell us by how much distances can vary when the input data varies by the amount of the margin of error. Kepler gives an indication of how this would turn out: in Chapter 53, he notes that the amount of error in distances determined from observations depends on the particular alignment of Mars. In the case of one observation, he writes: "the distances themselves are not to be trusted, owing to the angles being too small"; if this angle is "varied by one minute through an error in observing, as easily happens, we shall be in error by *a thousand units*" (*New astronomy*, 533; italics added). In other words, in one particular observational situation of the type considered in Chapter 53, a margin of error in the observations of one arcminute produces a margin of error in the Mars–Sun distance of 1000 units. For more favourable positions, Kepler notes, an uncertainty of one arcminute in Mars's geocentric position will produce an uncertainty of no more than 50 parts in the Mars–Sun distance calculated on the method of Chapter 53 (*New astronomy*, 533). This seems to tell us that the highest accuracy that we can hope for in determining the Mars–Sun distances is agreement to within 50 units, and that in the worst cases the distances can be off by as much as 1000 units.

Although at one point Donahue seems to be aware that the methods he is comparing give empirically indistinguishable results,[22] in claiming that the values in the table of Chapter 53 are "different" from what one obtains with the method of Chapter 53 he has neglected the margin of error in the observations, something about which Kepler was always acutely aware. Donahue's demand for exact numerical agreement between the table of Chapter 53 and his own calculations (it is implicit in Donahue's argument that this is the only condition under which Kepler would not be guilty of fraud) is therefore misguided. The average of the absolute values of the differences between Donahue's and Kepler's calculations is, as noted above, 34 parts, where the Earth–Sun distance is taken to be 100 000. But the Mars–Sun distance is more like 164 490 (for the six dates considered); so the ratio of the average distance difference to the average distance is about 0.000 211 — Donahue is effectively asking for agreement to six decimal places![23] It is unwarranted to demand this level of accuracy from Kepler; Brahe's data certainly cannot support it.[24] Even the *maximum difference* between Donahue's results and those reported in Kepler's table (60 units) is rather close to the *minimum error* in the distances (50 units). The conclusion we should draw here is that Donahue's results using the method of Chapter 53 and the Mars–Sun distances in the table of Chapter 53 (at an average difference of 35 units) must be taken to be *equivalent*, since they are empirically indistinguishable. The lack of precise numerical agreement does indeed show, as Donahue argues, that Kepler did not calculate the table by the method described in the chapter; but the margin of error in the observations does not allow us to distinguish the results from each other. Given this, we must say that the distance results reported in the table of Chapter 53 are (contrary to Donahue's claims) *not different* from the results Kepler would have obtained (and which Donahue did obtain) using the method described in Chapter 53. And since the results are empirically equivalent, the methods are interchangeable as calculational devices.[25]

The final step in the defence of Kepler is to show that we can interpret Kepler's procedure in Chapter 53 as an instance of something perfectly acceptable that Kepler does throughout the *New astronomy*. If this is right, Kepler is guilty only of not being explicit about what he was doing, and about why it was justified.

4. *Kepler, Observational Error and Approximative Methods*

The discrepancy Donahue has noticed in the table of Chapter 53 gives us an opportunity to take account of an important aspect of Kepler's scientific method, namely, that he has a sophisticated understanding of the role of observational error. He is sensitive to the role of observational error with respect to both (i) the epistemic limits it imposes on the support that empirical evidence can give to scientific hypotheses, and (ii) defining what alternative forms of calculation are permissible. (By (ii) I mean that the margin of error in the data defines how close an approximative method must come to the rigorous method in order for it to be an acceptable substitute for that rigorous method.) The goal of this section is to show that what Kepler has done with the table of Chapter 53 is consistent with the limitations imposed by observational error, and with his practice with regard to the use of approximative calculations elsewhere in the *New astronomy*.

Kepler's remarks about the epistemic limits imposed by observational error are often found in passages where he needs to find a way to approximate or abbreviate calculations that would otherwise be difficult or impossible. In Chapter 18, for example, Kepler writes: "I shall use that form of calculation which I explained above in Chapter 4, because it is easier to use. Also, it is indubitable that not half a minute (actually somewhat less) would be gained or lost by using the Copernican or Tychonic form" (*New astronomy*, 276). Here, he chooses a certain form of calculation *because* it is easier, and simultaneously demonstrates his sensitivity to the question of how much the results of different forms of calculation would differ from each other — his choice of the easier method turns on the fact that it differs from the stricter alternative by less than the margin of error in Brahe's observations.

Several of the instances in which Kepler needs to rely on approximative methods are cases where he simply does not know how to achieve the exact solution he needs. In these cases (for example, in Chapters 40, 47, 50, 59 and 60) Kepler challenges the geometers to come up with the methods or proofs in question — but since he obviously cannot wait for the geometers' reply, he needs to find some reasonable way to go on with his calculations. He needs, in other words, to come up with some approximative method. In Chapter 16, for example, after describing a long and inelegant solution, Kepler writes, "Let the [subtle geometers] ... go forth themselves and solve the figure geometrically, and they will be to me great Apolloes. For me it is enough ..., in getting from the labyrinth to the highway, to show, instead of a geometrical light, a contrived thread, which nonetheless will lead you to the exit" (*New astronomy*, 256–7). Here, Kepler is unable to find a geometrically elegant solution, and is forced to rely on an inelegant approximative method. That he does so shows that there are cases where getting *some* solution is more important than finding the exact solution.

It is important to notice that in these cases where he does not know how to perform the exact calculation, Kepler seeks approximations whose results differ from what would be obtained using the exact calculation by less than the amount of error in the observations. In Chapter 47, for example, Kepler finds that precise calculations with his oval are too difficult; the ellipse there makes its first appearance in the *New astronomy, as an approximation* to the oval path, and he justifies this substitution on the grounds that the two figures "hardly differ" (*New astronomy*, 469).

We shall briefly return to the example of the substitute ellipse after examining one of the most striking examples of Kepler's use of approximative methods, the case of the so-called "area law". The area law is first developed in Chapter 40 for orbits that are eccentric circles; as his model of planetary orbits changes throughout the *New astronomy*, Kepler modifies the area law and continues to use it to predict the equations, that is, to predict where a planet is at a given time, or a certain length of time after it was in a known position. This is a striking case for us in part because we are accustomed to thinking of the area law not as an approximation, but as a *law*: a line connecting a planet with the Sun sweeps out equal areas in equal times. This was not Kepler's original conception, however. The relation that he originally tried to characterize, and was forced to approximate with the area law, captures the physical reason that we now invoke to explain the area law, namely, that a planet moves fastest when it is closest to the Sun, and slowest when farthest away. That is, Kepler's first question was, what causes a planet to traverse *equal* arcs of its orbit in *different* times?

This way of thinking (see Figure 2) is grounded in Kepler's firm belief that the Sun itself is the cause of the motions of the planets; it quickly led to the realization that the "delays" (times to traverse a given arc of the orbit) are proportional to the planet's distance from the Sun. Kepler's geometrical and physico-philosophical arguments for this are given in Chapters 32 and 33. In Chapter 40, Kepler is concerned

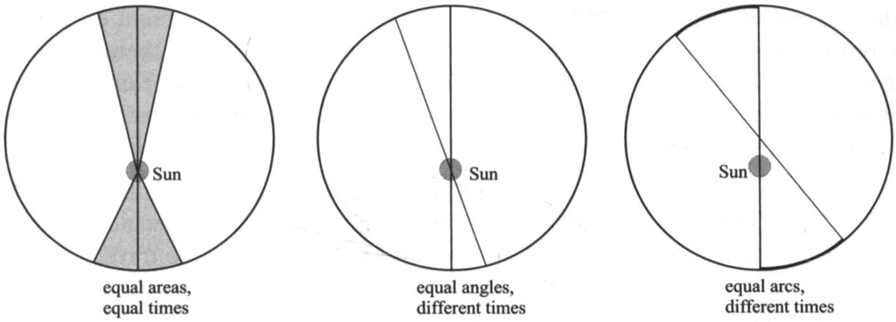

FIG. 2

to find a general solution to the question, how long will it take a planet to traverse a given arc of its orbit? This problem is made more difficult by the fact that in an eccentric orbit (or a non-circular one) the planet's distance from the Sun, on which its speed depends, is constantly changing. But given that a planet's speed at each point of the arc depends on its distance from the Sun at that point, the time to traverse the arc will be proportional to the sum of the distances of the planet from the Sun at each point of the arc: as Kepler puts it, "the whole sum of the distances is to the whole periodic time as any partial sum of the distances is to its corresponding time" (*New astronomy*, 417). Kepler therefore seeks to find a way to take the sum of the distances; his task, unfortunately, is made impossible (since integration was unknown at that time) by the simple fact that there are an infinite number of points in an arc of any length.

As usual, Kepler wants to try to find *some* solution to his general question, even though the exact solution is beyond his abilities. His first attempt at an approximation is to take the sum of the distances at one degree intervals around the orbit. For any arc of the orbit, the time to traverse the arc is to the whole periodic time as the sum of the distances at one degree intervals of the arc is to the similar sum of the whole (roughly, $t_a : \Sigma t_n :: d_a : \Sigma d_n$). He is not satisfied with this approximation, however, and therefore seeks a better one:

> [S]ince this procedure is mechanical and tedious, and since it is impossible to compute the equation given the ratio for one single degree without the others, I looked around for other means. And since I knew that the points of the eccentric are infinite, and their distances are infinite, it struck me that all these distances are contained in the plane of the eccentric.... Accordingly, instead of dividing the circumference, as before, I now cut the plane of the eccentric into 360 parts by lines drawn from the point where the eccentricity is reckoned [i.e., the Sun] (*New astronomy*, 418).

When Kepler says here that the number of points on the eccentric are infinite and that the distances are infinite, he means that the lines drawn to represent each are densely packed: lines drawn to each point of the eccentric to represent distances cover completely the plane of the eccentric. The *area* of the sector, therefore, is a good measure of the sum of those distances. This realization allows Kepler to approximate the distance–time relation, in which he is interested because it is physically meaningful, in terms of the purely abstract (non-physical) area–time relation.

> [I]t therefore seemed to me [that] I could conclude that by computing the area [of a sector] I would have the sum of the infinite [number of] distances [along the arc subtending the sector], not because the infinite can be traversed, but because I thought that the measure of the faculty by which the collected distances mete out the times is contained in this area, so that we would be able to obtain it by knowing the area without an enumeration of least parts (*New astronomy*, 419).

As the model of Kepler's use of approximative methods described here suggests he should, he quickly evaluates the empirical adequacy of the area law.

> [T]his method of finding the equations is not only very easy indeed, and based upon the natural causes of the motion explained above, but also agrees most precisely with the observations in the theory of the Sun or Earth. Nevertheless, it errs in two respects. First, it supposes that the orbit of the planet is a perfect circle, which, as will be demonstrated below in Chapter 44, is not true. Second, it uses a plane which does not exactly measure the distances of all points from the Sun. Nevertheless, as if by a miracle, each of these exactly cancels the effect of the other (*New astronomy*, 424).

Later, Kepler attempts to adapt this method to the oval of Chapter 45, but since he is unable to do the mathematics associated with the oval, he approximates the oval path with an elliptical one in Chapter 47. This particular approximation was especially fortuitous, since Kepler eventually noticed that the distance error in the substitute ellipse was opposite and roughly equal to the distance error in the original circle, which provided him with a strong hint regarding the shape and size of Mars's true orbit — namely, an ellipse falling halfway between the circle and substitute ellipse. If Kepler had known how to give an easy mathematical characterization of an arbitrary oval, he might have been even further delayed in reaching his final account of Mars's path!

The discussion in Chapter 21 of the extent to which false hypotheses can yield the truth is, interestingly, relevant to Kepler's use of approximations. There, Kepler argues that compensating errors in false hypotheses can sometimes balance each other off, leading to results that are consistent with the observations to within the margin of error. This, he suggests, is sufficient for practical purposes such as calculation and prediction, where one is not concerned with finding the exact truth. Kepler is, of course, ultimately concerned to discover the geometrical and physical truth about the planetary system, but he acknowledges that there are cases in which practical limitations make it impossible to know or use the truth, or where the truth of some small geometrical matter is not enough to warrant suffering the difficulty of the exact calculation. Where Kepler does not know the truth, he challenges the geometers and seeks an approximative method. Where it is not worthwhile to pursue the exact truth, due to the difficulty involved, he also sometimes seeks approximative methods. In both sorts of cases, he seeks approximations which agree with the exact calculation to within the margin of error. Approximations, it seems fair to say, are one of the kinds of false hypotheses that can yield the truth, when they are properly constrained. They are acceptable so long as we do not think that we achieve more than empirical adequacy (mere predictive accuracy, not a true description) when we use them. Because Kepler is ultimately concerned to determine Mars's *true* path, he is usually careful to say when his methods are mere approximations. Regrettably, Kepler is less careful than usual with regard to the table of Chapter 53.

Nevertheless, the case of the table at the end of Chapter 53 fits the pattern of Kepler's use of approximations. The method of obtaining Mars–Sun distances described in Chapter 53 is complicated, and it can only serve to confirm a result that is already known: it is to avoid having to go through the calculation for all 28 observations that Kepler proposes to report the results in a table in the first place. At the end of Chapter 53 he tells us, "It would be tedious to repeat the same method, using the same words, for all the years of the oppositions. And so, in the following table, I have placed the observations themselves which I have consulted, and added what resulted from the computations" (*New astronomy*, 535).[26] Donahue's calculations show that in the end Kepler simplified his task even further, by using the elliptical hypothesis to compute the table. Kepler has, in other words, used the elliptical hypothesis as an approximation to the exact distance calculation. Of course, this is not the same as computing the Mars–Sun distance directly from observations (the method of Chapter 53 *measures* the distances, while the elliptical hypothesis predicts them); but since the results are empirically indistinguishable from what would have been obtained using the method described in Chapter 53, what Kepler has done seems acceptable.

Kepler could have prevented any suspicion of impropriety just by telling us *in Chapter 53* how he actually calculated the table — so why didn't he? He should have, of course, and he should have made the point made here, namely that the two methods in question yield empirically equivalent Mars–Sun distances. The reason that he did not do so may simply be that Kepler was careless in his revision of Chapter 53, as Donahue suggests. Or it may be that Kepler felt that the rhetorical structure of the *New astronomy* — it is advertised to be a sequential account of the various stages of the War on Mars — demanded that the ellipse not be revealed until the appropriate place. In any case, the confusion created by what Kepler did in Chapter 53 is mitigated by the fact, remarked above, that when the ellipse is finally unveiled as the theory of Mars's true motion, he is quick to tell us that he used it to calculate the table of Chapter 53.

In this section it has been argued that it is reasonable to interpret what Kepler has done with the final table of Chapter 53 as an instance of his use of approximative methods. Kepler only uses approximative methods that give results that agree with the exact calculation to within the margin of error. We are justified in interpreting Kepler's procedure in Chapter 53 as an instance of his use of simplifying approximations because the case fits the pattern of Kepler's explicit use of approximations, and because such an interpretation lives up to the sophisticated awareness of the limits on proof imposed by the fact of observational error that Kepler demonstrates throughout the *New astronomy*.

5. *Some Further Remarks*

In addition to his discussion of the distances, Donahue also spends much time discussing the source of the heliocentric longitudes of Mars listed in the final table of Chapter 53. In this case, Donahue's calculations with the ellipse give him values that on average come within a few arc*seconds* of the longitudes in Kepler's table. This is very close agreement (well within the margin of error), close enough to make nearly indubitable Donahue's hypothesis that the longitudes in Kepler's table were calculated with the area law in the ellipse; what differences there are seem to be attributable to the fact that while Donahue used an electronic calculator, Kepler had to use trigonometric tables.[27] The charge of fraud arises for the longitudes because Kepler leads us to believe that they are derived from his so-called "vicarious hypothesis"; they are not, and we nowhere find an admission of this fact, as we do in Chapter 56 regarding the distances. "The vicarious hypothesis" is the name Kepler gives to the theory of Mars's heliocentric longitudes which he uses as a substitute until he discovers the true theory of Mars's orbit. Kepler knows that the vicarious hypothesis is false, because its predictions of Mars's geocentric *latitudes* (obtained by conjoining the vicarious hypothesis with a theory of the Earth) differ from what is observed by more than the margin of error in the observations. Nevertheless, he uses it for obtaining geocentric *longitudes*, because he knows that it gets them right to within the margin of error in the observations (in particular, it is very accurate at predicting *change of longitude* over short times, and this is the use to which it is put in the method of Chapter 53). The vicarious hypothesis is, in other words, an approximative method for finding longitudes of Mars.

There is no need to go into detail here, since an argument analogous to the one given above with regard to the distances suffices to show that Kepler was not perpetrating a fraud with the longitudes, either, even if the longitudes in the table were calculated with the ellipse, and not with the vicarious hypothesis as Kepler leads us to believe. Again, Donahue's own data (compare the tables on pages 232 and 233 of his article) show that the vicarious hypothesis and the final ellipse give longitudes that agree with each other to within the two-arcminute margin of error inherent in Brahe's observations. These two methods are empirically equivalent with regard to longitude predictions; Kepler is therefore justified in using one as a substitute for the other. Again, Wilson's argument applies: even longitude predictions that agree with the observations to within the margin of error are insufficient to prove the elliptical hypothesis from which the predictions come; the same observations are also consistent with the longitude predictions of competing hypotheses (the observations are, for example, also consistent with the longitude predictions of the vicarious hypothesis). Since Kepler is interested in Mars's true path through space, the only adequate theory of Mars, in his lights, is one that makes correct predictions of distance and longitude together, and which supplies a physical account of Mars's motion. It follows that there is nothing that the supposed fraud with regard to longitudes could accomplish, either: even the very good empirical adequacy that is demonstrated in the table is *in principle* insufficient to establish the truth of the hypothesis from which the results are computed. Furthermore, the longitude case also fits the pattern of Kepler's use of approximative methods.

Note that while the discussion of the longitudes is crucial to Donahue's demonstration that the table of Chapter 53 was calculated using the ellipse, in Chapter 53 itself the longitudes seem to be of only secondary importance. When Kepler comes back to them in Chapter 56 it is to draw the bland conclusion that since the distances in the table lead to computed positions that agree with the observations to within the margin of error, the distances must be correct (*New astronomy*, 546). We might consider this to be a further, purely trigonometric, *check* on the distances determined by the method of Chapter 53 — one not dependent on any particular theory of Mars's orbit, and therefore providing support for none. An objection to this view of things would be that the distances that lead to empirically adequate longitudes of Mars are in fact derived from the elliptical hypothesis. But since the distances that would be found using the method of Chapter 53 are empirically equivalent to the distances computed using the ellipse, they are also verified by this check. The success of the longitude predictions proves only that the *distances* are correct; it says nothing about the 'truth' of the *theory* from which the distances are derived. In this sense Donahue is wrong to say that the table is a *test* of the theory of Chapters 59 and 60: all that the results show is that the elliptical hypothesis is a member of the class of empirically adequate theories of Mars. The ultimate proof of the hypothesis must be found elsewhere than in its mere empirical success. Kepler's lack of commentary on this matter might mislead us into thinking that the table presents a direct check on the reliability of the method of Chapter 53; his lack of clarity is not fraud, though. He makes no exaggerated claims about the empirical success of the elliptical hypothesis, nor do the results in the table make the elliptical hypothesis look better than it is: rather, the table shows exactly how good the ellipse actually is. In fact, far from perpetrating a fraud, by not explaining what the table really does, Kepler has *understated* the empirical case for the ellipse, and left open the possibility that readers will think that the table of Chapter 53 *merely* confirms the result of Chapter 45, as the chapter itself is designed to do.[28]

6. *Conclusion*

To be clear, my complaint in this paper is not with Donahue's mathematical results, which convincingly demonstrate that the table of Chapter 53 was calculated using the final elliptical hypothesis. Donahue's speculative history of the writing of Chapter 53, which explains the origin of the mystery regarding that chapter, is also quite plausible. I have no objection, then, to any of Donahue's main results; what is at issue is what interpretation should be imposed on those results. Donahue writes that the table of Chapter 53 "is a fraud, a complete fabrication. It has nothing in common with the computations from which it was supposedly generated other than the mere dates and the data for the Sun".[29] For the several reasons discussed above, the judgement that this amounts to fraud is unwarranted. First, as Donahue himself notes,[30] in Chapter 56 Kepler admits that the distances in the table of Chapter 53 were calculated using the ellipse. Second, while Donahue focuses much energy on the longitudes presented in the table of Chapter 53, that chapter is meant to be only a double-check of the fact that the *distances* of Mars from the Sun establish that Mars's orbit is not circular; in any case, Kepler nowhere appeals to the longitudes *or* the distances in the table as *proof* of the elliptical hypothesis (this is appropriate, since it would be impossible for them to provide that proof). Third, to call the table of Chapter 53 "a fraud, a complete fabrication", is to ignore Kepler's insight into how evidence bears on theory; in particular, the charge cannot even be formulated without asserting that the numbers in the table are "different" from what one obtains using the method of Chapter 53, but that assertion can only be made if the margin of error in the data is illegitimately neglected. Fourth, there is no reason to demand one particular method of calculating the table rather than another — we should not begrudge Kepler the use of a calculation which makes his Herculean task in the *New astronomy* a bit easier. This is especially true since the approximating calculation in question yields Mars–Sun distances that are empirically identical to the results that would have been obtained had Kepler used the (much more tiresome) method of Chapter 53 (so the table *does* have something very important in common with the method by which it was supposedly generated — the reported distances are just what they would have been had Kepler not chosen to use the simpler way to compute the table). Finally, and I think most importantly, interpreting things along the lines suggested here permits us to see Kepler's procedure in Chapter 53 as an instance of his intelligent use of approximative methods. By jumping to the charge of fraud, Donahue has missed an opportunity to explore this important aspect of Kepler's work.[31] The table of Chapter 53 is not what it at first appears to be, but it is not fraudulent either.

Admittedly, it would have been better if Kepler had been more clear about how he actually calculated the table, and about why that alternative procedure was justified. It would be wrong, however, to think that Kepler purposefully hid this information in an effort to defraud his readers. Perhaps the clearest indication that Kepler's tendencies are not toward fraud is the following passage from Chapter 44, which immediately follows his demonstration that the distances given by the circular hypothesis are too long as compared with distances triangulated from observations:

> If anyone wishes to attribute this difference [of 350, 783 and 789 units] to the slippery luck of observing [i.e., to the uncertainty in the observations, which in Chapter 42 was reckoned to be 200 or at most 300 units], he must surely not have felt nor paid attention to the force of the demonstrations used hitherto, and will be shamelessly imputing to me the vilest fraud in corrupting the observations of Brahe (*New astronomy*, 452).

This passage is significant because in it we find combined the two issues that have been central to this paper: the way observational error bears on the relation of evidence to theory, and the issue of fraud. We should trust it, I think, as an indication of Kepler's character and intentions.

The *Times* article of 25 January 1990 reports that, "Experts, nearly unanimous in defending Kepler, say his act may be less reprehensible than it seems". The defences of Kepler there described are limp at best, amounting mostly to the claim that the standards of scientific practice were different in Kepler's time. The present analysis shows that Kepler was in fact using quite modern standards of evidence; furthermore, this analysis shows why it was wrong-headed to think of Kepler as having committed fraud in the first place. We should not view what we find in Chapter 53 of the *New astronomy* as an instance of scientific fraud, but as an example of Kepler's use of approximative methods, an example in which it is possible to see his acute awareness of the role of observational error in setting limits on what can be proved from observations, and in defining which alternative forms of calculation are acceptable. While he should have been more clear about what was going on, Kepler's procedure in Chapter 53 turns out to be not at all reprehensible: it is, rather, laudable for the sophisticated sensibilities it displays regarding the relation of evidence to theory.

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2. William Donahue, "Kepler's fabricated figures: Covering up the mess in the *New astronomy*", *Journal for the history of astronomy*, xix (1988), 217–37.
3. Johannes Kepler, *New astronomy*, translated with introduction and notes by William Donahue (Cambridge, 1992).
4. Volker Bialas ("Keplers komplizierter Weg zur Wahrheit: Von neuen Schwierigkeiten, die *Astronomia nova* zu lesen", *Berichte zur Wissenschaftsgeschichte*, xiii (1990), 167–76) takes exception to Donahue's claim that the area law in the ellipse is the *only* method by which Kepler could have calculated the table at the end of Chapter 53, and suggests another. Bialas's suggestion, and an argument about why it cannot be correct, will be discussed in ref. 19 below.
5. Donahue, *op. cit.* (ref. 2), 217, 232, and 234.
6. See Nicholas Jardine, *The birth of history and philosophy of science: Kepler's "A defence of Tycho against Ursus", with essays on its provenance and significance* (Cambridge, 1984).
7. Donahue, *op. cit.* (ref. 2), 221.
8. Kepler argues for this view in his *Defence of Tycho against Ursus* (see Jardine, *op. cit.* (ref. 6).
9. Curtis Wilson, "Kepler's derivation of the elliptical path", *Isis*, lix (1968), 4–25, reprinted in *Astronomy from Kepler to Newton: Historical studies* (London, 1989).
10. We find here a "real life example" of the Duhem–Quine thesis of the underdetermination of theory by evidence, familiar to many in the form of the "curve-fitting" problem: no matter how many data points you have, there are always many possible curves that will fit the data. (This problem is of course compounded when observational error is taken into account — even more curves will fit the data in the sense of passing through the error bars.) It follows that it is necessary to use extra-empirical considerations, such as simplicity (or, as in Kepler's case, physical arguments), to choose which line to draw through the data points.
11. See for example Wilson, *op. cit.* (ref. 9), 14 and 19.
12. Wilson, *op. cit.* (ref. 9), 21.
13. An excellent, more detailed, summary of the argument of Kepler's *New astronomy*, one not focused on contextualizing Chapter 53 as my summary is, can be found in Owen Gingerich's "Johannes Kepler", in *The general history of astronomy*, ii: *Planetary astronomy from the Renaissance to the rise of astrophysics*. Part A: *Tycho Brahe to Newton*, ed. by René Taton and Curtis Wilson (Cambridge, 1989), 54–78.
14. Donahue, *op. cit.* (ref. 2), 228–9 and 233.
15. The overall point would not be affected were these two dates included, but since there are grounds for excluding them, they are so excluded, since that allows the point to be made more forcefully.
16. Donahue, *op. cit.* (ref. 2), 229, italics added.
17. *Ibid.*
18. It is interesting, given that Donahue is worried about Kepler's integrity, that Donahue accepts Kepler's assertion in Chapter 56 without re-computing the *distances* using the elliptical hypothesis. It is also interesting to note (and I thank an anonymous referee for pointing this out to me) that the method of reciprocation is not uniquely in agreement with the ellipse. Whiteside (*Journal for the history of astronomy*, v (1974), 1–21; see especially p. 14) has shown that the method of reciprocation, also known as "libration", does not provide empirical justification for preferring the final ellipse over the *via buccosa*, the "puffy cheek" path. This is as we should expect, since we know (through Wilson's argument) that the margin of error in Kepler's data was too large for the choice of the ellipse to be a purely empirical matter. Whiteside notes (as Kepler did not) that the maximum optical difference between the *via buccosa* and

the final ellipse is just one arcminute, i.e., less than the margin of error in the observations, and it follows that the two orbits are not empirically distinguishable. But Kepler, in his argument against the "puffy cheek" orbit (Chapter 58), seems to be in too much of hurry, and leaves the impression that the method of reciprocation, since it is successful when used in the ellipse, *does* give sufficient empirical reason to reject the *via buccosa*, and he then "leaps" to the ellipse. What he fails to note is that the distances and equations in the *via buccosa* differ from those in the ellipse by less than the margin of error in the data, which means that in fact *no* empirical argument will be able to provide grounds for preferring the ellipse. This is another respect in which Kepler's argument to the ellipse is imperfect, and more evidence that it is really the *physical* argument (especially Chapter 57) that determines the choice of the ellipse over its (empirically indistinguishable) competitors.

19. Bialas (*op. cit.* (ref. 4)) suggests that Kepler could have calculated the distances in the table of Chapter 53 by taking the mean of the distances in the circle of Chapter 44 and in the oval of Chapter 45. For the results Bialas reports (pp. 173–4), this method yields good agreement with Kepler's table of Chapter 53. (Unfortunately, among the dates Bialas considers are the ones of December 1582 — observations which, Donahue argues, we have reason to think are corrupt.) If correct, Bialas's suggestion would absolve Kepler of fraudulently using the elliptical hypothesis as support for itself. But there are two problems. First, Donahue's hypothesis, that Kepler calculated the table of Chapter 53 using the ellipse, gains additional support from consideration of the longitudes in that table as well (see the fifth section of this paper); Bialas's suggestion can tell us nothing about the longitudes. Second, Bialas's explanation cannot make sense of Kepler's remarks in Chapter 56, telling us that he calculated the table of Chapter 53 using the elliptical hypothesis. It would seem, then, that Bialas's suggestion makes Kepler a dissimulator too, albeit in Chapter 56 instead of 53. Had Kepler used Bialas's method to calculate the table, and then in Chapter 56 claimed to have computed the table using the ellipse, intending to make the elliptical hypothesis seem to have greater empirical support, his fraud would be worse than the one with which Donahue charges him. (Whether or not Kepler used Bialas's method, that method will automatically yield results that are within the margin of error of the distances in the table of Chapter 53: we know by Chapter 51 that the true distances fall about halfway between the circle and the first oval!)
20. There are two ways to calculate this: (i) take the average of the differences between the two tables, or (ii) take the difference between the average distances. This second way yields a difference between Donahue's and Kepler's distances for these six dates (that is, leaving the signs of the differences intact) of a tiny $4\frac{1}{3}$ parts, where the distance being measured is about 164 500 parts! The first way, taking the average of the absolute value of each of the distance differences (as given in the text) is a fairer comparison, however, for the simple reason that the method of Chapter 53 is not a *theory* of Mars's orbit; it is, rather, designed to give the Mars–Sun distance very accurately given a very particular observing situation. Therefore, the distance results obtained by the two methods should be compared pair-wise, and not as an ensemble.
21. Compare Wilson, *op. cit.* (ref. 9), 13 *et passim.*
22. For example, in his discussion of the longitudes in the table (there will be more on longitudes in Section 5 of this paper) Donahue writes, "Although the agreement ... is within the limits of observational precision, we are concerned here with agreement of theories" (p. 229). The point is well taken: it is the lack of precise agreement that allows us to know that Kepler did not use the method of Chapter 53 to calculate the table. But to go on from there to a charge of fraud is to forget the fact that the results *are* empirically equivalent.
23. $34.7 \div 164\,490 = 0.000\,210\,955$.... Donahue's average Mars–Sun distance is 164 489, with a maximum distance of 166 412; corresponding numbers for Kepler are 164 493.3, and 166 400.
24. Kepler is not innocent of this sort of over-precision himself, insofar as he reports orbital distances as if all six digits were empirically meaningful. He at least demonstrates elsewhere that he is aware that the distances are accurate only to within a few hundred parts.
25. This assumes that Donahue is right that Kepler calculated the table with the ellipse — but *whatever*

method Kepler used to calculate the table, Donahue's calculations show that its results were empirically indistinguishable from the results of the method of Chapter 53. Kepler is therefore justified in using one method in place of the other. To be clear about the kind of interchangeability meant here, we should not ignore the fact that Kepler was not an instrumentalist about astronomical hypotheses, as is clear from his insistence that any theory of Mars's orbit must capture the planet's true motions through space, and not just its geocentric positions. That is why Kepler is interested in finding Mars's distance from the Sun in the first place. But the method described in Chapter 53 (which is not a *theory* of Mars's orbit) is empirically equivalent to the corresponding ellipse calculation of the Mars–Sun distance, and therefore the two are interchangeable *as devices for computing the table.*

26. The second sentence in this quotation, and Kepler's remarks that follow about the details of how the constants of the calculations were set, do indeed give the impression that the table was calculated using the method of Chapter 53. Donahue's hypothesis, that the chapter was written before the discovery of the ellipse and afterwards poorly revised, accounts for this discrepancy.
27. Donahue, *op. cit.* (ref. 2), 232ff.
28. An anonymous referee has posed a plausible objection: One could rightly object that "using the final theory — ellipse plus area rule — is inappropriate, when Chapter 53 is proposing to give 'another way of exploring Mars–Sun distances'. The table would not be giving the result of an empirical exploration at all. I would not cry 'fraud' here, but I would be unhappy, because the empirical justification for the ellipse and area rule have to be in question." I have several comments in response to this sort of objection, with whose spirit I must say I am sympathetic. I agree that we shouldn't be perfectly happy with what Kepler has done in Chapter 53, because he simply has not been clear enough about what is going on. The table indeed does not give the results of the advertised "other way" of exploring the distances, and this is clearly inappropriate given the lack of explanation. The results reported are, however, empirically indistinguishable from the promised results, and this seems to lessen the inappropriateness. It is important to notice, too, that Kepler makes no claim that the table provides empirical reason to prefer the elliptical hypothesis. *The justification for choosing the ellipse is, in the end, not an empirical one at all.* Merely to count as a competitor, any theory of Mars's orbit must give distances within the margin of error of those determined by the method of Chapter 53 (empirical adequacy is a minimum standard for theories), and in this respect *any number* of theories are equally empirically justified. (Empirical considerations have, therefore, a mainly negative force: they tell us which theories are non-starters.) Since empirical considerations are insufficient, we need extra-empirical reasons (Kepler uses physical arguments invoking the cause of planetary motion) to prefer one of these empirically adequate theories (the ellipse, say) over the others (like the *via buccosa*). This is what I meant above when I said that there is nothing that the supposed fraud could actually achieve.
29. Donahue, *op. cit.* (ref. 2), 233.
30. *Ibid.*, 229.
31. In the notes to his translation of the *New astronomy*, Donahue does not explicitly mention his own (1988) article or the accusation of fraud. He does, however, discuss the fact that the table of Chapter 53 was not calculated according to the method described in that chapter, and there are more (and longer) translator's notes to Chapter 53 than for almost any other part of the *New astronomy*. In addition, in the "Translator's Introduction" to the *New astronomy*, Donahue writes, "[A]lthough Kepler often seems to have been chronicling his researches, the *New astronomy* is actually a carefully constructed argument that skillfully interweaves elements of history and (it should be added) of fiction" (*New astronomy*, 3). The footnote to the passage just quoted remarks: "The entire table at the end of Chapter 53, for example, is based upon computed longitudes presented as observations" (*New astronomy*, 3). This seems to indicate that Donahue didn't change his mind about what Kepler had done in Chapter 53 between writing his (1988) article and publishing the (1992) translation of the *New astronomy*.