Michael R. Gardner

Realism and Instrumentalism in Pre-Newtonian Astronomy

1. Introduction

There is supposed to be a problem in the philosophy of science called "realism versus instrumentalism." In the version with which I am concerned, this supposed problem is whether scientific theories in general are put forward as true, or whether they are put forward as untrue but nonetheless convenient devices for the prediction (and retrodiction) of observable phenomena.

I have argued elsewhere (1979) that this problem is misconceived. Whether a theory is put forward as true or merely as a device depends on various aspects of the theory's structure and content, and on the nature of the evidence for it. I illustrated this thesis with a discussion of the nineteenth-century debates about the atomic theory. I argued that the atomic theory was initially regarded by most of the scientific community as a set of false statements useful for the deduction and systematization of the various laws regarding chemical combination, thermal phenomena, etc.; and that a gradual transition occurred in which the atomic theory came to be regarded as a literally true picture of matter. I claimed, moreover, that the historical evidence shows that this transition occurred because of increases in the theory's proven predictive power; because of new determinations of hitherto indeterminate magnitudes through the use of measurement results and well-tested hypotheses; and because of changes in some scientists' beliefs about what concepts may appear in fundamental explanations. I posed it as a problem in that paper whether the same or similar factors might be operative in other cases in the history of science; and I suggested that it might be possible, on the basis of an examination of several cases in which the issue of realistic vs. instrumental acceptance of a

This material is based upon work supported by the National Science Foundation under Grants No. SOC 77-07691 and SOC 78-26194. I am grateful for comments on an earlier draft by Ian Hacking, Geoffrey Hellman, Roger Rosenkrantz, Robert Rynasiewicz and Ferdinand Schoeman.

theory has been debated, to put forward a (normative) theory of when it is reasonable to accept a theory as literally true and when as only a convenient device.

For simplicity I shall usually speak of the acceptance-status of a theory as a whole. But sometimes it will be helpful to discuss individual hypotheses within a theory, when some are to be taken literally and others as conceptual devices.

In the present paper I would like to discuss a closely analogous case—the transition, during the Copernican revolution, in the prevailing view of the proper purpose and correlative mode of acceptance of a theory of the planetary motions. I shall briefly discuss the evidence—well known to historians—that from approximately the time of Ptolemy until Copernicus, most astronomers held that the purpose of planetary theory is to permit the calculations of the angles at which the planets appear from the earth at given times, and not to describe the planets' true orbits in physical space. For a few decades after Copernicus's death, except among a handful of astronomers, his own theory was accepted (if at all) as only the most recent and most accurate in a long series of untrue prediction-devices. But eventually the Copernican theory came to be accepted as the literal truth, or at least close to it. That this transition occurred is well known; why it occurred has not been satisfactorily explained, as I shall try to show. I shall then try to fill in this gap in the historical and philosophical literature. In the concluding section I shall also discuss the relevance of this case to theses, other than the one just defined, which go by the name "realism."

2. The Instrumentalist Tradition in Astronomy

The tradition of regarding theories of planetary motion as mere devices to determine apparent planetary angles is usually said to have originated with Plato; but it also has roots in the science of one of the first civilizations on earth, that of the Babylonians. Their reasons for being interested in the forecasting of celestial phenomena were largely practical in nature. Such events were among those used as omens in the conduct of government. Predictions of eclipses, for example, were thus needed to determine the wisdom of planning a major undertaking on a given future date. In addition, since the new month was defined as beginning with the new moon, one could know when given dates would arrive only through predictions of new moons. The Babylonians therefore appear to have developed purely arithmetical procedures for the prediction of apparent angles of celestial bodies. Their main technique was the addition and subtraction of quantities, beginning with some initial value and moving back and forth between fixed limits at a constant rate. Through such techniques they could forecast such quantities as lunar and solar velocity, the longitudes of solar-lunar conjunctions, the times of new moons, etc. Although numerous tablets have survived showing techniques for the computation of such quantities, and others showing the results of the computations, we have no evidence that these techniques were based on any underlying theory of the geometrical structure of the universe (Neugebauer 1952, Chapter 5). Moreover, the arithmetical character of the regularities in the variations of the quantities also suggests that the computations are based on purely arithmetical rather than geometrical considerations. Of course it is possible that the forecasting techniques were based upon statements about and/or pictures of the geometry of the universe that were not committed to clay or that do not survive. But it appears that in Babylonian mathematical astronomy—probably developed between about 500 and 300 BC—we have an extreme instance of instrumentalism: not even a theory to be interpreted as a prediction-device rather than as purportedly true, but only a set of prediction-techniques in the literal sense.

As I have said, another major source of the instrumentalist tradition in astronomy is Plato. I do not mean that he was a source of the instrumentalists' conception of the aims of astronomical theorizing, but rather of their strictures upon the permissible means for achieving those ends. Vlastos (1975, pp. 51-52) has assembled from various Platonic works an argument that runs as follows: celestial bodies are gods and are moved by their souls; these souls are perfectly rational; only uniform circular motions befit rationality; hence all celestial motions are uniform (in speed) and circular. To document this, we must note that in *Laws* Plato (in Hamilton and Cairns 1963) asserted that the sun's "soul guides the sun on his course," that this statement is "no less applicable to all . . . celestial bodies," and that the souls in question possess "absolute goodness"—i.e., divinity (898 C-E). Plato required in the argument the tacit assumption that goodness entails rationality. He could then conclude that since bodies "moving regularly and uniformly in one compass about one center, and in one sense" must "surely have the closest affinity and resemblance that may be to the revolution of intelligence," celestial motions must be uniform and circular (898 A-B). Plato was also in a position to argue for the rationality of a celestial soul on the grounds that it, unlike our souls, is not associated with a human body, which causes "confusion, and whenever it played any part, would not allow the soul to acquire truth and wisdom." (*Phaedo* 66A, in Bluck 1955)

Two clarifications are needed. Although Plato was not explicit on the point, he seems to have assumed that all these circular motions are concentric with the earth. This is presumably why at *Timaeus* 36D (in Cornford 1957) he says that the circles of the sun, moon, and five (visible) planets are "unequal"—so that they can form a nest centered about the earth, as Cornford suggests. (p. 79) Second, we are not to assume that these "circular" motions trace out simple circles. Rather, the complex observed paths are compounded out of a plurality of motions, each of which is uniform and circular. (*Timaeus* 36 C-D)

A story of uncertain reliability (Vlastos 1975, pp. 110-111) has it that Plato, perhaps aware of the empirical inadequacies of his own theory, proposed to astronomers the problem: "What uniform and orderly motions must be hypothesized to save the phenomenal motions of the stars?" (Simplicius, quoted in Vlastos 1975) The first astronomer to make a serious attempt to answer this question in a precise way was Plato's student Eudoxus. He postulated that each of the seven "wandering" celestial bodies was moved by a system of three or four earth-centered, nested, rotating spheres. Each inner sphere had its poles attached at some angle or other to the next larger sphere. The celestial body itself was located on the equator of the innermost sphere of the system. By adjustments of the angles of the poles and the (uniform) speeds of rotation, Eudoxus was able to obtain a fairly accurate representation of the motion in longitude (including retrogression) of three planets and the limits (but not the times) of the planets' motion in latitude. (Dreyer 1953, pp. 87-103)

In the extant sources on the Eudoxian theory (Aristotle and Simplicius), no mention occurs of any theories regarding the physical properties of these spheres—their material, thickness, or mutual distances—nor on the causes producing their motions. And even though the outermost sphere for each celestial body moves with the same period (one day), Eudoxus appears to have provided no physical connections among the various systems. (Dryer 1953, 90-91) The (admittedly inconclusive) evidence therefore suggests that Eudoxus regarded the spheres not as actual physical objects in the heavens, but as geometrical constructions suitable for computing observed positions. If this is correct, Eudoxus, the founder of quantitatively precise geometrical astronomy, was also an important early instance of some aspects of the instrumentalist tradition within that field. By this I do not mean that Eudoxus denied that his theory is true, or even that he was sceptical of its truth, but only that he appears to have limited the aim of astronomy to a geometrical representation (not including a causal explanation) of the observed phenomena. From this position it is only one step to the instrumentalist view that there *cannot* be a causal explanation of the orbits postulated by a given astronomical theory, and that the theory's descriptions of the orbits are therefore *not true*.

Whether Eudoxus held this view about the status of the spheres or not, it is quite clear that Aristotle held the opposite view. In *Metaphysics* XII, 8, he remarked that in addition to the seven spheres added to the Eudoxian system by Callipus for the sake of greater observational accuracy, additional spheres must be intercalated below the spheres of each planet, in order to counteract the motions peculiar to it and thereby cause only its daily motion to be transmitted to the next planet below. "For only thus," he said, "can all the forces at work produce the observed motion of the planets." (in McKeon 1941, 1073b-1074a) Clearly, then, Aristotle thought of the spheres as real physical objects in the heavens, and supposed that some of their motions are due to forces transmitted physically from the sphere of stars all the way to the moon.

At *Physics* II, 2 Aristotle expressed his realistic view of astronomy in a different way. He raised the question,

> Is astronomy different from physics or a department of it? It seems absurd that the physicist should be supposed to know the nature of sun or moon, but not to know any of their essential attributes, particularly as the writers on physics obviously do discuss their shape also and whether the earth and the world are spherical or not.
>
> Now the mathematician, though he too treats of these things, nevertheless does not treat of them as the limits of a physical body, nor does he consider the attributes indicated as the attributes of such bodies. That is why we separated them; for in thought they are separable from motion. . . .
>
> Similar evidence is supplied by the more physical of the branches of mathematics, such as optics, harmonics, and astronomy. These are in a way the converse of geometry. While geometry investigates physical lines, but not *qua* physical, optics [and, presumably, astronomy] investigates mathematical lines, but *qua* physical, not *qua* mathematical.

Although the message of this passage is somewhat ambiguous, its main point can be discerned. Aristotle thought that, generally, mathematicians deal with geometrical objects, such as shapes, as nonphysical entities, and thus they are presumably unconcerned with such questions as the constitution of objects and the forces upon them. Although astronomy is a branch of mathematics, it is one of the more physical branches; thus the foregoing generalization does not apply to astronomers, who must treat spheres, circles, etc. as physical objects. They cannot, then, as an astronomical instrumentalist would say they should, limit their concerns to the question of whether various geometrical constructions permit correct determinations of planetary angles.

The most influential astronomer before Copernicus was, of course, Ptolemy (fl. 127-150 AD); and it is therefore of considerable interest whether he regarded his theory as providing a true cosmological picture or as only a set of prediction-devices. This question, however, admits of no simple answer. To some extent, Ptolemy worked within traditions established by Plato and by Aristotle. Echoing (but not explicitly citing) Plato's postulate about uniform circular motions in the divine and celestial realm, he wrote in the *Almagest*, IX, 2:

> Now that we are about to demonstrate in the case of the five planets, as in the case of the sun and the moon, that all of their phenomenal irregularities result from regular and circular motions—for such befit the nature of divine beings, while disorder and anomaly are alien to their nature—it is proper that we should regard this achievement as a great feat and as the fulfillment of the philosophically grounded mathematical theory [of the heavens]. (Trans. Vlastos 1975, p. 65)

Again, he wrote, "we believe it is the necessary purpose and aim of the mathematician to show forth all the appearances of the heavens as products of regular and circular motions." (*Almagest*, III, 1)

Ptolemy followed Aristotle's division of the sciences, but diverged somewhat when it came to placing astronomy within it:

> For indeed Aristotle quite properly divides also the theoretical into three immediate genera: the physical, the mathematical, and the theological. . . .the kind of science which traces through the material and ever moving quality, and has to do with the white, the hot, the sweet, the soft, and such things, would be called physical; and such an essence [ousia], since it is only generally what it is, is to be found in corruptible things and below the lunar sphere. And the kind of science which shows up quality with respect to forms and local motions,

> seeking figure, number, and magnitude, and also place, time, and similar things, would be defined as mathematical. (*Almagest*, I, 1)

He then remarked that he himself had decided to pursue mathematics (specifically, astronomy), because it is the only science that attains "certain and trustworthy knowledge." Physics does not do so because its subject (sublunar matter) is "unstable and obscure." (*Almagest*, I, 1) Ptolemy, then, separated astronomy from physics more sharply than Aristotle did: he held that it is a branch of mathematics and did not say that it is one of the "more physical" branches. Such a claim, indeed, would make no sense in view of Ptolemy's claim that physics deals with the sublunar, corruptible realm.

Since Ptolemy denied that astronomy is a branch of physics, or even one of the more physical branches of mathematics, he did not put forward any theory analogous to Aristotle's of a cosmos unified through the transmission of physical forces productive of the planetary motions. Rather, he treated each planet's motion as a separate problem, and held that the motions are not explained in terms of physical forces at all, but in terms of the individual planet's essence: "The heavenly bodies suffer no influence from without; *they have no relation to each other;* the particular motions of each particular planet follow from the essence of that planet and are like the will and understanding in men." (*Planetary Hypotheses*, Bk. II; trans. Hanson 1973, p. 132)

But were these "motions" mere geometric constructions, or were they physical orbits? A key point is that Ptolemy did not claim in the *Almagest* to know how to determine the distances of the planets from the earth or even to be entirely certain about their order: "Since there is no other way of getting at this because of the absence of any sensible parallax in these stars [planets], from which appearance alone linear distances are gotten," he said he had no choice but to rely on the order of "the earlier mathematicians." (IX, 1) Although the angles given by Ptolemy's constructions in the *Almagest* are to be taken as purportedly corresponding to physical reality, the supposed physical distances were left unspecified. But this leaves open the possibility that Ptolemy thought his constructions gave the physical orbits up to a scale factor—i.e., that they gave the orbit's shape. Dreyer argues that Ptolemy's theory of the moon, if interpreted in this way, implies that the moon's distance and therefore apparent diameter vary by a factor of 2; and Ptolemy (like nearly everyone) must have been aware that no such variation in diameter is observed. (1953, p. 196) Against this it must be said that in the *Almagest* Ptolemy claimed that the moon's distance, unlike those of the five planets, can be determined from observational data via his model and that it varies from about 33 to 64 earth radii. (V, 13) He gave no indication that these numbers were not to be taken literally, despite what he obviously knew they implied about apparent diameter. He also took the numbers seriously enough to use them (and data on the apparent diameters of the sun, moon, and the earth's shadow) to obtain a figure for absolute distance to the sun—1210 earth radii. (*Almagest*, V, 15)

A stronger piece of evidence for Dreyer's view is Ptolemy's admission in *Planetary Hypotheses:* "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 by its proper hypothesis." (Trans. Dreyer 1953, p. 201) This quotation certainly suggests that Ptolemy did not think the totality of his geometric constructions was a consistent whole, and thus that he could not have thought that it (that is, all of it) represented physical reality.

Again, discussing the hypothesis that a given planet moves on an epicycle and the hypothesis that it moves on a deferent eccentric to the earth, he claimed: "And it must be understood that all the appearances can be cared for interchangeably according to either hypothesis, when the same ratios are involved in each. In other words, the hypotheses are interchangeable. . . . (*Almagest*, III, 3) This relation of "interchangeability" is apparently logical equivalence of actual asserted content; for when confronted with the identity of the two hypotheses' consequences for the appearances—i.e., planetary angles at all times—Ptolemy did not throw up his hands and say he could not decide between them (as would have been rational had he thought them nonequivalent). He straightway declared (in the case of the sun) that he would "stick to the hypothesis of eccentricity which is simpler and completely effected by one and not two movements." (*Almagest*, III, 4) In sum, his arguments in these passages appear to make sense only on the assumption that the genuine asserted content of the orbital constructions is exhausted by what they say about observed angles.

On the other hand, his use of the lunar model to determine distances makes sense only on a realist interpretation. Moreover, his planetary models are based partly on the assumption that maximum brightness (minimum distance) coincides with retrogression because both occur on the inner part of the epicycle (Pedersen 1974, p. 283); and this reasoning also presupposes that the model gives true orbits. We can only conclude that Ptolemy's attitude was ambivalent, or at least that the evidence regarding it is ambiguous.

Whatever we say about Ptolemy's attitude towards the planetary orbits, it would clearly be an exaggeration to say that his theory had no asserted content at all beyond what it implies about observed angles. For Ptolemy explicitly subscribed, of course, to a cosmology that placed the earth at rest in the center of an incomparably larger stellar sphere; and he argued for this conception on the basis of physical considerations—supposed effects the earth's motion would have on bodies on or near the earth. (*Almagest*, I, 2 and 7)

Moreover, some of his arguments are based on physical considerations regarding the nature of the ether (*Almagest*, I, 3) and heavenly bodies (XIII, 2)—their homogeneity, eternity, constancy, irresistibility, etc. (Lloyd 1978, p. 216) Thus the theory has considerable (literally-intended) cosmological and physical as well as observational content.

By the time he wrote *Planetary Hypotheses*, Ptolemy had changed his view about the possibility of determining the distances of the five planets despite their lack of visible parallax. He explained his procedure as follows: "We began our inquiry into the arrangement of the spheres [i.e., spherical shells] with the determination, for each planet, of the ratio of its least distance to its greatest distance. We then decided to set the sphere of each planet between the furthest distance of the sphere closer to the earth, and the closest distance of the sphere further (from the earth)." (Goldstein 1967, pp. 7-8) I shall call the assumption decided upon here the "nesting-shell hypothesis": no space between shells. Using the ratios mentioned here and his values for the lunar distances—both supplied by the theory of the *Almagest*—together with this new hypothesis, Ptolemy computed least and greatest distances for all the planets. And what was the evidence for the nesting-shell hypothesis? Ptolemy said only that "it is not conceivable that there be in nature a vacuum, or any meaningless and useless thing," and that his hypothesis (after some ad hoc fiddling with parameters) meets the weak constraint that it agree with his independently measured value for solar distance. He was apparently not entirely convinced by the argument himself; for he continued: "But if there is space or emptiness between the (spheres), then it is clear that the distances cannot be smaller, at any rate, than those mentioned."

To conclude on Ptolemy, then: in his post-*Almagest* writings, Ptolemy added a further quantity that was to be taken seriously and not just as an angle-predictor—namely, earth-to-planet distance. But he could compute the values of this quantity from observational data (on lunar eclipses) only through the use of a hypothesis for which he had almost no observational evidence.

In the period between Ptolemy and Copernicus, when Ptolemy's approach dominated astronomy, it is possible to distinguish several positions concerning the sense (if any) in which Ptolemaic astronomy should be accepted. One is the view—suggested by some of Ptolemy's own remarks—that the circles associated with the planets determine only observed angles and not orbits in physical space. A prominent example of a person who held such a view quite explicitly is Proclus, a fifth-century commentator. He indicated that he was alive to the distinction between the instrumentalist and realist interpretation of Ptolemaic astronomers by asking, "what shall we say of the eccentrics and the epicycles of which they continually talk? Are these only inventions or have they a real existence in the spheres to which they are fixed?" He concluded his discussion by conceding that some Ptolemaic system was acceptable at least with the former interpretation: "these hypotheses are the most simple and most fitting ones for the divine bodies. They were invented in order to discover the mode of the planetary motions which in reality are as they appear to us, and in order to make the measure inherent in these motions apprehendable." (Proclus 1909, VII 50 (236, 10); trans. Sambursky 1962, pp. 147-149) Earlier, however, Proclus had given a variety of arguments to show that no epicyclic-eccentric theory could be accepted as genuinely true. For example, he argued that such a theory, literally interpreted, is "ridiculous" because it is inconsistent with the principles of physics he accepted—specifically, those of Aristotle: "These assumptions make void the general doctrine of physics, that every simple motion proceeds either around the center of the universe or away from or towards it." (Proclus 1903, 248c [III 146, 17]; trans. Sambursky 1962, pp. 147-149) He argued further: "But if the circles really exist, the astronomers destroy their connection with the spheres to which the circles belong. For they attribute separate motions to the circles and to the spheres and, moreover, motions that, as regards the circles, are not at all equal but in the opposite direction." The point here is that the oppositeness of the deferent and epicyclic motions (for sun and moon) is physically implausible in view of their spatial interrelations (Proclus 1909, VII 50 (236, 10); trans. Sambursky 1962, pp. 147-149)

In addition, Proclus gave two arguments of an epistemological character purporting to show that we have insufficient evidence to justify acceptance of any Ptolemaic theory on a realistic interpretation. One of these is that the heavens are necessarily beyond the reach of merely human minds: "But when any of these [heavenly] things is the subject of investigation, we, who dwell, as the saying goes, at the lowest level of the universe, must be satisfied with 'the approximate.' " (Proclus 1903, I, pp. 352-353; trans. Duhem 1969, pp. 20-21) This argument appeals to the specific character of astronomy's domain; he bolstered his conclusion with a second epistemological argument that is, in principle, applicable to other sciences as well. It is that there could be, and in fact are, alternative systems of orbits (epicycle, eccentric, and homocentric) equally compatible with the observed data; hence which of these sets comprises the true physical orbits is unknowable: "That this is the way things stand is plainly shown by the discoveries made about these heavenly things—from different hypotheses we draw the same conclusions relative to the same objects. . . hypotheses [about]. . . epicycles. . . eccentrics. . . counterturning spheres." (Proclus 1903, I, pp. 352-353; trans. Duhem 1969, pp. 20-21)

A complication in Proclus's position is that whereas he conceded that the Ptolemaic theory is useful for predictions, even though it is not physically true, he did not think one can in the long run be satisfied with astronomical theories that are only conceptual devices and fail to provide causes: "For if they are only contrived, they have unwittingly gone over from physical bodies to mathematical concepts and given the causes of physical movements from things that do not exist in nature." (Trans. Lloyd 1978, p. 205. My interpretation follows Lloyd, in disagreement with Duhem.)

Essentially the same three arguments against realistic acceptance of epicycles and eccentrics—their violations of physical principles, the impossibility of human astronomical knowledge, and the existence of alternative observationally equivalent theories—are found in many writers in the period between Ptolemy and Copernicus. (See Duhem 1969, passim.) We should also take note of one further type of argument against accepting an epicyclic theory on a realistic interpretation: it is that the theory thus understood has consequences that have been observed to be false. Along these lines, Pontano, a widely read astronomer in the early sixteenth century, argued that if the epicycles were real physical orbits, they would have been formed (like the planets) from "solidification" of the spheres carrying them, and thus would be visible—as, of course, they are not. (Quoted in Duhem 1969, pp. 54-55)

Among those who conceded that the evidence showed Ptolemy's theory agrees with the observed celestial motions—and was thus acceptable in just that sense—we can distinguish a subgroup who made it clear that they nonetheless thought that since the theory could not be accepted on a realistic interpretation, it should be replaced with one that could. Some writers also maintained that such an astronomical revolution would also open the way to the sort of logical reconciliation between astronomy and physics attempted by Aristotle: real physical orbits, unlike Ptolemaic epicycles, must obey the laws of physics. An example of such a writer is the great Islamic philosopher Averroes (1126-1198):

> The astronomer must, therefore, construct an astronomical system such that the celestial motions are yielded by it and that nothing that is from the standpoint of physics impossible is implied. . . . Ptolemy was unable to see astronomy on its true foundations. . . . The epicycle and the eccentric are impossible. We must therefore, apply ourselves to a new investigation concerning that genuine astronomy whose foundations are principles of physics. . . . Actually, in our time astronomy is nonexistent; what we have is something that fits calculation but does not agree with what is. (Quoted in Duhem 1969, p. 31)

Another cognitive attitude towards the Ptolemaic theory, held by a few pre-Copernican thinkers, is that it should be accepted not only as an angle-determiner, but also as literally true. One possible argument for such a view is that if the theory (taken literally) were false, it would certainly have some false observational consequences. The thirteenth-century scholastic Bernardus de Virduno (1961, p. 70) gave such an argument: "And up to our time these predictions have proved exact; which could not have happened if this principle [Ptolemy's theory] had been false; for in every department, a small error in the beginning becomes a big one in the end." (Trans. Duhem 1969, p. 37) A second reason why someone who accepts a Ptolemaic type of theory as at least a convenient device may also accept it as true is that he, unlike Proclus, accepts a physical theory with which the astronomical theory in question is consistent. For example, Adrastus of Aphrodisius and Theon of Smyrna, near-contemporaries of Ptolemy, interpreted an epicycle as the equator of a sphere located between two spherical surfaces concentric with the earth. The smaller sphere moves around the earth between the two surfaces to produce the deferent motion, while it rotates on its axis to produce the epicyclic motion. (Duhem 1969, pp. 13-15) These three spheres are evidently to be understood as real physical objects, since Theon argued that their existence is demanded by physical considerations, specifically, the physical impossibility of the planets' being carried by merely mathematical circles: "the movement of the stars should not be explained by literally tying them to circles each of which moves around its own particular center and carries the attached star with it. After all, how could such bodies be tied to incorporeal circles?" (quoted in Duhem 1969, pp. 13-15) Since epicyclic motions violate the Aristotelian physical principle that all superlunar motions are circular and concentric with the earth, we can see that Theon rejected Aristotelian physics and apparently assumed that the celestial spheres roll and rotate in the same manner as terrestrial ones. This, then, is at least part of the reason why Theon would have been unmoved by the Aristotelian physical arguments on the basis of which Proclus adopted an instrumentalist interpretation of epicycles.

Let us sum up our results on the main types of cognitive attitude adopted towards the Ptolemaic theory in the period between Ptolemy and Copernicus, and the main reasons given in support of these attitudes. One possible attitude, of course, is that the theory should be rejected as even a device for the determination of observed angles. But because of the lack of a superior alternative, Ptolemy's accuracy was generally conceded before Copernicus. Another position is that the theory is acceptable as such a device but should not be accepted on a literal interpretation. Those who took this view generally did so on such grounds as that the theory (literally interpreted) is inconsistent with the physics they accepted, deals with a realm whose true nature is inaccessible to human minds, fits the observations no better than some alternative theories, or has false observational consequences. Persons who thought the theory was a convenient device but was untrue sometimes did, and sometimes did not, think it therefore needed to be replaced by a convenient device that was also a true theory. Finally, some persons thought some Ptolemaic type of theory gave a correct description of the actual physical orbits. Possible supporting grounds were that false theories always have false observational consequences, and that epicyclic orbits are compatible with the laws of a true (non-Aristotelian) physical theory.

3. Harmony

In the period between Copernicus and Kepler, a gradual transition occurred in the dominant view among astronomers of the purpose and content of planetary theory. Eventually certain Copernican theories came to be accepted as true descriptions of the actual orbits of the planets in physical space. Before attempting to determine the reasoning involved in this transition in acceptance-status, I shall explain and criticize what I take to be the most important existing attempts to explain the rationale for the acceptance of Copernicus's theory. Most of these do not distinguish between instrumental and realistic acceptance and so do not attempt to discern separate grounds for each. Still they provide a useful starting point in dealing with my more "refined" question.

There is a prima facie difficulty in accounting for such appeal as the Copernican system—at the time of its initial publication by Rheticus and Copernicus—had to the scientific community, or even to Copernicus and Rheticus themselves. Briefly, the difficulty is that the most obvious factors in appraisal—simplicity and observational accuracy—give no decisive verdict in Copernicus's favor. True, Copernicus did say of his theory and certain of its implications, "I think it is much easier to concede this than to distract the understanding with an almost infinite multiplicity of spheres, as those who have kept the Earth in the middle of the Universe have been compelled to do." (1976, Book I, chapter 10) As Gingerich (1973a) has shown by recomputing the tables used by the leading Ptolemaic astronomers of Copernicus's time, the familiar story that they were forced to use ever-increasing numbers of epicycles on epicycles is a myth. In fact, they used only a single epicycle per planet. Since Copernicus's theory of longitudes used two and sometimes three circles per planet (Kuhn 1957, pp. 169-170), his total number of circles—the most obvious but not the only measure of simplicity—is roughly comparable to Ptolemy's. Gingerich's computations of the best Ptolemaic predictions of Copernicus's time, together with the earliest Copernican predictions and with computations of actual positions based on modern tables, also enabled him to conclude that the latest Ptolemaic and earliest Copernican predictions did not differ much in accuracy (1973a, pp. 87-89). Indeed, Copernicus himself conceded that except in regard to the length of the year, his Ptolemaic opponents "seem to a great extent to have extracted. . . the apparent motions, with numerical agreement. . . ." (1976, prefatory letter)

Because of the difficulties in using accuracy and simplicity to account for the appeal of the Copernican system to its early supporters, a tradition has grown up among highly respectable historians that the appeal was primarily aesthetic, or at least nonevidentiary. Some of Copernicus's remarks certainly lend themselves to such an interpretation. For example, he wrote: "We find, then, in this arrangement the marvellous symmetry of the universe, and a sure linking together in harmony of the motion and size of the spheres, such as could be perceived in no other way." (1976, Book I, chapter 10) Koyré interprets this passage as reflecting Copernicus's alleged reliance upon "pure intellectual intuition"—as opposed, presumably, to superior evidence. (1973, pp. 53-54) Similarly, Kuhn—who says (1970, p. vi) Koyré was a principal influence on him—remarks that in his main arguments for his theory, including the foregoing quotation, Copernicus appealed to the "aesthetic sense and to that alone." (1957, p. 180) Gingerich falls in with this line when he remarks that Copernicus's defense of his theory in Book I, chapter 10, is "based entirely on aesthetics." (1973a, p. 97)

This interpretation of the Copernican revolution may well have been one of the things that produced the irrationalist tendencies in Kuhn's general theory of scientific revolutions—i.e., his comparisons of such revolutions to conversion experiences, leaps of faith, and gestalt-switches. Certainly the Copernican case is one he frequently cites in such contexts. (1970, pp. 112, 151, 158)

4. Positive Heuristics and Novel Predictions

Clearly the most effective way to counter such a view is to show that Copernicus's aesthetic language really refers to his theory's evidentiary support, the particular terms having perhaps been chosen merely for a scientifically inessential poetic effect. One attempt to show just this is contained in a paper by Lakatos and Zahar (1975). They try to show that acceptance of Copernicus's theory was rational in the light of Lakatos's "methodology of scientific research programs," as modified by Zahar (1973). According to this view, the history of science should be discussed, not it terms of individual theories, but in terms of "research programs," each of which is a sequence of theories possessing a common "hard core" of fundamental assumptions and a "positive heuristic" guiding the construction of variant theories. One research program will supersede another if the new program is, and the old one is not, "theoretically" and "empirically progressive." These terms mean that each new theory in the series exceeds its predecessor in content and also predicts some "novel" fact not predicted by its predecessor, and that such predictions are from time to time confirmed. (Lakatos 1970, pp. 118, 132-134) It is implicit in this claim that the only observational phenomena that have any bearing on the assessment of a research program are those that are "novel." Accordingly Lakatos claimed that a program's success or failure in accounting for non-novel facts has little or no bearing on its assessment. (1970, pp. 120-121, 137)

In their joint paper on Copernicus, Lakatos and Zahar (1975) use Zahar's (1973) criterion of novelty: a fact is novel with respect to a given hypothesis if "it did not belong to the problem-situation which governed the construction of the hypothesis." Elsewhere (1982) I have argued that a different criterion is preferable, but I shall discuss only Lakatos's and Zahar's attempt to use this one, and other Lakatosian principles, to give a rational rather than aesthetic interpretation of the Copernican revolution. They assert that Ptolemy and Copernicus each worked on a research program and that each of these programs "branched off from the Pythagorean-Platonic program." Evidently, then, we have three programs to discuss. Now the positive heuristic of the last-mentioned program, they say, was that celestial phenomena are to be saved with the minimum number of earth-centered motions of uniform linear speed. Ptolemy, however, did not follow this principle, they continue, but only a weaker version of it, which allowed (epicyclic and eccentric) motions to have centers other than the earth, and motions to be uniform in angular speed about a point (the "equant") other than their centers.

It is well known that Copernicus objected to this feature of Ptolemaic astronomy. (1976, prefatory letter) The objection might be taken as aesthetic in character, and/or as symptomatic of Copernicus's intellectual conservatism. (Kuhn 1957, pp. 70, 147) But Lakatos and Zahar think they can show that the objection is reasonable in light of their methodology, which requires that the positive heuristic of a program provide an "outline of how to build" sets of auxiliary assumptions through some "unifying idea" which gives the program "continuity." New theories that violate this requirement are said to be "*ad hoc*$_3$." (Lakatos 1970, pp. 175-176) In particular, Ptolemy's use of the equant, because it violates the Platonic heuristic, is said to be *ad hoc*$_3$, or to be an example of "heuristic degeneration." (Lakatos and Zahar 1975, Section 4)

What I do not understand is why, from the standpoint of the Lakatos-Zahar methodology, it is supposed to be an objection to one program that it deviates from the positive heuristic of an *earlier* program. I take it that it would not be at all plausible to say—and Lakatos and Zahar do not seem to be saying—that Ptolemy was working on the Platonic research program, since (despite his use of circular motions that are uniform in some sense) his assumptions and methods were so different from Plato's. If they *do* want to say this, they owe us a criterion of identity for research programs that makes the claim true. As matters stand, their criticism of Ptolemy has as much cogency as a criticism of Einstein for deviating from the Newtonian heuristic by using laws of motion analogous but not identical to Newton's.

They also assert that Copernicus had two further criticisms of the Ptolemaic program: (1) that it was un-Platonic in giving two motions to the stellar sphere, and (2) that it failed to predict any novel facts. But they fail to give any references to support the attribution to Copernicus of (1) and (2), which do not appear in his (1976, prefatory letter) principal summary of his criticisms of Ptolemy et al., or elsewhere in his writings, as far as I know.

In attempting to discern what reasons favored the Copernican program upon publication of *On the Revolutions*, Lakatos and Zahar first claim that Copernicus "happened to improve on the fit between theory and observation." (p. 374) Why they make this claim is puzzling, since relative numbers of anomalies are supposed, as we have seen, to be irrelevant to the rational assessment of a research program. In any case, they then proceed to list a group of facts which they claim to be predictions of the Copernican program that were previously known but novel in Zahar's sense: (a) the "planets have stations and retrogressions"; (b) "the periods of the superior planets, as seen from the Earth are not constant"; (c) each planet's motion relative to the earth is complex, and has the sun's motion relative to the earth as one component; (d) the elongation of the inferior planets from the sun is bounded; (e) "the (calculated) periods of the planets strictly increase with their (calculable) distances from the Sun." (Section 5)

But there are grave difficulties in construing these consequences of Copernicus's assumptions as novel in the sense of not being in the problem-situation, the facts he was trying to account for. Certainly (a), (b), and (d) were long-known facts, which any astronomer of Copernicus's time would have expected any planetary theory to account for. Thus Copernicus certainly was trying to account for them. Perhaps Lakatos and Zahar nonetheless think that Copernicus designed his theory to account for other facts and then discovered to his pleasant surprise that it also accounted for (a), (b), and (d). But they present not a single argument to show that this was the case. Instead they make an entirely irrelevant point: that (a) and (b) follow easily from Copernicus's assumptions. On the other hand, (c) is merely a result of a coordinate-transformation of Copernicus's theory, not a piece of (independently obtainable) evidence for the theory. Point (e) presents a different problem. Since, as Lakatos and Zahar are aware, the calculations it refers to are done by means of the Copernican theory, it is impossible to claim that the correlation (e) describes is a long-known observational result that supports Copernicus's theory. Since Copernicus's theory provided the first and, at the time, the *only* reasonably satisfactory way to determine the planetary distances, there was certainly no question of a prior or subsequent independent determination of the distances, whose agreement with Copernicus's results could support his theory. If his determination of the planetary distances was a crucial consideration in its favor (and we shall see later that it was), Lakatos and Zahar have not shown why—or much (if anything) else about the Copernican Revolution.

5. Simplicity and Probability

I now turn to Roger Rosenkrantz's (1977) attempt to give a Bayesian account of the rationality of preferring Copernicus's to Ptolemy's theory within a few decades after the former was published.

Rosenkrantz is a Bayesian in the sense that he thinks that the probabilities of hypotheses form the basis for comparing them, and that probability $P(H)$ of hypothesis H changes to $P(H/x)$ when evidence x is obtained, where this conditional probability is computed using Bayes's theorem. He is not a Bayesian, however, in the stronger sense that he thinks prior probabilities are subjective. Instead he maintains that the objectively correct distribution is to be computed by assuming that entropy is maximized subject to given constraints. If we have a partition of hypotheses H_i, then the *support* each receives from observation x is defined as the *likelihood* $P(x/H_i)$. By Bayes's rule the posterior probability is

$P(H_i/x) = P(H_i)P(x/H_i)/P(x)$. (1977, pp. vii-ix)

As a Bayesian, Rosenkrantz must hold that all supposed virtues of theories—in particular, simplicity—are such only to the extent that they manifest themselves somehow in high probabilities. His theory of simplicity begins from the observation that we make a theory more complex if we add one or more adjustable parameters. For example,

(1) $(\exists a)(\exists b)(y = a + bx)$

is less complex than

(2) $(\exists a)(\exists b)(\exists c)(y = a + bx + cx^2)$.

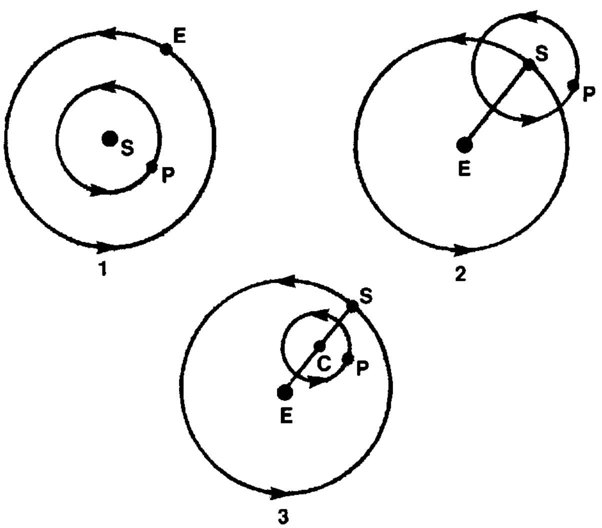
Moreover, if we take a *special case* of a theory—by which Rosenkrantz seems to mean that we set some of its free parameters equal to particular values—then we obtain a simpler theory, as when we set $c = 0$ in our examples. To make sense of such intuitions, Rosenkrantz introduces the concept of *sample coverage* of theory T for experiment X, defined as "the chance probability that the outcome of the experiment will fit the theory, a criterion of fit being presupposed." (1977, pp. 93-94) A criterion of fit will be of the form "$x \varepsilon R$," where $P(x \varepsilon R/T) \geq \alpha$, so that outcomes outside a certain high-likelihood region are said not to fit T. (1976, p. 169) "A 'chance' probability distribution is one conditional on a suitable null hypothesis of chance (e.g., an assumption of independence, randomness, or the like), constrained perhaps by background information." In the straightforward case in which the chance distribution is uniform, the sample coverage is just the proportion of experimental outcomes which fit the theory. In any case, simplicity (relative to the experiment, criterion of fit, and null hypothesis) is measured by smallness of sample coverage (1977, p. 94). It follows from this that (1) is simpler than (2) relative (e.g.) to an experiment yielding three points $\langle x,y \rangle$, to a reasonable criterion of fit, and to a uniform chance distribution. To deduce this (though Rosenkrantz does not say so), we need to make the number of outcomes finite by assuming that x and y have finite ranges divided into cells corresponding to the precision of measurement. Then the proportion of triples of these cells that fall close to some quadratic curve will obviously be larger than the proportion that fall near the subset of such curves that are straight lines.

To see the bearing of this on the evaluation of theories, consider (for ease of exposition) a theory H with adjustable parameter θ and special cases H_i ($i = 1, 2, \ldots, n$) corresponding to the n possible values of θ. Since $H \Leftrightarrow H_i \vee \ldots \vee H_n$,

$$P(x/H) = \sum_{i=1}^{n} P(x/H_i \wedge H)\, P(H_i/H).$$

Now let $\hat{H}$ be the H_i that maximizes $P(x/H_i)$—i.e., the best-fitting (best-supported) special case of H. Since $P(x/H)$ is a weighted average of $P(x/\hat{H} \wedge H)$ and quantities that are no greater, we can infer that "H is never better supported. . . than its best-fitting special case," and will be less well-supported if any of its special cases fit worse than $\hat{H}$, as happens in practice. The same conclusion holds if θ varies continuously, but the sum must be replaced by $\int P(x/\theta \wedge H)\, dp\, (\theta/H)$. We can therefore infer that the maxim "simplicity is desirable ceteris paribus" has its basis in the greater support which x affords $\hat{H}$ than H, $\hat{H}$ being a special case and hence simpler. (1977, p. 97)

Dissatisfied as we were with the accounts of Kuhn and of Lakatos and Zahar, Rosenkrantz now attempts to use his theory to show the rationality of the Copernican revolution. He represents the Copernican, Tychonic, and Ptolemaic systems for an inferior planet in Figures 1-3 respectively.



Figures 1-3. Reproduced by permission of publisher from Rosenkrantz 1977.

Since in the *Almagest* Ptolemy specified no values for the planetary distances, Rosenkrantz assumes that the distance EC to the epicyclic center C for planet P is a free parameter, constrained only in that C must lie on the line from the earth E to the sun S. Since he wants to maintain that all special cases of the Ptolemaic system fit the angular variations of P equally well, however, he evidently has in mind that the epicyclic radius extends proportionally as C moves towards S. Figure 2 is a special case of Figure 3, and "can be transformed into an equivalent heliostatic picture by the simple device of fixing S and sending E into orbit around S (Figure 1)." Thus Copernicus's theory is a special case of Ptolemy's. Considering only observations of planetary angles, all the special cases are equally well supported. Hence the Ptolemaic system (with free parameter) has exactly the same support as any of its special cases, including Figure 1. But if we consider Galileo's famous telescopic observation that Venus has a full set of phases, the only special cases of Figure 3 that fit at all well are those that have *P* moving in a circle centered (at least approximately) at S, as in Figure 2 or the "equivalent" Figure 1. (See Kuhn 1957, Figure 44.) Thus Copernicus's system is much better supported than Ptolemy's, whose support is obtained by averaging that of Copernicus's theory with many much smaller numbers. (1977, pp. 136-138) Note that this Bayesian view denies that there is any importance in the fact that the nature of Venus's phases followed from a theory proposed *before* they were observed. Cf. the common opposite opinion of Lakatos and Zahar 1975, p. 374 and Kuhn 1957, p. 224.

There are a number of simplifications in Rosenkrantz's version of the history. As he is aware (1977, pp. 156-159), all three astronomical systems were more complex than Figures 1-3 show, and they did not fit the angular data equally well. Let us assume that these simplifications are designed to enable us to concentrate on establishing the significance of the phases of Venus. This aside, one difficulty immediately strikes one. How could Copernicus's theory possibly be considered a special case of Ptolemy's, given that one asserts and the other denies that the earth moves? It would be irrelevant and false to say (and Rosenkrantz does not) that from a relativistic standpoint there is no distinction, since we and Rosenkrantz (1977, p. 140) are trying to articulate the underlying basis of the pro-Copernican arguments given in Copernicus's time. As far as I know, no one in the period covered by this paper—specifically, no one before Descartes—held that all motion is relative. And anyway, both special and general relativity distinguish accelerated from unaccelerated frames of reference, and Copernicus said the earth is accelerated. Nor can we say Copernicus's theory is a "special case" of Ptolemy's, meaning just that it has a smaller sample coverage; for Rosenkrantz's argument for the greater support of some special case requires defining "special case" as above in terms of fixing one or more parameters. But we cannot get a moving earth by fixing the position of *C* in a geostatic theory.

There is an easy way for Rosenkrantz to neutralize this objection. All he has to do is to replace the one-parameter theory of Figure 3 by a new theory obtained by disjoining it with its transformation into a heliostatic system. Then the disjunction of the Copernican and Tychonic systems does become a special case of the new theory—and, indeed, the best-fitting special case.

This change would cohere well with Rosenkrantz' remark that what Galileo's observations did is to narrow "the field to the Copernican and Tychonic alternatives." (1977, p. 138)

We can now face the really serious problems with Rosenkrantz's account. The first is that Copernicus did not in fact take himself to be faced with a Ptolemaic alternative with a free parameter *EC*. (1976, prefatory letter; Bk. 1, chap. 10) For he was well aware of the long tradition among Ptolemiac astronomers (section 2 above) of determining *EC* on the basis of the nesting-shell hypothesis. His objection to them on this score was not that *EC* was unspecified, but that it was specified on the basis of an "inappropriate and wholly irrelevant," "fallacious" assumption that caused their arguments on planetary order and distances to suffer from "weakness and uncertainty." In effect, Rosenkrantz has loaded the dice against Ptolemy and friends by inserting into their theory a nonexistent free parameter that lowers its support. He therefore fails to give an adequate analysis of the reasons why Copernicus attributed such importance to the relative merits of his and Ptolemy's determinations of the planetary distances.

The second serious problem with Rosenkrantz's account is that he fails to consider the bearing of the supposed prior probabilities of the Copernican and Ptolemaic theories on their evaluation. He writes that "simplifications of the Copernican system" such as the supposed one under discussion "are frequently cited reasons for preferring it to the Ptolemaic theory. Yet, writers from the time of Copernicus to our own, have uniformly failed to analyze these simplifications or account adequately for their force. The Bayesian analysis. . . fills this lacuna in earlier accounts. . . ." (1977, p. 140) But his Bayesian analysis asserts that preference among theories is governed by their probabilities, and support must be combined via Bayes's theorem with the prior probabilities of the theories and data to yield the posterior probabilities. And Rosenkrantz gives no indication of how these priors are to be computed from the maximum entropy rule (what constraints are to be used, etc.). Nor does he show how to compute the numerical values of the conditional probabilities so that the Bayes-theorem calculation can actually be done. Finally, he neglects to say what null hypothesis of chance and what background information define the chance distribution.

The last argument by Rosenkrantz (1977, pp. 143-148) we shall consider relates to a crucial passage by Copernicus (1976, Book I, chapter 10), which I shall discuss later:

> We find, then, in this arrangement the marvellous symmetry of the universe, and a sure linking together in harmony of the motion and size of the spheres, such as could be perceived in no other way. For here one may understand, by attentive observation, why Jupiter appears to have a larger progression and retrogression than Saturn, and smaller than Mars, and again why Venus has larger ones than Mercury; why such a doubling back appears more frequently in Saturn than in Jupiter, and still more rarely in Mars and Venus than in Mercury; and furthermore why Saturn, Jupiter and Mars are nearer to the Earth when in opposition than in the region of their occultation by the Sun and re-appearance. . . . All these phenomena proceed from the same cause, which lies in the motion of the Earth. (Copernicus 1976, Bk. I, chap. 10)

Rosenkrantz attempts to explain the force of the part of this argument which concerns the frequencies of retrogression as follows:

> . . . within the Ptolemaic theory, the period of an outer planet's epicycle can be adjusted at will to produce as many retrogressions in one circuit of the zodiac as desired. . . . while both theories fit this aspect of the data, the Copernican theory fits *only* the actually observed frequency of retrogression for each planet. . . . Hence, . . . qua special case of the geostatic theory, the heliostatic theory is again better supported. (Rosenkrantz 1977, pp. 143-148)

Let us handle the problem about "special case" as above, so that the argument is understood (as Rosenkrantz intends) as being for the disjunction of the Copernican and Tychonic theories. We can then easily see that the argument fails for essentially the same reason as Rosenkrantz's argument concerning the planetary distances. The best Ptolemaic theory of Copernicus's time did not, of course, merely assert that a given planet has one epicycle with *some* radius and *some* period. If this theory had been existentially quantified in this way, it would not have sufficed for the computation of the tables of observed positions, which Copernican astronomers sought to improve upon. Similarly, the Copernican theory could not fit just the observed frequencies of retrogression without specifying the radii and periods of the planets' revolutions. Obviously, then, Rosenkrantz is making an irrelevant and unfair comparison: between the Copernican theory *with* its specifications of radii and speeds, and a Ptolemaic theory weaker than Copernicus's real rival because of existential quantification over its fixed parameters. To obtain a Copernican account of the durations and angular widths of the retrograde motions, we need to specify radii and speeds; and we get a Ptolemaic account of the same appearances if we specify the comparable quantities.

I also cannot see why Rosenkrantz thinks his Bayesian account explains the basis of Copernicus's claim that his theory exhibits a "symmetry of the universe, and a sure linking together in harmony," or in Rosenkrantz's terms an "economy of explanation." Copernicus, as he explicitly said, was evidently referring to the fact that his theory explains a wide variety of phenomena on the basis of the hypothesis that the earth moves. Metaphorically, his theory "symmetrizes" or "harmonizes" these phenomena. But this virtue of his theory has nothing to do with the supposed fact that his theory is compatible only with certain specific observations. It could have this latter property even if it accounted for different sets of these observations on the basis of entirely different hypotheses.

To sum up then: despite the considerable merits that Rosenkrantz's Bayesianism has in connection with other problems in the philosophy of science, I do not think it helps us understand the Copernican revolution.

6. Bootstraps

The last published account of the Copernican revolution I shall discuss is based on Glymour's presentation of what he calls the "bootstrap strategy" of testing, which he says is a common but not universal pattern in the history of science. (1980, chapter 5) Briefly and informally, the strategy is this. One confirms a hypothesis by verifying an instance of it, and one verifies the instance by measuring or calculating the value of each quantity that occurs in it. In calculating values of quantities not themselves measured, one may use *other* hypotheses of a given theory—i.e., the theory can "pull itself up by its bootstraps." However, no hypothesis can be confirmed by values obtained in a manner that guarantees it will be satisfied. We cannot, then, test "$F = ma$" by measuring m and a and using this very law to calculate F; whereas we *can* use this procedure to obtain a value of F to substitute into "$F = GMm/r^2$" along with measured values of M, m, and r, and thereby test the latter law. So, Glymour concludes, Duhem-Quine holists are right in maintaining that hypotheses can be used jointly in a test, but mistaken in concluding that the test is therefore an indiscriminate one of the entire set. The described procedure, e.g., uses two laws jointly, but tests only the latter.

The falsification of an instance of a hypothesis may result solely from the falsity of other hypotheses used in testing it. Again, confirmation may result from compensatory errors in two or more hypotheses. Hence there is a demand for variety of evidence: preferably, a hypothesis is tested in many different ways—i.e., in conjunction with many different sets of additional hypotheses. Preferably also, these additional hypotheses are themselves tested.

As Glymour points out, his strategy of testing makes it possible to understand, e.g., why Thirring's theory of gravitation is not taken seriously by physicists. It contains a quantity

$$\eta_{\mu\nu} - f\psi_{\mu\nu}$$

which can be calculated from measurements using rods, clocks, test particles, etc. But the quantities $\psi_{\mu\nu}$ and $\eta_{\mu\nu}$ can be neither measured nor calculated from quantities that can. Either may be assigned any value whatsoever, as long as compensatory changes are made in the other. We might express this by saying these quantities are *indeterminate:* neither measurable nor computable from measured quantities via well-tested hypotheses. Consequently no hypotheses containing them can be confirmed, and Thirrings's theory is rejected.[1]

Glymour attempts to establish the importance of the bootstrap strategy by showing that it played a role in a variety of episodes in the history of science. He also applies it to the comparison of the Ptolemaic and Copernican planetary theories, but says that in this case (unlike his others) his arguments are merely intended as illustrative of the strategy and not as historically accurate accounts of arguments actually given around Copernicus's time. (Glymour 1980, chapter 6) I shall therefore not discuss here what Glymour does, but I shall consider instead to what extent the bootstrap strategy is helpful in enabling one to see the basis of Copernicus's own arguments against Ptolemaic astronomy and in favor of his own theory. Copernicus gives a helpful summary of his negative arguments in his prefatory letter:

> . . . I was impelled to think out another way of calculating the motions of the spheres of the universe by nothing else than the realisation that the mathematicians themselves are inconsistent in investigating them. For first, the mathematicians are so uncertain of the motion of the Sun and Moon that they cannot represent or even be consistent with the constant length of the seasonal year. Secondly, in establishing the motions both of the Sun and Moon and of the other five wandering stars they do not use the same principles or assumptions, or explanations of their apparent revolutions and motions. For some use only homocentric circles, others eccentric circles and epicycles, from which however the required consequences do not completely follow.

> For those who have relied on homocentric circles, although they have shown that diverse motions can be constructed from them, have not from that been able to establish anything certain, which would without doubt correspond with the phenomena. But those who have devised eccentric circles, although they seem to a great extent to have extracted from them the apparent motions, with numerical agreement, nevertheless have in the process admitted much which seems to contravene the first principle of regularity of motion. Also they have not been able to discover or deduce from them the chief thing, that is the form of the universe, and the clear symmetry of its parts. They are just like someone including in a picture hands, feet, head, and other limbs from different places, well painted indeed, but not modelled from the same body, and not in the least matching each other, so that a monster would be produced from them rather than a man. Thus in the process of their demonstrations, which they call their system, they are found either to have missed out something essential, or to have brought in something inappropriate and wholly irrelevant, which would not have happened to them if they had followed proper principles. For if the hypotheses which they assumed had not been fallacious, everything which follows from them could be indisputably verified. (1976, p. 25)

As Copernicus said more explicitly elsewhere, his first point is that his opponents had inaccurate theories of the precession of the equinoxes, which (with the solstices) define the seasons. (1976, Book III, chapters 1, 13) In particular, Ptolemy attempted to define a constant year by reference to the equinoxes, failing to account for the supposed fact that they recur (as Copernicus thought) at unequal intervals, and that only the sidereal year (with respect to the stars) is constant. Copernicus's second complaint is that his opponents disagreed among themselves in regard to the use of homocentric spheres as against epicycles and eccentrics. Moreover, he argued, those who used homocentrics had failed to achieve observational accuracy, because "the planets. . . appear to us sometimes to mount higher in the heavens, sometimes to descend; and this fact is incompatible with the principle of concentricity." (Rosen 1959, p. 57) Copernicus undoubtedly was referring here not to variations in planetary latitude, whose limits (but not times) Eudoxus *could* account for (Dreyer 1953, p. 103), but to variations in planetary distance ("height") and brightness, a long-standing problem for homocentrics. Although those who used epicycles and eccentrics (realistically interpreted) could handle this problem, they also used the equant, which violates the principle that the basic celestial motions are of uniform linear speed and which makes their system "neither sufficiently absolute nor sufficiently pleasing to the mind." (Rosen 1959, p. 57)

At first glance, it seems difficult to be sympathetic to this last argument—that is, to try (like Lakatos and Zahar) to find some generally acceptable pattern of scientific reasoning into which it fits. Kuhn explains part of what is going on at this point when he says that here Copernicus showed himself to be intellectually conservative—to feel that on this question at least "our ancestors" were right. (Copernicus, in Rosen, 1959, p. 57) Copernicus also evidently felt that his principle of uniformity had a "pleasing" intellectual beauty that lent it plausibility. The principle also derived some of its appeal from the false idea that trajectories that "pass through irregularities. . . in accordance with a definite law and with fixed returns to their original positions" must necessarily be compounded of uniform circular motions. Finally, he had arguments based on vague, implicit physical principles: irregularity would require "changes in the moving power" or "unevenness in the revolving body," both of which are "unacceptable to reason." (Copernicus 1976, Book I, chapter 4) The principle of uniform circularity does, then, rest after all upon considerations of a sort generally considered scientifically respectable: theoretical conservativism, theoretical beauty, mathematical necessities imposed by the phenomena, and consistency with physical principles and conditions.

In the remainder of the long quotation above, Copernicus made a fourth and a fifth critical point, without distinguishing them clearly. The fourth is that his opponents were not able satisfactorily to compute from their principles (together, presumably, with observational data) the distances of the planets from the earth and therefore the overall arrangement ("form") of the planetary system. They either omitted these "essential" quantities altogether, or else computed them using an "inappropriate" and "irrelevant" assumption—viz., the nesting-shell hypothesis (stated and criticized explicitly in Book I, chapter 10).

It is plain that we can interpret this Copernican criticism (which he regarded as his most important—"the chief thing") in the light of the principle, suggested by Glymour's theory, that it is an objection to the acceptance of a theory that it contains indeterminate quantities—i.e., nonmeasurable quantities that either cannot be computed from observational data at all, or cannot be computed via well-tested hypotheses. I wish to leave it open for the moment, however, whether well-testedness in the Copernican context can be interpreted on the basis of Glymour's approach.

Let us first consider why Copernicus thinks his own theory avoids his fourth objection to Ptolemy's. Copernicus showed how to use his heliostatic system of orbits, together with such data as the maximum angle between an inferior planet and the sun, to compute the relative distances between the sun and each planet. (1976, Book V; see also Kuhn 1957, pp. 174-176) He was referring to this fact about his theory when he said that it "links together the arrangement of all the stars and spheres, and their sizes, and the very heaven, so that nothing can be moved in any part of it without upsetting the other parts and the whole universe." (1976, prefatory letter) Contrary to Kuhn, this argument has little if anything to do with "aesthetic harmony," but concerns the determinateness of quantities. But were the hypotheses used to obtain the radii of these orbits well tested in a way in which the nesting-shell hypothesis was not? And does the bootstrap strategy describe this way? Or does the more familiar hypothetico-deductive (HD) strategy—roundly criticized by Glymour (1980, chapter 2) —provide a better basis for the comparison?

One of the cardinal points of the bootstrap strategy is that a hypothesis cannot be confirmed by values of its quantities that are obtained in a manner that guarantees they will agree with the hypothesis. By this standard the nesting-shell hypothesis was almost entirely untested. Except for the case of the sun (see section 2), Ptolemy had only one way to compute planetary distances—via the nesting-shell hypothesis—and there was therefore no way he could possibly obtain planetary distances that would violate it. If we ignore the difficulties in stating the HD method and follow our intuitions about it, we will say similarly that the nesting-shell hypothesis was untested by HD standards; for it was not used (perhaps in conjunction with other hypotheses) to deduce observational claims which were then verified. Ptolemy adopted it on the entirely theoretical ground that a vacuum between the shells is impossible (section 2 above).

But were the hypotheses Copernicus used in computing planetary distances well tested—and, if so, in what sense(s)? Let us begin with his principal axiom—that the earth moves (in a specified way). In his prefatory letter Copernicus had made a fifth and last criticism of his opponents: that they treated the apparent motion of each planet as an entirely separate problem rather than exhibiting the "symmetry" or "harmony" of the universe by explaining a wide variety of planetary phenomena on the basis of a single assumption used repeatedly. As the long quotation in section 5 (above) makes clear, Copernicus thought that his own theory did have this particular sort of unity. Essentially his claim was that his hypothesis E concerning the actual motion (around the sun) of the earth is used in conjunction with his hypothesis P_k describing the actual motion of each planet k in order to deduce a description O_k of the observed motion of planet k.

$$(*)\qquad \begin{array}{ccc} E & E & E \\ P_1 & P_2 & P_3 \\ \therefore O_1 & \therefore O_2 & \therefore O_3,\ \text{etc.} \end{array}$$

From the O_k's one can deduce various relations among the planets' sizes and frequencies of retrograde loops, and the correlation of maximum brightness with opposition in the case of the superior planets. One of Copernicus's main arguments for his theory, then, is that E is very well tested in that it is used (in conjunction with other hypotheses) to deduce the occurrence of all these observed phenomena. As he put it: "All these phenomena proceed from the same cause, which lies in the motion of the Earth." (Copernicus 1976, Book I, Chap. 10) Although Copernicus did not say so explicitly, the phenomena he listed are not only numerous, but also varied. Perhaps the phrase "all these phenomena" was intended to express this. And although he invoked no criterion of variety explicitly, a criterion that his argument satisfies is: the derivations (*) provide *varied* tests for E because different sets of hypotheses are conjoined with E to deduce the O_k's. In any case, the appeal in the argument from "harmony" is not merely to the "aesthetic sense and to that alone" (Kuhn 1957, p. 180), but to the nature of the observational support for the hypothesis E.

Despite the cogency of Copernicus's argument, Ptolemy could have made the reply that each of his determinations of a planetary angle was based on the hypothesis that the earth is stationary at the approximate center of the planet's motion, and that his hypothesis was therefore well tested in the same way E was. But I am just trying to discern Copernicus's testing strategy, and so shall ignore possible replies to it.

Now in some respects the argument from harmony sounds like the bootstrap strategy in operation: for Glymour's principle of variety of evidence also favors a hypothesis that is tested in more ways (other things being equal), where the ways are individuated by the sets of additional hypotheses used in the test. But the bootstrap strategy requires the measurement or computation (using other hypotheses) of every quantity occurring in a hypothesis in order to see if it holds in a given instance. And this does not appear to be what Copernicus had in mind in the argument we are discussing. We are not to observe the apparent position of some planet k at some time and then use this in conjunction with P_k to compute the actual position of the earth at that time to see if E holds then. Rather, the deductions are to proceed in the opposite direction: the "phenomena proceed from the cause" E, as Copernicus put it, and not vice versa. His method here is hypothetico-deductive, and involves no bootstrapping. That Copernicus thought his argument for the earth's motion was HD is also shown clearly by his statement (1976, Book I, chapter 11): "so many and substantial pieces of evidence from the wandering stars agree with the mobility of the Earth. . . the appearances are explained by it as by a hypothesis."

The set of deductions (*) can be thought of as parts of HD tests not only of E, but also of the descriptions P_k of the motions of the other planets. These, however, are not tested as thoroughly by (*) and the relevant data, each one being used to obtain only one of the O_k. But Copernicus also had other tests aimed more directly at the P_k, and they turn out to involve a complex combination of bootstrapping and hypothetico-deduction. We can illustrate his procedure by examining his treatment of Saturn (Copernicus 1976, Book V, chapter 5-9), which has the same pattern as his treatment of the other two superior planets. (see also Armitage 1962, chapter 6.) He began with three longitudes of Saturn observed at opposition by Ptolemy. He then hypothesized a circular orbit for Saturn eccentric to the earth's. From these data and hypotheses he deduced the length and orientation of the line between the two orbits' centers. He then repeated the procedure using three longitudes at opposition observed by himself, and noted with satisfaction that he obtained about the same length for the line between the centers. (Its orientation, however, seemed to have shifted considerably over the intervening centuries.) This is a bootstrap test of his hypothesis h concerning the length of the line. He used one triple of data and other hypotheses to obtain the quantity (length) in h. To have a test, the length had then to be obtained in a way that could falsify h; hence he used another triple of data and the same auxiliary hypotheses, computed the length anew, and thereby verified h.

This bootstrapping was only a preliminary step. Copernicus's final move was to use the computed length and orientation of the line between the centers to construct what he considered an exact description P_5 of Saturn's orbit (with one epicycle), and showed that P_5 and E yield (very nearly) the triples of data: "Assuming these [computed values] as correct and borrowing them for our hypothesis, we shall show that they agree with the observed appearances." (Book V, chapter 5) The positive results gave Copernicus sufficient confidence in P_5 to use it (with E) to construct extensive tables of observed positions for Saturn.

This procedure is somewhat peculiar, since Copernicus used the very same triples of data to obtain some of the parameters in P_5 and also to test P_5. Still the exercise is nontrival since (a) one of the parameters could be obtained from either triple of data; and (b) the additional parameters of P_5 might have resulted in inconsistency with the original data.

Peculiarities aside, the final step of the procedure is plainly HD: deducing from P_5 and E values for certain angles, comparing these with the data, and finally concluding that the hypotheses, because they agree "with what has been found visually [are] considered reliable and confirmed." (1976, Book V, chapter 12)

In arguing that Copernicus's method was mainly the HD and not the bootstrap, I have not been arguing against Glymour. For he freely admits (1980, pp. 169-172) that despite the difficulties in giving a precise characterization of the HD strategy, a great many scientists have nonetheless (somehow) managed to use it. What, then, are the morals of my story? The first moral is that the principle suggested by the bootstrap strategy, that indeterminate quantities are objectionable, has a range of application that transcends the applicability of the bootstrap concept of well-testedness. That is, we can see from Copernicus's arguments concerning the planetary distances that the computability of quantities from measured ones via well-tested hypotheses is considered desirable even if the testing in question is done mainly by the HD method. Indeed, Copernicus himself made this connection between the HD method and determinateness when he wrote that if his hypothesis is adopted, "not only do the phenomena agree with the result, but also it links together the arrangement of all the stars and spheres and their sizes. . . ." (1976, prefatory letter) Unfortunately the rationale for this principle within the bootstrap strategy—that its satisfaction makes tests possible or makes them better—is unavailable in an HD framework. What its rationale there might be I do not know.

To say exactly when a hypothesis is well tested in HD fashion would be a task beyond my means. But Copernicus's argument for a moving earth does at least suggest one principle whose applicability in other historical cases is worth examining—namely, that *a hypothesis is better tested (other things* *equal) if it is used in conjunction with a larger number of sets of other hypotheses to deduce statements confirmed by observation.* This principle of variety of evidence has a rationale similar to the one Glymour gives for his: that its satisfaction reduces the chance that one hypothesis is HD-confirmed in conjunction with a set of others only because of compensatory errors in them both.

7. Copernicus's Realistic Acceptance

Having completed our survey of various ways of accounting for such acceptance as the Copernican theory enjoyed down through the time of Galileo, and having made some preliminary suggestions about why the theory appealed to its author, I should now like to refine the problem by asking whether various astronomers accepted the theory as true or as just a device to determine observables such as angles, and what their reasons were. I shall begin with Copernicus himself.

It is well known that the question of Copernicus's own attitude concerning the status of his theory was initially made difficult by an anonymous preface to *De Revolutionibus,* which turned out to have been written by Osiander. The preface rehearses a number of the traditional arguments discussed earlier against the acceptability of a planetary theory (Copernicus's in this case) on a realistic interpretation: that "the true laws cannot be reached by the use of reason"; that the realistically interpreted theory has "absurd" observational consequences—e.g., that Venus's apparent diameter varies by a factor of four (Osiander ignored the phases' compensatory effect.); and that "different hypotheses are sometimes available to explain one and the same motion." (Copernicus 1976, pp. 22-23) Hence, Osiander concluded, Copernicus (like any other astronomer) did not put forward his hypotheses "with the aim of persuading anyone that they are valid, but only to provide a correct basis for calculation." And for this purpose it is not "necessary that these hypotheses should be true, nor indeed even probable." However, the older hypotheses are "no more probable"; and the newer have the advantage of being "easy." We should note that there is an ambivalence in Osiander's position. At some points he seems to be saying that while Copernicus's theory is at least as probable as Ptolemy's, we are uncertain and should be unconcerned whether it or any astronomical theory is true. But at other points he seems to be saying that the new theory is plainly false.

Despite Osiander's efforts, as he put it in a letter, to "mollify the peripatetics and theologians" (Rosen 1959, p. 23), it is quite clear that Copernicus believed that his hypothesis that the earth moves (leaving aside for the moment some of his other assertions) is a literally true statement and not just a convenient device. For example, in his statement of his seven basic assumptions in *Commentariolus*, he distinguished carefully between the *apparent* motions of the stars and sun, which he said do not in fact move, and the *actual* motions of the earth, which explain these mere appearances. (Rosen, pp. 58-59) Moreover, he felt compelled to answer the ancients' physical objections to the earth's motion, which would be unnecessary on an instrumentalist interpretation of this hypothesis. (1976, Book I, chap. 8)

Unconvinced by such evidence, Gingerich claims that Copernicus's writings are in general ambiguous as to whether his theory, or all astronomical theories, are put forth as true or as only computational "models." However, Gingerich holds that in at least three places, Copernicus was clearly thinking of his geometrical constructions as mere models "with no claim to reality": Copernicus sometimes mentioned alternative schemes (e.g., an epicycle on an eccentric vs. an eccentric on an eccentric) and "could have hardly claimed that one case was more real than the other"; and he did not use the same constructions to account for the planet's latitudes as for their longitudes. (Gingerich 1973b, pp. 169-170)

But the mere fact that someone mentions alternative theories does not show that he thinks that neither is true—i.e., that each is a mere device. He may simply think the available evidence is insufficient to enable one to decide which is true. Contrary to Gingerich, such an attitude towards particular alternatives is no evidence of a tendency in Copernicus to think that all his constructions—much less astronomical theories in general—are mere devices. And in fact, Copernicus appears just to have suspended judgment regarding his alternative systems for Mercury: the second, he wrote, was "no less credible" or "reasonable" than the first and so deserved mention. He did not say or imply that neither is true, or that no astronomical theory is true. (1976, Book V, chap. 32)

Perhaps my disagreement with Gingerich reflects only our differing concepts of what it is to regard a theory as a mere computation-device or (his term) "model"—i.e., to accept it instrumentally. If a person thinks that a theory permits the derivation of a given body of information but is simply unsure whether it is true, I do not say that he regards it as a mere device (accords it instrumental acceptance). I say this only if he actually *denies* that the theory is true—i.e., asserts its falsity or lack of truth value. I adopt this terminological stipulation to retain continuity with the traditional philosophical position of (general) instrumentalism, according to which all theories (referring to unobservables) are mere prediction-devices and are not true—that is, are false or truth-valueless (e.g., Nagel 1961, p. 118). But Gingerich's instances from Copernicus certainly have the merit of indicating that an intermediate attitude towards a particular theory sometimes occurs: that the theory is convenient for deriving certain information but may or may not be true.

In regard to the mechanism for determining a planet's longitudes vs. that for determining its latitudes, it is a mistake to interpret Copernicus either as holding that both are physically unreal or as being uncertain which of the two is physically real. Rather, Copernicus regarded the alternatives as two independent mechanisms that can be described separately for the sake of simplicity, but that operate together to produce the planet's exact trajectory. Thus, in giving his famous nonepicyclic account of retrogression, he wrote: "On account of the latitude it [the orbit of an inferior planet] should be inclined to AB [that of the earth]; but for the sake of a more convenient derivation let them be considered as if they are in the same plane." (1976, Book V, chapter 3) After describing all the longitude constructions as coplanar to a first approximation in Book V, he introduced the assumption that in fact these mechanisms reside in oscillating planes: "their orbits are inclined to the plane of the ecliptic. . . at an inclination which varies but in an orderly way." And he remarked in the case of Mercury:

> However, as in that case [Bk. V] we were considering the longitude without the latitude, but in this case the latitude without the longitude, and one and the same revolution covers them and accounts for them equally, it is clear enough that it is a single motion, and the same oscillation, which could produce both variations, being eccentric and oblique simultaneously. (1976, Book VI, chapter 2)

So in this case we do not have two equally "credible" mechanisms, but one approximate and one supposedly exact total mechanism.

Now the use of approximations or idealizations is connected with the traditional philosophical question of instrumentalism as a general thesis, and is thus at least indirectly related to our successor question of instrumental vs. realistic acceptance of particular theories. For example, one of the arguments for general instrumentalism discussed by Nagel is that "it is common if not normal for a theory to be formulated in terms of ideal concepts such as. . .perfect vacuum, infinitely slow expansion, perfect elasticity, and the like." (1961, p. 131) Contrary to Nagel, if this practice is only "common" and not universal, it does not provide much reason to say that scientific theories in general are just untrue prediction-devices. But certainly if a particular theory, such as Copernicus's coplanar treatment of longitudes, is presented explicity as an idealization, we must (obviously) conclude that it is being put forward for instrumental acceptance—as untrue but nonetheless convenient for accounting (at least approximately) for a specific body of information. But Copernicus's coplanar theory is not a very interesting example of a theory with instrumental acceptance, since Copernicus had in hand a replacement for it which he appears to have accepted realistically—in contrast with Proclus and Osiander, who asserted that realistic acceptance is always unreasonable.

I have claimed—what is not these days disputed—that Copernicus accorded realistic acceptance to the hypothesis that the earth moves. But it is quite another matter whether he accorded realistic acceptance to all the other features of his geometrical constitutions—leaving aside Gingerich's cases in which he was undecided or presented first approximations. In particular, did Copernicus really believe that the planets performed the remarkable acrobatic feat of moving in physical space on epicycles? Or were the epicycles mere computation-devices?

Let us consider this question briefly from the standpoint of generalized instrumentalism. Carnap held that a theory's "theoretical terms," unlike its "observational terms," receive no rules of designation. Hence sentences containing such terms can be "accepted" only in the sense that they can play a role in "deriving predictions about future observable events." They cannot also be accepted as true (of certain "theoretical entities") in some further sense than this. (Carnap 1956, pp. 45, 47) From such a philosophical perspective, it is perfectly clear which sentences of someone's theory are to be regarded as mere prediction-devices—viz., those which contain theoretical terms. This is clear, anyway, if we can somehow get around the well-known difficulties in distinguishing this class of terms.

But by anyone's standards it is a theoretical and not an observational question whether the earth moves at all or whether a planet moves on an epicycle (assuming our observations are done on the earth). Contrary to Carnap, however, this does not settle the question of whether Copernicus's assertions on these motions were put forward as true or as mere devices. Instead we must look at the specific scientific grounds (explicit or implicit) Copernicus had for adopting one view or the other in regard to each assertion. (Here I am indebted to Shapere 1969, p. 140)

Unfortunately I do not know of any place where Copernicus explicitly raised the issue of whether his epicycles were real physical orbits or convenient fictions, and then came down on one side or the other. Still, such evidence as there is favors the view that Copernicus considered his epicycles physically real. First, there is no place, as far as I know, where Copernicus (like Osiander) pointed out that his theory would have some false observational consequences if interpreted realistically. Second, recall that another sort of argument we have seen for instrumental acceptance is that the realistically interpreted theory is physically impossible. But Copernicus's physical theory (which was not fully articulated) allowed the compounding of uniform circular motions in epicyclic fashion. I call this principle of uniformity "physical" because part of the argument for it asserts the impossibility of "changes in the moving power." (Copernicus 1976, Book I, chapter 4) The third and most persuasive piece of evidence that Copernicus thought the actual physical orbits of some planets are epicyclic occurs in a passage of the sort Gingerich referred to, in which Copernicus discussed alternative constructions for the earth: "the same irregularity of appearances will always be produced and. . . whether it is by an epicycle on a homocentric deferent or by an eccentric circle equal to the homocentric circle will make no difference. . . . It is therefore not easy to decide which of them exists in the heaven." (1976, Book III, chapter 15) The implication here is that the epicyclic and the eccentric theories are logically incompatible despite the fact that they yield the same longitudes, that the two are about equally credible, and that exactly one of them is true. All this entails that the epicyclic theory has asserted content beyond its implications regarding longitudes—that its purpose is to describe the actual physical orbit of the earth.

Thus far I have said nothing about Copernicus's reasons for regarding his theory as true (except for some idealizations and uncertainties). Since he did not explicitly raise the issue of realistic vs. instrumental acceptance, it is not easy to determine what factors influenced his choice between the two. It will be best to defer consideration of this question, then, until we have seen what factors were operative in the thinking of those in whose writings the issue did arise explicitly. This will provide some basis, at least, for conjectures regarding Copernicus's own reasons.

8. The Instrumentalist Reception of Copernicanism

In the half-century or so after the publication of *De Revolutionibus*, Copernicus's theory was widely perceived as an improvement upon Ptolemy's in regard to observational accuracy and theoretical adequacy, and yet was almost universally regarded as untrue or at best highly uncertain. (Kuhn 1957, pp. 185-188; Westman 1972a, pp. 234-236) A concise expression of instrumental acceptance occurs in an astronomical textbook of 1594 by Thomas Blundeville: "Copernicus. . . affirmeth that the earth turneth about and that the sun standeth still in the midst of the heavens, by help of which false supposition he hath made truer demonstrations of the motions and revolutions of the celestial spheres, than ever were made before." (Quoted in Johnson 1937, p. 207)

Crucial in promoting this point of view, especially in the leading German universities, was a group of astronomers led by Phillipp Melanchthon (1497-1560) of the University of Wittenberg. Generally speaking, their opinion was that Copernicus's theory was credible primarily just in regard to its determinations of observed angles; that it was preferable to Ptolemy's in that it eschewed the abhorrent equant; but that the new devices needed to be transformed into a geostatic frame of reference, since the earth does not really move (Westman 1975a, pp. 166-167). For example, Melanchthon praised parts of Copernicus's theory in 1549 for being "so beautifully put together" and used some of his data, but held that the theory must be rejected on a realistic interpretation because it conflicts with Scripture and with the Aristotelian doctrine of motion. (Westman 1975a, p. 173) Similarly, Melanchthon's distinguished disciple Erasmus Reinhold was plainly more impressed by the fact that "we are liberated from an equant by the assumption of this [Copernican] theory" (as Rheticus had put it in Rosen 1959, p. 135) than by the theory's revolutionary cosmology. On the title page of his own copy of *De Revolutionibus*, Reinhold wrote out Copernicus's principle of uniform motion in red letters. And in his annotations he consistently singled out for summary and comment Copernicus's accomplishments in eliminating the equant, because of which (he said) "the science of the celestial motions was almost in ruins; the studies and works of this author have restored it." Thus Reinhold saw Copernicus entirely as the reactionary thinker he in some respects was, returning astronomy to its true foundations on uniform circular motions. In contrast, the paucity of Reinhold's annotations on the cosmological arguments of Book I indicates little interest, and in an unpublished commentary on Copernicus's work he maintained a neutral stance on the question of whether the earth really moves. But there was no doubt in his mind that Copernicus's geometric constructions provided a superior basis for computing planetary positions, and the many users of Reinhold's *Prutenic Tables* (1551) found out he was right, whatever their own cosmological views. (Westman 1975a, pp. 174-178)

An especially influential advocate of instrumental acceptance in our sense—i.e., with an explicit denial of truth—of the Copernican theory was Caspar Peucer, Melanchthon's successor as rector at Wittenberg. Like his predecessor and mentor, Peucer used Copernican values for various parameters, but denied the theory's truth on Scriptural and Aristotelian physical grounds in his popular textbook of 1553. He also suggested in 1568 that if certain parts of Copernicus's theory were reformulated in a geostatic frame, "then I believe that the same [effects] would be achieved without having to change the ancient hypotheses." (Quoted in Westman 1975a, pp. 178-181)

We have already seen one reason why the Wittenberg school refused to grant realistic acceptance to the Copernican theory: namely, that on a realistic interpretation the theory conflicts with Aristotelian physics and with Holy Scripture. (In section 2 above we saw that a parallel argument from Aristotelian physics had often been given against a realistically interpreted Ptolemaic theory.) But let us consider whether something else was involved as well. Westman makes some intriguing suggestions about this:

> . . . what the Wittenberg Interpretation *ignored* was as important as that which it either asserted or denied. In the writings both public and private of nearly every author of the generation which first received the work of Copernicus, the new analysis of the relative *linear* distances of the planets is simply passed over in silence. . . . questions about the Copernican ordering of the planets were not seen as important topics of investigation. In annotated copies of *De Revolutionibus* which are datable from the period *circa* 1543-1570, passages in Book I extolling the newly discovered harmony of the planets and the eulogy to the sun, with its Hermetic implications, were usually passed over in silence. (Westman 1975a, pp. 167, 181)

The reference is to the long quotation in section 5 above, and to adjacent passages.

Although Westman deserves our thanks for pointing this out, he makes no effort to explain why a lack of interest in Copernicus's planetary harmony and distances was associated with a witholding of realistic acceptance. Can we get any deeper? Consider first the question of distances, deferring that of harmony. In another paper (1979) I argued that a principle P operative in the nineteenth-century debates about the reality of atoms was that *it is an objection to the acceptance of a theory on a realistic interpretation that it contains or implies the existence of indeterminate quantities*. We have already seen that this principle played at least some role in astronomy: Ptolemy's theory had indeterminate planetary distances and tended to be refused realistic acceptance. One might well wonder, however, how this principle could explain refusal to accept the literal truth of Copernicus's theory, since he did make the planetary distances determinate. The answer appeals to a variant of P called P': *persons who either reject or ignore a theory's determinations of magnitudes from measurements via its hypotheses will tend to refuse it realistic acceptance*. P', though not precisely a corollary of P, is plausible given P: someone who rejects a theory's magnitude-determinations is likely to do so because he regards the hypotheses used as not well tested and hence regards the magnitudes as indeterminate; and someone who ignores the determinations is unaware of some of the support for realistic acceptance. Principle P' certainly fits the behavior as described by Westman of the first-generation response to Copernicus, especially among the Wittenberg group.

It also fits Johannes Praetorius (1537-1616), who studied astronomy at Wittenberg and later taught there and elsewhere. He expressed his instrumental acceptance of the Copernican theory as follows in a manuscript begun in 1592:

> Now, just as everyone approves the calculations of Copernicus (which are available to all through Erasmus Reinhold under the title *Prutenic Tables*), so everyone clearly abhors his hypotheses on account of the multiple motion of the earth. . . . we follow Ptolemy, in part, and Copernicus, in part. That is, if one retains the suppositions of Ptolemy, one achieves the same goal that Copernicus attained with his new constructions. (Westman 1975b, p. 293)

Like others associated with Wittenberg, Praetorius was most impressed by the improvements in observational accuracy over Ptolemy and even over Copernicus himself, that were achieved by Reinhold on Copernican assumptions, and by Copernicus's elimination of the "absurd" equant. But unlike the first generation at his school, he paid careful attention to

Copernicus's determinations of the planetary distances and to his evocations of the planetary system's "harmony" or "symmetry" (i.e., unified overall structure) that the new theory makes evident. Thus in lectures written in 1594 he listed Copernicus's values for the planetary distances and remarked about them: "this symmetry of all the orbs appears to fit together with the greatest consonance so that nothing can be inserted between them and no space remains to be filled. Thus, the distance from the convex orb of Venus to the concave orb of Mars takes up 730 earth semidiameters, in which space the great orb contains the moon and earth and moving epicycles." (quotations etc. in Westman 1975b, pp. 298-299)

Another point emerges from this quotation: Praetorius evidently followed the tradition of thinking of the planets as moving on solid spheres. This assumption created difficulties for him when he attempted to transform Copernicus's system into a geostatic one. For he found that using Copernicus's distances, "there would occur a great confusion of orbs (especially with Mars). . . . because it would then occupy not only the Sun's orb but also the great part of Venus'. . . ." Since intersections of the spheres are impossible, he argued, Copernicus's distances "simply cannot be allowed." He therefore roughly doubled the distance to Saturn, on the ground that there will still be plenty of distance to the stars, and claimed that with that done, "nothing prohibits us. . . from making Mars' orb greater so that it will not invade the territory of the Sun." (Westman 1975b, p. 298) Plainly, on Praetorius's version of the Copernican theory, the planetary distances are indeterminate: they are set through entirely theoretical considerations regarding the relative sizes of the spheres, instead of being computed from observational data via well-tested hypotheses. Because he rejected Copernicus's determinations of the distances, it is in accord with principle P' that he rejected Copernicus's theory on a realistic interpretation. And since the distances are indeterminate (even though specified) on his own theory, it is in accordance with our principle P that he granted instrumental acceptance to his own astronomical theory, since he refused realistic acceptance to any:

> . . . the astronomer is free to devise or imagine circles, epicycles and similar devices although they might not exist in nature. . . . The astronomer who endeavors to discuss the truth of the positions of these or those bodies acts as a Physicist and not as an astronomer—and, in my opinion, he arrives at nothing with certainty. (Westman 1975b, p. 303)

This quotation reveals that Praetorius was influenced by two additional factors we have observed earlier as counting against realistic acceptance of an astronomical theory. One is that *the theory is independent of physics:* that it is either outside the domain of physics, or if literally interpreted is inconsistent with the true principles of physics. (We have noted above, section 2, that Ptolemy, who seems sometimes to have been thinking of parts of his theory as mere devices, held to the first kind of independence; and that Proclus used the second as an argument against realistic acceptance of a Ptolemaic theory.) The second factor influencing Praetorius was the argument, also found in Proclus and others, that no realistically interpreted astronomical theory can be known to be true.

Another argument we found in the pre-Copernican period (e.g., in Proclus) against realistic acceptance of any planetary theory was that alternative systems of orbits may be compatible with the appearances; and that no particular system, therefore, can be asserted as literally true. A strengthened version of principle P—no theory T containing indeterminate quantities should receive realistic acceptance—is closely related to Proclus's principle, since various settings of the indeterminate parameters in T would in some cases produce various alternative theories equally compatible with the data. But although the notion that indeterminate quantities count against realistic acceptance continued to play a role in post-Copernican astronomy, Proclus's principle came under attack and seems not to have played much (if any) role in the thinking of instrumentally Copernican astronomers. For example, the influential Jesuit astronomer Christopher Clavius wrote in 1581 that it is not enough merely to speculate that there *may* be some other method than ours of accounting for the celestial appearances. For the argument to have any force, our opponents must actually produce the alternative. And if it turns out to be a "more convenient way [specifically, of dealing with the appearances] . . . we shall be content and will give them very hearty thanks." But failing such a showing, we are justified in believing that the best theory we actually have (Ptolemy's, he thought) is "highly probable"; for the use of Proclus's principle would destroy not just realistically interpreted astronomy, but all of natural philosophy: "If they cannot show us some better way, they certainly ought to accept this way, inferred as it is from so wide a variety of phenomena: unless in fact they wish to destroy . . . Natural Philosophy. . . . For as often as anyone inferred a certain cause from its observable effects, I might say to him precisely what they say to us—that forsooth it may be possible to explain those effects by some cause as yet unknown to us." (Blake 1960, pp. 31-37) Although this gets us ahead of and even beyond our story, eventually a principle very much like Clavius's appeared as Newton's fourth rule of reasoning in philosophy:

> *In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur by which they may either be made more accurate, or liable to exceptions.*
> This rule we must follow, that the argument of induction may not be evaded by hypotheses. (Newton 1934, p. 400)

Newton's rule is a stronger critical tool than Clavius's, since even if the alternative "hypothesis" is actually produced (but, by definition of "hypothesis," not by deduction from phenomena), Newton refused it consideration, whereas Clavius might even have preferred it if it proved to be more convenient, or better in accord with physics and Scripture.

Clavius thought that although Copernicus's theory was approximately as accurate as Ptolemy's, it was false because it conflicted with physics and Scripture. So he accorded the Copernican theory instrumental acceptance, and the Ptolemaic theory realistic acceptance as an approximation. In addition to consistency with physics and Scripture, he used one other consideration in favor of realistic acceptance which, I have argued elsewhere (1979), was also operative in the nineteenth-century atomic debates. It is progressiveness—i.e., the power of a theory to inform us of "novel" facts, of facts the theory's inventor did not know at the time of the invention. (See my 1982 paper on this definition of "novel.") Thus Clavius argued in favor of a realistic acceptance of Ptolemy's theory:

> But by the assumption of Eccentric and Epicyclic spheres not only are all the appearances already known accounted for, but also future phenomena are predicted, the time of which is altogether unknown: thus, if I am in doubt whether, for example, the full moon will be eclipsed in September, 1583, I shall be assured by calculation from the motions of the Eccentric and Epicyclic spheres, that the eclipse will occur, so that I shall doubt no further. . . . it is incredible that we force the heavens (but we seem to force them, if the Eccentrics and Epicycles are figments, as our adversaries will have it) to obey the figments of our minds and to move as we will or in accordance with our principles. (Blake 1960, p. 34)

Here we have as clear an example as could be desired of an explicit distinction being made between realistic and instrumental acceptance, and of progressiveness being used to decide between them.

The instrumentally Copernican astronomers discussed so far—i.e., astronomers who preferred Copernican angle-determinations but thought the theory needed a geostatic transformation—either ignored or rejected Copernicus's determinations of the planets' distances. But this is not true of the most famous of their group, Tycho Brahe. Like the members of the Wittenberg circle, with whom he had extensive contact, Tycho wrote in 1574 that although Copernicus "considered the course of the heavenly bodies more accurately than anyone else before him" and deserved further credit for eliminating the "absurd" equant, still "he holds certain [theses] contrary to physical principles, for example, . . . that the earth . . . move(s) around the Sun. . . ." He therefore invented his own system, which was essentially Copernicus's subjected to a transformation that left the earth stationary, the sun in orbit around it, and the other planets on moving orbits centered at the sun. Having become convinced that there are no solid spheres carrying the planets, since the comets he had observed would have to penetrate them, he did not share Praetorius's motivation for altering the Copernican distances, and therefore retained them. (Westman 1975b, pp. 305-313, 329; see Kuhn 1957, pp. 201-204)

Now the case of Tycho may seem to be anomalous from the standpoint of principles P and P': for Tycho accorded Copernicus's theory only instrumental acceptance, and yet neither ignored nor rejected Copernicus's determinations of the planetary distances. He would have conceded that these quantities were determinate, since he knew they could be computed from observations and certain of Copernicus's hypotheses on the *relative* positions of the planets, hypotheses that Tycho accepted and regarded as well tested. But this objection to P ignores its implicit ceteris paribus clause: P requires only that the determinateness of a theory's quantities should count in favor of realistic acceptance, and that indeterminateness should count against; there may nonetheless be countervailing considerations. Tycho's reasoning is entirely in accord with this notion. In one of his own copies of *De Revolutionibus*, Tycho underlined the passage (quoted in section 6 above) in which Copernicus stated that his theory links together the planetary distances, and commented on the passage: "The reason for the revival and establishment of the Earth's motion." And next to the passage (quoted in section 5 above) in which Copernicus spoke of the "symmetry of the universe" made evident by explaining so many varied phenomena in terms of the earth's motion, Tycho wrote: "The testimonies of the planets, in particular, agree precisely with the Earth's motion and thereupon the hypotheses assumed by Copernicus are strengthened." (Westman 1975b, p. 317) Despite these favorable remarks, Tycho rejected the Copernican theory on the sorts of grounds with which we are now familiar: that it conflicts with Scripture, physical theory, and certain observational data—specifically, Tycho's failure to detect the annual stellar parallax entailed by the earth's motion, and the relatively large apparent sizes of the stars given the great distances entailed by the undetectability of parallax. (Dreyer 1953, pp. 360-361) Although these considerations prevailed in his mind, it is still plain from the marginal notes just quoted that in accordance with principle *P* he counted it in favor of realistic acceptance of the hypothesis of the earth's motion that it made the planetary distances determinate and also (a point not yet discussed in this context) that *it satisfies Copernicus's principle of variety of evidence*, which in section 6 above I tentatively proposed explicating in terms of number of sets of auxiliary hypotheses.

That variety of evidence (metaphorically, "symmetry" or "harmony") counted in favor of realistic acceptance is also indicated by Westman's remark that in the period of instrumental acceptance of Copernicus's theory his remarks on harmony tended to be ignored.

We can sum up our discussion of the instrumental acceptance of the Copernican theory by listing the factors that, in the immediately post-Copernican period, were counted in favor of, or whose absence was counted against, acceptance on a realistic interpretation:

On such an interpretation, the theory

(1) satisfies the laws of physics,
(2) is consistent with other putative knowledge (e.g., the Scriptures),
(3) is consistent with all observational data,
(4) contains only determinate quantities,
(5) is able to predict novel facts,
(6) has a central hypothesis supported by a large variety of evidence,
(7) is within the realm of possible human knowledge.

Failing any of (1) - (3), a theory (if we assume it is still a convenient prediction-device in a certain domain) will tend to be accorded instrumental acceptance (with denial of truth). Supposed failure of (4) - (7) leads only to scepticism regarding truth.

We can now turn to those who accepted the Copernican theory on a realistic interpretation. If the foregoing is correct and complete, we should not find anything new.

9. Realistic Acceptance of Copernicanism

We saw in section 7 that the first person to accept the Copernican theory as literally true was Copernicus himself. We also saw that he argued explicitly that his theory satisfies the laws of physics, makes the planetary distances determinate, and has a central hypothesis supported by a wide variety of evidence. He also mentioned no observational data inconsistent with his theory, and implied in his prefatory letter to the Pope that Scripture conflicts with his theory only if "wrongly twisted." Since these considerations were all "in the air" in Copernicus's period as counting in favor of realistic acceptance, I think it is plausible to regard them as his reasons, although he did not make this more obvious by citing them in the context of an explicit distinction between realistic vs. instrumental acceptance.

The second astronomer to give realistic acceptance to Copernicanism was undoubtedly Georg Joachim Rheticus. He left Wittenberg to live and study with Copernicus (from 1539 to 1541), during which time he became familiar with the still unpublished Copernican theory. In 1540 he published *Narratio Prima*, the first printed account of the new theory of "my teacher," as he called Copernicus. In this work he nowhere indicated that he thought the theory to be just a convenient device. Moreover, he claimed (falsely) for unspecified reasons that at least some aspects of the Copernican theory could not be subjected to the sort of geostatic transformation (permitting instrumental acceptance) favored by others associated with Wittenberg: "I do not see how the explanation of precession is to be transferred to the sphere of stars." (Rosen 1959, pp. 4-5, 10, 164) Finally, in two copies of *De Revolutionibus* he crossed out Osiander's preface with red pencil or crayon. (Gingerich 1973c, p. 514) So it is obvious enough that his acceptance was realistic.

But why? First, he thought the theory is consistent with the most important relevant law of physics—uniform circularity of celestial motion—and all observational data:

> . . . you see that here in the case of the moon we are liberated from an equant by the assumption of this theory, which, moreover, corresponds to experience and all the observations. My teacher dispenses with equants for the other planets as well. . . . (Rosen 1959, p. 135)

> . . . my teacher decided that he must assume such hypotheses as would contain causes capable of confirming the truth of the observations of

> previous centuries, and such as would themselves cause, we may hope, all future astronomical predictions of the phenomena to be found true. (Rosen 1959, pp. 142-143)

It is somewhat puzzling that Copernicus's repudiation of the equant was a basis for both realistic and instrumental acceptance. The explanation is perhaps that his principle of uniformity can be thought of as an aesthetic virtue of a calculation-device—it is "pleasing to the mind" (Copernicus, in Rosen 1959, p. 57)—or as a physical principle (Copernicus 1976, Book I, chapter 4).

In the last quotation from Rheticus, he invoked the criterion of progressiveness (5), since he implied that the predictions were not known to be correct on some other ground (such as simple induction). He also contrasted the Copernican and Ptolemaic hypotheses in regard to the determinateness of planetary distances:

> . . . what dispute, what strife there has been until now over the position of the spheres of Venus and Mercury, and their relation to the sun. . . . Is there anyone who does not see that it is very difficult and even impossible ever to settle this question while the common hypotheses are accepted? For what would prevent anyone from locating even Saturn below the sun, provided that at the same time he preserved the mutual proportions of the spheres and epicycle, since in these same hypotheses there has not yet been established the common measure of the spheres of the planets. . . .
>
> However, in the hypotheses of my teacher, . . . (t)heir common measure is the great circle which carries the earth. . . . (Rosen 1959, pp. 146-147)

It will be noted that Rheticus ignored the nesting-shell hypothesis and took the Ptolemaic distances as entirely unspecified. Finally, Rheticus argued that Copernicus's central hypothesis that the earth moves was supported by a wide variety of evidence—specifically, the apparent motions of the five visible planets. "For all these phenomena appear to be linked most nobly together, as by golden chain; and each of the planets, by its position and order and every inequality of its motion, bears witness that the earth moves. . . ." (Rosen 1959, p. 165) God arranged the universe thus "lest any of the motions attributed to the earth should seem to be supported by insufficient evidence." (Rosen 1959, p. 161) Rheticus, then, appealed to criteria (1), (3), (4), (5), and (6) for realistic acceptance—criteria, I have argued, that were widely accepted in his period.

Westman is dissatisfied with "rational" explanations (such as the above) of Rheticus's behavior—i.e., explanations in terms of generally applicable and generally applied criteria regarding the theory and the evidence for it: "If today we might defend the rationality of this argument [from unity or harmony] on grounds of its empirical adequacy, its simplicity, and hence its considerable promise for future success, Rheticus went much further: he took it as evidence of the *absolute* truth of the entire theory of Copernicus." (Westman 1975a, pp. 184-186) Rheticus's "excessive zeal," then, requires not a rational but a "psychodynamic" explanation. We should note that Westman appears at this point to be following the psychohistorical analog of the "arationality assumption" widely accepted by sociologists of knowledge: *"the sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits"* (This formulation of the assumption and references to writers espousing it are in Laudan 1977, p. 202).

Westman finds himself at a disadvantage by comparison with professional psychoanalysts in that he cannot put Rheticus on the couch and get him to free-associate. Still, the psychohistorian has considerable information with which to work, including the fact that when Rheticus was fourteen his father was convicted of sorcery and beheaded. Westman thinks that part of the reason why Rheticus was attracted to Copernicus and thus to his theory was that "in Copernicus, Rheticus had found a kind and strong father with a streak of youthful rebellion in him: a man who was different, as Rheticus' father had been. . . ." But just as important was the fact that the Copernican system had the sort of *unity* that Rheticus's father so notably lacked after he had been beheaded. Rheticus had made "determined efforts—in the search for wholeness, strength, and harmony—to unconsciously repair the damage earlier wrought on his father." And Copernicus was a substitute father "who, like *the system he created,* had a head and a heart which were connected to the same body." (Westman 1975a, pp. 187-189)

Despite my considerable reliance upon and admiration for Professor Westman's brilliant contributions to our understanding of the Copernican revolution, I cannot follow him here. Leaving aside the question of the scientific merit of the general psychological theory on which his explanation of Rheticus's behavior is premised, the main problem with the explanation is that it simply is not needed. Rheticus argued explicitly that Copernicus's theory met certain criteria, and these were criteria used by many writers of his time (and other times) for realistic acceptance. Rheticus himself said that the importance of the "golden chain" unifying the planetary appearances was that it assures that there is sufficient evidence for the earth's motion. There is simply no need (and certainly no direct evidence) for an appeal to symbolic posthumous surgery on his father.

To make this criticism of Westman is not to accept the arationality assumption. In fact, I consider it absurd to deny that there can be *both* scientific reasons and psychosocial causes for a given cognitive attitude of a scientist. Still, when a psychosocial explanation lacks support for its initial conditions (here, the symbolic import of "unity"), so that the most that can be said for it is that we cannot think of any other explanation of the explanandum, then the existence of a documented rational account undermines the psychosocial one.

Laudan points out that a difficulty in using the arationality assumption is that one cannot apply it correctly unless one has an adequate theory of the rational merits of scientific theories; and, he says, use of an overly simple theory of rationality has been the cause of much confusion in the sociology (and presumably, the psychohistory) of ideas. (1977, pp. 205; 242, n.1) This, I believe, is what has gone wrong in Westman's discussion of Rheticus. "Empirical adequacy. . . simplicity. . . and. . . promise" is just not an adequate description of the scientific merits of Copernicus's arguments. Moreover, it is beside the point that today we think Copernicus's theory has only approximate and not "absolute" truth. Since we have much more astronomical evidence and know much more physics than Rheticus did, the rationality of his beliefs (in the light of his knowledge) cannot be assessed by reference to our knowledge.

Perhaps Westman would concede in response to my criticism that although the unity of the Copernican theory may have provided Rheticus with *reason* to believe it, still we need a psychodynamic explanation of why Rheticus was impressed by this unity in a way others of his generation generally were not. To this I would reply that a perfectly straightforward and plausible explanation of Rheticus's attitude is that he learned the theory from Copernicus's own lips; and since Copernicus obviously thought the "harmony" of his system was one of its main virtues, he no doubt forcefully called it to Rheticus's attention. This explanation is at any rate considerably less speculative than Westman's.

We find realistic acceptance and reasoning similar to that of Copernicus and Rheticus in another early Copernican, Michael Mästlin (1550-1631), whose support was crucial because of the pro-Copernican influence Mästlin exerted on his student Kepler. In his annotations of a copy of *De Revolutionibus*, which are consistently approving of Copernicus, he complained that Osiander's preface had made the mistake of "shattering [astronomy's] foundations" and suffered from "much weakness in his meaning and reasoning." This indicates that his acceptance was realistic. Commenting on Copernicus's attempts in Book I of *De Revolutionibus* to answer Ptolemy's physical arguments against the earth's motion, he wrote: "He resolves the objections which Ptolemy raises in the Almagest, Book I, chapter 7." So he evidently thought the Copernican theory satisfied the laws of physics. Finally, referring to Copernicus's arguments from determinateness of distances and variety of evidence, he wrote: "Certainly this is the great argument, *viz.* that all the phenomena as well as the order and magnitude of the orbs are bound together in the motion of the earth. . . . moved by this argument I approve of the opinions and hypotheses of Copernicus." (Quoted in Westman 1975b, pp. 329-334) Mästlin's realistic acceptance, then, was based at least on criteria (1), (4), and (6).

10. Kepler

The trend toward realistic acceptance of a heliostatic theory culminated in the work of Kepler. As Duhem wrote, "the most resolute and illustrious representative [of the realistic tradition of Copernicus and Rheticus] is, unquestionably, Kepler." (1960, p. 100) Kepler was quite explicit in making the distinction between realistic and instrumental acceptance of astronomical theories in general and of Copernicus's in particular, and explicit in saying where he stood on these issues. Not only Osiander, but Professor Petrus Ramus of the University of Paris, had asserted that Copernicus's theory used hypotheses that were false. Ramus wrote: "The fiction of hypotheses is absurd. . . would that Copernicus had rather put his mind to the establishing of an astronomy without hypotheses." And he offered his chair at Paris "as the prize for an astronomy constructed without hypotheses." Kepler wrote in his *Astronomia Nova* of 1609 that had Ramus not died in the meantime, "I would of good right claim [his chair] myself, or for Copernicus." He also indignantly revealed that Osiander was the author of the anonymous preface to *De Revolutionibus* and asserted: "I confess it a most absurd play to explain the processes of nature by false causes, but there is no such play in Copernicus, who indeed himself did believe his hypotheses true. . . nor did he only believe, but also proved them true." (Blake 1960, p. 43) Kepler intended to proceed in the same realistic spirit: "I began this work declaring that I would found astronomy not upon fictive hypotheses, but upon physical causes." (Quoted in Westman 1971, p. 128) Astronomy "can easily do without the useless furniture of fictitious circles and spheres." (Kepler 1952, p. 964) In taking such a view Kepler was consciously aware of contributing to a revolution not only in the prevailing theories in astronomy but also in the prevailing view of the field's purposes. He described his work as involving "the unexpected transfer of the whole of astronomy from fictitious circles to natural causes, which were the most profound to investigate, difficult to explain, and difficult to calculate, since mine was the first attempt." (Quoted in Gingerich 1973d, p. 304)

According to Kuhn, disagreement over the problems, aims, and methods of a field is one of the things that makes competing schools "incommensurable"—i.e., makes arguments for either of them circular, the choice a matter of "faith" or a "conversion experience" based, sometimes, on "personal and inarticulate aesthetic considerations" rather than on "rational" argument. (1970, pp. 41, 94, 151, 158) But in fact Kepler did present rational (if not always decisive) arguments for the shift in astronomical aims that he advocated, grounds for asserting that these aims could and should be achieved. Most of these grounds were neither inarticulate nor merely personal, but were explicitly stated attempts to show that a heliostatic theory could meet the widely used criteria for realistic acceptance.

We saw (section 8 above) that one such criterion, in this historical period and others, is progressiveness—prediction of novel facts. Thus we are not surprised to find Kepler arguing as follows in his *Mysterium Cosmographicum* (1596) for an essentially Copernican theory—i.e., one that has the planets somehow orbiting a stationary sun:

> My confidence was upheld in the first place by the admirable agreement between his conceptions and all [the objects] which are visible in the sky; an agreement which not only enabled him to establish earlier motions going back to remote antiquity, but also to predict future [phenomena], certainly not with absolute accuracy, but in any case much more exactly than Ptolemy, Alfonso, and other astronomers. (Quoted in Koyré, 1973, p. 129)

Kepler (*ibid*, p. 133) also echoed Copernicus's and Rheticus's appeals to criterion (6), well-testedness: "Nature likes simplicity and unity. Nothing trifling or superfluous has ever existed: and very often, one single cause is destined by itself to [produce] several effects. Now, with the traditional hypotheses there is no end to the invention [of circles]; with Copernicus, on the other hand, a large number of motions is derived from a small number of circles." (1973, p. 133) As Koyré pointed out, this claim about numbers of circles is an overstatement which had become traditional among Copernicus and his followers. But we can accept the part of this argument alluding to the variety of evidence for the earth's motion.

We also saw (sections 8-9 above) that realistic acceptance tended to be associated with belief and interest in the determinate values of the planetary distances that Copernicus's theory and data provided. There can be no doubt that Kepler had both belief and the most intense interest. In 1578 Kepler's teacher Michael Mästlin had published a theory of the motion of the comet of 1577 that asserted that it moved within the heliocentric shell of Venus, a theory that presupposed the Copernican arrangement of the inferior planets and his values for their distances: "I noticed that the phenomena could be saved in no other way than if. . . [the comet's radii] were assumed to be 8420 parts when. . . the semidiameter of the [earth's orbit] is 10,000; and likewise, when the semidiameter of Venus' eccentric is 7193." Kepler wrote in 1596 that Mästlin's theory that the comet "completed its orbit in the same orb as the Copernican Venus" provided "the most important argument for the arrangement of the Copernican orbs." (Quotations in Westman, 1972b, pp. 8,22) A second argument for Kepler's acceptance of (at least as approximations) and interest in Copernican determinations of the distances is that the main purpose of his *Mysterium Cosmographicum* was to explain the distances (as well as the number) of the planets on the basis of the assumption that their spheres are inscribed within a nest formed by the five regular solids. This work was published with an appendix by Mästlin containing improved Copernican calculations of the distances based upon the Prutenic Tables (Westman 1972b, pp. 9-10; Koyré, 1973, pp. 146-147). The third way in which approximately Copernican values for the sun-to-planet distances were important for Kepler was that he held that a planet's orbital period is proportional to its mean distance raised to the 3/2 power, and that its linear speed is inversely proportional to its distance from the sun. And from either premise he deduced his crucial conclusion that the planets are made to revolve in their orbits neither by their supposed souls (Plato) nor their intrinsic nature (Copernicus), but by a physical force orginating in the sun. (Kepler 1952, p. 895)

A corollary of our principle (4) governing realistic acceptance (section 8 above) is that a theory's failure to provide any means at all—let alone well-tested hypotheses—to determine some of its parameters counts against realistic acceptance. Kepler appealed to this corollary when he tried to show that Ptolemy believed his theory was more than a prediction-device: "to predict the motions of the planets Ptolemy did not have to consider the order of the planetary spheres, and yet he did so diligently."[2] (Kepler, forthcoming) Since the hypothesis from which Ptolemy obtained the order (that it corresponds to increasing orbital period) was entirely untested, I do not say that the order was determinate, but only specified.

We have seen earlier that such writers as Proclus and Praetorius argued against realistic acceptance of any astronomical theory on the grounds that knowledge of the full truth of such a theory exceeds merely human capacities and is attainable only by God. Aware of this traditional instrumentalist argument, Kepler felt obligated to argue on theological grounds that there is no *hubris* in claiming to know the true geometry of the cosmos:

> Those laws [governing the whole material creation] are within the grasp of the human mind; God wanted us to recognize them by creating us after his own image so that we could share in his own thoughts. For what is there in the human mind besides figures and magnitudes? It is only these which we can apprehend in the right way, and. . . our understanding is in this respect of the same kind as the divine. . . . (Letter of 1599, in Baumgardt 1951, p. 50.)

To supplement his theological arguments, Kepler also attempted to undermine such support as astronomical scepticism received from the unobservability of the planets' orbits in physical space: "But Osiander. . . (i)f [you say that] this art knows absolutely nothing of the causes of the heavenly motions, because you believe only what you see, what is to become of medicine, in which no doctor ever perceived the inwardly hidden cause of a disease, except by inference from the external bodily signs and symptoms which impinge on the senses, just as from the visible positions of the stars the astronomer infers the form of their motion." (Kepler, forthcoming)

Another epistemological argument we saw was popular among astronomical instrumentalists asserts that since there are, or may be, alternative systems of orbits equally compatible with all observational data, no one system can be asserted as physically correct. About twenty years after the Ptolemaic astronomer Clavius attacked the argument from observational equivalence, Kepler mounted his own attack from a Copernican viewpoint. First, he argued, sets of hypotheses—some true, some false—which have exactly the same observational consequences are found (if ever) far less frequently than those who use the argument from equivalence suppose:

> In astronomy, it can scarcely ever happen, and no example occurs to me, that starting out from a posited false hypothesis there should follow what is altogether sound and fitting to the motions of the heavens, or such as one wants demonstrated. For the same result is not always in fact obtained from different hypotheses, whenever someone relatively inexperienced thinks it is. (Kepler, forthcoming)

For example, Kepler argued, Magini attempted (1589) to produce a Ptolemaic theory agreeing with the *Prutenic Tables*, but failed to obtain Copernicus's prediction that Mars has a greater parallax than the sun. Kepler believed that whenever two conflicting hypotheses give the same results for a given range of phenomena, at least one of them can be refuted by deriving observational predictions from it in conjunction with new auxiliary hypotheses:

> "And just as in the proverb liars are cautioned to remember what they have said, so here false hypotheses which together produce the truth by chance, do not, in the course of a demonstration in which they have been applied to many different matters, retain this habit of yielding the truth, but betray themselves." (Kepler, forthcoming)

Thus a false hypothesis may occasionally yield a true prediction, but only when it chances to be combined with an auxiliary hypothesis containing a compensatory error, as when Copernicus proposed a lunar latitude and a stellar latitude, both too small by the same amount, and thus obtained a correct prediction for an occultation of the star by the moon. (Kepler, forthcoming) Conjoined with different auxiliary hypotheses, this one on the moon would certainly "betray itself." Kepler was arguing—exactly in accordance with (6) of section 8 above—that we have good reason to accept a hypothesis realistically when it is supported by a variety of phenomena, where this means "in conjunction with many sets of auxiliary hypotheses." Moreover, he appealed to the rationale for this idea, which in section 6 above we took from Glymour and adapted to the HD context—namely, that it reduces the chance of compensatory errors.

Kepler also considered a somewhat different way of stating the instrumentalist argument from observational equivalence. The Copernican and Ptolemaic hypotheses are concededly both compatible with, and both imply, e.g., the daily motion of the whole heaven. Ptolemy apparently inferred, partly because of this property of his theory, that it is true. But, the objection runs, the Copernicans think Ptolemy was thus led into error. "So, by the same token, it could be said to Copernicus that although he accounts excellently for the appearances, nevertheless he is in error in his hypotheses." (Trans. Jardine 1979, p. 157)

Kepler's rejoinder to this objection is as follows:

> For it can happen that the same [conclusion] results from two suppositions which differ in species, because the two are in the same genus and it is in virtue of the genus primarily that the result in question is produced. Thus Ptolemy did not demonstrate the risings and settings of the stars from this as a proximate and commensurate middle term: 'The earth is at rest in the centre'. Nor did Copernicus demonstrate the same things from this as a middle term: 'The earth revolves at a distance from the centre'. It sufficed for each of them to say (as indeed each did say) that these things happen as they do because there occurs a certain separation of motions between the earth and the heaven, and because the distance of the earth from the centre is not perceptible amongst the fixed stars [*i.e.*, there is no detectable parallactic effect]. So Ptolemy did not demonstrate the phenomena from a false or accidental middle term. He merely sinned against the law of essential truth *(kath'auto)*, because he thought that these things occur as they do because of the species when they occur because of the genus. Whence it is clear that from the fact that Ptolemy demonstrated from a false disposition of the universe things that are nonetheless true and consonant with the heavens and with our observations—from this fact I repeat—we get no reason for suspecting something similar of the Copernican hypotheses. (Trans. Jardine 1979, p. 158)

Kepler's point is this. Let H = "The heaven moves (in certain way) about a stationary earth." E = "The earth moves (in a certain way) within a stationary heaven," R = "The earth and heavens have a (certain) relative motion," and O = "The stars rise and set (in certain ways)." Since H implies O, it may appear that the verification of O supports H. But Aristotle required that in a premise of a "demonstration," or "syllogism productive of scientific knowledge," the predicate must belong to the subject "commensurately and universally," which he took to entail both that it belongs "essentially" and also that the subject is the "primary subject of this attribute." Aristotle's example of a violation of this latter requirement is the statement that any isosceles triangle has angles equal to 180°. (*Post. Anal.* I, 2,4; in McKeon 1941). Evidently, then, it would be a "sin" against this requirement to use a specific predicate when a more general one would yield the conclusion. (Thus far I follow Jardine 1979, p. 172). Kepler's application of this idea to Ptolemy is as follows: the syllogism that leads validly from H to O is not a proper demonstration, since the weaker premise R would suffice; hence the verification of O gives HD-support only to R, and the falsity of H casts no doubt on HD reasoning.

The supposedly erroneous reasoning that Kepler here attributed to Ptolemy is the same as what occurs in the "tacking paradox," an objection to the HD account of scientific reasoning discussed by Glymour (1980, chapter 2). The paradox is that if hypothesis h entails and is hence HD-supported by evidence e, then the same holds for "h & g," where g is an arbitrary, "tacked on" sentence. Ptolemy obtained support for R (i.e., "H or E"), but then tacked on $\sim E$ and claimed he had support for the conjunction and hence for H.

The difficulty with Kepler's argument (and with the HD account generally) is that it is quite unclear when tacking on is allowed and when it is not. We do not want to say, as Kepler's argument suggests, that no premise is ever HD-confirmed when a weaker one would have sufficed for the deduction. This would entail that whenever one applies, say, Newton's law of gravitation to objects within a certain region, one obtains no support for the law, but only for the hypothesis that the law holds within that region. On the contrary, we can tack on the law's restriction to the region's complement. But why? Thus Kepler's reply to the objection to HD reasoning answers it by revealing another, more damaging one.

When two hypotheses seem to be observationally indistinguishable, one way Kepler thought they could be distinguished is by relating them to what he called "physical considerations": "And though some disparate astronomical hypotheses may yield exactly the same results in astronomy, as Rothmann insisted. . . of his own mutation [geostatic transform] of the Corpernican system, nevertheless a difference arises because of some physical consideration." (Kepler, forthcoming). Physics, in this context, includes dynamics and cosmology—theories of the causes of motion and of the large-scale structure of the universe. His example here is cosmological: Copernicus's system and its geostatic transform differ in that the latter can "avoid postulating the immensity of the fixed stars" required by the former to explain the absence of detectable stellar parallax. Since Kepler had no way to show (without appeal to Copernicus's theory) that more sensitive instruments would detect stellar parallax due to the earth's revolution, this consideration provided no reason to prefer Copernicus's theory to a geostatic transform of it such as Tycho's. But this neutrality was appropriate in a piece titled "A Defence of Tycho Against Ursus."

In other works, however, Kepler used physical considerations—specifically, about the causes of planetary motions—to argue in favor of his own theory, which was Copernican in using a moving earth, non-Copernican in using elliptical orbits. In ancient and medieval astronomy the problem of why the planets move had either not arisen, or had been solved in a very simple way. To the extent that hypothesized motions were viewed as mere computation-devices, the problem of explaining them dynamically did not arise. Motions that were considered physically real, such as those of the spheres carrying the stars or planets, were usually explained as due to the spheres' "nature" or to spiritual intelligences attached to them. But Kepler, in part because be thought that God created nothing haphazardly but followed a rational plan knowable by man, sought to understand why the planets move as they do. (Koyré 1973, pp. 120-122)

In accordance with principle (1)—consistency with the laws of physics counts in favor of realistic acceptance—the ideas of physical explanation and realistic interpretation of planetary orbits were intimately connected in Kepler's mind:

> Consider whether I have made a step toward establishing a physical astronomy without hypotheses, or rather, fictions, ... The force is fixed in the sun, and the ascent and descent of the planets are likewise fixed according to the greater or lesser apparent emanation from the Sun. These, therefore, are not hypotheses (or as Ramus calls them) figments, but the very truth, as the stars themselves; indeed, I assume nothing but this. (Quoted in Gingerich 1975, p. 271)

> . . . Astronomers should not be granted excessive license to conceive anything they please without reason: on the contrary, it is also necessary for you to establish the probable cause of your Hypotheses which you recommend as the true causes of Appearances. Hence, you must first establish the principles of your astronomy in a higher science, namely Physics or Metaphysics. . . . (quoted in Westman, 1972a, p. 261)

In addition to the foregoing arguments based on criteria (1) and (4)-(7) of section 8 above for realistic acceptance—criteria that had been used by earlier writers—Kepler formulated two criteria of his own. One of these might be called "explanatory depth": it counts in favor of realistic acceptance of a theory that

(8) it explains facts that competing theories merely postulate.

After asserting the superior accuracy of Copernicus's retrodictions and predictions, Kepler remarked: "Furthermore, and this is much more important, things which arouse our astonishment in the case of other(s) [astronomers] are given a reasonable explanation by Copernicus. . . ." In particular, Ptolemy merely postulated that the deferent motions of the sun, Mercury, and Venus have the same period (one year). Copernicus, in contrast, could explain this equality on the basis of his theory that the planets revolve around the sun. Transformed into an earth-centered system, this theory yields components of the sun's and each planet's motions that are, as Kepler put it, "projections of the earth's proper motion on to the firmament." (Quoted in Koyré 1973, pp. 129, 136-137)

Second, Kepler argued that readers of Ptolemy should be astonished that the five planets, but not the sun and moon, show retrograde motion; whereas Copernicus can explain these facts, specifically by saying the five planets have epicycles of such speeds and sizes as to produce retrograde motion but the sun and moon do not. Kepler presumably meant that Copernicus's theory, when transformed geostatically, yields these statements about epicycles. Using Copernicus's figures for the radii and periods of all the planets' heliostatic orbits, one can show that their geostatic transforms will contain combinations of circles producing retrogression. In contrast, the moon, since it shares the earth's heliocentric motion, does not have a component of its geostatic motion that mirrors the earth's orbit and thereby yields retrogression, as does the epicycle of a superior planet. Finally, Kepler argued, since the earth's heliostatic orbit is circular with constant speed, it follows that the sun's geostatic orbit shows no retrogression. (Koyré 1973, pp. 136-137)

Kepler's third and fourth points are similar to ones made by Copernicus himself (section 5 above). The third is that Ptolemy postulates but cannot explain the relative sizes of the planets' epicycles, whereas their ratios can be obtained by transforming Copernicus's system into a geostatic one. The fourth is that Ptolemy postulates that, but does not explain why, the superior planets are at the closest point on their epicycles (and hence brightest) when at opposition with the sun, whereas this fact too results from a geostatic transformation of Copernicus's system.

Kepler sometimes said these four arguments are designed to show Copernicus' theory is preferable to Ptolemy's (Koyré 1973, p. 136), and sometimes to unnamed "other" astronomers' (p. 129). It is worth noting, however, that arguments from explanatory depth do not establish the superiority of Copernicus's system to Tycho's. Either of these can explain anything (regarding relative motions within the solar system) asserted by the other, by means of the appropriate transformation. (This symmetric relation does not hold between Copernicus's theory and Ptolemy's, since a heliostatic transformation of the latter would yield few if any features of the former. For example, the transform would have the planets orbiting a moving earth.) Of all Kepler's arguments, it is only the dynamical ones considered above—that there is a plausible physical explanation why the planets should move around the sun, but not why the sun should move around the earth carrying the other planets' orbits with it—that favor Copernicus's theory over Tycho's.

The last argument by Kepler that I shall consider has a remarkably contemporary ring. One of Ursus's arguments against Tycho had been an induction on the falsity of all previous astronomical theories. (Jardine 1979, p. 168) Kepler's reply was that despite the continuing imperfection of astronomical theories, cumulative progress had nonetheless been made at least since Ptolemy. Erroneous in other respects, Ptolemy's theory at least taught us that "the sphere of the fixed stars is furthest away, Saturn, Jupiter, Mars, follow in order, the Sun is nearer than them, and the Moon nearest of all. These things are certainly true. . . ." Tycho taught us at least that "the Sun is the centre of motion of the five planets," Copernicus that the earth-moon distance varies less than Ptolemy said, and unspecified astronomers established the "ratios of the diameters of the Earth, Sun and Moon. . . . Given that so many things have already been established in the realm of physical knowledge with the help of astronomy, things which deserve our trust from now on and which are truly so, Ursus' despair is groundless." (Kepler, forthcoming) Similarly, Kepler argued in favor of Copernicus that he "denied none of the things in the [ancient] hypotheses which give the cause of the appearances and which agree with observations, but rather includes and explains all of them." (Quoted in Jardine 1979, pp. 157-158). This last statement is part of Kepler's reply to the objection that Copernicus's theory might be false even though it saves the phenomena.

Clearly, then, Kepler was appealing to a principle we have not previously come across: it counts in favor of realistic acceptance of a theory that

(9) it agrees with some of the nonobservational claims of some previous theories purporting to explain the same observations.

I say "nonobservational" because the agreed-upon claims Kepler mentioned here were not observable phenomena such as brightnesses and angular positions, but unobservables such as the planets' orbits. The tacit assumption behind (9) seems to be that if the astronomical theories produced through history were merely a series of devices for predicting observations, there would be relatively little reason to expect them to contain any common non-observational parts: whereas this is what we would expect if the sequence of principal theories contains a growing set of true descriptions of astronomical reality.

11. Contemporary Realisms

I said this argument sounds contemporary because it is echoed with little change in an argument Hilary Putnam has recently discussed, attributing it to R. Boyd.[3] (Forthcoming) According to this argument, a new and better theory in a given field of science usually implies "the *approximate truth of the theoretical laws of the earlier theories in certain circumstances*." Further, scientists usually require this feature of new theories in part because they believe that (a) the "laws of a theory belonging to a mature science are typically approximately *true*"; and meeting this requirement is fruitful in part because this belief is true. (Putnam 1978, pp. 20-21) Like Kepler, then, Boyd thinks that there is a degree of agreement in the nonobservational hypotheses of successive theories in a given field, and that this tends to show that these hypotheses are (partially or approximately) true and are not just prediction-devices. And like Kepler, Putnam (1978, p. 25) thinks that this consideration helps rescue contemporary science from the charge, based on induction from past theories, that it is probably false. (1978, p. 25)

Boyd labels as "realism" the conjunction of (a) above with (b): "terms in a mature science typically *refer*." (Putnam 1978, p. 20) This essay is mainly about a quite different thesis also called "realism"—the thesis that *scientific theories in general are put forward as true, and accepted because they are believed to be true*. But we have just seen that our astronomical case study has at least some connection with Boyd's kind of realism as well. I should like to conclude with some remarks about the more general question of the relevance of this case-study to various versions of realism.

Let us call the version of realism I have mainly been discussing "purpose-realism," since it is based on a thesis about the purpose of any scientific theory, and identifies acceptance with belief that that purpose is fulfilled. This is the kind of realism stated (and criticized) by van Fraassen: "*Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it* *is true.*" (1980, p. 8) Van Fraassen's own opposing position, which he calls "constructive empiricism," is: "*Science aims to give us theories which are empirically adequate* [true regarding observables]; *and acceptance of a theory involves as belief only that it is empirically adequate.*" Now I claim to have shown above and in my 1979 paper that purpose-realism and constructive empiricism are both over-generalizations, and that each holds for some theories but not others. Neither of the theses stated by van Fraassen can accommodate the extensive historical evidence that scientists sometimes believe that a theory is true and sometimes only that it is empirically adequate, and that there are different sets of grounds for these two beliefs.

Consider now the question whether this case study has any further relevance for Boyd's thesis, which we might call "approximation-realism" because it appeals to the concept of approximate truth. We should note first that approximation-realism is very different from purpose-realism. It is one thing to talk about the purpose of scientific theories and what those who accept them therefore believe, and quite another to say when this aim has to some degree actually been accomplished. That this distinction is nonetheless insufficiently appreciated is clear from the fact that Boyd (1976) puts approximation-realism forward as a rival to an earlier version of van Fraassen's (1976) thesis, which refers to purpose. Another difference between the two theses is that although purpose-realism is sufficiently clear to be refuted, the obscurity of approximation-realism makes its assessment difficult and perhaps impossible. I leave aside the question of what might be meant by a "mature science," and say nothing of the vagueness of "typically." The more difficult question is whether it makes any sense to speak of a law or theory (Boyd 1976) as "approximately true." It certainly makes sense, although the statement is somewhat vague, to say that a particular *value* for a given magnitude is approximately correct —e.g., that "$\theta = 3.28$" is approximately true, because in fact $\theta = 3.29$. But speaking of a *law* or *theory* as "approximately true" raises serious problems. Usually a law or theory refers to several different magnitudes—their values and relations at various times and places. Given two theories, one may be more accurate with respect to some magnitudes, and the other theory more accurate with respect to some others. If this happens, it is quite unclear what it would mean to say that one theory is *on the whole* more accurate than another. Some weights would have to be assigned to the various quantities, and no general way to do this springs readily to mind. It might seem that this problem could be obviated at least in the special case in which, for theories T_1 and T_2 and *every* common magnitude, T_1*'s* predicted value is never further from (and is sometimes closer to) the true value than T_2*'s*. But Miller (1975) has shown that uniformly greater accuracy in this sense is impossible—at least where the two predictions never lie on different sides of the true value.

To be fair, we should note that Boyd himself concedes that (approximation-) realists still have considerable work to do in developing a concept of approximate truth suitable for stating their thesis. Boyd does not mention that some work relevant to this task has been done by Popper and various of his critics. (See Popper 1976, for references.) This body of work provides little help to Boyd, however. For Popper's critics have found fatal weaknesses in a number of his explications of "verisimilitude." Worse, Popper's original intuitive concept of verisimilitude was a confusing amalgamation of two quite distinct desiderata of theories—namely, *accuracy* and *strength*. As he put it, "we combine here the ideas of truth and content into. . . the idea of *verisimilitude*" (Popper 1968, pp. 232-233) Thus he supposed it to be evidence for greater verisimilitude that one theory has passed tests that another has failed (greater accuracy), and also that it explains more facts than the other does (greater strength). But combining these two properties into one has at least three disadvantages: (1) it makes the term "verisimilitude" (whose etymology suggests only accuracy) highly misleading; (2) it obscures the question of the relative importance of accuracy vis-à-vis strength as desiderata of theories; and (3) it makes verisimilitude irrelevant to Boyd's problem of explicating the concept of overall accuracy of a theory. If we had a theory which was perfectly accurate (over its domain), we would not say its *accuracy* could be increased by adding to it an arbitrary additional true statement.

I conclude, then, that approximation-realism is too obscure to be assessed and is likely to remain so. (This stricture does not apply to Kepler's somewhat similar view, since he says only that there is a cumulatively growing set of truths upon which the principal astronomers up to any given time have agreed—and this does not presuppose a concept of approximate truth.)

I shall end this essay by considering two final "realistic" theses. Boyd remarks, "What realists really should maintain is that *evidence for a scientific theory* is evidence that both its theoretical claims and its empirical claims are. . . approximately true with respect to certain issues."[4] (Boyd 1976, pp. 633-634). Similarly, Glymour (1976) defines "realism" as "the thesis that to have good reason to believe that a theory is empirically adequate is to have good reason to believe that the entities it postulates are real and, furthermore, that we can and do have such good reasons for some of our theories." (Glymour has in mind van Faassen's (1976) concept of empirical adequacy: roughly, that all measurement-reports satisfy the theory.) To avoid the difficulties just discussed, I shall ignore Boyd's use of "approximately" and define "empirical realism" as the thesis that *evidence for a theory's empirical adequacy is evidence for its truth*. This claim is logically independent of claims as to which theories are in fact approximately true, or as to what the purpose of science is, although it might have affinities to such claims. As we saw in section 6 above, Glymour rejects empirical realism, since he thinks two theories (e.g., Einstein's and Thirring's) might both be compatible with given evidence that nonetheless fails to test one of them—the one containing nonmeasurable, noncomputable quantities. And I also claim to have shown above and in my 1979 paper that a given body of data may be regarded as good evidence for the empirical adequacy but not for the truth of a theory—as when the theory conflicts with physics, contains indeterminate quantities, lacks proven predictive capability, etc.

Perhaps Boyd would not be much bothered by this argument and would say that the main concern of a "realistically"-minded philosopher is to assert the less specific thesis, which I will call "evidential realism," that *we sometimes have evidence that a theory is true (and not just empirically adequate)*. I claim to have shown that evidential realism is correct, and moreover to have spelled out at least some of the reasons—criteria (1)-(9) above, and acceptability of explanatory basis (in my 1979 paper)—that scientists have counted in favor of a theory's truth over very long historical periods. If someone wants to say that what scientists have considered to be reasons are not really reasons, or are not good enough, I can only reply that such a claim clashes with what I take to be one of the purposes of the philosophy of science—to state explicitly, clearly, and systematically the principles of reasoning that have been and are used in actual scientific practice.

Notes

1. Most of the last three paragraphs are taken from my (1979).
2. I am grateful to Nicholas Jardine for allowing me to see and quote his unpublished draft translation. His final, published version may be different.

3. I shall assume for the sake of argument that Putnam gives an accurate account of Boyd's thinking, or at least of some stage thereof.

4. From the context, and to avoid triviality, the phrase I have italicized has to be interpreted to mean "instances of (i.e., evidence for) a scientific theory's empirical adequacy."

References

Armitage, Angus. 1962. *Copernicus, The Founder of Modern Astronomy*. New York: A.S. Barnes.

Baumgardt, Carola. 1951. *Johannes Kepler: Life and Letters*. New York: Philosophical Library.

Bernardus de Virduno. 1961. *Tractatus super total Astrologiam*. Werl/Westf: Dietrich-Coelde-Verlag.

Blake, Ralph M. 1960. Theory of Hypothesis among Renaissance Astronomers in R. Blake (ed.), *Theories of Scientific Method*. Seattle: University of Washington Press, pp. 22-49.

Bluck, R.S. 1955. *Plato's Phaedo*. London: Routledge & Kegan Paul.

Boyd, Richard. 1976. Approximate Truth and Natural Necessity. *Journal of Philosophy*. 73:633-635.

Boyd, Richard. Forthcoming. *Realism and Scientific Epistemology*. Cambridge: Cambridge University Press.

Carnap, Rudolf. 1956. The Methodological Character of Theoretical Concepts. *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, in Herbert Feigl and Michael Scriven (eds.), *Minnesota Studies in the Philosophy of Science*, vol. I. Minneapolis: University of Minnesota Press, pp. 33-76.

Copernicus, Nicolaus. 1976. *On the Revolutions of the Heavenly Spheres*. Trans. Alistair M. Duncan. New York: Barnes & Noble. Orig. ed. 1543.

Cornford, Francis M. 1957. *Plato's Cosmology*. Indianapolis: Bobbs-Merrill.

Dreyer, J.L.E. 1953. *A History of Astronomy from Thales to Kepler*. New York: Dover.

Duhem, Pierre. 1969. *To Save the Phenomena*. Trans. Stanley L. Jaki. Chicago: University of Chicago Press. Orig. ed. 1908.

Gardner, Michael R. 1979. Realism and Instrumentalism in 19th-Century Atomism. *Philosophy of Science* 46:1-34.

Gardner, Michael R. 1982. Predicting Novel Facts. *British Journal for the Philosophy of Science*. 33:1-15.

Gingerich, Owen. 1973a. Copernicus and Tycho. *Scientific American* 229:87-101.

Gingerich, Owen. 1973b. A Fresh Look at Copernicus. In *The Great Ideas Today 1973*. Chicago: Encyclopaedia Brittanica, pp. 154-178.

Gingerich, Owen. 1973c. From Copernicus to Kepler: Heliocentrism as Model and as Reality. *Proceedings of the American Philosophical Society* 117:513-522.

Gingerich, Owen. 1973d. Kepler. In *Dictionary of Scientific Biography*, ed. C.C. Gillespie, vol. 7. New York: Scribners, pp. 289-312.

Gingerich, Owen. 1975. Kepler's Place in Astronomy. In *Vistas in Astronomy*, ed. A. & P. Beer, vol. 18. Oxford: Pergamon, pp. 261-278.

Glymour, Clark. 1976. To Save the Noumena. *Journal of Philosophy* 73:635-637.

Glymour, Clark. 1980. *Theory and Evidence*. Princeton: Princeton University Press.

Goldstein, Bernard. 1967. The Arabic Version of Ptolemy's *Planetary Hypotheses*. *Transactions of the American Philosophical Society*, n.s. vol. 57, part 4:3-12.

Hamilton, Edith and Cairns, Huntington. 1963. *The Collected Dialogues of Plato*. New York: Bollinger Foundation.

Hanson, Norwood R. 1973. *Constellations and Conjectures*. Dordrecht: Reidel.

Hutchins, Robert M. 1952. *Great Books of the Western World*, vol. 16. Chicago: Encyclopaedia Brittanica.

Jardine, Nicholas. 1979. The Forging of Modern Realism: Clavius and Kepler against the Sceptics. *Studies in History and Philosophy of Science* 10:141-173.

Johnson, Francis R. 1937. *Astronomical Thought in Renaissance England*. Baltimore: Johns Hopkins Press.
Kepler, Johannes. 1952. *Epitome of Copernican Astronomy*. Trans. C.G. Wallis. Orig. ed. 1618-1621. In Hutchins, 1952, pp. 841-1004.
Kepler, Johannes. Forthcoming. "A Defense of Tycho against Ursus." Trans. Nicholas Jardine. Written about 1601.
Koyré, Alexander. 1973. *The Astronomical Revolution*. Trans. R.E.W. Maddison. Ithaca: Cornell University Press. Orig. ed. 1961.
Kuhn, Thomas S. 1957. *The Copernican Revolution*. Cambridge, Mass: Harvard University Press.
Kuhn, Thomas S. 1970. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
Lakatos, Imre. 1970. Falsification and the Methodology of Scentific Research Programmes. In *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave, pp. 91-195. London: Cambridge University Press.
Lakatos, Imre and Zahar, Elie. 1975. Why Did Copernicus' Research Program Supersede Ptolemy's? In Westman (1975c), pp. 354-383.
Laudan, Larry. 1977. *Progress and its Problems*. Berkeley: University of California Press.
Lloyd, G.E.R. 1978. Saving the Appearances. *Classical Quarterly* 28:202-222.
McKeon, Richard. 1941. *The Basic Works of Aristotle*. New York: Random House.
Miller, David. 1975. The Accuracy of Predictions. *Synthèse* 30:159-191.
Nagel, Ernest. 1961. *The Structure of Science*. New York: Harcourt, Brace and World.
Neugebauer, Otto. 1952. *The Exact Sciences in Antiquity*. Princeton: Princeton University Press.
Newton, Isaac. 1934. *Mathematical Principles of Natural Philosophy*. Trans. Andrew Motte and Florian Cajori. Berkeley: University of California Press. Orig. ed. 1687.
Pedersen, Olaf. 1974. *A Survey of the Almagest*. Odense: Odense University Press.
Popper, Karl. 1968. *Conjectures and Refutations*. New York: Harper.
Popper, Karl. 1976. A Note on Verisimilitude. *British Journal for the Philosophy of Science* 27:147-164.
Proclus. 1903. *In Platonis Timaeum Commentaria*. Ed. E. Diehl. Leipzig.
Proclus. 1909. *Hypotyposis Astronomicarum Positionum*. Ed. C. Manitius. Leipzig.
Ptolemy, Claudius. 1952. *The Almagest*. Trans. R.C. Taliaferro. In Hutchins 1952, pp. 1-478.
Ptolemy, Claudius. 1907. *Planetary Hypotheses*, In *Claudii Ptolemae: opera quae extant omnia, volumen II, opera astronomica minora*, ed. J.L. Heiberg, pp. 69-145. Leipzig: B.G. Teubneri.
Putnam, Hilary. 1978. *Meaning and the Moral Sciences*. London: Routledge & Kegan Paul.
Rosen, Edward, trans. 1959. *Three Copernican Treatises*. New York: Dover.
Rosenkrantz, Roger. 1976. Simplicity. In *Foundations of Probability Theory, Statistical Inference, and Statistical Theories of Science*, ed. William A. Harper and C. Hooker, vol. I, pp. 167-203. Dordrecht: Reidel.
Rosenkrantz, Roger. 1977. *Inference, Method, and Decision*, Dordrecht: Reidel.
Sambursky, Samuel. 1962. *The Physical World of Late Antiquity*. London: Routledge & Kegan Paul.
Shapere, Dudley. 1969. Notes Towards a Post-Positivistic Interpretation of Science. In *The Legacy of Logical Positivism*, ed. Stephen Barker and Peter Achinstein, pp. 115-160. Baltimore: Johns Hopkins Press.
van Fraassen, Bas. 1976. To Save the Phenomena. *Journal of Philosophy* 73:623-632.
van Fraassen, Bas. 1980. *The Scientific Image*. Oxford: Clarendon.
Vlastos, Gregory. 1975. *Plato's Universe*. Seattle: University of Washington Press.
Westman, Robert. 1971. *Johannes Kepler's Adoption of the Copernican Hypothesis*. Unpublished doctoral dissertation. Ann Arbor: University of Michigan.
Westman, Robert. 1972a. Kepler's Theory of Hypothesis and the 'Realist Dilemma'. *Studies in History and Philosophy of Science* 3:233-264.

Westman, Robert. 1972b. The Comet and the Cosmos: Kepler, Mästlin and the Copernican Hypothesis. In *The Reception of Copernicus' Heliocentric Theory*, ed. J. Dobrzycki, pp. 7-30. Dordrecht: Reidel.

Westman, Robert. 1975a. The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory. *Isis* 66:165-193.

Westman, Robert. 1975b. Three Responses to the Copernican Theory: Johannes Praetorius, Tycho Brahe, and Michael Maestlin. In Westman (1975c), pp. 285-345.

Westman, Robert. 1975c. *The Copernican Achievement*. Berkeley: University of California Press.

Zahar, Elie. 1973. Why Did Einstein's Research Programme Supersede Lorentz's? *British Journal for the Philosophy of Science* 24:95-123, 223-262.