THOMAS NICKLES

FROM COPERNICUS TO PTOLEMY: INCONSISTENCY AND METHOD*

Abstract: In recent years several philosophers and other science studies experts have adopted a somewhat more Ptolemaic than Copernican view of theories, models, and scientific research, namely, the "semantic" conception of theories and their applications. On the old, "Copernican" view, theories are deductively integrated, interpreted logical calculi, in standard symbolic logic, and science is a theory-centered enterprise that aims for a comprehensive, single picture of the universe. Accordingly, consistency becomes a hard constraint, a *sine qua non* of useful, rational inquiry. Proponents of the semantic conception are somewhat more "Ptolemaic" in treating theories as collections of models and in placing the solution of local problems in restricted domains ahead of grand, spectator theories of the universe. They focus as much on the process of inquiry as upon the logical structure of its products. Remarkably, in scientific practice we find inconsistencies and near-inconsistencies of various kinds apparently popping up everywhere, suggesting that one cannot lay a tight grid of standard logic over scientific practice. However, deflating unitary theories into collections of models makes inconsistency harder to define and locate precisely—but also less serious. Consistency often becomes a soft, negotiable constraint—one constraint among others. In practice, inconsistency rarely leads to complete disaster, for there are various ways to tame it. I urge paraconsistency and inconsistency-tolerant logicians to pay attention to real scientific examples of scientists' responses to inconsistency and quasi-inconsistency, e.g., conceptual incongruities such as conceptual "blowups" and pragmatic inconsistencies such as the simultaneous use of mutually inconsistent models.

1. INTRODUCTION

Methodological treatments of (in)consistency in the empirical sciences usually take for granted a theory-centered conception of inquiry according to which theories are the primary units of and for analysis. Theories are supposed to provide correct (realist) representations of the universe or at least unified engines of prediction and explanation for a universal domain of phenomena. Since the 1960s, however, this *Copernican paradigm* (as we may call it) has been challenged by numerous studies of historical and contemporary scientific practice, especially experimental practice. Some science studies experts now favor something closer to a *Ptolemaic paradigm*, according to which the theories (if any) of a scientific field are tools in a toolbox alongside various other tools for solving the variety of problems that arise in that field. Such a view is more characteristic of those who take a pragmatic, problem-solving approach to scientific work than of those who take a universal-truth-seeking,

* *Acknowledgement.* I thank the U.S. National Science Foundation for support on projects concerning heuristic appraisal and knowledge pollution. Thanks also to Joke Meheus for helpful suggestions. She is not responsible for infelicities that remain.

J. Meheus (ed.), Inconsistency in Science, 1—33.

theory-propounding approach (Nickles 1988). As my label implies, the former group focuses on the practices, the ongoing processes, of scientific investigation rather than upon the theory structures that are the occasional products of these processes.[1]

Roughly speaking, the new accounts give more attention to local problem solving and the construction of models of experiments and of phenomena than to grand unified theories. These accounts tend to deflate theories themselves into collections of exemplary models or applications. Accordingly, the new accounts tend to blur the distinction between theory and applications. Some of these accounts reject the old view of theories as collections of general lawlike statements that bear tight deductive relations to more specific laws and to applications. For example, Giere (1999a) conceives of a science without laws, and Schurz (1995) argues that the relation between a theory and its applications is nonmonotonic in the sense that the theory is not absolutely fixed in advance. The applications can react back to alter the theory, nonadditively.

Methodologists of science now appreciate more than ever that the empirical sciences (and even the fields of mathematics, to some degree; see Lakatos 1976) are not monotonic, foundational disciplines but rather nonmonotonic enterprises in which well justified results are routinely overturned or seriously qualified by later results. And 'nonmonotonic' implies 'temporally inconsistent' if we suppose (plausibly) that the results become known successively in time and are logically detachable from their evidence bases.

Clearly, these new conceptions of scientific work, which find failure of monotonicity everywhere, call for increased logical and methodological exploration of nonmonotonic forms of inquiry—which is virtually to say inquiry full stop. Hence, the new conceptions transform the discussion of (in)consistency in science. Traditionalists may draw the conclusion that inconsistency problems are more serious than ever, because we find inconsistency and conflict of various kinds arising everywhere, and not only in exotic theories. My own inclination is to infer that they are less serious than everyone used to think, precisely because they are now anticipated products of ongoing, self-corrective investigation and neither productive of general intellectual disaster nor necessarily indicative of personal or methodological failure. In any case we are left with the task of better understanding how inconsistency and neighboring kinds of incompatibility are tamed in scientific practice and the corresponding task of better modeling idealized practice in the form of inconsistency-tolerant logics and methodologies. We here face a successor to the old problem of scientific change that became the focus of attention in the 1960s.

My purpose in this paper is to explore some of the consequences for the consistency debate of this pragmatic turn in the treatment of scientific inquiry. I shall not attempt to resolve the issues between the older and newer accounts of theories and research, between the "Copernicans" and the "Ptolemaists". In fact, I suspect that many scientists move back and forth between (or among) the various stances on these issues, depending upon their problem situation and the audience. In

[1] This is not an absolute dichotomy, of course, since nearly everyone regards theories as problem solutions of a sort. However, problem-solving accounts of research tend to convey a quite different picture than do theory-centered accounts. The difference is philosophically, and logically, important.

other words, their stance is not consistent even at the metalevel, insofar as 'consistent' implies fixed. Although this makes the work of science studies more difficult, it (again) does not seem to produce intellectual disaster. On these issues, from time to time we probably all feel strongly both ways!

2. PTOLEMY AND COPERNICUS

> So I should like your Holiness to know that I was induced to think of a method for computing the motions of the spheres by nothing else than the knowledge that the Mathematicians are inconsistent in these investigations. ... [I]n determining the motions of [the Sun and Moon] and of the other five planets, they use neither the same principles and hypotheses nor the same demonstrations of the apparent motions and revolutions. ... Nor have they been able thereby to discern and deduce the principal thing—namely the shape of the Universe and the unchangeable symmetry of its parts. With them it is as though an artist were to gather the hands, feet, head, and other members for his images from diverse models, each part excellently drawn, but not related to a single body, and since they in no way match each other, the result would be monster rather than man.

So reads a fragment of Copernicus's famous letter to Pope Paul III that prefaces his *De Revolutionibus* (quoted from Kuhn 1959, 138 f).

The inconsistencies that Copernicus complained of were of two related kinds. First, different Ptolemaic mathematicians employed distinct methods and constructions to explain the same phenomena, constructions that are mutually incompatible if realistically interpreted, e.g. an equant versus more epicycles. Second, the same mathematician could use incompatible constructions to explain different phenomena, or different aspects of the same phenomenon or object. For example, the constructions used to predict the size of the moon at a given time were incompatible with those used to predict its location or its speed. Ptolemaic astronomy made no demand that these methods of calculation should be compatible.

Now even though many phenomena could be predicted as accurately as you please (given the limits of observational accuracy) by means of some construction or other, this sort of empirical adequacy was not enough for Copernicus. For him, construction methods were not merely tools for prediction; rather, they generated representations of nature that we should be able to piece together into one consistent and complete picture of the cosmos. Copernicus took it as a given that nature itself is consistent and had been made so by a rational and aesthetically sensitive God.

> I pondered long upon this mathematical uncertainty in establishing the motions of the system of the spheres. At last I began to chafe that philosophers could by no means agree on any one certain theory of the mechanism of the Universe, wrought for us by a supremely good and orderly Creator. ...

What is wanted, Copernicus said in effect, is not a toolkit of curve-fitting devices but rather a single, unitary theory that correctly represents and explains the phenomena. He sought a unified, intuitively visualizable representation of the cosmos that captures what is really there and is therefore aesthetically pleasing, given that it *is* a rationally ordered cosmos.

Copernicus's aesthetic motivation is well known. He desired to recapture the Greek ideal of the universe as a perfectly spherical cosmos consisting primarily of a harmonious compounding of uniform, perfectly circular motions. It is often remarked that Copernicus was so revolutionary because he was so conservative, even reactionary. Although his work initiated the so-called scientific revolution, Copernicus himself looked backward more than forward. His was still a Greek conception of the universe, including a sharp, Greek distinction between theory and practice. Accordingly, the sort of mathematical tinkering engaged in by the Ptolemaic mathematicians was inappropriate. Indeed, they were not doing genuine science at all. In astronomy a mathematical construction should be representational, not fictional or merely instrumental.[2]

We may pause here to note that Copernicus's serious charges of inconsistency were not self-evidently correct in the context of his time—or (as we shall see) in our time either. Ptolemaic practices had long been reasonably successful, and no one until Copernicus considered them riddled with logical fallacy. Copernicus's rhetoric of inconsistency was just that—a rhetorical attempt to shift the aims and ideals of the astronomical enterprise. Such a shift is not simply dictated by logic and by the astronomical data. In that sense, insofar as Copernicus succeeded in making his consistency charges stick, we can regard those consistency problems as social constructions resulting from deliberate choices.

Copernicus had an additional objection to Ptolemaic astronomy. Not only was it overdetermined to the point of being mutually inconsistent but also underdetermined in the sense of being incomplete, insufficiently integrated.

> [In my system, by contrast] the orders and magnitudes of all stars and spheres become so bound together that nothing in any part thereof could be moved from its place without producing confusion in all the other parts and in the Universe as a whole. [Kuhn 1959, 142]

As Kuhn notes, Copernicus here put his finger on the most striking difference between his and the Ptolemaic theories. The latter permitted one to expand and shrink the orbit of a given body at will, leaving the others fixed. Copernicus and his followers believed that a realist interpretation would help to solve both problems (inconsistency and incompleteness) by imposing available but hitherto ignored constraints on astronomical problem solving.

From this point in Copernicus's preface, we may draw a direct line to contemporary physicist Stephen Weinberg, writing in *Dreams of a Final Theory* (1994):

> Our present theories are of only limited validity, still tentative and incomplete. But behind them now and then we catch glimpses of a final theory, one that would be of unlimited validity and entirely satisfying in its completeness and consistency. ... [p. 6]
>
> [In addition to multiple symmetries, a final theory would manifest] the beauty of simplicity and inevitability—the beauty of perfect structure, the beauty of everything

[2] Lloyd (1978) rejects the widespread view that Ptolemy himself and the commentators cited by Duhem (1969) were instrumentalists rather than realists. Laudan (1983, §2) notes that many astronomers had abandoned Aristotelian demonstrative science with its aim of deducing causal explanations of celestial motion from self-evident first principles. (1) They gave up attempting to provide causal explanations in terms of essences, and (2) they adopted a hypothetico-deductive method of testing their models. Thus Copernicus and Kepler were conservative revolutionaries in trying to return astronomy to the status of a genuine science from that of a calculative craft.

> fitting together, of nothing being changeable, of logical rigidity. It is a beauty that is spare and classic, the sort that we find in the Greek tragedies. [p. 149]

Kuhn had already sketched a similar picture of the Copernican revolution:

> There are many variations of the Ptolemaic system besides the one that Ptolemy himself embodied in the *Almagest*, and some of them achieved considerable accuracy in predicting planetary positions. But the accuracy was invariably achieved at the price of complexity—the addition of new minor epicycles or equivalent devices—and increased complexity gave only a better approximation to planetary motion, not finality. No version of the system ever quite withstood the test of additional refined observations, and this failure, combined with the total disappearance of the conceptual economy that had made cruder versions of the two-sphere universe so convincing ultimately led to the Copernican Revolution. [Kuhn 1959, 74]

Other authors, and Kuhn himself in other passages, have been less confident of the superior accuracy of Copernican theory. Thanks initially to Kuhn's book, we now appreciate that, at that time, the Copernican theory did not obviously possess all the advantages often claimed for it (simplicity, predictive accuracy, elimination of epicycles, etc.).[3] For example, Copernicus still retained epicycles, although he eliminated Ptolemy's five major epicycles as well as the equant (Kuhn 1959, chap. 2). Nor was Ptolemaic astronomy in crisis, although, in the later *Structure of Scientific Revolutions* (1962), Kuhn insisted that every revolution must be preceded by a crisis. Moreover, in the "Postscript" to the second edition (1970) of *Structure*, Kuhn himself devalued overarching theories based on principles such as $F = ma$, in favor of concrete problem solutions or "exemplars". Accordingly, it is no longer clear whether we should count Copernicus's achievement as an exemplar (a paradigm in Kuhn's "small" sense) or as the creation of a disciplinary matrix (a paradigm in the "large" sense). While it conveyed a revolutionary new worldview, only much later was it parlayed into a new disciplinary matrix. Be that as it may, the Copernican Revolution remained paradigmatic of a scientific revolution in Kuhn's own development.[4]

The word 'epicycle' has become pejorative, signifying a degenerating program that saves itself through otherwise arbitrary, ad hoc patches. Yet, even here, Copernicus's and Kuhn's complaints about the Ptolemaic tradition can be challenged. For the Ptolemaic practice of introducing new epicycles (or any other constructions that might be convenient) in order to provide a tighter model of a phenomenon is not completely out of keeping with current scientific practice (Nickles 1987).

Of course, the Copernican debate is not my present concern. I do not here question the usual story of scientific progress from Copernicus to Newton's theory with its integration of terrestrial and celestial mechanics. But I do want to raise the question whether the Copernican paradigm, including Copernicus's demand for total consistency, should be taken as a model for all of science.

[3] See Westman 1975 for an entry into this debate.

[4] Kuhn stood with one foot in the aesthetic, unitary-representational-theory tradition and the other in the practice tradition, depending partly on whether we focus on large or small paradigms. See Rouse 1987, chap. 2, and Nickles 1998.

Springer

http://www.springer.com/978-1-4020-0630-2

Inconsistency in Science
Meheus, J. (Ed.)
2002, IX, 223 p., Hardcover
ISBN: 978-1-4020-0630-2