Ravetz, Jerome R.

Traditional and innovatory elements in the cosmology of Nicolaus Copernicus

Organon 2, 49-59

1965

Jerome R. Ravetz (Great Britain)

TRADITIONAL AND INNOVATORY ELEMENTS IN THE COSMOLOGY OF NICOLAUS COPERNICUS

I am honoured to participate in this Symposium, joining scholars to whose work my own speculations are little more than a footnote. Since I am only a visitor to the history of astronomy, my best course to-day is to review the subject of the Symposium from the point of view of someone engaged in thoughts on the general problem of continuity and change in scientific knowledge.

REFLECTIONS ON "TRADITION"

I think that we should expect to find that nearly every separate element in the cosmology of Copernicus is "traditional". By this I mean that Copernicus, like any other scientist, had to start his work from a base of facts, problems, and regulative principles of method and value inherited from his predecessors. We know that the stock of important scientific knowledge in any field frequently undergoes what seems to be a revolutionary change, and the associated technical problems change almost as radically. Yet the deeper problems, and the principles of what a particular science is about, evolve more slowly, in complex patterns not rigidly tied to those particular problems which happen to be successfully exploited at any given moment. Moreover, when historians have been able to dissect in detail the background to any particular great discovery, they have always discovered the precepts and suggestions of a teacher, the problems that were "in the air" and the anomalous facts that others struggled with, but only the one gifted investigator explained.

Indeed, this continuity of scientific knowledge is so strong that it is possible, at least in the first stage of scholarly investigation, to build up a picture of evolution by insensible degrees. One can plausibly reduce any given revolution in science to two stages: the collection of well-tried ideas; and the creation of a school of propagandists. As a corrective to fables of creation *ex nihilo,* such studies have at least created the basis for historical investigation. But revolutions in science cannot be wished away, especially since in science, even more than in politics, it is misleading to say: *plus ça change, plus la même chose.*

We may start to resolve the dilemma of "innovation and tradition", or "revolution and evolution" if we examine more closely what are the elements of a tradition in science as it affects the production of novelty. For a tradition in science is necessarily complex, even more so than the activity of setting and solving new problems. In the case of Copernicus, one can distinguish traditions in several sets of astronomical problems. A partial list includes the following: mathematical techniques; the computing of ephemerides; the construction of tables as the basis for ephemerides; the development of mathematical models for the calculation of such tables; the consideration of the long-term motions affecting the accuracy of the tables; specifically calendrical problems; problems of the structure of the heavens; problems of the causal agencies of their motions; problems of the terrestrial effects of the heavenly bodies; theological consequences of these last problems; and also discussion of the nature of astronomical science itself.

This is a sizeable list of problems even as it stands; and we cannot know which of them were dominant in any period without examining the literature of the time. To pick on a special one as the key problem on the basis of debates which took place some generations later, is historically naive.

For a comprehensive study of the tradition that Copernicus inherited, one would also have to consider the technical efficiency with which these problems were studied (in most cases it was low), and also the different schools in which they were studied, each with its own characteristic style. But I do not wish to elaborate the impossibly large list of categories which are relevant to this historical problem; for this problem like any other in history, is not capable of being studied exhaustively and conclusively.

The point of my illustrating the complexity of "tradition" is to show that one cannot speak of it as an undifferentiated entity which a scientist must simply "accept" or "reject". Of course, one aspect of the tradition may be so basic, and so deeply embedded in the thought of the time, that its rejection constitutes a great revolutionary step (if it is successful), even though this rejection was brought about by a reliance on other aspects of the same tradition. William Harvey appreciated this subtle interpenetration of acceptance and rejection; one can paraphrase his view as the principle that only an investigator with a trained sense is capable of seeing the facts that are there, and this training is of course done within the framework of traditional theories. But, continues Harvey, once the senses are trained and fully competent, the facts they perceive are absolute, and no traditional theory can be considered a more reliable basis for knowledge than the evidence of trained sense.

The tradition is not merely divided up into classes of separate problems which are studied with varying intensity at different times and places. Within each class of problems there may be found rival and antagonistic lines of solution, and this conflict within the tradition may sometimes be a most fruitful source of fresh thinking. When studying such conflicts, especially in a period when natural philosophy did not subsist of itself, we must expect them to be reflections of deeper conflicts in philosophy and theology, and to exhibit the sudden appearances and vanishings, rapid changes of front, and complex affiliations of ideas which are characteristic of such epiphenomena.

Thus the two poles "acceptance" and "rejection" of tradition represent extremes, whose adoption leads to inaccurate copying on the one hand and fruitless speculation on the other. To understand a tradition involves interpreting and adapting it, and even to overthrow a tradition, or a dominant part of it, involves first being schooled in its various techniques. The master of science, or even of a tradition-preserving field of scholarship, is he who can assess the strength and value of the manifold and contradictory components of the tradition he has inherited, to know what to accept, what to modify, what to reject and what to ignore.

It is usually astonishingly easy to distinguish the man who achieves a revolution in science from his "precursors". The innovating ideas which seem so similar to the real thing when plucked out of their context, are quickly seen to be tentative, incidental, or incoherent, in the writings of those who might have done the great work but in fact did not. When the genuine advance is made, it rings strong and clear, in spite of its unresolved problems, and even with its bits of scaffolding remaining from the tradition it has transformed. It is in the setting and solution of the most deep and difficult problems, lying at the foundations of the existing tradition, which brings into being the most revolutionary advances in disciplined knowledge. Such work calls for a master of his craft; and such, I believe, was Nicolaus Copernicus.

Our task in examining tradition and innovation is not a labelling of "progressive" and "reactionary" elements in the thought of Copernicus. Rather, I believe, we should try to see how he interpreted the tradition in setting and investigating his problems; and where he found obstacles, and where encouragement, for the cosmological conclusions to which he was driven by his astronomical work.

A WELL-KNOWN BUT NON-EXISTENT TRADITION

Let me now touch briefly on some aspects of the tradition in astronomy and cosmology as they are relevant to the work of Copernicus. Perhaps I should first draw attention to one set of problems where an active tradition did *not* exist: this is in the elaboration and refinement of the kinematic models for short-term planetary motions deriving from the *Almagest*. Many recent interpreters and popularizers of Copernicus have assumed that epicycles were being added to the original models, rather like higher-order frequencies in an harmonic analysis, in bewildering variety, and that Copernicus's aesthetic sense was outraged by this. Indeed, one of my colleagues in England has coined the term "epicyclic" to describe an over-ripe system in need of drastic simplification through being stood on its head. It is a nice term, and I wish it were accurate. But unfortunately it is not. To be sure, there are texts in the writings of Copernicus which can be interpreted as describing such a situation. But each of these texts can equally plausibly be given another interpretation, and one which has better support from the history of astronomy of the period.

If anyone wants evidence for my denial of the multiplication of epicycles, let him merely consult Professor Zinner's thorough bibliography of astronomical literature in the German culture-area. I would go further, and say not merely that epicycle-research was absent, but that it could not exist at the turn of the sixteenth century. The science of astronomy was simply not strong enough to support it, either organizationally or technically.

Let me give you one little example, which may be somewhat unpopular, as it reveals Copernicus as a master-astronomer as well as cosmologist. Accurate computation of the position of a planet on an epicycle model involves the determination of the direction of its line of apsides. This can be obtained from observations of the planet's position at three oppositions. The techniques are worked out in the *Almagest*, and are described by Pannekoek. From the time of Ptolemy until 1523 it was universally accepted that the apsides of the planets moved in unison with the fixed stars; or, put in more physical language, the eccentric spherical shells bearing the planetary spheres, shared the motions of the stellar sphere. In the course of preparing the *De Revolutionibus* Copernicus took observations of the planets and was the first astronomer in recorded history to discover that this assumption is false, starting with Mars in the year mentioned above.

We must ask why this discovery, depending as it does on essentially straightforward observations and calculations, was left to Copernicus. We cannot simply say that his predecessors adhered so slavishly to the words of Ptolemy that they never dared to test this assumption; for from early Islamic times it was believed that Ptolemy's views on a related problem, the length of the year, were overly simple and in need of modification. No, we must say that before Copernicus, and certainly in fifteenth-century Europe, the most pressing problems faced by the tiny handful of competent astronomers lay in other directions. When Copernicus was revered by his successors as "The Restorer of Astronomy", they knew what they meant; only after his work was there a basis for a hope of an effective science of astronomy. Concerning the historical myth of multiplying epicycles, Professor Rosen may be able to enlighten us. From a cursory glance at the astronomical work of Tacquet in the seventeenth century, I would hazard a guess that the criticism of "complexity" of anti-Copernican astronomy (confused with anti-Keplerian astronomy) may date from then.

That lengthy excursion into a non-existent tradition may serve to show how careful we must be, to avoid being blinded by hindsight.

IMPORTANT TRADITIONS

TEACHING

Now I should like to sketch a few aspects of the tradition in astronomical thought which I think will help to enhance our understanding of Copernicus's great innovation. I shall limit myself to five subjects: the teaching tradition; earlier cosmological speculation; problems of reference-frames for observational astronomy; problems of the structure of the heavens; and finally the question of the nature of astronomical science.

It is not out of courtesy to our hosts that I say that the Jagiellonian University of Cracow deserves a good share of the credit for the immortal achievement of Copernicus. The extent and quality of astronomical teaching there, unequalled by any other University in Europe at the time, may have been crucial in setting Copernicus on his path. At a young and impressionable age, he was given a thorough grounding in the techniques of astronomy and introduced to its basic problems. He did not have to spend the years when the intellect is quickest and most daring, on a search for manuscripts or in laborious self-education. When one learns how pitifully few were the competent astronomers of the period, how scarce and haphazard were the teaching materials, one appreciates the importance of having the techniques presented to one by a group of masters.

Some of you may know that I have considered whether the *Commentariolus* itself may date from Cracow. The fact that it shows no detailed acquaintance with the *Epitome in Almagestum* of Regiomontanus is for me a telling point. This is reinforced by its style, starting with the traditional debate between the world-systems of Aristotle and Ptolemy, and announcing, "I considered", with never a reference to the classical pedigree which would make the startling cosmology so much more acceptable to a Humanist audience. If this conjectured early dating for the *Commentariolus* is correct, then the role of the astronomical school at Cracow becomes of vital importance. In his later years Copernicus knew only too well that he could not offer a conclusive proof of his system; and if he had not discovered it so early in life as to be fully identified with its truth, we may wonder whether he would have devoted his talents to the long and wearying task of consolidation.

COSMOLOGICAL THOUGHT

As Duhem has shown, speculations on a partially sun-centred geostatic system were extant right through the medieval period. Some of these could quite easily have been transmitted to the young Copernicus by his teachers. The cosmological argument in Book I, Chapter 10 of the *De Revolutionibus* may reflect an actual chain of speculation, and it starts with the inclusion of Mercury and Venus in the "sphere" of the sun.

However, such a line of reasoning would not of itself produce a Copernican system. Extending sun-centred orbits to the other planets in a geostatic system only yields the monster of Tycho's compromise. Only if the earth is already in rotation and the heavens freed of their diurnal motion, can it be neatly transferred to the space between Venus and Mars.

We all know of the fourteenth-century discussions of the possible rotation of the earth. Perhaps knowledge of these survived through the next century, so putting the idea "into the air", and making it easier for Copernicus to conceive it as a way out of his astronomical dilemmas. If so, then in spite of himself Oresme may have played a part in the preparation of the revolution in cosmology. It is of course ridiculous to put the arguments of Oresme in the same class as those of Copernicus; Copernicus had to believe in the rotation of the earth as a necessary physical fact before proceeding to the full development of his system. However, as we know from the later history of the Copernican revolution, the ideas discussed and conclusions reached by any man play a role in the thought of his successors which bears as little relation to his own, as do their problems to his.

REFERENCE-FRAMES

Next I mention briefly two topics which, as you may already know, are dear to my heart: reference-frames and structure. At the time of writing this, I did not yet have the benefit of the criticism of my colleagues here, concerning my speculations. Hence it is impossible for me to discuss it in my prepared text. Whether or not my thesis will survive the examination of those more knowledgeable than myself, I can claim to have brought certain hitherto obscure matters into some sort of light.

The problem of reference-frames reminds us that astronomical observation was then, as it is now, a highly sophicticated procedure. It is clear that the *Almagest* of Ptolemy was far from a collection of unrelated computing routines, but was rather a fully articulated *Système du Monde,* each set of observations and theories laying an explicit and firm foundation for what followed. The *De Revolutionibus* is analogously structured, but with a significant non-parallelism to the *Almagest.*

It is highly unlikely that either Copernicus or Ptolemy derived the parameters of their models from observations in precisely the fashion set out in their masterpieces; and it is equally unlikely that many astronomers in the intervening centuries had such full command of the techniques as to make a competent appraisal of the problem of the structure of observational astronomy. However, calendrical problems, if nothing else, forced a primary attention to the motions of the sun and moon, and the "motions of the eighth sphere" were discussed whenever the level of astronomical competence rose to the point of making them appear comprehensible.

There was certainly a tradition of attempting to "save" the complex motions of the eight sphere by a very slow reciprocating motion of the earth. Duhem describes the efforts of Albert of Saxony in this direction, and the obscure fragments of the views of Regiomontanus on the motions of the earth can be explained as being concerned with such a problem. If this tradition was alive in Cracow, then it may well have been most fruitful. For once Copernicus was convinced that the precession of the equinoxes, rather than the motion of the eighth sphere, is the necessary explanation of the changes in stellar longitudes, a consideration of such slow terrestrial motions would lead him inexorably to the rotation of the earth. It is impossible to discuss this in any detail here; let it suffice that for these phenomena, the mathematical representations of the different cosmologics are not observationally equivalent. I might just add that the tradition of concern with these problems stretches as far back as Hipparchus; although Ptolemy refers to him as supporting motions of the eighth sphere, the books he quotes are clearly committed to a precession of the equinoxes.

As some of you already know, I have relied on this particular problem, and its tradition, very strongly in my conjectured reconstruction of Copernicus's path of discovery. It is because here, and only here, that we find a problem whose investigation would lead by natural stages to a conviction of the earth's rotation. And the regulative principle involved in making the conclusion inescapable is not a trivial counting of circles, but rather the fundamental one of the possibility of a science of astronomy. On this too there was a tradition, which I shall discuss at the end of my paper.

In the technical form it took, this problem of "the motions of the eighth sphere", involving motions of "trepidation" to save the irregular variations, was of course a pseudo-problem. Tycho satisfied himself that the motions were uniform, and as it turned out, he was correct. Since than that problem-tradition has had a very bad press from historians. But concerns were real ones, and I would claim that Copernicus's work on this was as fully "scientific" as, say, the precise measurement of atomic weights before the discovery of isotopes.

STRUCTURE

Similarly, the structure and disposition of the "spheres" can be dismissed as entirely misconceived. But out of investigations consciously in this tradition came Kepler's third law. And every time that Bode's law flickers back into scientific respectability we are reminded of the genuineness of the basic concern.

The argument between the homocentric spheres of the Aristotelian tradition, and the eccentric spherical shells deriving from Ptolemy's *Hypotheses of the Planets*, is well known to historians who do not rely exclusively on Dreyer. It is given pride of place by Copernicus in the *Commentariolus,* and may perhaps have been the sort of contradiction within a tradition, too deep to be smothered over by teachers, which would arouse the curiosity and ambition of a brilliant youth. Also, the comments by Copernicus on the unsatisfactory state of planetary theory (the "monster" of the dedication of the *De Revolutionibus*) seem to refer more naturally to the incoherencies of its structure than to mathematical elaborations of the kinematical models, which did not yet exist anyway.

Finally, the discussion of the problems of structure found in Chapter 10 of Book I of the *De Revolutionibus* may reflect (in simplified form of course) Copernicus's progress from a geocentric to a heliocentric world-system. For there we have a natural progression starting with a statement of the insolubility of the problems within the framework of the old hypothesis, and proceeding to the well known modification of the orbits of Mercury and Venus. Then there is an extension to the higher planets, thus creating a system where the "great circle" of the sun's orbit is the privileged centre of all planetary motions. There is no observational difference between such a system and the traditional one, and the kinematic models are nearly identical, except that in such a system there is no place for an equant.

I believe that there is evidence for the thesis that Copernicus went so far in his study of this thoroughly traditional problem, and then stopped to consolidate by working out parameters for the kinematic models. What should then have driven him to completion, placing the earth-moon system in motion in the space between Venus and Mars? A problem which may have prepared his mind for this, and which requires no flashes of a complex insight, could have been the traditional one of the harmonious relations of distances and velocities of the planet, ascribed to Euclid and made precise by Kepler. Thus at every stage, Copernicus may well have progressed by using certain traditional ideas and regulative principles, on a traditional problem whose new context (a rotating earth) made possible a drive through to the greatest of all astronomical innovations.

SOZEIN TA PHAINOMENA?

Whether or not my own reconstruction has any historical validity, there is no doubt that Copernicus was concerned to do more than merely "save the phenomena". This commitment to the possibility of a rational science of the heavens is by no means trivial; and without such a commitment it is doubtful whether Copernicus would have dared to make the sun stand still. One would think it unlikely that a young man would put his energies into finding the master-key that would unlock the riddles of the cosmos, if each and every source of information and guidance available to him denied that such a key existed, reducing mathematical astronomy to approximations and physical astronomy to probabilities. In the tradition which has been the most extensively described, this was indeed the view; Professor Edward Grant's studies on the fourteenth-century thinkers establish this quite firmly. Of course a "realist" position existed; if no one else, Ptolemy adhered to it, as a study of any of his works besides the kinematic models will reveal. But through what channels this tradition opened into the thinking of Copernicus, is something on which I appeal to the scholars around me for enlightenment.

I would not be surprised if these conflicts of "probabilism" and "realism" in mathematical and physical astronomy are very difficult to fit into coherent traditions. They may well cut across identifiable positions of "Platonism" and "Aristotelianism". Stands taken on these issues, which were after all not central to the ideological conflicts of the time, may have been influenced by philosophical and theological concerns. Because of this, individuals might in a few years reverse their position on the problem; it would seem to me that the Cusa who denied the possibility of a true calendar reform in 1436, and the Cusa who wrote the *De Staticis Experimentibus* in 1450 were the same man, but one who was living in radically different philosophical worlds at the two times.

I suspect also that if we look for a continuous tradition of "realism" in astronomical matters, we may find it an uncongenial place: among the astrologers. Here Ptolemy, with his *Tetrabiblios,* occupies an honoured place. As Professor Grant has shown, Oresme's brilliant work on incommensurable quantities was used by himself and his successors against the astrologers and calendar-reformers — were the two classes distinct? Might we not conjecture whether this was an important part of the motive for engaging in this work?

A study of astrology, or rather of the defences of astrology, bristles with methodological difficulties on top of the normal historical ones. It will no longer do, to be shocked at the participation of otherwise respectable astronomers in this disreputable activity; but I am not sure that we can simply say that in astrology everything was all right except for the *Weltanschauung* (although, as it happens, they were right about the cause of the tides). The whole problem touches rather too closely on our deepest ideas concerning the nature of science; this is both uncomfortable, and irrelevant to our discussions today. I have thought a little about pseudo-sciences of the past and present, and I am still troubled by the picture of a Medieval physician given by W. C. Curry: using astrology for his diagnosis and special amulets, and also prescribing various drugs. I am not happy in calling the former pseudo-science and the latter proto-science, since in the event it was the astrological part of the treatment which conveyed more psychological benefit, and less physical damage, than the pharmaceutical part.

But with that I must leave astrology, for the documents we have from Copernicus give no evidence that he took it seriously either in his astronomy or in his medicine. And if this evidence is conclusive, it raises the question: "Why not?". This is not an unimportant question, for non-astrological astronomers (to use a modern terminology) were distinctly in the minority between Ptolemy and Copernicus.

THE INNOVATIONS OF COPERNICUS

Perhaps in this feature of the thought of Copernicus, we may find one of the deepest of his innovations. To believe that God's world is fundamentally rational and harmonius was to participate in a long and distinguished tradition; but to accept that these harmonies are n o t put there for us humans, for our guidance and in the image of our human thoughts, was to adopt a *Weltanschauung* which, when made explicit, was the heart of the ideology of the Scientific Revolution a century later.

With this last point I have finally wandered back to something close to the idea of "cosmology" on which I was supposed to say something in this Symposium. I hope I have shown how much was "traditional", or at least available for taking from tradition, in the problems Copernicus worked on, and in the materials, in the form of theories and principles, that he used. What was "innovatory" in his work was the technical mastery and cosmological daring that he brought to bear on these problems and materials. So let it be with Copernicus as with any other great genius of science: rooted in the past, and out of it creating the future.