THE ELUSIVE SYNTHESIS: AESTHETICS AND SCIENCE

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1. EMPIRICAL AND AESTHETIC CONSIDERATIONS IN THEORY-CHOICE

Scientists choose among alternative available theories in part on empirical considerations, but in part also on aesthetic considerations. That is, their choices to adopt one theory in preference to another are determined partly by the degree to which they regard the theories in question as "beautiful", "elegant", or "aesthetically attractive". This paper is a contribution to the study of the aesthetic considerations to which scientists appeal in theory-choice, and of their role especially in revolutionary times.

In general, scientists decide cases of theory-choice by referring to a notion of what counts as an acceptable theory. This notion of acceptability will refer to a number of criteria by which theories may be evaluated. A theory's score on these criteria will determine whether it will prove acceptable to a particular scientist. Undoubtedly, the notion of acceptability to which most scientists have held is constituted partly by empirical criteria. Most scientists, through history, have possessed a concept of the empirical performance of theories, and have aimed to choose theories whose empirical performance will be good. They have identified properties of theories that are conducive to good empirical performances, and their empirical criteria have attached weight to these properties. Present-day scientists generally cite internal consistency, predictive accuracy, breadth of scope, degree of simplicity, and explanatory power as empirical properties of theories that they value particularly. (For a philosophical account, see e.g. Newton-Smith, 1981, pp. 226–232.) Although scientists in previous centuries have given differing analyses of theories' empirical properties, most scientific disciplines since the Renaissance have valued properties of theories akin to our "predictive accuracy", for instance.

However, in constructing their notion of what makes a theory acceptable, many scientists refer to concerns other than for the empirical performance of theories. Some of these concerns are aesthetic. Many scientists have possessed a concept of the beauty of theories; they have subjected to aesthetic appraisal the intellectual constructs that make up theories, and the verdicts of these appraisals have contributed to determining whether they deemed each theory acceptable.

It is impossible to give a straightforward list of properties of theories that scientists regard as conferring aesthetic value to a theory, since scientists in different disciplines and at different times have found wildly different sets of properties attractive. Any attempt to list properties conferring beauty to

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theories would thus have to be specific to discipline and time, at least. However, some general attempt can be made to analyse the experience that a scientist has when perceiving a theory as beautiful. Scientists commonly describe perceiving beauty in a theory as receiving an impression of aptness; they regard a theory as beautiful if they regard its properties as apt. Some scientists have explicitly characterized the beauty of theories in terms of aptness: for instance, Werner Heisenberg speaks of a theory as being beautiful if it shows "the proper conformity of the parts to one another and to the whole" (Heisenberg, 1970, p. 174). Heisenberg's characterization of beauty in theories has been endorsed by various scientists (e.g. Lipscomb, 1982, p. 4; Chandrasekhar, 1987, p. 70). Many others, while not offering any so explicit characterization of theoretical beauty, have alluded to feelings of aptness when describing their aesthetic response to theories.

Explicating the perception that a theory is beautiful as the perception that its properties are apt is consistent with many treatments of beauty in art criticism. We commonly speak of properties of an artwork as being appropriate, fitting, proper, or seemly. When we say of the conclusion of a musical composition or of a play that it is just what was demanded or could not have been different, we are signalling that we regard these elements of them as apt. In the evaluation of a work of art, the apparent aptness of its properties is a standard justification for attributing to it aesthetic value. Aptness has, in fact, been central to concepts of beauty since classical times. Greek art theorists, including Plato, knew it as *prepon*, and Roman writers, such as Vitruvius, as *decor*. It is because of the importance given to these concepts that, for instance, the consistent use of architectural orders in building was seen as ensuring beauty in an edifice (Pollitt, 1974: see pp. 217–218 on *prepon*, and pp. 341–347 on *decor*).

Of course, different properties of theories will strike different observers as being apt. For instance, while one scientist may experience a sense of aptness in discerning that a theory exhibits particular symmetries, another might experience it in discerning that a theory offers a visualization of phenomena in familiar terms. This fact explains why scientists have regarded many different properties as securing aesthetic value to theories.

I shall refer to any property of theories that is capable of producing a sense of aptness in one or another scientist as an "aesthetic property" of those theories. Under this terminological convention, calling a property "aesthetic" falls short of claiming that a given scientist will find theories that exhibit that property beautiful: rather, a scientist regarding a theory experiences a sense of aptness only upon discerning in it specified aesthetic properties, viz., those to which he or she attaches the value of beauty. Again, this usage conforms with standard talk. In passing an aesthetic appraisal of a building, for instance, we examine it for its aesthetic properties; however, what is required if we are to consider it beautiful is not that it should possess any aesthetic properties whatsoever, but that it should possess some of the aesthetic properties to which we attach value. I take it that which particular aesthetic properties of theories a scientist attaches value to is specified by that scientist's aesthetic evaluative criteria.

I consider scientists' aesthetic criteria to be just as central to scientists' notions of the acceptability of theories as are their empirical criteria. This interpretation of scientists' aesthetic considerations will find little favor with those philosophers of science who identify the "scientific" with the "empiricist". On their view, scientific activity is the construction of logically consistent theories to explain empirical data: all other concerns are external influences acting on science. For instance, Philipp Frank drew a distinction between two sets of criteria for theory-evaluation, which he termed the "scientific" and the "extra-scientific". His "scientific" criteria are "agreement with observations and logical consistency": all other criteria, doubtless including aesthetic criteria, are extra-scientific (Frank, 1957, p. 359). Such views of science would, I believe, lead us to misrepresent the construction of scientists' standards for the acceptability of theories, ignoring the role within them of various sets of considerations that cannot properly be described as empirical. In fact, both aesthetic and empirical criteria take part in determining scientists' notions of the acceptability of theories. This does not mean, of course, that we can draw no useful distinction between scientists' empirical and aesthetic considerations; but it does mean that the distinctions we draw between them cannot be portrayed as a demarcation between the scientific and the extra-scientific.

Once we have established that scientific communities evaluate theories both on their empirical and on their aesthetic properties, many interesting questions arise. The one on which this paper will focus is: what contributions do evaluations of these two sorts make to shaping the historical development of science? The answer today usually given to this question is that scientists' aesthetic preferences inspire them to inventiveness and iconoclasm, while their empirical norms hold them to continuity and conservatism. Perhaps this view descends from the images, widespread in present-day Western culture, of aesthetics-governed activity as the realm of imagination and daring and of data-governed activity as the realm of proof and punctiliousness. In the course of this paper, I shall outline a contrary view: I shall claim that, at crucial junctures of the history of science, scientists' empirical concerns lead them to invention and novelty in theorizing, while their aesthetic preferences among theories curb and resist innovation. We begin by examining one of the most elaborate formulations of the standard view, given by Thomas S. Kuhn.

2. KUHN'S VIEW OF AESTHETIC FACTORS

Kuhn sees in science two modes of development: the normal mode, in which scientists cultivate their discipline by progressing from theory to theory, and a revolutionary mode, in which scientists switch not just between theories but between paradigms. Kuhn considers that in both modes of development scientists choose among these intellectual constructs partly in the light of their empirical and aesthetic properties. However, these sets of properties play different roles in the two modes of development.

According to Kuhn, aesthetic factors play no decisive role in theory-choice within normal science. He says that, in the puzzle-solving of which normal science consists, the usual stimulus for scientists' coming to embrace a new theory is its being demonstrated empirically superior to its competitors. Kuhn has formulated five criteria, including those of predictive accuracy and degree of simplicity, on which one theory may be judged empirically superior to another (Kuhn, 1977, pp. 321–323).

By contrast, a new paradigm's empirical properties will typically not enable it to poach adherents from a better-established paradigm, Kuhn believes. After all, he says, a mature paradigm will have developed problem-solving resources that new paradigms are unable to match. Therefore, scientists in a revolutionary crisis will typically find their estimates of the competing paradigms' empirical properties weighing in favor of their current paradigm, and inhibiting paradigm-switch (Kuhn, 1962, pp. 156–157).

Kuhn identifies the factors that tend to induce paradigm-switch in arguments of a different sort: "These are the arguments, rarely made entirely explicit, that appeal to the individual's sense of the appropriate or the aesthetic – the new theory is said to be 'neater', 'more suitable,' or 'simpler' than the old" (*ibid.*, p. 155). Kuhn suggests that, without the contribution of such arguments, it might be impossible for a world-view to develop into a paradigm dominant in its community:

> The importance of aesthetic considerations can sometimes be decisive. Though they often attract only a few scientists to a new theory, it is upon those few that its ultimate triumph may depend. If they had not quickly taken it up for highly individual reasons, the new candidate for paradigm might never have been sufficiently developed to attract the allegiance of the scientific community as a whole. (*Ibid.*, p. 156)

This means that, at times when scientists are deliberating whether to switch paradigm, their empirical and aesthetic considerations weigh on opposite sides. Empirical considerations will militate in favor of preserving the status quo, since the well-established paradigm will generally have superior problem-solving capability. But aesthetic considerations can sometimes outweigh this conservative bias:

> Something must make at least a few scientists feel that the new proposal is on the right track, and sometimes it is only personal and inarticulate aesthetic considerations that can do that. Men have been converted by them at times when most of the articulable technical arguments pointed the other way. When first introduced, neither Copernicus' astronomical theory nor De Broglie's theory of matter had many other significant grounds of appeal. (*Ibid.*, p. 158)

Kuhn is willing to test these claims against the history of science. An appropriate test is performed as follows. We must identify a theory of which the adoption, we agree, constituted a revolution in some branch of science. We must then ascertain what role the empirical and aesthetic properties of that theory and its displaced predecessor played in either inducing or inhibiting the paradigm-switch. Kuhn's model of revolutions would be shown to accord with this episode of the history of science just if we found that the paradigm-switch was inhibited by empirical considerations and induced by aesthetic factors.

As a suitable test-case, Kuhn picks the transition from Ptolemy's to Copernicus's theory in mathematical astronomy, which he maintains constituted a revolution (Kuhn, 1957, p. 134; 1962, pp. 149–150). He reconstructs the grounds on which mid-sixteenth-century mathematical astronomers decided between these theories. Kuhn claims that the Copernican theory could not have won adherents from Ptolemy's theory on the grounds of either predictive accuracy or degree of simplicity: "Judged on purely practical grounds, Copernicus' new planetary system was a failure; it was neither more accurate nor significantly simpler than its Ptolemaic predecessors" (Kuhn, 1957, p. 171). Rather, Kuhn believes that Copernican theory gained adherents on the strength of its aesthetic properties. According to Kuhn, the arguments advanced in *De revolutionibus* show that Copernicus himself was aware that he could attract Ptolemaic astronomers to his theory most effectively by stressing its aesthetic virtues:

> Each argument cites an aspect of the appearances that can be explained by *either* the Ptolemaic *or* the Copernican system, and each then proceeds to point out how much more harmonious, coherent, and natural the Copernican explanation is. (. . .) Copernicus' arguments are not pragmatic. They appeal, if at all, not to the utilitarian sense of the practicing astronomer but to his aesthetic sense and to that alone. (. . .) The harmonies to which Copernicus' arguments pointed did not enable the astronomer to perform his job better. New harmonies did not increase accuracy or simplicity. Therefore they could and did appeal primarily to that limited and perhaps irrational subgroup of mathematical astronomers whose Neoplatonic ear for mathematical harmonies could not be obstructed by page after page of complex mathematics leading finally to numerical predictions scarcely better than those they had known before. (*Ibid.*, p. 181, emphasis as in the original; see also *ibid.*, p. 172)

Kuhn concludes that Copernicus's theory established itself in virtue primarily of its aesthetic properties and despite being able to demonstrate no empirical superiority over Ptolemy's theory. Therefore, he judges that, *qua* paradigm-switch, the transition from Ptolemaic to Copernican mathematical astronomy accords with his view of the role of aesthetic factors in revolution.

I shall try to show that Kuhn has misread the roles played by empirical and aesthetic factors in scientific revolutions. I trace my disagreement out as follows. Kuhn has advanced three interrelated claims about scientists' aesthetic preferences and scientific revolutions:

1. Theory-succession within a paradigm (i.e., in normal science) is typically prompted by empirical factors; paradigm-switch in a scientific revolution is typically prompted by aesthetic factors and inhibited by empirical factors.
2. The transition from Ptolemy's to Copernicus's theory constituted a revolution in mathematical astronomy.
3. In the choice open to mid-sixteenth-century mathematical astronomers between Ptolemy's theory and Copernicus's, the switch to Copernicus's

theory was encouraged by aesthetic factors (viz., the aesthetic properties of Copernicus's theory) and inhibited by empirical factors (viz., comparisons of the empirical capabilities of the two theories).

Claim 2 specifies how, in Kuhn's view, the transition from Ptolemy's theory to Copernicus's may be adduced as evidence in evaluating claim 1. On the assumption of claim 2, claim 3 presents that historical episode as evidence favorable to 1. In what follows, I endorse claim 3, but reject 1 and 2. I shall argue that Kuhn's own findings about the transition from Ptolemaic to Copernican theory ought to persuade us that this episode constituted no revolution. Copernican theory was able to attract adherents through the appeal of its aesthetic properties precisely because of its conservatism, its fulfilment of aesthetic canons that had long shaped the preferences of mathematical astronomers. I shall proceed to sketch a revised model of scientific revolution, that suggests that the factors which Kuhn sees as tending to induce revolutions tend in fact to inhibit them, and vice versa. I shall support some elements of this model by further reference to early modern mathematical astronomy.

3. HOW DID COPERNICAN THEORY ATTRACT ADHERENTS?

If mid-sixteenth-century mathematical astronomers had been attracted to Copernicus's theory on empirical considerations, we would expect them to have been able to portray it as superior to Ptolemy's theory in such qualities as predictive accuracy or degree of simplicity.

There are two classes of predictions in which the accuracy of Copernicus's theory could be compared to that of Ptolemy's: quantitative predictions of the positions of the celestial bodies, and qualitative predictions of the appearance of the night sky from the Earth.

Take first the quantitative predictions of Copernicus's theory. There is little evidence either that Copernicus's work was motivated by dissatisfaction with the current accuracy of astronomical predictions, or that it brought an improvement in that accuracy. The quantitative track record of the Ptolemaic theory in the sixteenth century was still generally perceived as good: the claim advanced by some old historiography that by the mid-sixteenth century the Ptolemaic theory had led astronomy into an "empirical crisis", which Copernicus resolved, is not tenable (Gingerich, 1975). At the opening of both the *Commentariolus*, a treatise which he composed probably in 1510–14, and *De revolutionibus*, Copernicus pronounces himself content with the accuracy of the predictions of planetary positions yielded by Ptolemaic theory (Swerdlow, 1973, p. 434; Copernicus, 1543, p. 4). The numerical predictions of the Ptolemaic and the Copernican theories have been compared by several historians of mathematical astronomy, who have found the latter no more accurate than the former (Price, 1959, pp. 209–212; Gingerich, 1975, pp. 85–86; Cohen, 1985, pp. 117–119). Furthermore, a comparison of the predictive accuracy of the Ptolemaic and Copernican theories would have required data more precise than were available in Copernicus's lifetime, or for decades to follow. Thus, even if the Copernican theory had yielded quantitative predictions more accurate than its Ptolemaic competitor, this superiority would not have been apparent to astronomers of the time.

Copernicus's theory failed to establish a clear superiority over its Ptolemaic competitor also in qualitative predictions about the appearance of the heavens. For instance, many of his contemporaries reasoned that, if the Earth truly moved, the apparent positions of stars viewed from the Earth should oscillate, by the effect of parallax. The fact that no such oscillations could be observed supported Ptolemaic theory better than Copernican. As a second example, take the problem of accounting for the observation that the apparent luminosity of Venus is approximately constant. The present-day explanation of this fact is that the apparent luminosity of Venus depends both on its distance from the Earth and on its phase, or proportion of the planet's orb which reflects light towards the Earth; and the effects of these two quantities compensate for one another almost exactly. But the phases of Venus were first detected only in 1610 by Galileo. Ptolemaic theory suggested that the distance of Venus from the Earth varies greatly: if that were true, one would expect the apparent luminosity of Venus to vary correspondingly widely. But Copernicus's theory predicts a similar variation in the Earth-Venus distance, and offers no separate explanation for the constancy of the apparent luminosity of Venus. So the Ptolemaic and Copernican theories fared about equally poorly in accounting for this observation (Price, 1959, pp. 212–214).

On the basis of such considerations, Robert Palter concludes that Copernicus's theory was not perceptibly superior to Ptolemy's in predictive accuracy. "In order to square this fact with the putative reality of a 'Copernican revolution' ", according to Palter, "one is constrained to fall back on the criterion of simplicity" (Palter, 1970, pp. 114–115). If Copernicus's theory had a degree of simplicity greater than Ptolemy's, although it may offer no greater predictive accuracy, it could still be portrayed as empirically superior to the latter.

Many historians and philosophers of science have suggested that the degree of its simplicity was the chief virtue of the Copernican theory, and the property on the strength of which it in fact attracted support. For instance, Hans Reichenbach writes: "Copernicus (. . .) was able, in fact, to cite as a distinct advantage only the greater simplicity of his system" (Reichenbach, 1927, p. 18). All too often, however, present-day estimates of the relative degrees of simplicity of the Ptolemaic and Copernican theories have been naive, consisting merely of a count of the circles in the geometrical constructions to which the theories appealed: Ptolemy's 80-odd circles are routinely contrasted with the 30 or so required by Copernicus's theory (e.g. Kordig, 1971, p. 109; for further details and examples of the count see Palter, 1970, pp. 94 and 113–114; and Cohen, 1985, p. 119). While overall numbers of circles may contribute to determine the theories' degrees of simplicity, they cannot be taken as the definitive measure of this parameter. (For some problems encountered in comparing theories in simplicity, see McAllister, 1991.) Indeed, on another set of criteria, Owen Gingerich judges that "the Copernican system is slightly more complicated than the original Ptolemaic system" (Gingerich, 1975, p. 87).

Among the many different standpoints from which the degrees of simplicity of the Ptolemaic and Copernican theories could be compared, let us choose one that might hold significance for Renaissance mathematical astronomers themselves. Their typical task was to calculate the apparent position of a planet viewed from the Earth. No calculation of this sort on Ptolemaic theory required the use of all its 80-odd circles: it needed no more than the six or so circles governing the motions of the planet to which the problem referred. Copernicus's theory, by contrast, supposed that both the Earth and the planet, whose apparent position was required, were in motion. Therefore, the position of a planet as seen from the Earth at some moment could not be calculated on Copernican theory without referring to the circles governing the motions of both bodies. In this sense, as a set of solutions to individual problems, Ptolemaic theory is simpler and more convenient – if somewhat less systematic – than that of Copernicus (Hanson, 1961, pp. 175–177).

In fact, the Copernican theory was not at the time of its enunciation reputed to be any simpler than the Ptolemaic theory (Cohen, 1960, p. 58; Neugebauer, 1968). There is evidence that, in his maturity, even Copernicus realized he could claim on behalf of his system a degree of simplicity no greater than that of the Ptolemaic theory. His early work, the *Commentariolus*, had suggested that his theory was simpler than the Ptolemaic theory (Swerdlow, 1973, pp. 434–436). If Copernicus had maintained this belief, he would surely have repeated and elaborated on it in his more systematic treatise, *De revolutionibus*, just as most of the other arguments in defense of the Copernican theory that appear in the *Commentariolus* receive an extended treatment in the later work. Instead, *De revolutionibus* omits claims that Copernicus's theory was simpler than Ptolemy's (Pera, 1981, pp. 157–159).

Neither predictive accuracy nor degree of simplicity thus emerge as decisive factors in favor of the Copernican theory. On what grounds then did Copernicus's theory prove preferable to Ptolemy's? As Kuhn and other present-day historians have documented, these grounds were primarily its aesthetic properties (Neugebauer, 1968, p. 103; Neyman, 1974, p. 9; Gingerich, 1975, pp. 89–90; Hallyn, 1987, pp. 73–103; Hutchison, 1987, pp. 109–136; and Westman, 1990, pp. 171–172). Evidence that sixteenth-century astronomers were attracted to Copernican theory by its aesthetic properties is contained in, for instance, the admiration for Copernicus's achievement that Tycho Brahe expressed in a letter of 1587 to the astronomer Christoph Rothmann:

> Copernicus (. . .) had the most perfect understanding of the geometrical and arithmetical requisites for building up this discipline (of astronomy). Nor was he in this respect inferior to Ptolemy; on the contrary, he surpassed him greatly in certain fields, particularly as far as the device of fitness and compendious harmony in hypotheses is concerned. (Quoted in the translation of Moesgaard, 1972, p. 38)

Copernicus himself expected that his theory would win support on its aesthetic virtues. He claims as the chief merit of his theory an internal harmony greater than that of Ptolemy's:

Those who devised the eccentrics seem thereby in large measure to have solved the problem of the apparent motions with appropriate calculations. But meanwhile they introduced a good many ideas which apparently contradict the first principles of uniform motion. Nor could they elicit or deduce from the eccentrics the principal consideration, that is, the structure of the universe and the true symmetry of its parts. On the contrary, their experience was just like some one taking from various places hands, feet, a head, and other pieces, very well depicted, it may be, but not for the representation of a single person; since these fragments would not belong to one another at all, a monster rather than a man would be put together from them. (Copernicus, 1543, p. 4; see also p. 22. For further discussion, see Rose, 1975, and Westman, 1990, pp. 179–182)

We may thus join Kuhn in his conviction that Copernicus's theory owed its adoption to aesthetic factors. However, the import of this conclusion for models of scientific revolutions does not emerge until it has been ascertained whether the adoption of Copernicus's theory in fact constituted a revolution.

4. THE CONSERVATISM OF COPERNICUS

In the fourth century B.C., Aristotle had enunciated three principles in cosmology: a principle of geocentricity; a principle of distinct physical regions, which held that the physical nature of the sublunary region (composed of the Earth and its atmosphere) differs from that of the supralunary region (containing the celestial bodies); and a principle of the circularity and uniformity of celestial motions, which held that the heavenly bodies move with uniform linear velocities along paths that are circles or compounds of circles.

The latter two principles, in particular, were deeply intertwined in Aristotelian natural philosophy. Objects in the sublunary region, composed of the four elements traditionally cited in ancient cosmologies, were subject to violent or forced motions, in which they were displaced from their natural locations. By contrast, celestial bodies were composed of a fifth element or quintessence, ether, which gave them perfection and ensured that they moved only with motions natural to them. These motions were circular and uniform (Randall, 1960, pp. 153–162).

Aristotle's corpus did not contain a theory of mathematical astronomy, but his three cosmological principles imposed constraints on how the heavens might be mathematically described. Mathematical astronomy in succeeding centuries was profoundly influenced by them, but not equally strongly by all three. The principle most frequently disputed was that of geocentricity. Pythagorean astronomers, such as Aristarchus of Samos in the third century B.C., flatly rejected it, embracing heliocentrism in its place (Heninger, 1974, pp. 127–128). Heliocentrism was supported also by forms of sun-worship, which retained its popularity into the Renaissance. Another astronomical system watered down geocentrism by portraying the sun, moon and outer planets as orbiting the Earth, but Mercury and Venus as orbiting the sun. This theory, first propounded by Heraclides of Pontus in the fourth century B.C., was widely endorsed by learned people throughout the Middle Ages.

Aristotle's most faithful disciples were naturally anxious to supplement his physical cosmology with a theory of mathematical astronomy which adhered as closely as possible to all his cosmological principles. Such astronomers as Apollonius of Perga (third century B.C.) and Hipparchus (second century B.C.) accepted the constraints of the principles of geocentricity and of the circularity and uniformity of celestial motions: they described the motions of celestial bodies by appeal to systems of circles centered at least roughly on the center of the Earth. Further, since their theories were primarily mathematical models that advanced few physical claims, they did not conflict either with Aristotle's principle of distinct physical regions.

The chief difficulty encountered by astronomers in this tradition was in accounting satisfactorily for observational data. Several times it occurred that a theory was recognized to be incapable of accommodating the data to acceptable accuracy with its arrangement of circles, and was succeeded by a still more intricate geometrical system. Eventually, around A.D. 150, it was concluded by Ptolemy that a satisfactory accord with data required the introduction of a new adjustable geometrical device.

Consider all cases of bodies moving along a circle with angular (not linear) velocity that is uniform about a particular geometrical point; and call this, as did Ptolemy, the "equant point". In some of these cases, the body moves also with uniform linear velocity along its circle: these are the cases in which the equant point coincides with the center of the circle. In constructing an astronomical theory, it may be stipulated that the equant point governing a certain body's motion should coincide with the center of the circle along which that body travels; this is what, in effect, Ptolemy's predecessors had stipulated in stating that celestial bodies travel with uniform linear velocity. By contrast, Ptolemy allowed himself the extra degree of freedom of locating the equant point so as to optimize the system's fit with the data: the equant point will then coincide with the center of the circle only occasionally and accidentally.

Thanks partly to this extra degree of freedom, Ptolemy's theory was much better than its predecessors' at according with the observational data. However, reference to equant points amounted to relaxing somewhat the commitment to the principle of the circularity and uniformity of celestial motions, since the theory no longer represented heavenly bodies as moving along their orbits with uniform linear velocities.

To Copernicus, this relatively late innovation in Hellenistic astronomical thinking was unacceptable. Copernicus considered that subscribing to the principle of the circularity and uniformity of celestial motions was mandatory for astronomical theory (Brackenridge, 1982, pp. 118–121). Appeals to equant points violated this fundamental principle, and Copernicus wished to rid astronomical theory of them. This intention is visible in both his polemics and his positive theorizing. First, he attacked Ptolemaic astronomy in both the *Commentariolus* and *De revolutionibus* not as a heliocentrist astronomer criticizing a geocentric theory, but on the grounds that Ptolemy had adhered insufficiently strictly to the principle of the circularity and uniformity of celestial motions. Secondly, he constructed a theory which, by avoiding use of equant points, more fully satisfied the principle of the circularity and uniformity of celestial motions, as well as being consistent with the principle of distinct physical regions. He retraced his reasoning at the opening of the *Commentariolus*:

> The theories concerning these matters that have been put forth far and wide by Ptolemy and most others, although they correspond numerically (with the apparent motions), also seemed quite doubtful, for these theories were inadequate unless they also envisioned certain equant circles, on account of which it appeared that the planet never moves with uniform velocity either in its deferent sphere or with respect to its proper center. Therefore a theory of this kind seemed neither perfect enough nor sufficiently in accordance with reason.
>
> Therefore, when I noticed these (difficulties), I often pondered whether perhaps a more reasonable model composed of circles could be found from which every apparent irregularity would follow while everything in itself moved uniformly, just as the principle of perfect motion requires. (Swerdlow, 1973, pp. 434–435; interpolations by Swerdlow)

In other words, Copernicus sought to formulate an astronomical theory that was more Aristotelian than Ptolemy's had been.

This achievement, it is true, involved sacrificing the principle of geocentricity and the belief that the Earth is immobile; and these changes too were opposed by Aristotelian natural philosophers (Grant, 1984). However, the principle of geocentricity was, as we have seen, the least deeply entrenched and most widely disputed of the three principles of Western astronomy. In relaxing adherence to the principle of geocentricity, Copernicus was therefore following a tradition relatively familiar to his readers. Indeed, in *De revolutionibus* Copernicus cited Pythagorean heliocentrism as a precedent for his own proposal (Copernicus, 1543, pp. 5 and 12); and many contemporaries interpreted Copernicus straightforwardly as having revived Pythagoreanism in astronomy (Heninger, 1974, p. 130).

Copernicus seems to have believed that the fact that his theory adhered more faithfully than Ptolemy's to the principle of the circularity and uniformity of celestial motions would prompt astronomers to transfer their allegiances from Ptolemy's theory to his own, despite the fact that it was able to demonstrate no clear empirical superiority. He was largely correct in this expectation. There is good evidence that many late-sixteenth-century mathematical astronomers found this feature of Copernican theory so attractive as to outweigh any reservations they may have had against the theory on other grounds. Examples of this attitude are offered by Erasmus Reinhold and by Tycho. Reinhold, one of the leading astronomers of his time, who endorsed Copernicus's theory primarily on the ground of its elimination of the equant point and its restoration of uniform circular motions; the fact that it placed the sun rather than the Earth at the center of the universe appears not to have greatly influenced his opinion of the theory (Gingerich, 1973, pp. 55–59). Similarly, in his letter of 1587 to Rothmann, after having paid tribute to Copernicus's ability to attain "fitness and compendious harmony in hypotheses", Tycho wrote:

(Copernicus's) apparently absurd opinion that the Earth revolves does not obstruct this estimate, because a circular motion designed to go on uniformly about another point than the very center of the circle, as actually found in the Ptolemaic hypotheses of all the planets except that of the Sun, offends against the very basic principles of our discipline in a far more absurd and intolerable way than does the attributing to the Earth one motion or another (. . .). There does not arise from this assumption so many unsuitable consequences as most people think. (Quoted in the translation of Moesgaard, 1972, p. 38)

In the light of its relation to the Aristotelian cosmological principles, Copernicus's theory was seen by contemporaries as a return to long-established values in the construction of astronomical models: as a restoration more than a revolution (Neugebauer, 1952, p. 206; Hanson, 1961; Cohen, 1985, pp. 123–125). As Robert S. Westman claims, far from being perceived as iconoclastic, the Copernican theory was respectfully welcomed into what Kuhn would term the "normal science" of mid-sixteenth-century mathematical astronomy (Westman, 1975, pp. 191–192).

This finding alerts us to inadequacies in Kuhn's account of aesthetic factors in science. Having drawn attention – with much justice – to the part played by the aesthetic properties of Copernicus's theory in winning it support, Kuhn is led to portray Copernicus's theory as a revolution. Under that portrayal, after all, the reception of Copernicanism would support his view that paradigm-switch is typically induced by aesthetic factors and inhibited by empirical factors. But the historical evidence suggests overwhelmingly that, for any reasonable construal of "scientific revolution", Copernicus's theory did not constitute a revolution in mathematical astronomy. A model of aesthetic considerations which regarded them as a conservative factor would yield a more adequate view of the history of Copernicanism: it would enable us both to acknowledge fully the role of aesthetic considerations in the reception of Copernican theory, and to explain why contemporaries regarded the theory as non-revolutionary.

5. REVOLUTION AS THE ABANDONMENT OF AESTHETIC COMMITMENTS

The formation of scientists' aesthetic preferences is patently backward-looking. Scientists tend to regard as beautiful those theories that have long been established, or that at least resemble theories that have long been established, in their community. Even a theory that is deemed aesthetically unattractive upon its first formulation can win aesthetic acceptance and admiration, if it remains established in the community for long enough. The history of Newtonian mechanics, of quantum mechanics, even of quantum field theory exhibits aspects of this phenomenon.

Of course, what enables a theory to win increasing aesthetic acceptance is not simply the passage of time: I do not suggest that scientists' taste is simply for the ancient. Rather, I hypothesize that a scientist's aesthetic preferences are shaped by the factor that ensures that certain theories remain long established in a community: their enduring empirical success. The mechanism by which scientists' aesthetic preferences are constructed and revised, I suggest, is inductive. A community constructs its aesthetic canon at a certain date from among the aesthetic features of all past theories by attributing to each feature a weighting roughly proportional to the degree of empirical success scored up to that date by the theories which have embodied that feature. (The degree of empirical success scored by theories is, of course, judged by the application of the community's empirical criteria of theory-evaluation.) The collection of aesthetic features and weightings thus assembled forms the community's aesthetic canon, which is thereafter used in judging new theories (McAllister, 1989, pp. 36–41).

This inductive mechanism ensures that aesthetic canons in science are conservative: they will tend to attribute greater value to, and to recommend for adoption, theories which duplicate the aesthetic features embodied by the empirically more successful theories of the recent past. The conservative bias of aesthetic canons has some interesting implications for models of scientific revolutions.

During certain time spans, its aesthetic evaluative canon will not hinder a community from adopting the empirically best-performing theories on offer. This situation holds as long as the aesthetic features of an empirically successful theory differ to only a small extent from those of its predecessors: in such a case, the aesthetic canon is able to evolve fast enough that, at any time, it expresses preference for the aesthetic features of the empirically most successful theory then available. By contrast, if the aesthetic features of one empirically successful theory differ too greatly from those of its predecessors, the community's aesthetic canon will not be renewed sufficiently quickly to reflect those changes. The canon will lag behind developments, continuing to express greater preference for aesthetic features that were exhibited by the community's former best theories, but that are not shown by the current best theories.

This lag of aesthetic canons behind technical resources is apparent also in the applied arts such as architecture and industrial design, which – unlike other arts – possess a notion of empirical performance. When technical resources in the applied arts progress at a high rate, aesthetic canons may fail to renew themselves fast enough to ensure that the most powerful resources available can always be given a form considered seemly. In the following passage, Maxwell Fry discusses the implications for architectural design of the high rate of technical progress seen during the nineteenth century:

> The rapidity of this change cut the ground away from under the architect's feet. (. . .) If the structural developments which have led to our present technical skill were to continue at the same pace into this century, at a pace, that is, exceeding our capacity as artists to assimilate them, then our hopes of establishing a workable architecture would be slight. (Fry, 1944, p. 122)

This incapacity of architectural canons to keep pace with technical developments is analogous to the lag of aesthetic canons in a science in a revolutionary crisis.

In these circumstances, a scientist who rigorously applied the established aesthetic canons would be unable to adopt the empirically best-performing theories available. In their reactions to this fact, members of the community will show two patterns of behavior. Some will deem it advantageous to suspend their allegiance to the established aesthetic canon, and to conduct theory-choice on empirical criteria alone. Others will find this too deep a rupture with tradition, and will allow their theory-choices to continue to be determined by their aesthetic commitments, even though this decision delivers them theories that are empirically less successful than other available theories. Eventually, the gap in empirical performance between the theories chosen by these two factions will widen to such an extent that retention of the old aesthetic preferences is no longer a defensible option. When this occurs, the entire community will align itself with the theory-choices of the more empirically-minded faction, and the period of controversy will come to a close. In the years that follow, of course, the community gradually forms a fresh aesthetic canon, through the renewed operation of the inductive mechanism, and the historical cycle recommences.

This, I suggest, is how a scientific revolution should be interpreted: as the forced repudiation of aesthetic constraints which a community had become accustomed to imposing on its theory-choices. This model of revolutions explains several features of such episodes. For instance, it explains the sensation of many scientists in a pre-revolutionary period that they face irreconcilable demands: their wish to maximize empirical performance is frustrated by their allegiance to the established aesthetic canons. Secondly, it explains why the theories adopted in a revolution "look strange" to many contemporaries: aesthetic or perceptual criteria played no part in their selection. Thirdly, it explains why, against Kuhn's expectations, the theories adopted before and after a revolution are not entirely incommensurable: although they do not have the same aesthetic style, there are common criteria on which their empirical performance can be compared (McAllister, 1989, pp. 41–47).

In the light of this model of scientific revolutions, I read developments in early-modern mathematical astronomy as follows. In adopting Copernicus's theory, mathematical astronomers rightly saw themselves as holding to long-established aesthetic commitments of their community. This theory therefore did not constitute a revolution. The early-modern revolution in mathematical astronomy occurred only at the hands of Kepler, as we shall next see.

6. THE ICONOCLASM OF KEPLER'S ELLIPSES

Kepler's *Astronomia nova* of 1609 sets out his first two laws of planetary motion. These were the fruit of his "war on Mars", the effort which he undertook between 1600 and 1605 to discover a mathematical law to describe the motion of the sun's fourth planet. Kepler had at his disposal the observational data collected by his former employer, Tycho: they had an accuracy of around 1%, substantially higher than any previous comparable data. Kepler appears to have reached his first law – that every planet's orbit is an ellipse having the sun at one focus – by, roughly speaking, an alternation of theoretical hypotheses and empirical tests: he proposed a succession of candidate-paths for the orbit of Mars and gauged the accord of each with Tycho's data.

Kepler tested first the hypothesis that Mars moved in a circle. He found that its angular coordinates would then have departed by as much as 8% from those recorded by Tycho. This discrepancy was in Kepler's view sufficiently large for a circular orbit to be ruled out (Whiteside, 1974, pp. 6–7). The distribution of discrepancies along the orbit suggested to Kepler in 1602 the curve which he should next consider: "The orbit is not a circle, but (passing from aphelion) enters in a little on either side (at quadratures) and goes out again to the breadth of the circle at perihelion, in a path of the sort called an oval" (Quoted *ibid.*, p. 8; interpolations by Whiteside). However, Kepler could not reconcile even this hypothesis to his satisfaction with the data. He concluded in 1604 that the true orbit must be a curve contained between the circle and the oval, and in the same breath suggested which curve this was:

> In the middle longitudes (. . .) the perfect circle prolongs (the true orbital path) by about 800 or 900 (parts in 152350, the mean radius of orbit) too much. My ovality curtails by about 400 too much. The truth is in the middle, though nearer to my ovality (. . .) just as though Mars's path were a perfect ellipse. (Quoted *ibid.*, p. 11)

The first law of planetary motion that Kepler published in the *Astronomia nova* expressed this conclusion.

The role played in Kepler's reasoning by empirical factors is evident: the theory that he published in 1609 was the empirically best-performing of the candidates that he had examined. Empirical considerations were responsible equally for the theory's gradual acceptance in the community. Initially, many astronomers were unable to evaluate the empirical quality of Kepler's theory: they were much less familiar with the properties of the ellipse than with those of the circle, and found it difficult to deduce from the theory predictions to test against observation. The theory's empirical performance became more obvious after 1627, when Kepler published the *Tabulae Rudolphinae* (Russell, 1964, pp. 7 and 20). This was a compilation of tables and rules for predicting the positions of the moon and planets, based on Kepler's laws: in essence, it was a tabulation of the observational consequences of Kepler's theory, which by this means opened itself to easy empirical test. Use of the *Tabulae Rudolphinae* quickly demonstrated that Kepler's theory was very successful at predicting the positions of the planets – even those of Mercury, the planet that had thus far proved most recalcitrant to astronomical theory.

The effect of the empirical performance of Kepler's theory on the community's opinion of it is illustrated by the conversion of Peter Crüger, Professor of Mathematics at Danzig. In the early years after the publication of Kepler's theory, he recoiled from it. He wrote for instance in 1624: "I do not subscribe to the hypotheses of Kepler. I trust that God will grant us some other way of arriving at the true theory of Mars" (Quoted *ibid.*, p. 8 where also more evidence is given of Crüger's early unfavorable response to Kepler's theory.) Once the *Tabulae Rudolphinae* had appeared, however, Crüger revised his opinion. Writing to the astronomer Philipp Müller in 1629, Crüger expressed the impact on him of the empirical corroboration of Kepler's theory:

You hope that someone will give these tables (the astronomical tables of Longomontanus) a further polishing and you say that all astronomers would be grateful for this. But I should have thought that it would be a waste of time now that the Rudolphine Tables have been published, since all astronomers will undoubtedly use these. (. . .) I am wholly occupied with trying to understand the foundations upon which the Rudolphine rules and tables are based, and I am using for this purpose the Epitome of Astronomy previously published by Kepler as an introduction to the tables. This epitome which previously I had (. . .) so many times thrown aside, I now take up again and study (. . .). I am no longer repelled by the elliptical form of the planetary orbits (. . .). (Quoted *ibid.*, p. 8)

This is evidence that Kepler's theory won adherents on the strength of its empirical properties. The last sentence of this passage, moreover, contains a clue that Kepler's theory had initially encountered opposition because of other features of it: its aesthetic and metaphysical properties.

The Aristotelian principle of the circularity and uniformity of celestial motions, whose power in the mid-sixteenth century we discussed in Section 4, still retained some influence in the early seventeenth century. It continued to captivate natural philosophers such as Galileo (Koyré, 1939, p. 144; Panofsky, 1954, p. 25); even Kepler had not been wholly immune to it (Brackenridge, 1982). Kepler's claim that planets moved in elliptical orbits stood in clear violation of this principle, and was accorded a correspondingly hostile reception by conservative astronomers. They did not try to justify their hostility on empirical grounds: they did not claim that postulating non-circular orbits rendered astronomical theory incapable of accounting with sufficient accuracy for observational data. Rather, they argued on a non-empirical, or metaphysical, criterion, that only the postulation of circular orbits could produce the harmony demanded in planetary astronomy. For instance, Tycho had written to Kepler in 1599:

The orbits of the planets must be constructed exclusively from circular motions; otherwise they could not recur with a uniform and equal constancy, eternal duration would be impossible; moreover, the orbits would be less simple, would exhibit greater irregularities and would not be suitable for scientific treatment and practice. (Quoted in the translation of Mittelstrass, 1972, p. 210)

Hostility to the suggestion that planetary orbits were elliptical was heightened by the perceived aesthetic contrast between ellipses and circles: whereas today we tend to describe the circle as the special case of the ellipse in which the two axes have equal length, the sixteenth and early seventeenth centuries saw the ellipse as a distorted and imperfect circle.

Let us return to the passage by Crüger that I quoted. I interpret its last sentence as indicating that Crüger renounces one of the criteria upon which he had previously objected to Kepler's theory: he no longer opposes the theory on the grounds that it describes the planetary orbits as elliptical. The reason why Crüger feels he cannot now afford to reject Kepler's theory on these grounds is, as the rest of the passage makes clear, that the theory had manifested through the *Tabulae Rudolphinae* a high degree of empirical accuracy. The effect of the aesthetic properties of Kepler's theory on its reception is unambiguous: far from contributing to the appeal of the theory initially, they proved to be a hindrance to the theory's acceptance, and the hostility that they generated had gradually to be overcome by demonstrations of the theory's empirical power. This fact leads me, on the model of revolutions that I outlined in Section 5, to portray Kepler's theory as constituting a revolution.

There is in fact good evidence, independent of any philosophical model of scientific revolutions, that Kepler's theory represented a deeper innovation in mathematical astronomy than Copernicus's. As Norwood R. Hanson characterized this period: "The line between Ptolemy and Copernicus is unbroken. The line between Copernicus and Newton is discontinuous, welded only by the mighty innovations of Kepler" (Hanson, 1961, p. 169). Certainly Kepler's theory is far more than a "version of Copernicus' proposal", as Kuhn characterizes it (Kuhn, 1957, p. 219).

7. THE EFFECT OF AESTHETIC FACTORS UPON THE COURSE OF SCIENCE

The evidence of early-modern mathematical astronomy can now be used in a comparative evaluation of the view of aesthetic factors proposed by Kuhn and the one that I have been defending in this paper. Kuhn takes scientists' aesthetic considerations as prompting them to innovation, and thereby as inducing scientific revolutions. I regard scientists' aesthetic preferences as *shaped by the aesthetic features of previous empirically successful theories*, and thereby as imposing a conservative check on styles of theorizing.

I feel that the evidence from early-modern planetary astronomy that we have examined accords with my view better than with Kuhn's. My view suggests appraisals of the theories of Copernicus and Kepler that agree with those given by their contemporaries. On my view, we come to see Copernicus's as a theory that, although not offering any improvement in predictive performance, was deemed worthy of adoption in virtue of satisfying more fully than its Ptolemaic predecessor certain aesthetic desiderata. For from sparking a revolution, this theory was both intended and received as an orthodox contribution to the established paradigm in mathematical astronomy. By contrast, we come to see Kepler's as a theory that merited adoption in virtue of delivering superior empirical performance, but could be adopted only if the aesthetic canon that had dominated the discipline since Ptolemy's time was abandoned. This rupture with the past was the true revolution in early-modern mathematical astronomy.

The history of science records many theories of which the reception echoes that of Copernicus's theory, and many that won adherents in a manner recalling

Kepler's theory. For instance, Einstein's special theory of relativity stands to classical physics much as Copernicus's theory stands to sixteenth-century mathematical astronomy: it was an essentially conservative theory that won acceptance largely in virtue of its aesthetic properties. By contrast, quantum theory performed a break with classical physics as deep as that which Kepler's theory performed with pre-existing mathematical astronomy, and had to rely on its empirical virtues to attract physicists who were repelled on aesthetic grounds.

The role of aesthetic factors in helping determine the course of science cannot be properly appreciated unless their relation with empirical factors is understood. If I am right, the usual suggestion that aesthetic factors are the spirit of innovation in science, while empirical factors are the spirit of continuity, is the contrary of the truth.

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