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Author(s): Giora Hon
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Is There a Concept of Experimental Error in Greek Astronomy?

GIORA HON*

The attempt to narrow the general discourse of the problem of error and to focus it on the specific problem of experimental error may be approached from different directions. One possibility is to establish a focusing process from the standpoint of history; such an approach requires a careful scrutiny of the history of science with a view to identifying the juncture when the problem of experimental error was properly understood and accounted for. In a study of this kind one would have to examine the evolution of the method of experimentation and related topics so that clear criteria would underlie the analysis.

This is not what I propose to do, if only for the reason that one cannot do justice to an historical study of this kind in a single paper. Rather, I propose to bring the problem of experimental error to the fore by contrasting two different attitudes towards it. I have addressed myself elsewhere to the question as to why it was not permissible for Kepler to ignore a discrepancy of eight minutes of arc; eight minutes which, in Kepler's words, 'led the way to the reformation of the whole of astronomy'.[1] In contrast to Kepler's perception, I shall discuss in the present paper a few suggestive cases from Greek astronomy in which such an awareness of error is at best only implicit or indeed lacking altogether. I shall attempt further to set this contrast against a philosophical and methodological background so that the essential elements which are conducive to the understanding of the problem of experimental error will be at hand. My principal argument is that in a science where empirical results are used more often for the purpose of illustrating and supporting theories rather than testing them, one would not expect a clear grasp of the concept of experimental error.

In a famous passage in the *Republic*, Plato (428–348 B.C.) expresses a view which prima facie amounts to a categorical objection to the observational method, and by implication to the method of experimentation. Notwithstanding his acceptance of the view that 'the stars that decorate the sky . . . [are] the finest and most perfect of visible

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[1] 'Nunc quia contemni non potuerunt, sola igitur haec octo minuta viam praeiverunt ad totam Astronomiam reformandam, suntque materia magnae parti hujus operis facta.' Quoted by Koyré. (A. Koyré, *The Astronomical Revolution: Copernicus–Kepler–Borelli*, (tr. R.E.W. Maddison), London, 1973, p. 401, note no. 22.) See G. Hon, 'On Kepler's Awareness of the Problem of Experimental Error', *Annals of Science* (1987), **44**, pp. 545–591.

*Department of Philosophy, Haifa University, Mt. Carmel, Haifa 31999, Israel.

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things', they are, Plato maintains, 'far inferior, just because they are visible, to the pure realities'.[2] For Plato it is the true relative velocities, in pure numbers and perfect figures, of the planets and their orbits, which constitute realities; and these are perceptible, in his view, to reason and thought but not visible to the eye.[3] He therefore argues that 'if anyone tries to learn anything about the world of sense whether by gaping upwards or blinking downwards, I don't reckon that he really *learns*—there is no *knowledge* to be had of such things'.[4] Astronomy should be treated, according to Plato, like geometry; that is, a discipline which sets problems for solution.[5] Thus, in order to make a genuine study of this subject, one should ignore the visible heavens.[6] Plato indeed applies this principle further and comments that the Pythagoreans are 'wasting their time on measuring audible concords and notes against each other'.[7] He does not think much of these people who 'torment the strings and try to wring the truth out of them by twisting them on pegs'.[8] He seems to despise the attempt to 'look for numerical relationships in audible concords'.[9] In sum, concerning both astronomy and harmonics, Plato appears, in these passages of the *Republic*, to object to the preference of the senses over mind.

However, as F.M. Cornford points out, 'Plato's primary purpose here is not to advance physical science, but to train the mind to think abstractly'.[10] In this sense, astronomy should be considered a study which can make the Guardians cultivate reason rather than the senses.[11] Nevertheless, Plato's didactic injunction to ignore the visible heavens was taken out of context in antiquity, as it has been again in modern times, to be construed as a ban on observational methods as a whole.[12] It appears that this doctrine of Plato has had a great influence upon the interpretation of Greek sciences. But as O. Neugebauer argues,

> if modern scholars had devoted as much attention to Galen or Ptolemy as they did to Plato and his followers, they would have come to quite different results and they would not have invented the myth about the remarkable quality of the so-called Greek mind to develop scientific theories without resorting to experimental or empirical tests.[13]

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[2] Plato, *Republic*, 2nd edn, rev. (tr. with an introduction D. Lee), London, 1974, p. 338 (529d).

[3] Ibid.

[4] Ibid., (529b–c), emphasis in translation.

[5] Ibid., p. 339 (530b).

[6] Ibid., (530b–c).

[7] Ibid., p. 340 (531).

[8] Ibid., (531b).

[9] Ibid., p. 342 (531c).

[10] F.M. Cornford, *The Republic of Plato*, Oxford, 1966, p. 241.

[11] G.E.R. Lloyd, *Magic, Reason and Experience*, Cambridge, 1979, p. 132.

[12] Ibid., p. 133. For detailed studies of this issue see G.E.R. Lloyd, 'Plato as a Natural Scientist', *Journal of Hellenic Studies* (1968), 88, pp. 78–81; A.P.D. Mourelatos, 'Plato's "Real Astronomy": *Republic* 527D-531D', in J.P. Anton (ed.) *Science and the Sciences In Plato*, with an Introduction by J.P. Anton, New York, 1980, pp. 33–73; I. Mueller, 'Ascending to Problems: Astronomy and Harmonics in *Republic* VII', in Anton, ibid., pp. 103–122; and G. Vlastos, 'The Role of Observation in Plato's Conception of Astronomy', in Anton, ibid., pp. 1–31. I am grateful to G.E.R. Lloyd for bringing the last three articles to my attention.

[13] O. Neugebauer, *The Exact Sciences of Antiquity*, 2nd edn, New York, 1969, p. 152; see also p. 69.

In Neugebauer's view,

> it is not because of philosophical prejudices that the Ptolemaic system dominated astronomy for about 1500 years but because of the solidity of its empirical foundations.[14]

Indeed, as G. E. R. Lloyd has convincingly demonstrated,[15] the notion that Plato steered Greek sciences away from empirical grounds, cannot be sustained by a careful study. It is now established that Greek sciences included many observational results—obtained either directly or through experimentation—which were incorporated into theories. However, a crucial question arises as to the way observational results were incorporated: were they considered a critical means of testing theories, or a mere corroborative device for the purpose of persuasion; a device over which theories could take precedence?

From the point of view of the problem of experimental error, this question is all the more important since its answer can afford a clue to the understanding of the limited awareness the Greeks had of the problem of experimental error. The following suggestive cases from the history of Greek astronomy indicate that the answer is the latter; namely, that the context in which observational results were incorporated into theories, at least in Greek astronomy, was not that of testing but rather corroborating. Hence the conclusion that the Greek astronomers had at best only an implicit awareness of the concept of experimental error; an awareness which never developed into explicit methodological procedures designed to account for the occurrences of experimental errors.

According to Neugebauer, Aristarchus (310–230 B.C.) can be considered the first astronomer who demonstrated that out of a few observational data combined with purely mathematical arguments, one could glean information about the sizes of the moon, the sun and their distances from earth. Aristarchus thus established a new methodological principle which is based on empirical and rational arguments.[16] Yet, much of his astronomy shows, as Neugebauer puts it, 'a lack of interest in empirical numerical data in contrast to the emphasis on the purely mathematical structure'.[17]

In his only preserved treatise, *On the Sizes and Distances of the Sun and Moon,*[18] Aristarchus deduced the result that the distance of the sun from earth is between eighteen and twenty times as great as that of the moon from earth. This result, which prima facie indicates a certain awareness of what a physical measurement consists of, namely, that it gives upper and lower bounds, was arrived at through correct geometrical, and thus theoretical, considerations but on the basis of an *impracticable* observational method. As a consequence, this result involved incorrect magnitudes of astronomical parameters.

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[14] O. Neugebauer, 'Notes on Hipparchus', in S.S. Weinberg (ed.), *The Aegean and the Near East*, New York, 1956, p. 296. See also O. Neugebauer, *Astronomy and History, Selected Essays*, New York, 1983. Quoted by R. Palter, 'An Approach to the History of Early Astronomy', *Studies in History and Philosophy of Science* (1970), 1, p. 127 note no. 3. However, see op. cit. (49).

[15] Lloyd, op. cit. (11).

[16] O. Neugebauer, *A History of Ancient Mathematical Astronomy,* Studies in the History of Mathematics and Physical Sciences, (eds M.J. Klein and G.J. Toomer), No. 1, 3 vols, Berlin and New York, p. 659.

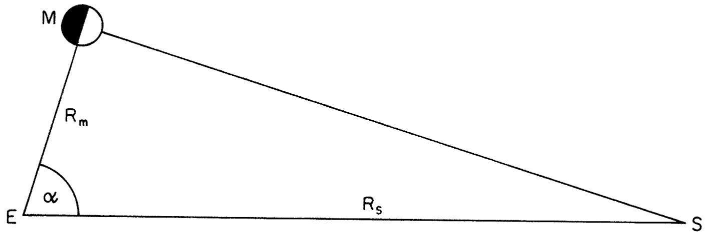
[17] Ibid., p. 271.

[18] T. Heath, *Aristarchus of Samos, the ancient Copernicus* (A history of Greek astronomy to Aristarchus together with Aristarchus' treatise on the sizes and distances of the sun and moon), Oxford, 1913.

Furthermore, according to Aristarchus' geometrical construction, the ratios of the sizes and distances which he set himself to calculate are trigonometrical. In Aristarchus' time such ratios had not been calculated, nor had a reasonably close approximation to the value of π been obtained. Being unable to perform exact calculations, Aristarchus apparently resolved to locate the sought ratios within upper and lower bounds.[19]

Specifically, his combined observations and calculations yielded the result: $18R_m < R_s < 20R_m$, which is in fact a direct consequence of the inequalities, $1/20 < \cos 87° < 1/18$. These inequalities are indeed correct, but the upper and lower bounds are obviously of mathematical origin and do not therefore reflect any physical consideration.[20]

Theoretically, the problem Aristarchus attempted to solve is quite simple once the construction of a right triangle, EMS, has been justified.



The problem comprises the solution of this triangle, in particular the ratio $R_m/R_s = \cos\alpha$. If one were to calculate, as indeed Aristarchus did, that $C_2 < \cos\alpha < C_1$, where C_1 and C_2 are constant, then one would get the result, $C_1R_m < R_s < C_2R_m$.[21]

In contrast to the theoretical simplicity of this problem, the practical difficulties are enormous. The measurement of the elongation α at the moment of dichotomy—that is, the moment when the moon is half illuminated—is fundamental to this determination. However, as Neugebauer categorically states, such a measurement 'is totally impracticable'.[22] Since the elongation of the moon changes one degree in about two hours, it is desirable to establish the moment of dichotomy within at least one hour. However, one would consider oneself lucky to determine the night in which dichotomy occurs. In fact, it seems that the magnitude Aristarchus assigned to the elongation α, that is eighty-seven degrees, is completely fictitious (that angle is thought to be 89° 51′).[23]

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[19] Ibid., p. 328.

[20] Ibid., pp. 333–334.

[21] Ibid. Cf. Neugebauer, op. cit. (16), pp. 634–643.

[22] Neugebauer, ibid., p. 642.

[23] Ibid. Boyer describes Aristarchus' method as unimpeachable;'the result,' he writes, 'being vitiated only by the error of observation in measuring the angle MES as 87 degrees.' (C.B. Boyer, *A History of Mathematics*, New York, 1968, p. 177.) By disregarding the enormous practical difficulty which the measurement of angle MES involves, Boyer misses a crucial element of this method of Aristarchus, namely, that for all intents and purposes, Aristarchus' measurement is a mathematical exercise. Cf., G.E.R. Lloyd, 'Observational Error in Later Greek Science', in J. Barnes *et al.* (eds) *Science and Speculation, Studies in Hellenistic Theory and Practice*, Cambridge, 1982, p. 153.

Moreover, it seems unlikely that the apparent diameter of the moon—a parameter which Aristarchus had to introduce in order to obtain the distances in terms of earth radii—was the result of a direct measurement. One may speculate that any attempt to measure it would have given Aristarchus a better estimate than the two degrees which he used. In fact, it appears that Aristarchus himself knew that two degrees is a gross overestimate; for Archimedes (286–212 B.C.) reports in his treatise the *Sand-Reckoner*, that 'Aristarchus discovered that the sun's apparent size is about one 720th part of the zodiac circle',[24] that is half a degree. As one of the physical assumptions in Aristarchus' calculation is that the moon and the sun are of equal apparent diameter, it seems strange that he did not use that value in his calculations, or amend them in the light of his new observational result.

However, there would be no surprise if one were to view Aristarchus' measurements as a purely mathematical exercise. 'If Aristarchus chose for the apparent diameter of the sun a value which he knew to be false, it is clear,' Tannery commented in 1883 that this

> treatise was mainly intended to give a specimen of calculations which require to be made on the basis of more exact experimental observations, and to show at the same time that, for the solution of the problem, one of the data could be chosen almost arbitrarily. He secured himself in this way against certain objections which might have been raised.[25]

Tannery seems to suggest that Aristarchus, being dissatisfied with the quality of the physical parameters, proceeded to illustrate his method with an arbitrary numerical value for the apparent diameter of the sun.

Whether or not Aristarchus envisaged much more exact observations and thus, by implication, knew the importance of securing accurate astronomical parameters, cannot be historically established. However, as Neugebauer holds, it is certainly the case that Aristarchus' 'measurement' of the sizes and distances of the sun and the moon 'has as little to do with practical astronomy as Archimedes' *Sand-Reckoner* in which he demonstrates the capability of mathematics of giving numerically definite estimates even for such questions as the ratio of the volume of the universe to the volume of a grain of sand'.[26] In his treatise, Aristarchus appears to assume numerical data which are, in Neugebauer's words, 'nothing but arithmetically convenient parameters, chosen without any consideration for observational facts', and he proceeds to elaborate a pedantic mathematical formulation which is 'unrelated to the complexities of empirical data'.[27] Aristarchus, in other words, treats astronomy as a field of study which, like geometry, sets problems to be solved; the hallmark of Plato cannot here be ignored.

Although Archimedes develops in his *Sand-Reckoner,* like Aristarchus before him, a pedantic and rigorous mathematical demonstration while ignoring the practical significance of the problem, he does introduce some practical innovations which indicate a certain concern with physical and technical aspects. However, this new perspective does not in itself indicate a substantial divergence from the trend of early Greek astronomy to which Aristarchus' method belongs.

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[24] Heath, op. cit. (18), p. 311.

[25] Quoted by Heath, ibid., pp. 311–312.

[26] Neugebauer, op. cit. (16), p. 643.

[27] Ibid.

According to Archimedes' formulation in the *Sand-Reckoner,* the problem of establishing the volume of the universe requires one physical parameter which has to be secured through observation; this is the apparent solar diameter. To measure this parameter Archimedes contrived a dioptra of which he gave only a sketchy description. It operates with a small vertical cylinder which can be moved on a horizontal ruler into a position which covers exactly the solar disk at sunrise. In addition, he experimented with two very small cylinders in order to determine the width of the observer's pupil.[28] Thus, it appears that Archimedes was not satisfied with the traditional geometrical optics, and tried to combine it with a result from physiological optics. As Neugebauer explains,

> the apparent diameter of the sun is . . . measured as the angle between two tangents to the first mentioned cylinder and the little space which corresponds to the width of the pupil determined in the second experiment.[29]

Archimedes discloses that in his own attempts to determine by means of instruments the angle subtended by the sun, he realized that

> this angle is not easy to determine precisely because neither eyes nor hands nor the instruments necessary for the determination are sufficiently free from error to render it exact. But as this point has been frequently made, it is hardly appropriate to discuss it further at this time.[30]

However, neither Archimedes' writings nor any other early work which has been preserved bear this point out. The question as to whether this revealing remark concerning actual practice was so common a point as not to be worth pursuing in the above context, should therefore remain open.

Archimedes found that the angle subtended by the sun's diameter is between 1/164th and 1/200th part of a right angle.[31] On the basis of this result he proves that the diameter of the sun is greater than the side of a chiliagon (a regular polygon with 1000 sides) inscribed in its orbit. In this proof Archimedes abandons the traditional view that the earth is a point in relation to the sphere in which the sun moves (Aristarchus regarded the earth as a point even with respect to the sphere in which the moon moves); he thereby demonstrates his awareness of the phenomenon of parallax in the case of the sun.[32]

However, these careful and subtle considerations stand in stark contrast to Archimedes' employment of crude roundings which are perfectly justified in view of his sole objective: to obtain a secure upper bound for the volume of the universe. For instance, he multiplies the commonly accepted circumference of the earth by a factor of ten; he also more than doubles the diameter of the sun in relation to the diameter of the moon, and he replaces a regular polygon of 812 sides by a 1000-gon.[33]

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[28] Ibid., p. 647. Cf., Lloyd, op. cit. (23), p. 136.

[29] Neugebauer, ibid.

[30] Quoted by Palter, op. cit. (14), p. 121.

[31] Neugebauer, op. cit. (16), p. 644.

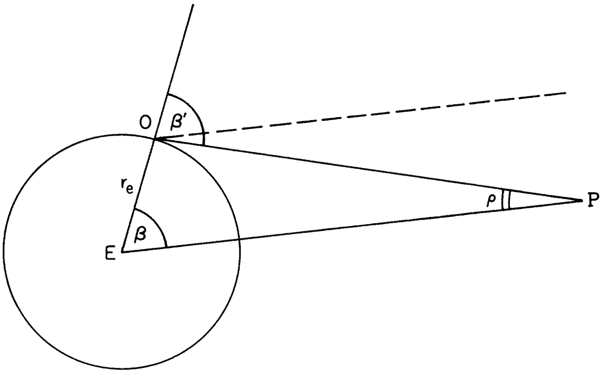
[32] Heath, op. cit. (18), p. 348. Heath suggests that Archimedes was the first to recognize the phenomenon of parallax with respect to the sun. (Ibid.) Neugebauer, on his part, maintains that we do not know who introduced the concept of parallax into Greek astronomy. (Neugebauer, op. cit. (16), p. 322.)

[33] Neugebauer, ibid., p. 644.

'And yet', as Neugebauer remarks, Archimedes 'undertakes a rigorous geometric discussion about the change of an angle observed from the earth's surface when shifted to the center of the earth,'[34] not to mention the measurement of the width of the observer's pupil. To amplify the accuracy of only some parts of the calculation which is, as a whole, based on crude roundings, does not render the calculation more accurate. On the contrary, it shows a lack of insight into the relationship between the abstract power of mathematics and the practice of physics. The width of the observer's pupil and the phenomenon of parallax are, from the standpoint of the degree of accuracy demanded by the problem, simply irrelevant to Archimedes' result that the volume of the universe contains less than 10^{51} grains of sand.[35] This kind of excessive rigour is essentially erroneous. It may be called erroneous rigour; a practice by no means rare at the present time.

A new insight into the interplay between theory and practice, between mathematics and physics, and, moreover, a recognition of the limitation of knowledge and its pitfalls, are displayed in the work of Hipparchus (190–125 B.C.).

In his attempt to determine the distances of the sun and the moon, Hipparchus distinguished, it seems for the first time, between the theoretical and the practical aspect of the phenomenon of parallax. From the theoretical point of view, the problem of parallax is very simple and straightforward: if β is the zenith distance of P, a celestial object, with reference to the point E, the centre of the earth, an observer in O will find a zenith distance $\beta' > \beta$. The difference $\rho = \beta' - \beta$ is the parallax of P.



The phenomenon of parallax is intimately related to the problem of determining the distances of celestial objects; for if β and ρ were known one could compute the ratio of EO = r_e and EP; in other words, one could find the geocentric distance of P measured in earth radii. However, the practical aspect of this phenomenon, that is, the measurement of ρ, is anything but straightforward: with ρ so small, errors of observation were bound to prevail given the observational techniques available in antiquity.[36]

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[34] Ibid.

[35] Ibid., p. 646. Cf., Lloyd, op. cit. (23), pp. 153–155.

[36] Neugebauer, ibid., pp. 100, 1235, Fig. 92.

I suggest that the comprehension of this distinction between theory and practice constitutes a turning point in Greek astronomy, indeed in science, with regard to the awareness of possible observational, and for that matter experimental, errors. However, with the advent of the Ptolemaic system and its powerful mathematical description, this insight of Hipparchus was lost.

In view of the observational difficulties, Hipparchus, it appears, resolved to find limits within which the solar parallax must lie in order to get observation and calculation to agree in the case of solar eclipse. In Book 1 of his *On Sizes and Distances,* Hipparchus started from the fact that there is no observable solar parallax. He therefore assumed the extreme situation in which the solar parallax was zero; that is, that the sun was, for practical purposes, infinitely distant. Using then the data from the eclipse of 14 March 189 B.C., he derived a *minimum* distance of the moon (seventy-one earth radii at least distance). As G. J. Toomer points out, Hipparchus

> was well aware of the unreliability of his premisses: for first, the fact that no solar parallax could be observed did not mean that the parallax was in fact zero; secondly, a small change, to five-sixths or three quarters, in the figure for the size of the eclipse at Alexandria, would cause an increase or decrease of 20% in the resulting lunar distance.[37]

Indeed, at the end of Book 1, Hipparchus forewarned the reader that he should not suppose that the question of the moon's distance had been resolved. He disclosed that further investigations would show the moon's distance to be *less* than what he had just computed (notice that what Hipparchus computed was a *minimum*).[38] He thus openly acknowledged a contradiction in his parallax calculation. In Book 2, Hipparchus assumed that the solar parallax was the maximum possible, namely 7′. He thus computed the sun's *minimum* distance and the corresponding *maximum* distance of the moon. He found the latter to be 67 1/3 r_e in the mean. He then showed that as the sun's distance increased, the moon's distance decreased towards a limit of fifty-nine earth radii. He was thus able to establish the moon's distance between quite close limits.[39]

The problem of finding accurately the distance of the sun and, as a consequence, its actual size was altogether beyond the instrumental means of astronomers until the invention of the telescope.[40] It is thus to the credit of Hipparchus that he attacked the problem from, so to speak, both ends; a method which enabled him to establish not mathematical but rather *physical* limits for the value sought. Furthermore, he acknowledged the

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[37] G.J.Toomer, 'Hipparchus on the Distances of the Sun and Moon', *Archive for History of Exact Sciences* (1974), 14, p. 139. Pappus notes in his account of Hipparchus' procedure that Hipparchus 'takes the following observation: an eclipse of the sun, which in the Hellespontine region was an exact eclipse of the whole sun, such that no part of it was visible, but at Alexandria by Egypt approximately four-fifths of the diameter was eclipsed'. (Quoted by Toomer, ibid., pp. 126–127.)

[38] Pappus' commentary; quoted by Toomer, ibid., 126–127.

[39] Toomer, op. cit. (37), p. 139. Cf., Neugebauer, op. cit. (16), pp. 109–112, 327–329.

[40] J.L.E. Dreyer, *A History of Astronomy from Thales to Kepler*, 2nd edn (revised with a Foreword by W.H. Stahl), New York, 1953, p. 184. Neugebauer remarks that 'it is not surprising that the early attempts at determining the size and distance of sun and moon in relation to the earth ended with wrong results. The ancient methods are of necessity based on trigonometric arguments in combination with visual estimates of very small angles and one naturally had the tendency to falsify such estimates in the wrong direction.' (Neugebauer, op. cit. (16), p. 634.)

indefinite nature of his measurements which effectively prevented the solution of the problem. This acknowledgement indicates not only an insight into the roles of theory and practice, but also a scientific honesty, for Hipparchus did not erase his conflicting results. He disclosed that the 'maximum distance' in Book 2, that is, 67 1/3 r_e, had turned out to be smaller than the 'minimum distance' in Book 1, that is, 71r_e; these values are nevertheless of the same order of magnitude, and—for the first time in the history of astronomy—in the right region.[41] As Toomer remarks, this kind of openness is rare; it can also be found in the works of Kepler.[42] Kepler, in fact, intended to entitle his planned systematic treatise on astronomy—a treatise similar in its comprehensive goal to the *Syntaxis* of Ptolemy—with the name *Hipparchus*, in honour of this great astronomer.[43]

In Toomer's view, Hipparchus' treatise 'is a model of the use of a few observations to squeeze out a reliable result, while retaining due distrust of the accuracy of the observations'.[44]

One source of error in Hipparchus' procedure lies in his a priori assumption of a perceptible solar parallax, a hypothesis which Ptolemy (A.D. 100–170) considered highly questionable.[45] 'In the case of the sun it is quite uncertain,' Ptolemy maintains, 'not merely how great a parallax it has, but whether it has any at all.'[46] However, he himself did not improve on it, on the contrary, as will be observed, he made a distinctly retrograde step which fixed an incorrect solar parallax for almost 1500 years.[47] From the point of view of the practice of observation, Neugebauer remarks that

> the exaggerated value of the solar parallax is of little importance [with regard to the theory of eclipses and planetary motion] compared, e.g., to the effects of refraction and to errors of measurement of times and angles.[48]

Nevertheless, with the improvement of observational techniques and the accumulation of observational records, one would have expected that the scale of the planetary system could have been gradually enlarged. But as it happened, more than a millennium had to pass before the determination of the solar parallax was improved. It was Kepler who by amassing really refined observations, realized that a reduction to 1/3 of the incorrect ancient solar parallax should be introduced.[49]

[41] Toomer, op. cit. (37), pp. 139–140. Dreyer, ibid.

[42] Toomer, ibid.

[43] However, Kepler did not carry out his plan and wrote instead an elementary text-book of astronomy, *Epitome Astronomiae Copernicanae*. (Dreyer, op. cit. (40), pp. 403.)

[44] Toomer, op. cit. (37), pp. 139–140.

[45] Neugebauer, op. cit. (16), p. 329.

[46] Quoted by Toomer, op. cit. (37), p. 126.

[47] Dreyer, op. cit. (40), pp. 184–185.

[48] Neugebauer, op. cit. (16), p. 111.

[49] Ibid. In Neugebauer's view 'Muslim astronomers . . . restricted themselves by and large to the most elementary parts of Greek astronomy: refinements in the parameters of the solar motion, and increased accuracy in the determination of the obliquity of the ecliptic and the constant of precession'. (Ibid., p. 145.) However, Neugebauer remarks that 'the conceptual elegance of Ptolemy's cinematic models and the logical consistency of the derivation of the fundamental parameters from carefully selected observations made it extremely difficult to introduce more than insignificant modifications of the basic theory'. Thus, Neugebauer continues, 'every attempt at a revision of the foundations of the planetary theory must have appeared, rightly, as a gigantic task, not lightly to be undertaken in view of the consistency of the structure erected in the *Almagest*'. (Ibid.) For Neugebauer 'it is not surprising that a cosmological theory of such impressive internal consistency was not conducive to serious scrutiny'. (Ibid., p. 919.)

However innovative, Hipparchus' treatment of the problem of solar parallax does not surpass in its insight his great discovery of the precession of the equinoxes. Babylonian and early Greek astronomy does not distinguish between the sidereal year (the periodic time in which the sun returns to the same position with respect to the fixed stars whence it departed) and the tropical year (the time interval that elapses between the sun's two successive passages through the same tropic: equinoctial or solstitial point).[50] In other words, this astronomy presupposes the equivalence and constancy of the time intervals which these two distinct ways of describing the periodicity of the sun's motion exhibit. Hipparchus' great discovery is the recognition that the sun returns sooner to the vernal point than to the same fixed star; that is, Hipparchus discovered that the tropical year is shorter than the sidereal year, a discovery which is, in effect, the discovery of the precession of the equinoxes.[51]

To conceive the possibility of such a distinction requires, first and foremost, a conviction that, as Neugebauer puts it, 'no periodic time interval should be accepted as exactly constant without empirical confirmation through observations distant as far as possible from one another'.[52] Holding to this methodological principle, Hipparchus scrutinized earlier records of fixed star distances with respect to equinoxes and solstices, and data concerning the moments of equinoxes and solstices. Such records, about 150–170 years old, were available to him, and he compared these observations with his own results.[53] In performing this comparison Hipparchus exhibits not only an awareness of the importance of accurate observations, but also the ability to carry this understanding into effect; that is, to attempt to evaluate the errors in all of these observations and thus to assess the validity of the observational results.[54] Hipparchus published the results of this attempt in the treatise *On the Length of the Year,* in which he came to the conclusion that 'the equinoctial points move *at least* 1° per century in a direction opposite to the order of the zodiacal signs'.[55]

Ptolemy reports that in assessing the validity of the observational results, Hipparchus realized that errors could easily account for a shift of up to a quarter of a day.[56] Adhering to his methodology, Hipparchus did not exclude a priori the possibility of variations in the lengths of the years: either sidereal or tropical, or both.[57] It was therefore a problem for him whether or not these periodic time intervals are constant. 'It is clear... from these

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[50] Ibid., pp. 54, 369, 529, 543 note no. 13, 1082–1083.

[51] Ibid., pp. 807 note no. 15, 1082–1083.

[52] Ibid., p. 54.

[53] Ibid., pp. 292–298.

[54] However, see the criticism of Aaboe and Price, particularly the discussion of the different accuracy obtained in solstice and equinox observations. (A. Aaboe and D.J. de Solla Price, 'Qualitative Measurement in Antiquity: the derivation of accurate parameters from crude but crucial observations', in A. Koyré, *L'aventure de la Science, Mélanges A. Koyré,* Vol. I, Paris, 1964, pp. 6–10. Cf. op. cit. (138).

[55] Neugebauer, op. cit. (16), p. 293, my emphasis. Apparently, this discovery led Hipparchus to introduce real ecliptic coordinates because longitudes increase proportionally with time whereas latitudes remain unchanged. (Neugebauer, op. cit. (13), p. 69.)

[56] Lloyd, op. cit. (11), p. 181 note no. 295. Lloyd, op. cit. (23), p. 141. Neugebauer, op. cit. (16), p. 294.

[57] Neugebauer, ibid., p. 298.

observations,' Hipparchus commented,

> that the differences of the years have been very small. But as regards the solstices I do not despair of my and Archimedes' being in error both in observation and in calculation even up to the fourth part of a day. But the irregularity of the early periods can be accurately apprehended from observations made on the bronze ring set up in Alexandria in the so-called Square Hall.[58]

Thus, Hipparchus seems to have found that real variations in the length of the tropical year must be admitted, notwithstanding his awareness of possible errors of up to six hours arising from either observations or calculations, or both.[59]

The achievement of Hipparchus lies in his attempt to assess, theoretically as well as practically, earlier observations; that is, to determine their reliability and accuracy. In the case of the length of the tropical year, Hipparchus was aware of the possibility of explaining the apparent variations as due to the occurrences of experimental errors. However, he concluded that in this case a new phenomenon has to be acknowledged, namely, that the tropical year is not constant. This conclusion is incorrect; it was Ptolemy who correctly established the constancy of the tropical year.[60] Nevertheless, Hipparchus' methodology points in the right direction: it does take the problem of experimental error into account in however rudimentary and unsuccessful a fashion. Moreover, since he had at his disposal only a few observations, neither very old nor very accurate, he formulated his results, as Neugebauer puts it, 'very cautiously and in a preliminary form'.[61] He, for example, questioned the suggestion that the poles of the ecliptic are the centre of the motion of the precession, as he could not demonstrate it from the very limited empirical material that he had, a suggestion which Ptolemy did not doubt any longer. For Ptolemy it was an established fact that the slow motion of precession proceeds about the pole of the ecliptic and not about the pole of the equator.[62]

Characteristically, Hipparchus was aware that his limited data could not support a definite determination of the magnitude of the precession. He thus resolved to set a lower limit and considered it to be one degree per century. This judgement was vindicated later since Hipparchus reports in his later treatise, *On the Displacement of the Solstitial and Equinoctial signs,* that he 'found Spica to be six degrees from the autumnal equinox, while Timocharis had found the distance to be eight degrees'.[63] Timocharis had observed Spica in 294 and 283 B.C., while Hipparchus observed it in 129 B.C., thus the change amounts to 45″ or 46″ a year, that is, about 1 1/4 degrees per century.[64]

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[58] Quoted by Ptolemy. See Lloyd, op. cit. (23), p. 141.

[59] Hipparchus adduces another proof for variation in the length of the tropical year from calculations based on eclipse data. However, Ptolemy criticizes this proof and considers it circular. (Ibid., pp. 142, 156. Neugebauer, op. cit. (16), p. 295. See op. cit. (90).)

[60] Neugebauer, ibid. Cf., op. cit. (85, 86). Copernicus also did not realize that errors of observation were quite sufficient to account for the difference between the various values of the constant of precession. (Dreyer, op. cit. (40), p. 329.)

[61] Neugebauer, ibid., p. 294.

[62] Ibid., pp. 294, note no. 15, 296. See also op. cit. (84).

[63] Dreyer, op. cit. (40), p. 203.

[64] Ibid.

Almost two and a half centuries after Hipparchus had introduced the requirements for new standards in astronomical studies, Ptolemy succeeded in casting the observations and calculations into a so-called system, namely the Ptolemaic system. There is no doubt that Ptolemy drew from Hipparchus' works, be they methodological, theoretical or observational.[65] Indeed, it seems that Hipparchus had anticipated a Ptolemy who would put his results to use, for he consciously prepared the ground for further work to be carried out on the basis of his systematized observations.[66] As Ptolemy writes, it was because he

> had not received from his predecessors as many accurate observations as he has left to us, that Hipparchus, who loved truth above everything, only investigated the hypotheses of the sun and moon, proving that it was possible to account perfectly for their revolutions by combinations of circular and uniform motions, while for the five planets . . . he has not even commenced the theory, and has contented himself with collecting systematically the observations and showing that they did not agree with the hypotheses of the mathematicians of his time.[67]

The greatest achievement of Hipparchus was not the prediction of future eclipses for 600 years, as Pliny—apparently following a certain tradition—would have us believe, but rather the arrangement and classification of the material at his disposal from the past 600 years.[68] Hipparchus laid a solid foundation for the development of theoretical astronomy and made it possible for Ptolemy to take full advantage of the accumulated observational results.[69] Indeed, without the work of Hipparchus 'one could never have hoped to predict eclipses with reasonable accuracy and to test the foundations of theoretical astronomy'.[70] According to Neugebauer, Hipparchus was 'fully conscious of the fact that many of the parameters as well as the theoretical models at his disposal were only approximations in need of refinement by future generations'.[71]

The fact that Hipparchus sought to establish a sound and solid foundation for astronomy by providing observations and arranging them for proper analysis by future generations,[72] did not escape the perceptive eye of Kepler. Kepler drew the attention of Maestlin, his teacher, to the following parallel:

> You can see in what manner God disposes of his gifts; one man cannot do everything. Tycho Brahe has done what Hipparchus did; he has laid the foundations of the edifice, and has accomplished an enormous amount of work. Hipparchus had need of a Ptolemy who built thereon [the theories] of the five planets. I have done as much whilst he [Tycho Brahe] was still alive.[73]

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[65] E.g., Neugebauer, op. cit. (16), p. 89.

[66] Dreyer, op. cit. (40), pp. 161, 166–167.

[67] Quoted by Dreyer, ibid., pp. 165–166.

[68] See Neugebauer, op. cit. (16), pp. 319–321.

[69] In his 'Notes on Hipparchus', Neugebauer concludes that 'it is our good luck to be able to see in the *Almagest* how Ptolemy utilized this material with supreme skill'. (Neugebauer, op. cit. (14), p. 296.)

[70] Neugebauer, op. cit. (16), p. 321.

[71] Ibid., p. 320.

[72] Neugebauer, op. cit. (14), p. 296.

[73] Quoted by Koyré, op. cit. (1), p. 398, note no. 4. Neugebauer puts it this way: 'One may perhaps say that the role of Apollonius, Hipparchus, and Ptolemy has a parallel in the positions of Copernicus, Brahe and Kepler.' (Neugebauer, op. cit. (16), p. 309.)

Ptolemy, however, had not consolidated the methodological and observational achievements of Hipparchus, as much as Kepler did *vis-a-vis* Tycho Brahe's. Unlike Hipparchus, Ptolemy neither acknowledged the limitations of his results nor did he examine them critically; he did not pursue his studies along an open path but rather saw to it that his system would account for the phenomena. Ptolemy did not make explicit the criteria upon which he judged some observations more accurate and reliable than others. He thereby exposed his methodology, as Lloyd points out, to the charge of circularity: 'the observations are judged accurate because they confirm the theories (Hipparchus' or his own) and the theories are accepted on the grounds that the "best" observations confirmed them'.[74]

Ptolemy, like Hipparchus, determined the lunar distance as fifty-nine earth radii; but unlike Hipparchus he rendered it exact.[75] Admittedly, this value is in the right region as the accepted value is 60 1/3 r_e; however, it appears that Ptolemy's result is approximately right only because a series of errors in observation and theory cancelled each other.[76] Endorsing this view, Neugebauer holds that in general 'it is only the accidental interplay of a great number of different inaccuracies of empirical data and of computations that lead to nearly correct results'.[77] But was it accidental? In view of the fact that Ptolemy knew in advance at what value of the lunar distance he should arrive, namely, Hipparchus' result, it seems incredible that this happened fortuitously. In other words, it is not unlikely that Ptolemy selected those observations which he had thought he could manipulate to produce exactly Hipparchus' result and thereby render his own result exact.[78]

This kind of circular procedure, in which results are adjusted to tally with the theory, is not unheard-of in classical time. In acoustics, for example, results of real or purported experiments are invariably presented, as Lloyd puts it,

> in the form of ratios that *exactly* correspond to what acoustic theory demanded—and they do so even when the tests referred to could not conceivably have yielded anything like those results.[79]

Indeed, Ptolemy himself perfected, so to speak, this circular method of research in his investigation of the phenomenon of refraction which has a great bearing upon the accuracy of astronomical observations. In his *Optics*, Ptolemy describes detailed experiments which are designed to determine the refraction that occurs when light passes from air to water, from air to glass and from water to glass. The results are set in tables and although some of the results are qualified as 'very nearly', they all tally exactly with a general law

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[74] Lloyd, op. cit. (23), p. 158.

[75] Toomer, op. cit. (37), p. 131.

[76] Ibid., p. 131, note no. 25.

[77] Neugebauer, op. cit. (16), p. 106.

[78] Toomer, op. cit. (37), p. 131. Lloyd suggests that Ptolemy settled on a one-value parameter, instead of a bounded one in order to simplify the computations. (Lloyd, op. cit. (23), p. 155.) Cf., op. cit. (115).

[79] Lloyd, op. cit. (23), p. 151, emphasis in the original. However, Lloyd points out that 'in acoustics, as in astronomy, it was sometimes recognised that different observers will get different results'. (Ibid., p. 132, note no. 8.) Indeed, when Plato discusses harmonics in the *Republic*, he remarks that 'some say they can distinguish a note between two others, which gives them a minimum unit of measurement, while others maintain that there's no difference between the notes in question'. (Plato, op. cit. (2), p. 340 (530).)

which, however, is not stated. To be sure, the law is not correct and it appears that here, as Lloyd puts it, 'the observations have been interpreted *before* they are recorded'.[80]

It now becomes clear why Ptolemy determined the solar parallax and distance so confidently. For having arrived at what he thought to be the *exact* lunar distance and thus parallax, he proceeded to calculate the solar parameters and assigned confidently to the parallax the value 2′51″. He thereby ignored the cautious methodology of Hipparchus and established an incorrect value, about nineteen times too great, which conforms to his world picture of nested planetary orbits: a geocentric model that lasted for almost 1500 years.[81]

Another example is concerned with the determination of the magnitude of the precession of the equinoxes whose discovery is due, it may be recalled, to Hipparchus. This case bears all the traits of the previous one: whereas Hipparchus had determined the precession to be *at least* one degree per century, Ptolemy concluded that it is *very nearly* one degree and he adopted this convenient round number for ordinary working purposes.[82] Hipparchus is not only methodologically correct; in view of the accepted value (about 1.4 degrees), he is also factually correct. Ptolemy's value for the precession produces a deviation of more than one degree in three centuries and thus a noticeable discrepancy would have resulted comparatively soon, if only there had been a careful observer to look for it.[83]

To the credit of Ptolemy it should be noted that he realized that at his disposal were sufficient observations for demonstrating that the slow motion of precession proceeds about the pole of the ecliptic and not about the pole of the equator.[84] Furthermore, he held that the observations did not confirm fluctuations in the length of the tropical year; he thus considered the amount of precession constant. He argued that

> we are sure by the continuous instrumental observations we have made of tropics and equinoxes that these periods [the time between successive tropics or equinoxes] are not unequal. For we find them differing by no appreciable amount from the additional quarter day, but at times by about as much as could be attributed to the error due to the construction or position of the instruments.[85]

In Ptolemy's view a deviation of six minutes of arc from the equatorial plane in the position of the instrument, generates an error of six hours in the determination of the time of the equinox. Ptolemy in fact considered unreliable the instrument at Alexandria to which Hipparchus had referred.[86]

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[80] Lloyd, op. cit. (23), p. 151, emphasis in the original. Cf., Lloyd, op. cit. (11), p.197; Neugebauer, op. cit. (16), pp. 892–896; Palter, op. cit. (14), pp. 121–122; A.M. Smith, 'Ptolemy's Search for a Law of Refraction: A Case-study in the Classical Methodology of "Saving the Appearances" and its Limitations', *Archive for History of Exact Sciences* (1982), 26 no. 3, pp. 221–240.

[81] Neugebauer, op. cit. (16), pp. 112, 634, 917–922.

[82] Ptolemy, *Almagest,* (trs. and ann. G.J. Toomer), London, 1984, Bk. VII, Ch. 2. Toomer, op. cit. (37), p. 131 note no. 25. Cf., Dreyer, op. cit. (40), p. 203; Neugebauer, op. cit. (16), pp. 54, 160; Lloyd, op. cit. (23), pp. 147–149.

[83] O. Pedersen, *A Survey of the Almagest,* Odense, 1974, p. 248. Neugebauer, ibid., pp. 986, 1037.

[84] Neugebauer, ibid., p. 34.

[85] Quoted by Palter, op. cit. (14), pp. 122–123.

[86] Lloyd, op. cit. (11), p. 181 note no. 295; Lloyd, op. cit. (23), pp. 140–142, 145. See op. cit. (58, 59).

Although Ptolemy concluded that there is no variation in the tropical year, he admitted that its actual length of time is difficult to determine and he emphasized what Hipparchus had already realized, namely that to determine accurately periods of return it is necessary to use as far as possible observations which are distant in time one from the other.[87] 'The period of return will be obtained as nearly exactly as possible', Ptolemy rightly maintains, 'the longer the time between the observations compared.'[88] Moreover, as Lloyd points out, Ptolemy occasionally stated the need to base conclusions on as many observations as possible.[89] And since he realized that alternative methods may be used to obtain the same result (in the case of the determination of the length of the tropical year, Ptolemy cited both direct observations of solstices and equinoxes and results arrived at indirectly by calculations based on eclipse data),[90] he recommended their use to provide a checking procedure; a very powerful method indeed which Kepler also used.[91]

Though Ptolemy has emerged as the creator of a dogmatic astronomy much enhanced by his mathematical genius, it is none the less true that, like Archimedes, he was also interested in problems of observation. Prominent amongst them in astronomy is the assignment of limits for permissible discrepancy between observation and calculation. It appears that for Ptolemy the limits of tolerance of discrepancy between observation and calculation are 10′ of arc.[92] Although this important consideration is only implied, it does indicate that there is after all a methodological difference between his optical and astronomical works. As Lloyd explains,

> unlike the *Optics*, the *Syntaxis* does not, as a whole, present results that have already been tailored to match the theory precisely. The problem there is not that discrepant observational data are corrected, in a bid to obtain perfect fit with the theories, but rather that they are tolerated—along with a very broad tolerance of other sources of imprecision in the purely mathematical part of the calculations.[93]

Another case in point is Ptolemy's clear grasp of the impossibility of establishing accurately absolute—as distinct from relative—planetary distances. Ptolemy explicitly states that the problem of planetary distances could only be solved if direct measurements

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[87] Lloyd, op. cit. (23), pp. 142, 146–147.

[88] Quoted by Lloyd, ibid., p. 142.

[89] Ibid., p. 145. However, as Lloyd stresses, it is not in dispute that the paucity of the actual observations cited in Ptolemy's detailed accounts of the movements of the planets in Books IX to XI is remarkable. For each planet he cites almost the minimum number of observations that are necessary to determine the parameters of what is after all a complex model. (Lloyd, op. cit. (11), p. 186.) Ptolemy is in general quite confident that his theories work well; indeed, he considers approximate or uncorrected figures adequate for the exposition of his model. (Ibid., p. 187 note no. 325.)

[90] However, Ptolemy criticized Hipparchus' indirect method of determining the length of the tropical year using the data of lunar eclipses. He argued that these calculations presuppose correct determinations of equinoctial points, and cannot be carried out independently of assumptions about the sun's position. Ptolemy thus exposed the circularity of this method. (Lloyd, op. cit. (23), pp. 142, 156. Neugebauer, op. cit. (16), p. 295.)

[91] Lloyd, ibid., p. 145. Cf. Hon, op. cit. (1), pp. 557–559.

[92] Dreyer, op. cit. (40), p. 195; Neugebauer, op. cit. (16), p. 99; Palter, op. cit. (14), p. 126; Toomer, op. cit. (37), p. 129. However, Lloyd points out that Ptolemy does not always set out his workings in such a way that one can see precisely what margin of error he allowed himself. (Lloyd, op. cit. (23), p. 149.)

[93] Lloyd, ibid., p. 152.

of all the various parallaxes were available.[94] However, such measurements were at Ptolemy's time impossible, and thus as far as Ptolemy was concerned this problem should have remained unsolved.[95] Notwithstanding this recognition, Ptolemy thought it fit to construct on the basis of his evaluation of the solar distance—which needless to say is erroneous due to incorrect solar parallax obtained indirectly—a planetary shell structure that came to prevail till the advent of the Copernican system. It is worth noting that the description of the *apparent* planetary motions, as projected onto the celestial sphere, does not require absolute distances. But Ptolemy, it appears, could not resist the temptation of solving the very problem he himself had suspended; that is, the determinations of *absolute* planetary distances.[96]

Ptolemy in fact showed that he could be quite critical with regard to practical procedures and the validity of their results. To measure, for example, the apparent diameter of the sun and the moon, one would employ an instrument of the dioptra type. Aristarchus and, later on, Archimedes had arrived by such a method at one 720th part of the circumference of the zodiac circle and half a degree respectively; in Ptolemy's time this was generally accepted as the correct value. Yet, as Neugebauer reports, it became fashionable to embellish this direct measurement with the timing of the rising of the solar disc by means of a water-clock. The claim was that the resultant quantity of water was one 720th of the total daily outflow. To obtain such a result one has to guarantee an accuracy of 1/1000 in the measurement of the daily outflow; a requirement which was then all but impossible. Ptolemy was aware of this folly and exposed its fictitiousness.[97]

But the most convincing evidence for Ptolemy's interest in practical, in addition to theoretical, problems comes from his study of optics. Ptolemy did not confine himself to the area of strictly geometrical optics. With his experiments on binocular vision,[98] on the origin of colour sensation (e.g., the mixture of colours on rotating discs),[99] on the refraction of light[100] and on optical illusions (such as the apparent magnification of celestial objects near the horizon),[101] Ptolemy went far into the field of physiology of sight and optics at large.

In his studies of astronomy and optics, Ptolemy exhibits a great power of analysis and practical inventiveness which rightly makes him one of the greatest figures in the history of science. However, the available historical material does not furnish enough evidence to reach confident conclusions concerning some aspects of his procedures.[102] In his

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[94] Neugebauer, op. cit. (16), p. 148.

[95] Neugebauer, op. cit. (13), pp. 155–156.

[96] Neugebauer, op. cit. (16), pp. 148, 917–922, 1088; Lloyd, op. cit. (11), p. 199. Cf. Hon, op. cit. (1), pp. 562–563.

[97] Neugebauer, ibid., pp. 103, 657–658. Ptolemy in fact adduces an array of arguments against this method: (1) the hole of the clepsydra gets stopped up; (2) the quantity of water that flows out in a night or a day is not necessarily an exact multiple of the quantity taken at the rising; (3) it is inexact to take the chord as equal to the arc it subtends. (Lloyd, op. cit. (23), p. 143.)

[98] Neugebauer, op. cit. (16), pp. 893–894.

[99] Ibid., p. 894.

[100] Op. cit. (80), and (137); but see (139).

[101] Neugebauer, op. cit. (16), p. 894; op. cit. (136). Ptolemy lists in the *Optics* many illusory phenomena and he attempts to account for them. Far from concluding that sight is deceptive, he stresses the difference between exceptional and normal sight. (Lloyd, op. cit. (23), p. 161.)

[102] Lloyd, ibid., p. 147.

astronomical studies Ptolemy had presumably some other data besides those he quotes, but one remains totally in the dark as to his selective criteria.[103] Lloyd maintains that it is largely a matter of guesswork to determine 'how far he is prepared to adjust his data or to ignore conflicting evidence: how far he systematically biases what he records in favour of preconceived conclusions'.[104] Yet, from the *Syntaxis* itself, it is abundantly clear, as Lloyd observes, 'that he does not submit his results to rigorous and extensive controls'.[105] In Lloyd's view, 'there can be little doubt that as a whole he sought to *confirm* earlier results as far as possible, particularly those of Hipparchus'.[106] By giving the minimum number of observations required to determine the parameters and by making adjustments such as discounting minor discrepancies in the reported observational data and roundings in the calculations, Ptolemy weakens the 'confrontation' between theory and empirical results. It is not therefore reassuring that he professes to select the more accurate observations of those recorded by his predecessors, as these are the observations which tend to corroborate his theory.[107] But at the same time, he was prepared, as Lloyd continues to observe, 'to modify the current theory at certain points—to obtain a better fit with such evidence as he had at his disposal'.[108] Lloyd, however, concludes that the deductive nature of the *Syntaxis* cannot be disputed: 'it is an exercise in geometrical demonstration and that is where its great strength lies'.[109]

The few cases I have outlined show that in Greek astronomy one can find evidence of some significant concern with the problem of obtaining accurate observational data, though these evidences occur, so to speak, late in the day.[110] Ptolemy was in fact explicitly critical of most ancient observations from which he had to draw on for his planetary theories; in his view they had been recorded 'inattentively and at the same time in a rough and ready fashion'.[111] Indeed, he, and Hipparchus before him, were aware of particular sources of error in observations and calculations.[112] However, as R. Palter

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[103] Ibid., p. 150.

[104] Lloyd, op. cit. (11), p. 198.

[105] Ibid.

[106] Ibid., emphasis on the original.

[107] Ibid., p. 192; Lloyd, op. cit. (23), pp. 147, 157.

[108] Lloyd, op. cit. (11), p. 198. In his account of Venus, Ptolemy claims that the observational data required the introduction of the equant: the 'centre for the eccenter which produces the uniform motion', to use Ptolemy's own definition. (Ibid., p. 192; Neugebauer, op. cit. (16), p. 1102.) In Neugebauer's view, the introduction of the equant was an 'important step in the history of the theory of planetary motion . . ., a step which was eliminated by philosophical reasons in Copernicus' theory but again fully recognized in its importance by Kepler'. (Neugebauer, ibid., p. 171; cf., Hon, op. cit. (1), p. 559.)

[109] Lloyd, ibid., p. 198.

[110] The existence of a Greek star-catalogue of over 1000 stars which gives longitude, latitude and magnitude determinations for each star, is considered another evidence—regardless of the controversy concerning its origin—of sustained observational work. (Ibid., pp. 183–184, 200; Dreyer, op. cit. (40), pp. 202–203; Neugebauer, op. cit. (13), pp. 68–69; Neugebauer, op. cit. (16), pp. 53–54, 280–292, 577, 836, 1087; Palter, op. cit. (14), p. 126.)

[111] Quoted by Lloyd, op. cit. (23), p. 133.

[112] Ibid., pp. 156–157; Lloyd, op. cit. (11), p. 182.

remarks,

> repetition of experiments, cross-checks of experimental findings, rigid control over measurement procedures, scrupulous reporting of all measurements: these must have been exceptional, if they occurred at all, in ancient astronomy.[113]

Lloyd concurs with this view in concluding that although it is not doubtful that Ptolemy realized the importance of obtaining trustworthy data, parts of the *Syntaxis* show, nevertheless,

> little awareness of the need for the rigorous and repeated checking and control of results against accumulated evidence—or of the need for the meticulous recording and presentation of that evidence.[114]

To be sure, here one has to guard against the historical mistake of passing a negative judgement on the standard of ancient observations in the light of the requirements of modern procedures; there is however significant evidence of retrograde steps e.g. an abandonment of the proper methods available and a disregard for glaring discrepancies.

One such proper method which is very suitable for handling inaccurate data, namely the bracketing of a measurement result between upper and lower bounds, was never put to extensive use in Greek astronomy. This method which had originated in Greek mathematics and gained a proper physical basis in the work of Hipparchus, apparently lost its appeal once a planetary system became available. Lloyd suggests that a reason for this step is that the use of this method would have resulted in such complex computations that they would have become quite unmanageable.[115] It is indeed easier to operate with a parameter to which one value has been assigned rather than a dual one. However, it is one thing to consider such one-value parameter a tentative quantity for working purposes, and quite another to regard it as representing 'very nearly' the *true* value.

The lunar theory which had originated in Hipparchus' work and was later developed by Ptolemy, demands excessive variations in the moon's geocentric distance, and thereby in its apparent diameter.[116] However, this expected phenomenon never occurs in reality, and as J. L. E. Dreyer maintains, 'it cannot possibly have escaped Hipparchus and Ptolemy';[117] yet, they took no notice of it.[118] This lack of consideration for a glaring discrepancy between the observed phenomenon and the result of a theory, shows that Hipparchus and Ptolemy 'did not look upon their work as a real system of the world, but

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[113] Palter, op. cit. (14), p. 122.

[114] Lloyd, op. cit. (11), p. 200.

[115] Lloyd, op. cit. (23), p. 155. Lloyd thus holds that the deductive articulation of Ptolemy's theories has effectively ruled out in most cases the use of upper and lower limits for the main fundamental parameters. (Ibid., p. 156.) Neugebauer on his part observes that Ptolemy 'resorted to mere approximations when higher accuracy implied too heavy a burden of numerical computations'. (Neugebauer, op. cit. (16), p. 145.)

[116] The epicycle-eccentric model of Hipparchus and Ptolemy for the sun and moon has been hailed as 'the outstanding example, from the ancient world, of a theory that combined the mathematical rigour the Greek scientists demanded with a detailed empirical base'. (Lloyd, op. cit. (11), p. 200.)

[117] Dreyer, op. cit. (40), p. 201. Ptolemy indeed records his awareness of this discrepancy. (Lloyd, op. cit. (23), p. 139.)

[118] Dreyer, ibid., p. 196.

merely as an aid to computation'.[119] Dreyer argues that Ptolemy's epicyclic theory 'was merely a means of calculating the apparent places of the planets without pretending to represent the true system of the world, and it certainly fulfilled its object satisfactorily, and, from a mathematical point of view, in a very elegant manner'.[120] Indeed, Ptolemy generally begins the theory of a particular aspect of a planet's motion by saying 'let us imagine . . . a circle'.[121] Even in the *Planetary Hypotheses*—Ptolemy's other astronomical treatise in which he attempted to establish a true physical account of planetary motions[122]—Ptolemy admits in the introduction that 'I do not profess to be able thus to account for all the motions at the same time; but I shall show that each by itself is well explained in its proper hypothesis'.[123] This admission of Ptolemy strengthens the view that Ptolemy's system is not really a system, let alone a physical system, but rather a string of mathematical hypotheses.[124]

It seems safe to conclude that in Greek astronomy the context in which observational results were incorporated into theories is not that of testing but rather corroborating. In their paper which bears the significant title, 'Qualitative Measurement in Antiquity', A. Aaboe and D. J. de Solla Price arrive at the conclusion that

> the role of instruments in antiquity was to serve convenience rather than precision, and that the characteristic type of measurement depended not on instrumental perfection but on the correct choice of crucial phenomena. If such phenomena could be welded together in a matrix of mathematics, the agreement between observation and theory was perfect. Needless to say, if one phenomenon did not fit, it had to be rejected as inaccurate and imperfect.[125]

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[119] Ibid., p. 201. The phenomenon of annular solar eclipse is another case in point. Since Ptolemy assumed that the apparent lunar diameter equals the apparent solar diameter when the moon is at its maximum geocentric distance (previous astronomers had assumed equality for the moon at mean distance), he in effect denied the possibility of annular solar eclipse. However, in all probability such a phenomenon was observed still in his lifetime. But, as Neugebauer remarks, 'neither then nor during the next 1400 years was the obviously necessary modification . . . undertaken'. (Neugebauer, op. cit. (16), pp. 104, 111.) Kepler studied carefully reports of such an eclipse and considered them correct. (Hon, op. cit. (1), p. 579.) In general, Kepler did not rest until he was able to reconcile all aspects of theory and observations, whereas Ptolemy's theory had been accepted for centuries without any attempt to eliminate its defects. (Neugebauer, op. cit. (16), p. 98.) 'I have built up a theory of Mars', Kepler writes to his teacher, Maestlin, 'such that there is no difficulty about agreement between calculation and the accuracy of observational data'. (Quoted by Koyré, op. cit. (1), p. 397 note no. 4.)

[120] Dreyer, ibid., p. 196.

[121] Ibid., p. 201.

[122] On the *Planetary Hypotheses* see Pedersen, op. cit. (83), pp. 391ff; Lloyd, op. cit. (11), p. 199; Neugebauer, op. cit. (16), pp. 900ff.

[123] Quoted by Dreyer, op. cit. (40), p. 196.

[124] Lloyd disagrees with this interpretation. In his view, Ptolemy's work 'is *not* simply and solely a piece of pure mathematics'. (Lloyd, op. cit. (11), p. 198, my emphasis.) According to Lloyd, Ptolemy 'hoped for a true physical account, indeed one that covered not just the kinematics, but also the dynamics, of heavenly movement'. (Lloyd, ibid., p. 199.) However, in Neugebauer's view, the *Planetary Hypotheses* seems on the face of it to suggest some mechanism which connects the motions of the planets within a larger cosmic system, but in fact nothing of this kind is achieved. 'No planet is influenced by the motion of any other one and the only unifying principle is their confinement into contiguous but strictly separated compartments . . .' (Neugebauer, op. cit. (16), p. 922.)

[125] Aaboe and Price, op. cit. (54), pp. 3–4.

Thus, when Neugebauer writes that 'the ancient astronomers rightly had greater confidence in the accuracy of their mathematical theory than in their instruments',[126] he appears to be giving these astronomers undue credit as this judgement implies that they fully appreciated problems of observation—particularly the limitations of their instruments—and the interplay that exists between mathematical theories and observations. As Aaboe and Price show, the accuracy which ancient astronomers sometimes present through complicated numbers is in fact an illusion; this illusion of accuracy is the result of crude numerical data having been fed into the mathematical machinery.[127] 'Far from any march towards precision by way of instrumental improvement in antiquity, we find,' write Aaboe and Price, 'but a predominant concern with the mathematical niceties of such theory'.[128]

In sum, elaboration of theory, particularly its mathematical basis and geometrical constructs, constituted the central concern of the Greek astronomers. However, this preoccupation with theory at the expense of observation should not be construed as signifying an insight into the limitations of the available observational techniques. Rather, it is an indication that observations played a secondary role: they were used mainly as illustrations, not as a means of testing.

In view of this analysis of Greek astronomy, my principal argument may be formulated thus: in a science where empirical results are more often used for the purpose of illustrating and supporting theories rather than testing them,[129] one would not expect a clear grasp of the concept of experimental error. To be sure, one cannot really deny that the Greek astronomers had some implicit notion of the problem of experimental error. However, this implicit notion did not develop into explicit methodological procedures intended to be applied rigorously to account for the occurrence of experimental errors. Rather, this notion remained stagnant, if it did not regress. Again, in an astronomy where the observations, as Lloyd puts it, 'are cited to illustrate and support particular doctrines'; where 'the observations are sometimes already interpreted in the light of the theories they were meant to establish', and where the support is in many cases exaggerated:[130] in such an astronomy there is no room for a developed concept of experimental error. This concept can emerge only in a context where the empirical results are given their due weight as a means of testing theories, and the theories are construed as reflecting real physical relations and not mere mathematical relations between abstract entities.

I have argued elsewhere that in Kepler one can find a combination of ideas which was conducive to this proper understanding of empirical results and the role experimental errors play in their evaluation. This understanding, I have argued, emerged not only from

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[126] Neugebauer, op. cit. (13), p. 185. Elsewhere Neugebauer writes that 'it makes no sense to praise or to condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data'. (Neugebauer, op. cit. (16), p. 108.)

[127] Aaboe and Price go on to say that 'the simple numbers however produce results that agree remarkably well with the facts, so that we must marvel at the way in which the choice and simple numbers were injected into suitably interlocking chains'. (Aaboe and Price, op. cit. (54), p. 20.)

[128] Ibid.

[129] Lloyd, op. cit. (11), p. 200.

[130] Ibid., p. 221.

the pioneering work of Tycho Brahe who had heralded the modern method of observation—the continuous observation to the point of overdetermining the phenomenon[131]—but also from the genius of Kepler in which the idea of unity was combined with the belief in physical realism.[132]

By way of conclusion, I should like to extend a distinction which Lloyd has drawn to facilitate the discussion on errors of observation. In his paper on observational error in later Greek science, Lloyd distinguishes between 'problems that arise from the conditions under which the object is to be observed or from the nature of the object itself, and those that relate to the means or method of observation'.[133] Lloyd is aware that the distinction is broad and cannot always hold firmly.[134]

Under the first category, that is the category of conditions of observation, Lloyd includes any type of interference arising from atmospheric conditions.[135] Ptolemy, for example, suggests that

> the same angular distances appear greater to the eye near the horizon, and less near the zenith, and so for this reason it is clear that they can be measured sometimes as greater and sometimes as less than the real angular distance.[136]

However, it is worth noting that although Ptolemy recognized and indeed studied the phenomenon of refraction, he does not make any systematic correction to accommodate this hindrance.[137] As an example for problems which arise in the conditions created by the object itself, Lloyd suggests the case of determining solstices and equinoxes. At a solstitial point, the sun is either at its maximum or minimum declination. Two days away from a solstitial point, the sun's declination differs but 1′ from the extreme value; and after five days the declination changes by as little as 6′. By contrast, the declination of the sun changes at an equinox by about 24′ per day. Thus, whereas an equinoctial point can be determined to an accuracy of 1/4 day (allowing for an inaccuracy of 6′), a solstitial point cannot be located directly to an accuracy better than some three or four days.[138] Ptolemy was aware of this difference and indeed remarks on the greater accuracy of equinox observations.[139]

The second category of Lloyd's classification of observational errors consists of problems which arise in the means and method by which the phenomenon is observed. Here Lloyd includes the use of sighting aids or other instruments.[140] The experiments of Archimedes on the dioptra, as reported in the *Sand-Reckoner*, constitute such a case.

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[131] Aaboe and Price, op. cit. (54), p. 16; Neugebauer, op. cit. (16), p. 1089.

[132] Hon, op. cit. (1). Cf., N. 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.

[133] Lloyd, op. cit. (23), p. 133.

[134] Ibid., pp. 133–134.

[135] Ibid., pp. 134–135.

[136] Quoted by Lloyd, ibid., p. 135; cf. ibid., note 12.

[137] Ibid., p. 134. However, as Neugebauer remarks, 'it should be remembered how difficult the problem still appeared to Brahe and Kepler when it was taken up around 1600'. (Neugebauer, op. cit. (16), p. 896.)

[138] Aaboe and Price, op. cit. (54), p. 9.

[139] Lloyd, op. cit. (23), p. 135.

[140] Ibid., pp. 136ff.

Another example is Ptolemy's rejection of the unsound attempt to establish the apparent diameter of the sun or moon by means of the water-clock.[141]

This broad classification which Lloyd introduces, constitutes a preliminary step towards a general taxonomy of types of experimental error. Although the distinction Lloyd draws applies only for observational errors, the motivation is the same: to clarify the problem of experimental error, one may classify different types of errors which arise in different contexts; or, to use Wittgenstein's expression, 'mustn't one make a distinction between the ways in which something "turns out wrong"?'[142] Lloyd classifies, as we have seen, two types of observational errors: those that are associated with either the external or internal conditions of the observed object, and those that pertain to a particular method of observation. In the light of my discussion on Kepler's awareness of the problem of experimental error, it becomes apparent that a general and yet refined classification—more than the one Lloyd applies to Greek astronomy—is needed in order to truly reflect Kepler's novel understanding of the problem of experimental error. Such a classification will have to take into consideration not only errors that pertain to observation, but also errors that may arise in the theoretical background—especially in the underlying assumptions and the theory of the apparatus—the actual employment of the apparatus as well as the reduction of the recorded data and their interpretation.[143]

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[141] Op. cit. (28, 30, 97).

[142] L. Wittgenstein, *On Certainty,* (eds G. E. M. Anscombe and G. H. von Wright, trs D. Paul and G. E. M. Anscombe), Oxford, 1977, p. 84e (#641).

[143] Hon. op. cit. (1), p. 591. Cf., G. Hon, 'On the Concept of Experimental Error', Ph.D. Thesis, London University, (1985), Ch. IV: A Classification of Types of Experimental Error; G. Hon, 'Towards a Typology of Experimental Errors: an Epistemological View', *Studies in History and Philosophy of Science,* **20.**