Was There a Crisis Before the Copernican Revolution? A Reappraisal of Gingerich's Criticisms of Kuhn[1]

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In this essay I will discuss and appraise two conflicting answers to the question of whether there was a crisis in Ptolemaic astronomy prior to the Copernican revolution. I will begin by giving a brief account of why anybody should be interested in this question. I will discuss the two conflicting answers of Kuhn (1962, 1970), who claims to present evidence which shows that Ptolemaic astronomy was anomaly-ridden at the time of Copernicus, and of Gingerich (1975), who claims that the supposed anomalies, which have been passed down in the historical literature due to poor scholarship, are fictitious. Finally I will appraise Gingerich's criticism of Kuhn, giving my own evaluation of Gingerich's arguments. I conclude that Gingerich's arguments against the existence of a technical crisis in Ptolemaic astronomy prior to Copernicus appear to be, either arguments against the efficacy of the Copernican system, or arguments based on definitions of complexity which are not directly attributable to Kuhn.

The general picture of science presented by Kuhn in *The Structure of Scientific Revolutions* (1962, 1970) has profoundly influenced the way philosophers, historians, and sociologists perceive scientific change. Kuhn initiated a tradition in the philosophy of science, perpetuated and refined by Gutting (1980), Lakatos (1978), Laudan (1977), and Feyerabend (1975), which makes any philosophical account of scientific change responsible to the historical evidence. Since 1962, there has been considerable disagreement over the capacity of Kuhn's picture to accurately describe scientific change. (Laudan et. al., 1986; Gholson and Barker, 1985)

According to Kuhn, scientific change takes place by a process of revolution in which the ontology, epistemology, and methodology, provided by one paradigm, is replaced by a new one. The old paradigm is called into question when a sufficient number of sufficiently severe experimental failures (anomalies) throw the paradigm into a crisis state. The old paradigm is then replaced with a new one during a revolutionary period in the history of that discipline, in which scientists may shift their commitment from the old ontology, epistemology, and methodology, to a new one. Then follows a period of normal science in which the new paradigm is further articulated.

When historians and philosophers look closely at historical episodes, however, they often cannot identify one or more of Kuhn's stages of scientific change. Particularly damaging to Kuhn's theory have been tests using the historical episodes which Kuhn

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himself uses to support his theory. One of the pillars of historical evidence which supports Kuhn's account of scientific change is the Copernican revolution. Any argument against the existence of a crisis in Ptolemaic planetary astronomy prior to Copernicus, based on a new interpretation of the historical episode, would seriously undercut the historical support for Kuhn's philosophical account.

There have been two conflicting views of the state of science prior to the Copernican Revolution. Kuhn (1962, 1970) finds that a technical crisis in Ptolemaic astronomy did exist prior to Copernicus. Copernicus is supposed to have perceived this technical crisis and so motivated to search for a replacement. But the historian of science Owen Gingerich, writing in 1975, claimed that a close examination of the historical record does not reveal that a technical crisis existed. As perhaps the leading U.S. scholar of Copernicus, Gingerich's criticisms carry special weight.

I will deal exclusively with the technical component of the crisis in this discussion for several reasons. First, Kuhn believes that a technical breakdown in the Ptolemaic system of calculating planetary position was at the core of the crisis. Second, Gingerich criticizes Kuhn because he cannot find historical evidence for a technical breakdown, at least according to the definitions of Kuhn (1962, 1970) and de Vaucouleurs (1957). Third, they both agree that an 'aesthetic' component, to use Gingerich's term, motivated Copernicus to introduce a new system, although they disagree to some extent over the importance of that component. According to Kuhn, Copernicus' prefatory statement in *De Revolutionibus* (1543), that he had inherited an astronomical tradition which had finally created only a monster, is primarily a statement on the technical condition of the Ptolemaic paradigm. Any 'aesthetic' connotations are therefore secondary. According to Gingerich, bringing about a fixed symmetry of the parts of the universe, i.e. eradicating the monster from astronomy, was first and foremost a part of Copernicus' grand aesthetic view. It was only secondarily a response to technical crisis, if at all. Resolving this disagreement between Kuhn and Gingerich would entail determining the impact of aesthetic considerations on Copernicus' decision to undertake the development of a new planetary theory. I gladly leave this problem and return to the issue of technical crisis.

According to Kuhn (1962, 1970), the state of Ptolemaic astronomy was a scandal before Copernicus. Predictions made with Ptolemy's system of calculating planetary positions never quite conformed to the best available observations of planetary position, or with the precession of the equinoxes. Therefore, the activity of normal science within the Ptolemaic paradigm consisted of attempts to reduce and eventually eliminate the discrepancies between theory and observation. Kuhn says that the attempts were made by some particular adjustment of the system of compounded circles. However, the complexity which resulted from the adjustments increased more rapidly than the reduction of discrepancies. In fact, discrepancies were merely displaced and showed up elsewhere. Several astronomers, including those working on calendar reform in the thirteenth century under the patronage of King Alfonso X of Castille, and Domenico da Novara and Copernicus in the sixteenth century, were skeptical that the Ptolemaic system did not describe the true state of nature. The system was just too complex.

Two remarks on the account which Kuhn presents are in order at this point in the discussion. First, Derek De Solla Price (1959, 1960) claims that Ptolemy's mathematical explanation of nature, in the *Almagest*, worked perfectly within the limits of any observations possible with the naked eye during the Hellenistic period. This contradicts Kuhn's statement that theory *never quite fit* observations.

Second, Kuhn (1962, 1970) does not tell us who actually made the adjustments in the Ptolemaic system of compounded circles which increased the complexity of the system. Nor does he tell us in *The Copernican Revolution* (1957). He refers to

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evidence presented in *A History of Astronomy from Thales to Kepler* (1953) by J.L.E. Dreyer. Upon review of the evidence which Dreyer presents I find that he does not provide conclusive support for Kuhn's point, that attempts to reconcile Ptolemaic theory with observation caused the complexity of the theory to increase more rapidly than the reduction of the discrepancies. According to Dreyer, all attempts at rebellion against the Ptolemaic system (Kuhn would say they were within the Ptolemaic paradigm) by Oriental astronomers, turned out to be failures.[2] It was impossible to find anything better than what Ptolemy had produced. This seems to have been the case in the West also. Dreyer points out that although Peurbach and Regiomontanus wrote treatises on Ptolemaic astronomy neither made any significant theoretical advances. In other words, although attempts were made to adjust the Ptolemaic system of compounded circles the adjustments were minor failures or complete failures which were rejected as being useless. Therefore, the planetary theory described by Ptolemy in the *Almagest* remained relatively unadulterated during the "revival" of astronomy in Europe during the fifteenth and sixteenth centuries.

In Kuhn's account of the crisis state which precipitated the Copernican revolution, the early sixteenth century found the Ptolemaic paradigm failing in application to its own traditional problems. According to Kuhn, Copernicus' prefatory remarks in *De Revolutionibus* are an indication that he had recognized the persistent failure of the Ptolemaic paradigm and that this recognition had motivated him to search for an alternative. Several technical failures prompted the search. First, 'mathematicians' were so unsure of the movements of the sun and the moon that they could not even explain or observe the length of the seasonal year. Second, there was no consensus on which method was appropriate for determining the positions of the five planets, some used only homocentric spheres, others used eccentrics and epicycles. The homocentric sphere approach had failed to save the phenomena. The eccentric and epicycle approach had saved the phenomena, but in due course had admitted the use of the equant which seemed to Copernicus to violate the first principle of uniform motion. Thus, Copernicus was conscious of a technical crisis in the Ptolemaic paradigm and responded in a manner consistent with Kuhn's theory of scientific change.

From Kuhn's account of the technical crisis which supposedly precipitated the Copernican revolution, I now turn to Gingerich's. Gingerich (1975) claims that the Ptolemaic system was not in a state of technical crisis in the early 1500s. He makes three arguments against the existence of such a technical crisis. First, errors in calculating planetary orbits were the same using a Ptolemaic and a Copernican system of calculation. Gingerich graphs the errors in the planetary positions predicted by two leading sixteenth century ephemeris-makers. Johannes Stoeffler (1452-1531), who used the Ptolemaic system of calculation, extended the planetary predictions made earlier by Regiomontanus. Johannes Stadius was the first computor to adopt the Copernican parameters for a major ephemeris. His tables, probably generated in the 1560s, were the successors to Stoeffler's.

According to Gingerich, a map of the errors for each ephemeris reveals that the errors reached approximately the same magnitude before and after Copernicus. For example, in the ephemerides of Regiomontanus and Stoeffler the error in longitude for Mars reached nearly 5 degrees. Gingerich argues that failure of the Ptolemaic system to conform to observation could not have produced a technical crisis, because the Copernican theory was accepted with an equal failure to conform.

Second, Gingerich argues that concern over the complexity of the Ptolemaic system could not have initiated Copernicus' search and introduction of his new system. Copernicus could not have been profoundly concerned with simplifying technical complexity because he himself introduced a system in which the count of circles for the longitudinal mechanisms of the Sun, Moon, and planets was 18, three more than the

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Ptolemaic system. The substance of Gingerich's argument is if Copernicus had been profoundly concerned with complexity, he would have found his own system unacceptable.

Third, Gingerich claims that the increase in complexity of the Ptolemaic system, due to a multiplication of circles, did not contribute to the technical crisis because multiplication is a myth. An examination of the Alfonsine tables of the thirteenth century reveals that they were constructed using a pure Ptolemaic system, that is, with an eccentric, an equant, and a single epicycle for the superior planets. An examination of the tables calculated by Stoeffler reveals that he too used an unembellished Ptolemaic system, which he inherited from Regiomontanus. This point confirms Dreyer's opinion that Regiomontanus' treatise on Ptolemaic astronomy made no significant theoretical contributions. Thus, Stoeffler inherited a system of calculation from Regiomontanus which was a virtually unchanged version of the original system set out in the *Almagest*. Gingerich concludes that this evidence dispels the myth that the number of circles in the Ptolemaic system of calculating planetary position multiplied, causing an unacceptable level of complexity which contributed to the crisis state.

Gingerich appears to effectively dismiss Kuhn's argument that a technical crisis existed prior to Copernicus. However, a closer examination of the arguments reveals that this may not be the case. Gingerich's first argument is really one against the efficacy of the Copernican paradigm, not against the existence of a crisis which preceded its introduction. He concludes, from discovering that errors in predictions of planetary positions were as great by a Copernican method as by a Ptolemaic method, that these errors could not have been an important factor in the technical crisis. However, Copernicus may have recognized that the potential for his system to reduce the errors was greater than that of the Ptolemaic system.

Gingerich's second argument is similar to the first. Again, the argument that Copernicus' system was in some respects more complex than the Ptolemaic is *directly* an argument against the efficacy of the Copernican system, and not against the existence of a crisis state before Copernicus. If construed *indirectly*, as an argument that Copernicus would never have introduced a more complex system if complexity had been an important factor in the crisis, we can again say that Copernicus may have introduced it for its heuristic value.

Gingerich's third argument appears to be the most damaging. He attempts to dispel the myth that the Ptolemaic system precipitated a technical crisis because, by the sixteenth century, it had become intolerably complex. One problem with this argument is that he uses de Vaucouleurs' (1957) definition of complexity, that complexity was a *multiplication* of circles, and not Kuhn's definition, that the complexity was brought about by *adjusting* Ptolemy's system of circles. Nowhere in *The Structure of Scientific Revolutions* (Kuhn 1962, 1970) does Kuhn equate complexity with a multiplication of circles. I cannot find this equation in *The Copernican Revolution* (1957) either. Furthermore, Copernicus does not attribute complexity to a multiplication of circles in his prefatory justification for seeking an alternative to the Ptolemaic system. So although Gingerich shows that the number of circles did not multiply, it is not clear that Kuhn equates complexity with a multiplication of circles in the first place.

Is there an alternative basis for saying that the Ptolemaic system became more complex without attributing complexity directly to a multiplication of circles? Gingerich (1975) himself provides a clue. He says that in order to generate the Alfonsine tables, astronomers had augmented precessional motion with a trepidation device. Although Gingerich excludes this device when considering the complexity of the Ptolemaic system, he earlier implies that Copernicus' use of a trepidation device contributed to the complexity of his system.

Let me turn briefly to a source of support for my earlier point that Copernicus introduced his system as a replacement for Ptolemy's despite complexity and prediction problems, because he recognized its heuristic potential. This will also allow me to show that Gingerich dispels only part of the complexity myth. In Gingerich's argument against the existence of a technical crisis he claims that Stadius' Copernican ephemeris had the same degree of error as Stoeffler's Ptolemaic one. But Stoeffler was part of only one school of ephemeris-makers, the school which included Peurbach and Regiomontanus who employed eccentrics and epicycles. A second possible basis for ephemerides were the astronomical models using only homocentric spheres, revived in Europe by Fracastaro and Amici shortly before Copernicus death. We are told by Dreyer that Fracastaro, in his system, assumed the number of spheres to be 79.

Copernicus states in his preface that he was not reacting against the technical complexity of only one school, as Gingerich leads us to believe. Rather he was reacting against the technical complexity of having two competing schools, and against a school that Gingerich does not mention in his argument, which generated technical complexity far greater than that of either Ptolemy's or Copernicus' system. At the very least, this evidence indicates Copernicus recognized that his system had the potential to reduce the technical complexity of two systems into one. More likely, this realization was coupled with the knowledge that he was actually reducing the complexity of the system revived by Fracastaro and Amici, whom he mentions specifically in the Preface.

In conclusion, Gingerich's arguments against the existence of a technical crisis before Copernicus appear to be, either arguments against the efficacy of the Copernican system, or arguments based on definitions of complexity which should not be attributed to Kuhn. A strong interpretation of the evidence presented to show the incompleteness of Gingerich's argument against the existence of technical complexity which precipitated the crisis further undermines Gingerich's overall criticism of Kuhn.

I believe that Gingerich has misinterpreted Kuhn on crisis. However, this is in part a consequence of Kuhn's own vague description of the crisis state which preceded the Copernican revolution. Recall that Kuhn does not tell us in *The Structure of Scientific Revolutions* who was responsible for generating the technical complexity in the Ptolemaic system. He merely refers us to the work of Dreyer (1953) who discusses, both the school of ephemeris-makers which based its predictions on eccentrics and epicycles, and the school which based its predictions on homocentric spheres. We have to return to Copernicus prefatory remarks in *De Revolutionibus*, included in Kuhn's *The Copernican Revolution* (1957) to determine that Kuhn (1962) is describing Copernicus' reaction both to users of epicycles and eccentrics, and to users of homocentric spheres.

Notes

[1]Special thanks are due to Professor Peter Barker who provided critical comments during preparation of an earlier version of this paper for his Philosophy of Science Course at Virginia Polytechnic Institute and State University.

[2]Current historical research disputes Dreyer's conclusion about Oriental Astronomers. The members of the Maragha school, in particular, are now regarded as having made real advances over Ptolemy-some of which may have been known to Copernicus. For a review of current knowledge see Swerdlow and Neugebauer (1984) 41-8.

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