Our Common Future.
1987 World Commission on Environment and Development. Oxford: Oxford University Press.

Raa, J.
1987 "Faglig utarming av universitetene." Forskningspolitikk, 10, 1:15—17.

Radnitzky, G.
1983 "Science, Technology, and Political Responsibility." Minerva, 21, 2—3: 234—264.

Rahman, A.
1972 Anatomy of Science. Delhi: National.

Restivo, S.
1988 "Modern Science as a Social Problem." Social Problems, 35, 3 (June): 206—225.

Rosenthal, M. M. et al. (eds.)
1974 Geschichte der marxistischen Dialektik: von der Entstehung des Marxismus bis zur leninschen Etappe. Berlin: VEB Deutscher Verlag der Wissenschaften.

Seppälä, E-O.
1987 "Suomen korkeakoulujen tutkimusrahoituksen kehitys 1970-luvulta lähtien." (Development of research funding in Finnish universities since 1970s). Tiedepolitiikka, 3: 29—31.

Shapere, D.
1986 "External and internal factors in the development of science." Science and Technology Studies, 4, 1: 1—9.

Shiva, V. and Bandyopadhuay, J.
1980 "The Large and Fragile Community of Scientists in India. Minerva, 18, 4: 575—594.

Stolte-Heiskanen, V.
1987 "Comparative Perspectives on Research Dynamics and Scientific Performance: a View from the Periphery." R&D Management, 17, 4: 253—262.

Suomen Akatemian toimintalinjat.
(The guidelines of the Academy of Finland)
1988 Helsinki: Valtion painatuskeskus.

Titmonas, A.
1974 "K voprosy o predposylkah institutsionalizatsii nauki" (On the preconditions of institutionalization of science). Pp. 158—173 in V. Kelle and S. R. Mikulinski (eds.), Sotsiologitsheskije Problemy Nauki (Sociological Problems of Science). Moskva: Nauka.

Trent, J.
1978 "Internationalization? Interdisciplinarity? Development?" Social Science Information, 17, 2: 337—342.

Tsurumi, K.
1977 "Some Potential Contributions of Latecomers to Technological and Scientific Revolution: A Comparison between Japan and China." Pp. 147—173 in Rolf Dahrendorf et al., Scientific-technological Revolution: Social Aspects. London: Sage (Studies in InternationalSociology 8).

Volkov, H.
1976 Istoki i gorizonty progressa (Origins and horizons of progress). Moskva: Izdatelstvo polititsheskoi literatury.

Vsesojuznaja konferentsija "Metodologitsheskije aspekty vzaimodeistvija obsheshestvennyh, jestestvennyh i tehnitsheskih nauk v svete reshenij XXV Sjezda KPSS" (Federal conference on "Methodological aspects of the interaction of social, natural and technical sciences in the light of XXV Party congress decisions").
1978 Moskva-Obninsk: Sovetskoje natsionalnoje objedinenie istorii i ilosofii jestestvoznanija i tehniki (i dr.).

Whitley, R.
1974 "Cognitive and social institutionalization of scientific specialties and research areas." Pp. 69—95 in Richard Whitley (ed.), Social Processes of Scientific Development. London and Boston: Routledge & Kegan Paul.
1984 The Intellectual and Social Organization of the Science. Oxford: Clarendon Press.

Yakhiel, N.
1985 "Nauka i burzhuaznaia sotsiologitsheskaja mysl: ot normativnoi k interpretivnoi sotsiologii nauki" (Science and bourgeois sociological thought: from normative to interpretative sociology ofscience). Voprosy Filosofii, 39, 5: 64—73.

Yearley, S.
1989 "Colonial science and dependent development: the case of the Irish experience." The Sociological Review, 37, 2: 308—331.

Ziman, J.
1984 "The Collectivization of Science." EASST Newsletter, 3, 2: 5—11.

Erkki Kaukonen and Pirkko-Liisa Rauhala
University of Tampere
Department of sociology and social psychology
P.O. Box 607
33101 Tampere, Finland

Raimo Lehti

The usefulness of Monsters when running a Scientific Revolution

The first case study of Thomas Kuhn concerning scientific revolutions treated the 'big bang' brought about by Copernicus. The paradigmatic role this revolution has played in Kuhn's thought is manifested in Kuhn (1968: 66—67) when he turns to study "Crisis and the Emergence of Scientific Theories". He writes: "If awareness of anomaly plays a role in the emergence of new sorts of phenomena, it should surprise no one that a similar but more profound awareness is prerequisite to all acceptable changes of theory. On this point historical evidence is, I think, entirely unequivocal. The state of Ptolemaic astronomy was a scandal before Copernicus' announcement". The emergence of Copernican astronomy is hailed as "a particularly famous case of paradigm change", and is characterized as follows: "By the early sixteenth century an increasing number of Europe's best astronomers were recognizing that the astronomical paradigm was failing in application to its own traditional problems. That recognition was prerequisite to Copernicus' rejection of the Ptolemaic paradigm and his search for a new one. His famous preface still provides one of the classic descriptions of a crisis state" (Kuhn 1968: 68—69). Also later Kuhn (1977: 277) makes use of the 'scandalous' state of astronomy in the early sixteenth century to back up his philosophical position.

We limit our discussion to the question of how far the Copernican Revolution has been realistically described in (Kuhn 1957). Whether or not (p. 134) "that Revolution is in many respects typical" for "any other major conceptual upheaval in the sciences" is a question we leave open.

An old view of the Copernican Revolution — still to be found in antiquated popularizations — is that Copernicus must have exposed the errors of Earth-centered astronomy with new and better observations; how else could he have arrived at his revolutionary innovation? This view is incorrect. Copernicus' own observations did not give any better reasons to decide for heliocentrism than the observations made by Ptolemy about 1400 years earlier. Hence, supposing there existed a breach dividing Copernicus from Ptolemy, the new from the old, where are the origins of this breach to be found?

The origin of the breach, according to Kuhn, was the crisis of Ptolemaic astronomy, growing to a scandal in the sixteenth century. Epicycles had been heaped upon epicycles until the system had degenerated to an unbearably complicated and ugly ramshackle which no longer fulfilled its task: it did not satisfactorily represent the observed motion of the planets, and could not meet such practical requirements as preparing a correct calendar. Referring to the preface of De Revolutionibus, Kuhn repeatedly assures that the Ptolemaic astronomy had grown to a diffuse and inaccurate monster, and that the recognition of the monster, and discontent with it, were the first steps toward the Copernican Revolution (Kuhn 1957: 141). That Copernicus was the one to recognize the monster was, according to Kuhn, due to the fact that he was 'converted' to neoplatonism, and therefore saw the problem of the planets in a different light than his predecessors had done.

The development of Ptolemaic astronomy as background to Copernicus' innovation

The central point of Kuhn's interpretation of the Copernican revolution is the supposed technical failure of Ptolemaic astronomy. "After thirteen centuries of fruitless research a perceptive astronomer might well wonder, as Ptolemy could not have, whether further attempts within the same tradition could conceivably be successful" (Kuhn 1957: 140; see also Krige 1980: 149—152). The unsuccessful attempts of the astronomers are described as follows (Kuhn 1968: 68): "Given a particular discrepancy, astronomers were invariably able to eliminate it by making some particular adjustment in Ptolemy's system of compounded circles. But as time went on, a man looking at the net result of the normal research effort of many astronomers could observe that astronomy's complexity was increasing far more rapidly than its accuracy and that a discrepancy corrected in one place was likely to show up in another."

We return later to the question of whether Copernicus himself saw something monstrous in his astronomical heritage. Copernicus' subjective attitude, however, is not the moot question in Kuhn's interpretation. The failure of Ptolemaic astronomy must have been something recognized by the astronomical community. Hence we begin with the question of how far the state of Ptolemaic astronomy really was a scandal before Copernicus (cf. Kuhn 1968: 68), and especially, what documentation Kuhn gives for this assertion.

Leaving for the time being aside the preface of De Revolutionibus and the calendar problem, we concentrate upon the question of the general increase of complications within the Ptolemaic system. As documentation, Kuhn (1968: 68) refers only to chapters 11 and 12 of the well-known book by Dreyer (1953), hence it is of interest to take a look at these chapters. Doing so, we find to our surprise that the evidence obtainable for the addition of complicating epicycles is meagre indeed. In chapter 11, devoted to Indian and Arabic astronomers, we learn that some astronomers (Al Battani, Al Zarkali) had treated the motion of solar apsides (Dreyer 1953: 250—251), but this long-period motion, present also in Copernicus' models, can hardly be considered a shocking complication. The number of epicycles was increased practically only by Nasir al-Din al-Tusi and his pupils (the Maragha school) when they eliminated the Ptolemaic equant and replaced it with uniformly rotating circles. It would be comical to characterize that change of the Ptolemaic models as a monstrous complication, as Copernicus himself accepted it and defended it with gusto[1]. Moreover, the works of the Maragha school were not generally known in Europe before Copernicus.

One aspect where the Arabs diverged from the presentation given in Ptolemy's Almagest (but not from that given in his Planetary Hypotheses) was the representation of planetary motions using solid spherical shells (Dreyer 1953: 257—262, Swerdlow—Neugebauer 1984: 33—45). This cosmological assumption meant no change of the system as a device for computing planetary positions. The change attempted by al-Bitruji was more radical, because it replaced the Ptolemaic epicycles with a totally antiquated Aristotelian-Eudoxian system of concentric spheres (Dreyer 1953: 263—268). This cosmological undertaking had no effects on serious astronomical work, and the question of whether or not it created a complicated and inaccurate monster, is irrelevant to the history of Ptolemaic astronomy.

The only point where the Arabs really complicated the models of the Almagest, was something detached from the planetary models proper, namely the motion of the sphere of the fixed stars. The erroneous equinox observations (if they were observations) of Ptolemy together with earlier and later more correct observations had lead to an illusory view of changes in the precession rate and the length of the solar year. These complications — which the contemporaries of Copernicus knew from the works of Thabit ben Qurra — have nothing to do with structural deficiences of Ptolemaic planetary models. Copernicus himself did by no means find the changing precession rate a scandal, but included it, translated into a motion of the Earth, in the third book of De Revolutionibus[2].

Dreyer (1953: 272) concludes: "Arabian astronomers who really wished to follow in detail the celestial motions were therefore obliged to adopt the Ptolemaic system altogether." Such were the authors of the Alphonsine Tables, the most important heritage of Arab astronomers to medieval Europe

(272—275).

According to Kuhn, chapter 11 of Dreyer (1953) should give documentation of how the Ptolemaic system had been complicated with addition of epicycles. Dreyer, however, closes the chapter concluding: "... enough has been said to show that when Europeans again began to occupy themselves with science they found astronomy practically in the same state in which Ptolemy had left it in the second century. But the Arabs had put a powerful tool into their hands by altering the calculus of chords of Ptolemy into the calculus of sines or trigonometry, and hereby they influenced the advancement of astronomy. in a most important manner."

The same conclusion can be drawn from more specialized literature, or from other general accounts, eg. Zinner (1988: 70—75). So much for the Arabs as monster-breeders.

Neither do we find the promised traces of growing monstrosity in chapter 12 of Dreyer (1953: 281—304), entitled "The Revival of Astronomy in Europe". Dreyer reviews the major works of Renaissance astronomers. He writes that Peurbach in his Teoricæ Novæ Planetarum does not give any new developments of theory, but adopted as a cosmological doctrine the system of solid spheres (288—289). Similarly Regiomontanus accepted every detail of the Ptolemaic system, and so did Francesco Maurolico still in 1543 (290, 295). Dreyer (1953: 295—296) characterizes the books of 'De Sphera' -literature as follows: "They show that the work of the Alexandrian astronomers was not well known and appreciated in Europe, but they show at the same time that no attempt had yet been made to continue and extend that work." The same conclusion is confirmed from a study of original sources to which we refer later in this article.

Hence, from Dreyer's book we find no evidence for the widely spread belief that the Ptolemaic system would have been made more complicated with addition of epicycles. For other types of less successful cosmological attempts we find evidence enough. Some Italians tried to construct planetary mechanisms using only the concentric spheres suggested by al-Bitruji. These constructions may have fulfilled all requirements we might put up for an astronomical system to be a monster; they were both complicated and inaccurate, but they had nothing whatever to do with the Ptolemaic system. No astronomer took them seriously, they were motivated solely by the dogms of natural philosophy. The best known specimen, given by Girolamo Fracastoro in his Homocentrica (Venedig 1538), uses 79 spherical shells to turn around the celestial bodies. Dreyer comments (1953: 297):

"It is to be hoped that Fracastoro understood his own system in every particular, but he certainly had not the gift of making his readers get a clear idea of every detail of the cumbersome machinery he offered as a substitute for the elegant geometrical system of Ptolemy."

Further (301): "And this system was supposed to be more reasonable than that of Ptolemy."

Enterprises like that of Fracastoro cannot be taken as evidence for a 'crisis' of the Ptolemaic system. We might as well point to Velikovsky to prove that our present astronomy is in crisis.

Reference to any secondary source, even to such an excellent one as Dreyer (1953), is of course no sufficient evidence against the thesis postulating a crisis of Ptolemaic Astronomy. The preceding paraphrase of Dreyer only shows that his book certainly can not be used as evidence supporting such a thesis. Kuhn, however, refers only to that book as a document for complications of Ptolemaic planetary mechanisms with the addition of epicycles. A confirmation of the non-existence of such complications in standard expositions of Ptolemaic Astronomy can be got by reading the medieval and renaissance text-books. In an article of limited scope the non-existence of anything is difficult to document to the satisfaction of everybody, hence the burden of proof stays with those who maintain that such complications can be found. Regrettably, the more extensive exposition given in Lehti (1989) is in Finnish, and therefore scarcely available to all readers. I attempt to improve the situation by giving in the following some references to the original texts for support of my critical attitude to the supposed crisis of Ptolemaic planetary theory, and thus to render the present article as self-contained as possible.

The most important source of Ptolemaic astronomy which Copernicus used, was the Epytoma of Regiomontanus. A study of that work (presently available as a facsimile Regiomontanus 1972) confirms the evaluation given by Dreyer. Regiomontanus attempts to clarify the central features of the Ptolemaic system in a form more readily assimilated by the reader. He leaves out much tabulary and other inessential material and concentrates upon the main questions. The only changes of any consequence he makes to the Almagest concern the possible slow changes of the solar orbit and the changing precession (Regiomontanus, 1972: 105, 178—179). The reader finds no hint of the 'scandalous' state of Ptolemaic astronomy[3].

In Kuhn (1957: 126—127) the reader finds a different account of the works of Peurbach and

Regiomontanus. We read that when the scientific classics of Greece became known in authentic Greek versions "the Ptolemaic system's recognized failure correctly to predict celestial motions could no longer be blamed upon errors accumulated in transmission and translation". After Peurbach had failed when trying to "reconstruct a more adequate and complete account of Ptolemy's system than any known before", Regiomontanus studied the Greek manuscripts, discovering that "even Ptolemy's original formulation was inadequate. By making available sound texts of ancient authors, fifteenth-century scholars helped Copernicus' immediate predecessors to recognize that it was time for a change."

A reader interested in the history of astronomy wonders who these 'immediate predecessors' of Copernicus are. Who were the astronomers who found the Ptolemaic system inadequate for predicting planetary positions? Regiomontanus was not among them, and, as we will later show, neither was Copernicus himself. The changes renaissance astronomers made to the Ptolemaic system affected minor details: introduction of new values of the parameters or slow change of the parameters. Such changes do not give evidence of any failures of the fundamental structure. A study of the astronomical literature reveals that before and contemporaneously with Copernicus, the knowledge of and interest in the system of the Almagest flourished more than ever before. This state of affairs can scarcely be characterized as a crisis[4]. For Copernicus' innovation the renaissance revival of Ptolemaic astronomy was of decisive importance, as will be seen later.

Neoplatonism and Copernicus' innovation

We proceed to the second component of the monsterological explanation of the Copernican Revolution, according to which the Ptolemaic system frightened Copernicus not only because of its inaccuracy, but also because of its ugliness. This type of esthetic argumentation has as such little relevance for the thesis that the failure of puzzle-solving is a prerequisite of a scientific revolution (cf. Heidelberger 1980: 275—277). For the present we ignore such philosophical subtleties: we are simply interested in the historical problem of how far the use of proper esthetics of the cosmos, as argument for heliocentrism, has explanatory value.

It is somewhat surprising that the system of the Almagest is often condemned as clumsy, complicated, and disagreeable. No thorough investigator of the Almagest has pronounced such a judgement — neither the past astronomers nor the modern historians. There are unnecessarily complicated details in the Almagest, but as a rule it is 'complicated' in exactly the same sense as any advanced and detailed mathematical representation of natural phenomena, say a text-book of Newtonian celestial mechanics.

In Kuhn (1957: 126—133) we find a setting where a new attitude to what must be required of a satisfactory cosmological structure led to criticism of the Ptolemaic system, and hence to the Copernican Revolution. The roots of the new attitude are located in the renaissance neoplatonism.

"Neoplatonism completes the conceptual stage setting for the Copernican Revolution, at least as we shall examine it here. For an astronomical revolution it is a puzzling stage, because it is set with so few astronomical properties. Their absence, however, is just what makes the setting important. Innovations in a science need not be responses to novelties within that science at all. No fundamental astronomical discovery, no new sort of astronomical observation, persuaded Copernicus of ancient astronomy's inadequacy or of the necessity for change. Until half a century after Copernicus' death no potentially revolutionary changes occurred in the data available to astronomers. Any possible understanding of the Revolution's timing and of the factors that called it forth must, therefore, be sought principally outside of astronomy, within the larger intellectual milieu inhabited by astronomy's practitioners...." (Kuhn 1957: 132).

This key text gives rise to several questions:

— Are there no reasons for the Copernican innovation inherent in the astronomy proper of the sixteenth century?

— Are there no reasons for 'the Revolution's timing' inherent in Copernicus' scientific heritage?

— Are such reasons to be found in neoplatonism?

We postpone the first two questions, and concentrate upon some specified components of neoplatonism explicitly brought up by Kuhn and by several other authors both before and after Kuhn[5]. It is maintained that the requirement for mathematical simplicity was initiated by the neoplatonists, and it led the astronomers to recognize the ugliness of the Ptolemaic monster, and hence to an attempt to construct something simpler in its stead. It is also maintained that the idea of heliocentrism grew up from the adoration of the Sun typical of the neoplatonists. A thorough analysis of both assertions would need an extensive study of renaissance thought. Such cannot be attempted here, so we must restrict the argumentation to a few remarks

(for a somewhat more extended exposition see Lehti 1989, esp. pp. 139—175).

The solar cult of renaissance neoplatonists had its roots in classical antiquity; it was founded on the one hand on the obvious everyday observation of the Sun's dominating role upon earthly affairs, on the other hand on the astronomer's discovery of how the motions of the planets depended upon that of the Sun. A typical formulation is that found in the Natural History II 12—13 of Pliny (1979: 176—179):

"In the midst of these [the planets] moves the sun, whose magnitude and power are the greatest, and who is the ruler not only of the seasons and of the lands, but even of the stars themselves and of the heaven. Taking into account all that he effects, we must believe him to be the soul, or more precisely the mind, of the whole world, the supreme ruling principle and divinity of nature."

Similar encomia are found in several authors; in latin Europe that given by Macrobius was in influence comparable to that of Pliny. The posing of the Sun 'in the midst' of the planets meant that three planets, Mars, Jupiter, and Saturn, were thought to be above it, and three others, Venus, Mercury, and the Moon, below it. The Sun itself was called 'the heart of the world', or the 'chorus-leader' who directed the dance of the wandering stars[6].

This was also the view of the Sun's cosmic position, which inspired the neoplatonists of Italian renaissance and their followers, from Ficino through Telesio and Dee to Fludd. Sometimes we read that some of these hermetics adopted the heliocentric idea or were at least sympathetic to it. With the exception of Giordano Bruno, these assertions are unfounded (cf. Lehti 1989: 139—163). The dominating role of the Sun was seen in mystical terms; the dependence of planetary motions upon the Sun was seen as a sign of the glorious authority of the Celestial Majesty, not as a trivial consequence of its position in the geometrical center. This attitude seems sooner to have hampered than advanced the progress of heliocentrism; it was for instance characteristic of Tycho Brahe and his pupil Longomontanus, and gave them a cosmological frame with which to resist the Copernican heliocentrism (Lehti 1989: 164—175).

In sum. even if we allow some traces of neoplatonism in the thinking of Copernicus and Kepler, that does not help to explain their heliocentrism, as the great majority of ardent neoplatonists remained convinced geocentrists and interpreted the dominating role of the Sun in the traditional dreamy manner. This holds true both for the predecessors and the followers of Copernicus. Assertions such as the following one have no foundations in historical reality: "During the sixteenth century the heliocentric view was accepted only within the Pythagorean-Hermetic tradition. On Hermetic assumptions the central place of the sun in the universe seemed axiomatic because it was 'fitting'." (Kearney 1971: 104; see also pp. 39, 100—101, 106—107). For criticism of such views see Lehti 1989: 186—196.

The question of what role the neoplatonists or hermetists played in the rise of appreciation of mathematics in natural philosophy, is a complicated one. Several of them, e.g. Giordano Bruno, John Dee, and Robert Fludd, wrote glowing eulogies about the glory of mathematics, and this attitude — typical also of Galileo — may have counteracted the traditional Aristotelian mistrust of mathematics as a way to knowledge, and hence opened the way for mathematization of science. On the other hand, what these authors meant with mathematics was something quite different from what the mathematicians proper, and the appliers of mathematics, have both before and after meant with that word. For the Hermetics mathematics was intuitive peering into the realm of otherworldly reality, using number- and figure-mysticism as a weapon. When John Dee criticized the teaching in the universities, he lamented over the absence of such sciences as 'De numeris formalibus' and 'De Mensuris Divinis'. He himself agreed with other hermetics in accepting the operation of numbers in three worlds, the elemental, the celestial, and the supercelestial one. When he himself applies mathematics to astronomical phenomena, he gives only senseless programs without any attempt to carry them through[7]. Nevertheless, Dee was the one of the Hermetists who for some unknown reason was characterized as a mathematician; the achievements of the others are even worse[8]. How an attitude like that of the Hermetists or neoplatonists could have inspired Copernicus, or any other astronomer, remains a mystery.

The critical attitude of the Hermetists to the constructions of Ptolemaic astronomy has been taken as a proof of their importance for the Copernican Revolution. Cornelius Agrippa wrote about 1530 concerning the astronomers of his time: "Their vain disputes about Eccentricks, Concentricks, Epicycles, Retrogradations. Trepidations, accessus, recessus, swift motions and circles of motion ... (are) the works neither of God nor Nature, but the Fiddle-Faddle and Trifles of Mathematicians" (Shumaker-Heilbron 1978: 99). Giordano Bruno wrote later in his De l'infinito universo et mondi in a similar vein: "Now see, you Astrologers together with supporting philosophers, have you not with your circles, attempting to describe your nine fictitious moving spheres, so completely imprisoned your reason that you resemble a lot of parrots in a cage ...". At the end of the dialogue Bruno cheers himself up: "Destroy these fictitious concave and convex surfaces which bound and separate the elements from the heavens. Heap ridicule upon the spheres of the deferents and the fixed stars" (Bruno 1888: 304, 400).

This ridicule, however, was directed, not specifically at Ptolemy, but at all attempts to describe mathematically the planetary motions. When Copernicus appeared on the stage, the Hermetists critisized him with the same gusto. Even the only Copernican among them, Bruno himself, had little sympathy with Copernicus' mathematics. In La cena de le ceneri he writes about Copernicus: "Yet he himself did not much transcend it [the commonly received philosophy]; for being more a student of mathematics than of nature he was not able to penetrate deeply enough to remove the roots of false and misleading principles and, by disentangling all the difficulties in the way, to free both himself and others from the pursuit of empty enquiries and turn their attention to things constant and certain." (Bruno 1888: 124; translation from Yates (1964): 236).

Those stressing the role of neoplatonism as the source of heliocentrism, identify Copernicus' Italian teacher Domenico Maria de Novara as the person who introduced Copernicus to the neoplatonic criticism of Ptolemaic astronomy[9]. Novara was a pupil of Regiomontanus, and from the comparison of his own observations with some results given by Ptolemy, he suggested some changes in the details of the Almagest; these concerned a possible slow motion of the celestial axis, and changes in the inclination of the ecliptic (Zinner 1988: 161—165). Such details may well have influenced Copernicus in his study of the Almagest, but they have nothing to do with the change of paradigm which is assumed to have been a necessary condition for the 'revolution'. We read in Kuhn (1957: 129): "Novara himself was among the first to criticize the Ptolemaic planetary theory on Neoplatonic grounds, believing that no system so complex and cumbersome could represent the true mathematical order of nature". As we have seen, such criticism was typical of the Hermetic-neoplatonic phantasts, but it would be most curious from a competent astronomer like Novara whose criticism of Ptolemy was directed upon minor details. The authors who consider Novara as the link between neoplatonism and Copernicus have not given documentary evidence for the assertion that Novara had rejected the Ptolemaic astronomy on neoplatonic grounds.

The Calendar Reform as a possible motive for Copernicus

The errors accumulated in the Julian calendar are often offered as a motive for expectation of a reform in astronomy in the sixteenth century. In Kuhn (1968: 152) we read: "Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis. ... Copernicus thus claimed that he had solved the long-vexing problem of the length of the calendar year. ..." Also (Kuhn 1968: xii): "One need, however, look for no further than Copernicus and the Calendar to discover that external conditions may help to transform a mere anomaly into a source of acute crisis." In Kuhn (1957: 125—126) we get a more detailed version of the relation of Calendar reform to Copernicus' work, leading to a conclusion: "Reform of the calendar demanded, said Copernicus, reform in astronomy. The preface of his De Revolutionibus closed with the suggestion that his new theory might make a new calendar possible. The Gregorian calendar, first adopted in 1582, was in fact based upon computations that made use of Copernicus' work." — Similar assertions are found in many other presentations[10].

The picture thus formed of the role of the Calendar reform in the Copernican innovation needs revision to which we turn point for point.

We read (Kuhn 1957: 125): "Agitation for calendar reform had an even more direct and dramatic effect on the practice of Renaissance astronomy, for the study of calendars brought the astronomers face to face with the inadequacy of existing computational techniques. The cumulative errors of the Julian calendar had been recognized much earlier, and proposals for calendar reform date from the thirteenth century or before. But these proposals were ineffective until the sixteenth century, when the increasing size of political, economic, and administrative units placed a new premium upon an efficient and uniform means of dating."

There is, however, scarcely any evidence that the Julian calendar would have been unsatisfactory for any political, economic or administrative use. The place where it failed was the determination of Easter according to the old rules. The rules for the dating of Easter had a more or less astrological origin; they presented a problem for the ecclesiastics but scarcely for the astronomer or the practical man.

In Hall (1956: 16) the errors of the Julian calendar are laid at the door of antiquated astronomical tables. "The current astronomical tables had been computed at the order of King Alfonso the Wise of Castille at the end of the thirteenth century, and were out of date." To figure out what kind of crisis preceded the Gregorian calendar reform we must take a look at the earlier unsuccessful attempts.

Already in the thirteenth century Johannes de Sacrobosco, Robert Grosseteste, and Roger Bacon had recognized the error in the length $(365+{}^1/_4)^d$ of the Julian year, but they were unable to replace it with anything better than the tropical year $(365+1/4—1/300)^d$ used in the Ptolemaic tables (Duhem 1973; 45—50); Sacrobosco uses in fact an approximation $(365+{}^1/_4—1/288)^d$ (Sacrobosco (1568): fol L4v—L5v, M7v—M8r). The Alphonsine Tables became known in Paris around the year 1300. They use a tropical year of the length $(365+{}^1/_4—1/134)^d=365^d5^h49^m16^s$, easily calculable from the excerpt of the Tables reproduced in Grant (1974): 480. About the year 1345 Jean de Murs and Firmin de Belleval made a proposal to pope Clementius VI for a calendar reform using that length of the year, and also the Alphonsine length of the synodic month. The main burden of the proposal concerned the lunisolar periods necessary for the ecclesiastic calendar, especially the dating of Easter. This was the moot point of the reform, but it had no relevance for astronomy and little interest for an astronomer as an astronomer. The only astronomical information needed consisted of the two constants, the lengths of the tropical year and the synodic month; the rest meant just combinatorial juggling with these constants to obtain a rule of dates necessary for a calendar. The proposal of 1345 did not lead to any action; neither did a similar proposal which cardinal Pierre d'Ailly presented for the Council of Constance in 1414 (Duhem 1973: 51—60, 168—182). In both cases the failure of the reform was due to the fear of the ecclesiastical officials to introduce an innovation contrary to what the Council of Nicea had sanctioned. That council had decreed an ecclesiastic calendar with the Julian length of the year, and rules for Easter. These gave the date of the full moon on days which already in the sixteenth century were wildly erroneous. These errors had nothing to do with 'inadequacy of existing computational techniques'. Using the techniques available to the astronomer, Regiomontanus had, beginning already in 1474, published calendars which gave the dates of full and new moons pretty correctly, and such unofficial but correct calendars were published regularly after that date (cf. Regiomontanus 1972: 535—564). If the Church could not accept such calendars as official, that was due to the authority of the Nicean Council. A crisis there was, but it was not a crisis of astronomy, it was a crisis of ecclesiastic conservatism.

Of special interest for our present theme is the question of Copernicus' relation to the calendar reform. After the few mentions in Copernicus' and Rheticus' writings, soon to be discussed, Copernicus seems to have been connected with the calendar reform first in Galileos celebrated letter to Grand Duchess Christina, reproduced in Drake (1957). There we read (p. 178):

"... He [Copernicus] was in fact so esteemed by the church that when the Lateran Council under Leo X took up the correction of the church calendar, Copernicus was called to Rome from the most remote parts of Germany to undertake its reform. At that time the calendar was defective because the true measures of the year and month were not exactly known. The Bishop of Culm, then superintendent of this matter, assigned Copernicus to seek more light and greater certainty concerning the celestial motions by means of constant study and labor. With Herculean toil he set his admirable mind to this task, and he made such great progress in this science and brought our knowledge of the heavenly motions to such precision that he became celebrated as an astronomer. Since that time not only has the calendar been regulated by his teachings, but tables of all the motions of the planets have been calculated as well."

This text of Galileo is presumably the original source of the widely spread belief that concern for the calendar reform was an important motive for Copernicus, and that Copernicus' undertaking was an important factor in rendering the reform possible. Both assumptions are refuted in the article by Rosen (1958). After analyzing these as well as other misstatements Galileo made about Copernicus, Rosen comes (p. 330) to the conclusion:

"... If we compare Galileo's five misstatements with the truth, we see that each of them tended to bind Copernicus more closely to the Roman Catholic church.

Galileo made these five misstatements at a time when he was fighting hard to prevent his church from denouncing Copernicanism as heretical. This farseeing and loyal purpose dominates his entire Letter to the Grand Duchess, an eloquent (albeit unavailing) effort to save the Roman Catholic church from committing a grievous error. ...

It was not any deliberate desire to distort the facts, but rather the intensity of his struggle against bigoted and narrow-minded coreligionists that, in my opinion, led Galileo astray into these five misstatements. ..."

Copernicus' role and interest in the calendar reform can best be inferred from his own works. The best document for his original motives when starting his innovative work is the early Commentariolus (Rosen 1959: 57—90); there the calendar question is never mentioned. The same can be said for the bulk text of De Revolutionibus. In chapter 3, where Copernicus thoroughly studies the length of the tropical year, he does this motivated by a desire for confirmation of his (mainly erroneous) views of several variabilities of the Earth's motion; no word is said about the possible relevance of these studies for the calendar (Copernicus 1976: 140—185; cf. Swerdlow-Neugebauer 1984; 127—179). The single Copernican reference to the calendar reform occurs at the end of his prefatory letter to Pope Paul III, where he writes, as translated by Duncan (Copernicus 1976: 27; for a German version see Zinner 1988: 251—252):

"... Mathematics is written for mathematicians, to whom this work of mine, if my judgement does not deceive me, will seem to be of value to the ecclesiastical Commonwealth over which your Holiness now holds dominion. For it is not long ago that under Leo X the question of amending the ecclesiastical calendar was debated in the Lateran Council and then remained undecided solely for the reason that the lengths of the years and months and the motions of the Sun and Moon were considered not yet adequately measured. Since that time I have devoted my attention to observing them more accurately, on the advice of the Right Reverend Paul, Bishop of Sempronia, who was then in charge of the project. However, what I have accomplished in that respect I leave to the judgement of your Holiness in particular and of all other learned mathematicians, and lest I seem to your Holiness to promise more for the utility of the work I can perform, I now pass to my task."

A similar story of how Copernicus' clerical friends urged him to work upon the calendar problem is given by Rheticus in his Narratio Prima, written about two years earlier than Copernicus' preface (Rosen 1959: 192).

The statement that Copernicus would have begun and accomplished his work because of some bishops concern for calendar reform, is obvious nonsense. From Copernicus' text we immediately infer that his reason for making such an astonishing assertion was the same as the one Rosen has found for Galileo's misstatements. Copernicus wished to introduce his precarious theories with a declaration that they may be 'of value to the ecclesiastical commonwealth'. We return later to the fact that the entire prefatory letter seems to have been written from apologetic motives; hence it is of doubtful value as a document of Copernicus' goals and procedures.

Even as it stays, the preface does not close "with the suggestion that his new theory might make a new calendar possible" (Kuhn 1957: 126). That would have been too curious a statement even for a propagandist to make. Copernicus expressed a hope that his book could be of some use. That much would anybody have bowed to the calendar reform, even without any real interest in the project. There is no evidence of such interest from Copernicus, for, as already said, after the few words in the preface he drops the question, never to return to the calendar in De Revolutionibus. There was no reason why he should, for from the perspective of astronomy, the calendar problem was trivial.

Quite the opposite is stated in Kuhn (1957: 271): "Copernicus' concern with the calendar therefore led him to a serious study of precession, and thus to an intimate knowledge of that aspect of astronomy about which the Ptolemaic astronomers were in the greatest disagreement." Here the cause and effect have been turned around. Copernicus was intensely interested in the precession as one of the motions of the Earth, and his study of the assumed change of length of the solar year was motivated by that interest. This becomes unequivocally clear to anybody making a study of the third book of De Revolutionibus.

One detail of Galileo's story has later reappeared in several ramifications, namely the assertion that "Copernicus was called to Rome from the most remote part of Germany to undertake (the calendars) reform". As a matter of fact, Copernicus was not called to Rome, he was not even asked for any special advice; he just answered an inquiry sent in general to European astronomers and theologians (Rosen 1958: 321).

There remains the question of how far the Gregorian calendar was based upon computations that made use of Copernicus' work. We have already mentioned that the early proposals for calendar reform, made in the fourteenth and fifteenth centuries, were based on constants from the old Alphonsine Tables. For the Gregorian reform, the most informative and authoritative account is found in the collected works of the Jesuit mathematician Christopher Clavius, especially in Clavius (1612a): 64—81 and (1612b): 5—29. The author of the reform was Aloysius Lilius (Giglio), and his proposal is reproduced in Clavius (1612a): 3—12. When Lilius calculated the finally accepted cycles, he decided against the Copernican variable year and favoured the Alphonsine year which "is an average of various measurements and therefore less subject to error" (Clavius 1612a: 3, cf. Rosen 1958: 329).

The only change Lilius made to the Alphonsine year was a replacement of the Alphonsine $(1/134)^d=10^m44.8^s$, which removes one leap year for every 134 years, with $(3/400)^d=10^m48^s$, which more conveniently removes 3 leap years in every 400 years. The protestant opponents of the calendar reform critisized the innovation because of the use of the fixed Alphonsine year instead of the (erroneously!) variable Copernican year. (The real motivation of the criticism was, of course, hostility against an innovation brought about by a decree of the Pope.) Hence, the main effect Copernicus had in the Calendar reform was to produce a pretence for its bigoted protestant opponents.

Father Clavius refuted the criticism pointing out that in a calendar one must use constant mean motions of the Sun, not eventual oscillations about the mean motions. Clavius mentions repeatedly that the Prutenic tables, calculated by Erasmus Reinhold according to the Copernican theory, give for the tropical year the maximum length $365^d5^h55^m37^s40^{ter}$, and the minimum length $365^d5^h42^m55^s7^{ter}$, and their mean value is the Alphonsine year $365^d5^h49^m16^s$ (Clavius 1612a: 70, 77—78; 1612b: 18—19).

All the astronomical facts (two constants!) needed for the Gregorian calendar reform are therefore found already in the Alphonsine tables calculated in the thirteenth century. The renaissance astronomers did not contribute any new facts relevant to that problem. Hence we must ask: why was the reform not brought about much earlier? There are certainly many reasons for the delay; we may propose the following ones:

Although the medieval tables already gave the lengths of the solar year and the synodic month with sufficient accuracy, that fact was maybe not so clear to those using these or other astronomical tables, for Ptolemy and al-Battani had given different values. Copernicus presumably strengthened the confidence in the Alphonsine year by producing a theory of a variable year which gave the same mean length. The coincidence, however, is due neither to chance nor to independent inquiries. In his prefatory letter to De Revolutionibus, cited above, Copernicus writes that motivated by the calendar problem he has "devoted (his) attention to observing (the motions of the Sun and the Moon) more accurately". Such assertions must be taken with more than a grain of salt. Copernicus obtained most of his numerical parameters by manipulation of earlier sources. Comparing the Alphonsine tables for mean solar motion (Grant 1974: 480) with the corresponding tables in Copernicus (1976): 164, we are lead to surmise that Copernicus has copied and simplified the medieval tables. Neither Clavius nor other contemporaneous astronomers seem to have suspected this; they presumably imagined that Copernicus had independently verified this mean solar motion and therefore the mean solar year. The authority of Copernicus may therefore have given additional confidence in the Alphonsine tables as a basis for the reform. All the astronomical credit for the reform belongs, of course, to the medieval astronomers responsible for the Alphonsine Tables. Incidentally, Copernicus' reliance on these tables illuminates his own indifference concerning the whole calendar problem. He had more important questions to concentrate upon.

Moreover, the problems of the calendar reform were not limited to its few astronomical aspects. The correct length of the year gave only a necessary 'initial value' for dating the festivals of the ecclesiastical year. The real problem was to formulate some unequivocal arithmetical rules for the dates of equinoxes and full moons, to be used at the dating of Easter. The Council of Nicea had formulated such rules relying on the supposition that the 19-year Metonic cycle consists of 235 months. After the Julian year was replaced by the Gregorian one, requiring a more complicated rule for leap years, and after a more exact value was used for the synodic month, a new and much more complicated Easter rule must be figured out. The construction of such a rule was the main feature of the proposal made by Aloysius Lilius. Already a glance at the calendar works Clavius (1612a), (1612b), shows that the incomparably greatest effort needed in the reform consisted of the combinatorics of the dates of church festivals: a problem with no relevance for astronomy.

The decisive delaying factor, however, was the wariness of later councils to break the authority of Nicea. The councils of the fifteenth and sixteenth centuries had at hand such an abundance of controversies, that they dared not increase the discord with additional seeds of dissent. The Councils implicitly admitted their inability to arrive at a decision, when they at last pushed the whole misery onto the personal responsibility of the Pope, to be decided by his decree.

To conclude this lengthy discussion: The role of the calendar reform as a motivation for astronomical reform, and especially for Copernicus' innovation, has been greatly exaggerated. As the main occupation of renaissance astronomers was the computation of planetary positions, the problems inherent in the Julian calendar had next to no effects on their practice. This may oppose a tradition of history writing of Renaissance Astronomy. So much the worse for a tradition which seems to follow the good old scholastic practice of repeating from author to author an error once generated. Edward Rosen began his analysis of the historical errors about Copernicus with a wish: "To impede their further spread is the aim of this article" (Rosen 1958: 319). Alas, the spread of historical errors is not easily impeded!

The Monster

The Copernicus-interpretation in Kuhn (1957) is founded upon the criticism of earlier astronomical systems expounded in the prefatory letter to pope Paul III in De Revolutionibus. There Copernicus moans: "... the mathematicians are so unsure of the movements of the Sun and Moon that they cannot even explain or observe the constant length of the seasonal year." He characterizes his astronomical heritage as follows: "Those who have relied on homocentrics, though they have proven that some different motions can be compounded therefrom, have not thereby been able fully to establish a system which agrees with the phenomena. Those again who have devised eccentric systems, though they appear to have well-nigh established the seeming motions by calculations agreeable to their assumptions, have yet made many admissions [...] which seem to violate the first principle of uniformity in motion. Nor have they been able thereby to discern or 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"[11].

This text is the King's witness upon which Kuhn reconstructs the Copernican Revolution as an example of what Scientific Revolutions in general shall be. According to this reconstruction, the recognition of the monster made clear to Copernicus that the geocentric astronomy could not solve the problem of the planets:

"... The traditional techniques of Ptolemaic astronomy have not and will not solve that problem: instead they have produced a monster; there must, he [Copernicus] concludes, be a fundamental error in the basic concepts of traditional planetary astronomy. For the first time a technically competent astronomer had rejected the time-honored scientific tradition for reasons internal to his science, and this professional awareness of technical fallacy inaugurated the Copernican Revolution" (Kuhn 1957: 139).

Copernicus' prefatory letter to pope Paul describes, according to Kuhn, brilliantly the causes of the transformation which made it possible to see the astronomical tradition as a monster although it had not previously seemed to be one. Since Ptolemy, one mathematician after another "had added or subtracted a few small circles", until "there were so many variant systems that the adjective 'Ptolemaic' had lost much of its meaning". In spite of the redundance: "None of the 'Ptolemaic' systems which Copernicus knew gave results that quite coincided with good naked-eye observations."

"Diffuseness and continual inaccuracy — these are the two principal characteristics of the monster described by Copernicus." The recognition of this monster and discontent with it lead to the Revolution. This is a Kuhnian pattern typical for all scientific revolutions, but the Copernican one climbs to a paradigmatic status because of the explicitness of Copernicus' preface. "Such explicit recognitions of breakdown are extremely rare, but the effects of crisis do not entirely depend upon its conscious recognition". This helps us to "recognize crisis as an appropriate prelude to the emergence of new theories"[12].

The material referred to in the previous paragraphs indicates that this description of the monster is fictitious. The Ptolemaic system had not been complicated with adding one epicycle here, another there. There was no such abundance of Ptolemaic systems that the fundamental principles of calculating astronomical predictions would have become equivocal. On the contrary, the current text-books of Ptolemaic astronomy presented a clarified exposition of the system practically as given in the Almagest; especially the planetary mechanisms had remained unchanged. The most important of the Ptolemaic text-books was the Epytoma of Regiomontanus, already referred to; others include e.g. The Commentaria in Novas theoricas planetarum Georgi Purbachii by Erasmus Oswaldus Schreckenfuchs, the Cosmographia of Francesco Maurolico, the various books explaining the construction and use of Æquatoria, for instance those by Petrus Apianus and Johannes Schoener, the Margarita Philosophica by Gregor Reisch, the Theoricæ planetarum by Nicolaus Simus, and several other similar books, including the bulky compilations of various commentaries to the Sphere of Sacrobosco and the Theoricæ Novæ Planetarum by Georg Peurbach. For bibliographical and other descriptions see the catalogue Lehti (1984: 27—30, 35, 44—46, 51—52, 80—83 etc.)

The only changes made to Ptolemy's original version concerned the problematic rate of precession, and the possible slow changes of some parameters. Neither of these was a failure of Ptolemaic planetary mechanisms, nor could they be helped by fingering those mechanisms, still less by groaning about monsters. The Ptolemaic system had not failed as a puzzle-solving device. Copernicus himself says as much in the selfsame preface, cited by Kuhn: "Those again who have devised eccentric systems [=the Ptolemaic astronomers] ... appear to have well-nigh established the seeming motions by calculations agreeable to their assumptions, ..." (Kuhn 1957: 138). A similar statement is found in Copernicus' earlier Commentariolus, to the effect that the "planetary theories of Ptolemy and most other astronomers" were "consistent with the numerical data" (Rosen 1959: 57). The reason why Copernicus deviated from Ptolemy had nothing to do with the supposed failure of geocentric astronomy as a puzzle-solving device.

Neither had it anything to do with the difficulties of the Julian calendar, although they are repeatedly given as a special sign of the crisis of pre-Copernican astronomy (Kuhn 1957: 11, 125—126, 196, 271; Kuhn 1968: xii, Kuhn 1977: 206). As we have argued, it was no crisis for the astronomers, it was a crisis for the ecclesiastic administrators. All in all, in the world of astronomy in Copernicus' time there were scarcely any traces of monstrosity, a result confirmed in Heidelberger (1980): 275—277.

It remains to turn to Copernicus' own work to look for possible traces of the infamous monster.

Copernicus as an unsuccessful monster-killer

Having identified the motive of the Copernican Revolution as the abhorrence created by the Ptolemaic monster, Kuhn describes the system which Copernicus offered as a substitution. The exposition ends with an appraisal:

"Even this brief sketch of the complex system of interlocking circles employed by Copernicus to compute planetary position indicates the third great incongruity of the De Revolutionibus and the immense irony of Copernicus' lifework. The preface to the De Revolutionibus opens with a forceful indictment of Ptolemaic astronomy for its inaccuracy, complexity, and inconsistency, yet before Copernicus' text closes, it has convicted itself of exactly the same shortcomings. Copernicus' system is neither simpler nor more accurate than Ptolemy's. ... Those features of the ancient tradition which had led Copernicus to attempt a radical innovation were not eliminated by that innovation. ... Judged on purely practical grounds, Copernicus' new planetary system was a failure: it was neither more accurate nor significantly simpler than its Ptolemaic predecessors." (Kuhn 1957. 171, see also Kuhn 1977: 322—323).

However we interpret the monster, be it an esthetic disgust or a failure of puzzle-solving tradition, Copernicus did not succeed in conquering it. "His full system was little if any less cumbersome than Ptolemy's had been. ... his cumbersome sun-centered system gave results as accurate as Ptolemy's, but it did not give more accurate results. Copernicus did not solve the problem of the planets" (Kuhn 1957: 169).

If we accept that for Copernicus the 'problem of the planets' was primarily an attempt to construct an esthetically satisfactory and numerically accurate presentation of observed celestial motions, we must agree with Kuhn. Then we must accept the 'irony' of Copernicus' lifework: he failed to arrive at what he had staked out as his goal. Hence, when Copernicus in 1543 published his work, and already in the decades he had spent in finishing it, he must have been conscious of the futility of the attempt. Is it really plausible that Copernicus spent about thirty years in hard work fostering a brainchild which was, and which he himself must have known to be, a similar monster as the Almagest?

We naive non-philosophers, however, might understand something quite different with the 'problem of the planets', namely the question of their real and true spatial configurations and motions. Supposing that this common usage was also that of Copernicus, he could with some justification maintain that he had solved the problem of the planets; moreover, it makes sense to say: "For Copernicus the motion of the earth was a by-product of the problem of the planets" (Kuhn 1957: 144). We are quite justified to lament the complexities and inaccuracies of Copernicus' solution, but a solution it was. The solution has nothing to do with killing of any monsters, neither real nor fictitious.

Hence the lifework of Copernicus can be characterized in simple terms: he discovered that the Earth revolves around the Sun, and in his book he attempted to give vindications for this discovery. A similar conclusion is drawn in Krige (1980: 97, 100), in spite of some traces left by the monsterological interpretation (pp. 149—152).

It is an embarrassing fact that this pretty self-evident statement seems to come as a surprise to many present philosophers and sociologists of science. If the most eminent feature of Copernicus' innovation, the insistence on the reality of Earth's motion, is hidden in a corner of the stage, the drama of the Copernican Revolution resembles Hamlet without the Prince of Denmark.

Did Copernicus himself hide the Prince of Denmark?

Now we must ask: How is it possible to read out from Copernicus' work that his main motive and accomplishment was not to prove the reality of the Earth's motion, but to start a crusade against a Ptolemaic monster?

"So far as possible we shall discover those [Copernicus'] contributions [to the Revolution] in Copernicus' own words drawn from De Revolutionibus, the book that presented the new astronomy to the world", writes Kuhn (1957: 134). However, he deplores that De Revolutionibus is an intrinsically difficult 'problem text' which must be dealt with in a 'relatively nonmathematical paraphrase'. Accepting this necessity the reader waits for a paraphrase where the implications of the whole work, even of technical details, are elucidated. Moreover, some notice could be given also to some other less technical and 'relatively nonmathematical' texts, for De Revolutionibus was not the only text which 'presented the new astronomy to the world'.

Arguments for the view that Copernicus' innovation was a reaction to the crisis of Ptolemaic astronomy are brought forward in the paragraph "Motives for Innovation — Copernicus' Preface". Kuhn finds a "significant incongruity of the De Revolutionibus, the disproportion between the objective that motivated Copernicus' innovation and the innovation itself". The motive was an "attempt to reform the techniques employed in computing the planetary position", but the result was "the revolutionary conception of the earth's motion". In the previous paragraph we questioned the view that the motive of the innovation can be found in any other directions than the discovery of the Earth's motion. Kuhn, however, does find another motive from "the prefatory letter that Copernicus prefixed to the De Revolutionibus in order to sketch the motive, the source and the nature of his scientific achievement" (Kuhn 1957: 137).

To justify the assertion that Copernicus did not look at Earth's motion as his central thesis, Kuhn declares that from De Revolutionibus we find only very concealedly and implicitly that point, which later was accepted as the core of the Copernican Revolution, namely the explanation of planetary retrogradations and other apparent irregularities as perspective phenomena caused by the Earth's motion. We read (Kuhn 1957: 150) that Copernicus never demonstrates the advantages of heliocentrism qualitatively for a nonmathematical reader, but obscures them behind "the abstruse quantitative details of the retrograde motions of each individual planet." Therefore Copernicus' thesis: 'Mathematics are for mathematicians' leads to a contradiction between the work and its later influence. "The Copernican revolution, as we know it, is scarcely to be found in the De Revolutionibus, and that is the second essential incongruity of the text" (Kuhn 1957: 155).

It is somewhat unjust to criticize Copernicus for such abstruseness. In chapters 1, 2, 3 of the fifth Book of De Revolutionibus we find a rather non-technical and comprehensible account of how heliocentrism explains the retrogradations (Copernicus 1976: 233—242). In his earlier Commentariolus Copernicus had formulated the central idea of heliocentrism still more explicitly. About the apparent retrogradations of the outer planets he writes as follows (Rosen 1959: 77—78):

"... There is a second inequality, on account of which the planet seems from time to time to retrograde, and often to become stationary. This happens by reason of the motion, not of the planet, but of the earth changing its position in the great circle. For since the earth moves more rapidly than the planet, the line of sight directed toward the firmament regresses, and the earth more than neutralizes the motion of the planet. This regression is most notable when the earth is nearest to the planet, that is, when it comes between the sun and the planet at the evening rising of the planet. On the other hand, when the planet is setting in the evening or rising in the morning, the earth makes the observed motion greater than the actual. But when the line of sight is moving in the direction opposite to that of the planets and at equal rate, the planets appear to be stationary, since the opposed motions neutralize each other; this commonly occurs when the angle at the earth between the sun and the planet is 120°. In all these cases, the lower the deferent on which the planet moves, the greater is the inequality. Hence it is smaller in Saturn than in Jupiter, and again greatest in Mars, in accordance with the ratio of the radius of the great circle to the radii of the deferents. The inequality attains its maximum for each planet when the line of sight to the planet is tangent to the circumference of the great circle. In this manner do these three planets move."

Copernicus has also a similar story to tell about the motions of Venus and Mercury (Rosen 1959: 83—85). The Commentariolus remained in a manuscript, although rather widely distributed, but

Copernicus' assistant and collabolator Joachim Rheticus published in two editions already before 1543 the Narratio Prima, a popular exposition of the central features of Copernicus' innovation.

The story told by Rheticus about the motions of the planets (Rosen 1959: 162—178) is more revealing than those presented by Copernicus himself. Copernicus did not care to stress that the main point of his innovation was a reinterpretation of facts already given in the Almagest, namely the "solar relations" which introduced to the motion of every planet a major term corresponding to the motion of the Sun (For Venus and Mercurius see Almagest IX 3, for outer planets IX 6, X 6, see also XII 1; Ptolemaios 1984: 425, 444, 480—484, 555—562.) Rheticus is quite explicit when pointing at the Ptolemaic solar relations:

"The ancients attributed to the epicycles of the three superior planets the entire inequality of motion which they discovered that these planets had with respect to the sun. ... So long as the ancients strove to retain the earth in the center of the universe, they were compelled by the observations to affirm that, just as Venus revolved with his own special motion on the epicycle, but by reason of the eccentric advanced with the mean motion of the sun, so conversely the superior planets in the epicycle were related to the sun, but moved with special motions on the eccentric" (Rosen 1959: 165—166).

The true explanation of these phenomena is what proves the correctness of Copernicus' assumptions. Rheticus writes (pp. 164—165):

"With regard to the apparent motions of the sun and moon, it is perhaps possible to deny what is said about the motion of the earth, although I do not see how the explanation of precession is to be transferred to the sphere of the stars. But if anyone desires to look either to the principal end of astronomy and the order and harmony of the system of the spheres or to ease and elegance and a complete explanation of the causes of the phenomena, by the assumption of no other hypotheses will he demonstrate the apparent motions of the remaining planets more neatly and correctly. For all these phenomena appear to be linked most nobly together, as by a golden chain; and each of the planets, by its position and order and every inequality of its motion, bears witness that the earth moves and that we who dwell upon the globe of the earth, instead of accepting its changes of position, believe that planets wander in all sorts of motions of their own. ..."

The "golden chain" that connects the apparent motions of the planets is the motion of the Earth. Rheticus writes (p. 168): "Consequently (as I pointed out in the reasons for revising the hypotheses) the entire inequality in the apparent motion of the planets which seems to occur in their positions with respect to the sun is caused by the annual motion of the earth on the great circle." The techniques of Copernicus consist, in the case of superior planets, of shifting the center of their revolutions from the earth to the vicinity of the Sun (Rosen 1959: 169):

"Now since we look up at the motions of the three superior planets as from the center of the earth, ... the centers of the deferents of the planets may properly be brought into relation with the center of the great circle [the orbit of the Earth]; and from this point we may then quite correctly transfer all the motions and phenomena to the center of the earth. ..."

For Venus, the innovation of Copernicus means "rejecting the deferent, which is replaced by the great circle", and further "the scheme of motions for Mercury agrees in general with the theory of Venus, ..." (p. 170).

Rheticus' account is, of course, also complicated by the details of the motions, for which Copernicus' theory indeed did not mean any simplification of Ptolemy's theory. In spite of that Rheticus gives a general account of how we see the planets from the moving Earth, and his story leaves scarcely anything to be desired on explicitness. We can not reproduce here the whole exposition (Rosen 1959: 171—178), but must be content with some excerpts:

"The foregoing is very nearly the whole system of hypotheses for saving the entire real inequality of the motion in longitude of the planets. ... But we, as dwellers upon the earth, observe the apparent motions in the heavens from the earth. Hence we refer all the motions and phenomena to the center of the earth as the foundation and inmost part of our abode, by drawing lines from it through the planets, as though our eye had moved from the center of the great circle [practically the Sun] to the center of the earth. Clearly it is from this latter point that the inequalities of all the phenomena, as they are seen by us, must be calculated. ...

When, as the earth advances with the motion of the great circle, it reaches a position where it is on a straight line between the sun and one of the three superior planets, the planet will be seen at its evening rising; and because the earth, when so situated, is at its nearest to the planet, the ancients said that the planet was at its nearest to the earth and in the perigee of its epicycle. ...

Moreover, in the hypotheses of the three superior planets the great circle takes the place of the epicycle attributed to each of the planets by the ancients. ...

motion, is hidden in a corner of the stage, the drama of the Copernican Revolution resembles Hamlet without the Prince of Denmark.

Did Copernicus himself hide the Prince of Denmark?

Now we must ask: How is it possible to read out from Copernicus' work that his main motive and accomplishment was not to prove the reality of the Earth's motion, but to start a crusade against a Ptolemaic monster?

"So far as possible we shall discover those [Copernicus'] contributions [to the Revolution] in Copernicus' own words drawn from De Revolutionibus, the book that presented the new astronomy to the world", writes Kuhn (1957: 134). However, he deplores that De Revolutionibus is an intrinsically difficult 'problem text' which must be dealt with in a 'relatively nonmathematical paraphrase'. Accepting this necessity the reader waits for a paraphrase where the implications of the whole work, even of technical details, are elucidated. Moreover, some notice could be given also to some other less technical and 'relatively nonmathematical' texts, for De Revolutionibus was not the only text which 'presented the new astronomy to the world'.

Arguments for the view that Copernicus' innovation was a reaction to the crisis of Ptolemaic astronomy are brought forward in the paragraph "Motives for Innovation — Copernicus' Preface". Kuhn finds a "significant incongruity of the De Revolutionibus, the disproportion between the objective that motivated Copernicus' innovation and the innovation itself". The motive was an "attempt to reform the techniques employed in computing the planetary position", but the result was "the revolutionary conception of the earth's motion". In the previous paragraph we questioned the view that the motive of the innovation can be found in any other directions than the discovery of the Earth's motion. Kuhn, however, does find another motive from "the prefatory letter that Copernicus prefixed to the De Revolutionibus in order to sketch the motive, the source and the nature of his scientific achievement" (Kuhn 1957: 137).

To justify the assertion that Copernicus did not look at Earth's motion as his central thesis, Kuhn declares that from De Revolutionibus we find only very concealedly and implicitly that point, which later was accepted as the core of the Copernican Revolution, namely the explanation of planetary retrogradations and other apparent irregularities as perspective phenomena caused by the Earth's motion. We read (Kuhn 1957: 150) that Copernicus never demonstrates the advantages of heliocentrism qualitatively for a nonmathematical reader, but obscures them behind "the abstruse quantitative details of the retrograde motions of each individual planet." Therefore Copernicus' thesis: 'Mathematics are for mathematicians' leads to a contradiction between the work and its later influence. "The Copernican revolution, as we know it, is scarcely to be found in the De Revolutionibus, and that is the second essential incongruity of the text" (Kuhn 1957: 155).

It is somewhat unjust to criticize Copernicus for such abstruseness. In chapters 1, 2, 3 of the fifth Book of De Revolutionibus we find a rather non-technical and comprehensible account of how heliocentrism explains the retrogradations (Copernicus 1976: 233—242). In his earlier Commentariolus Copernicus had formulated the central idea of heliocentrism still more explicitly. About the apparent retrogradations of the outer planets he writes as follows (Rosen 1959: 77—78):

"... There is a second inequality, on account of which the planet seems from time to time to retrograde, and often to become stationary. This happens by reason of the motion, not of the planet, but of the earth changing its position in the great circle. For since the earth moves more rapidly than the planet, the line of sight directed toward the firmament regresses, and the earth more than neutralizes the motion of the planet. This regression is most notable when the earth is nearest to the planet, that is, when it comes between the sun and the planet at the evening rising of the planet. On the other hand, when the planet is setting in the evening or rising in the morning, the earth makes the observed motion greater than the actual. But when the line of sight is moving in the direction opposite to that of the planets and at equal rate, the planets appear to be stationary, since the opposed motions neutralize each other; this commonly occurs when the angle at the earth between the sun and the planet is 120°. In all these cases, the lower the deferent on which the planet moves, the greater is the inequality. Hence it is smaller in Saturn than in Jupiter, and again greatest in Mars, in accordance with the ratio of the radius of the great circle to the radii of the deferents. The inequality attains its maximum for each planet when the line of sight to the planet is tangent to the circumference of the great circle. In this manner do these three planets move."

Copernicus has also a similar story to tell about the motions of Venus and Mercury (Rosen 1959: 83—85). The Commentariolus remained in a manuscript, although rather widely distributed, but

Copernicus' assistant and collabolator Joachim Rheticus published in two editions already before 1543 the Narratio Prima, a popular exposition of the central features of Copernicus' innovation.

The story told by Rheticus about the motions of the planets (Rosen 1959: 162—178) is more revealing than those presented by Copernicus himself. Copernicus did not care to stress that the main point of his innovation was a reinterpretation of facts already given in the Almagest, namely the "solar relations" which introduced to the motion of every planet a major term corresponding to the motion of the Sun (For Venus and Mercurius see Almagest IX 3, for outer planets IX 6, X 6, see also XII 1; Ptolemaios 1984: 425, 444, 480—484, 555—562.) Rheticus is quite explicit when pointing at the Ptolemaic solar relations:

"The ancients attributed to the epicycles of the three superior planets the entire inequality of motion which they discovered that these planets had with respect to the sun. ... So long as the ancients strove to retain the earth in the center of the universe, they were compelled by the observations to affirm that, just as Venus revolved with his own special motion on the epicycle, but by reason of the eccentric advanced with the mean motion of the sun, so conversely the superior planets in the epicycle were related to the sun, but moved with special motions on the eccentric" (Rosen 1959: 165—166).

The true explanation of these phenomena is what proves the correctness of Copernicus' assumptions. Rheticus writes (pp. 164—165):

"With regard to the apparent motions of the sun and moon, it is perhaps possible to deny what is said about the motion of the earth, although I do not see how the explanation of precession is to be transferred to the sphere of the stars. But if anyone desires to look either to the principal end of astronomy and the order and harmony of the system of the spheres or to ease and elegance and a complete explanation of the causes of the phenomena, by the assumption of no other hypotheses will he demonstrate the apparent motions of the remaining planets more neatly and correctly. For all these phenomena appear to be linked most nobly together, as by a golden chain; and each of the planets, by its position and order and every inequality of its motion, bears witness that the earth moves and that we who dwell upon the globe of the earth, instead of accepting its changes of position, believe that planets wander in all sorts of motions of their own. ..."

The "golden chain" that connects the apparent motions of the planets is the motion of the Earth. Rheticus writes (p. 168): "Consequently (as I pointed out in the reasons for revising the hypotheses) the entire inequality in the apparent motion of the planets which seems to occur in their positions with respect to the sun is caused by the annual motion of the earth on the great circle." The techniques of Copernicus consist, in the case of superior planets, of shifting the center of their revolutions from the earth to the vicinity of the Sun (Rosen 1959: 169):

"Now since we look up at the motions of the three superior planets as from the center of the earth, ... the centers of the deferents of the planets may properly be brought into relation with the center of the great circle [the orbit of the Earth]; and from this point we may then quite correctly transfer all the motions and phenomena to the center of the earth. ..."

For Venus, the innovation of Copernicus means "rejecting the deferent, which is replaced by the great circle", and further "the scheme of motions for Mercury agrees in general with the theory of Venus, ..." (p. 170).

Rheticus' account is, of course, also complicated by the details of the motions, for which Copernicus' theory indeed did not mean any simplification of Ptolemy's theory. In spite of that Rheticus gives a general account of how we see the planets from the moving Earth, and his story leaves scarcely anything to be desired on explicitness. We can not reproduce here the whole exposition (Rosen 1959: 171—178), but must be content with some excerpts:

"The foregoing is very nearly the whole system of hypotheses for saving the entire real inequality of the motion in longitude of the planets. ... But we, as dwellers upon the earth, observe the apparent motions in the heavens from the earth. Hence we refer all the motions and phenomena to the center of the earth as the foundation and inmost part of our abode, by drawing lines from it through the planets, as though our eye had moved from the center of the great circle [practically the Sun] to the center of the earth. Clearly it is from this latter point that the inequalities of all the phenomena, as they are seen by us, must be calculated. ...

When, as the earth advances with the motion of the great circle, it reaches a position where it is on a straight line between the sun and one of the three superior planets, the planet will be seen at its evening rising; and because the earth, when so situated, is at its nearest to the planet, the ancients said that the planet was at its nearest to the earth and in the perigee of its epicycle. ...

Moreover, in the hypotheses of the three superior planets the great circle takes the place of the epicycle attributed to each of the planets by the ancients. ...

But when the earth reaches the part of the great circle that is nearer to the planet, the direction of its motion at once becomes westward, so that the apparent motion of the planet forthwith seems slower to us. Moreover, because the earth mounts toward the planet, ..., the planet is thought to approach us, as though it were descending from its upper circumference. However, the motion of the planet seems to be direct, until the center of the earth reaches the point, ... For there, since the two motions neutralize each other, the planet appears to remain at its first stationary point for a number of days, ... Then, as the earth moves from this position nearer to the planet, we believe that the planet retrogrades and moves in precedence, since the regression of the line of the true place of the planet perceptibly exceeds the real motion of the planet. This apparent retrogradation continues until the earth reaches the true perigee of the planet with respect to the great circle, where the planet, at the mid-point of regression, is in opposition to the sun and nearest to the earth. ...

The foregoing is the first use made of the great circle [the Earth's motion] in the study of the planetary motions; by it we are freed from the three large epicycles in Saturn, Jupiter and Mars. What the ancients called the argument of the planet, my teacher calls the planet's motion in commutation, for by means of it we explain the phenomena arising from the motion of the earth on the great circle. These phenomena are clearly caused by the great circle, as the parallaxes of the moon are caused by the ratio of the radius of the earth to the lunar circles. ...

Moreover, we shall find the second of the uses of the great circle, no less important than the first, in the theory of Venus and Mercury. For since we observe these two planets from the earth as from a lookout, even if they should remain fixed like the sun, nevertheless, because we are carried about them by the motion of the great circle, we would think that they, like the sun, traverse the zodiac in motions of their own. ...

But since we do not observe the motions of the planets from the center of the great circle [practically from the Sun], nor does the annual motion of the earth cease, it will be quite clear why these phenomena appear in such great variety to us who inhabit the earth. In accordance with the size of their circles, Venus and Mercury outrun the earth by their swifter motion, while the earth follows them in its annual motion. Therefore Venus overtakes the earth in about sixteen months, and Mercury in four; with these intervals as their period, the planets show us again and again all the phenomena which God desired to be seen from the earth.

... We think that Venus and Mercury move on their circles with the motion with which the ancients said that they moved on the epicycle. But since this motion is merely the difference by which the swifter planet exceeds the mean motion of the earth or sun, my teacher calls this excess the motion in commutation, for exactly the same reasons as in the three superior planets. ..."

For everyone who cares to read Rheticus' book, no equivocation remains for the main idea of Copernicus' innovation. In the sixteenth century, readers certainly studied that introduction to Copernicanism, as two editions were published already before De Revolutionibus, and then the book was added as an expository appendix to the second edition of Copernicus' own technical work.

The one thing which may make it difficult for a modern reader to follow Rheticus' exposition is the lack of figures to illustrate the geometric situation. Even the figures given in other early defenses of heliocentrism do not satisfactorily visualize how the annual revolution of the Earth brings forth the stations and retrogradations of the planets (see Copernicus 1976: 239, 241; Kepler 1981: between pp. 85, 86; Stevin 1961: 182, 192, 196). It seems that Rheticus' verbal description got, in the case of upper planets, its first convincing illustration in the Dialogue of Galileo (Galilei 1967: 343). Galileo was, after all, a pedagogue of no mean talent.

A knowledgeable sixteenth-century reader was, however, presumably better acquainted with the concepts and terminology of planetary theory than a modern reader with customary education is, and to such a reader the text of Rheticus appeared comprehensible and illustrative enough. Kepler provides evidence that such was the case. When he in his scientific firstling gave a similar exposition of the main idea of Copernicanism he apologizes this with his previous ignorence of Rheticus' exposition: "I collected ... little by little ... the advantages which Copernicus has mathematically over Ptolemy. I could easily have been relieved of this toil by Joachim Rheticus, who has briefly and penetratingly treated the particular points in his Narratio Prima" (Kepler 1981: 63).

In the light of Rheticus' text it is somewhat unfair to accuse Copernicus of a lack of clear demonstration of his main point and the astronomical advantages of heliocentrism in a way accessible to the nonmathematical lay reader (cf. Kuhn 1957: 150). Granted, the Narratio Prima was not written by Copernicus, but it was written under his direct influence and probably surveillance. It was generally accepted as an introduction to De Revolutionibus,

and it gave an exposition of the new system which was extremely explicit about exactly those points we find central in the Copernican Revolution.

Therefore, from Commentariolus and Narratio Prima we get a different picture both of Copernicus' motives and his manner of presentation than from the citations given in Kuhn (1957) and taken from the preface and Book I of De Revolutionibus. Although Kuhn has used the 1939 -edition of Rosen (1959), which contains the translations of Commentariolus and Narratio Prima, his few references to these works do not include anything to document Copernicus' motives and achievements. (Cf. Kuhn 1957: 138, 171, 186, 196, 280.)

Copernicus in the disguise of a Monster-killer

We turn to the question of where Kuhn finds the evidence for Copernicus' fear and disgust of the Ptolemaic monster. From Book I of De Revolutionibus Kuhn gives references to the chapters concerning the possibility of physics in a Copernican world, and some parts of chapter 10 to prove the Hermetic influence upon Copernicus[13]. With the exception of the latter, only the prefatory letter to pope Paul III is used as a document of Copernicus' aims and ideas. In the preface we find the lamentations about the various astronomers and the monster they have produced. It is quite revealing to compare the pathetic style of the preface with the sober presentation given in the beginning of the Commentariolus (Rosen 1959: 57). In the preface, and only there, Copernicus mentions the calendar reform, but not even there does he maintain that his innovation would solve the calendar problem, neither does he accuse the Ptolemaic system of inaccuracy.

Are we justified to conclude that Copernicus in his prefatory letter to De Revolutionibus has sketched "the motive, the source and the nature of his scientific achievement" (Kuhn 1957: 137)? Every author of long works knows that the prefaces are the last pieces to be written. We have evidence that the preface to De Revolutionibus was written in the summer of 1542 (Zinner 1988: 451). Hence, if we are interested in Copernicus' motives when he began his work, we must turn to the early Commentariolus where Copernicus in a simple and unequivocal way begins with the heliocentric assumptions and their sufficiency "to explain so many apparent inequalities in the heavens" (Rosen 1959: 58—59).

The motive of Copernicus' prefatory letter to pope Paul is quite different, and can be inferred from its first words: "I may well presume, most Holy Father, that certain people, as soon as they hear that in this book about the Revolutions of the Spheres of the Universe I ascribe movement to the earthly globe, will cry out that, holding such views, I should at once be hissed off the stage." The entire preface is an attempt to prevent such accusations. Copernicus turns aside the accusation that he has written "contrary the received opinion of Mathematicians", pointing that "the mathematicians are so unsure", and the "philosophers could by no means agree on any one certain theory of the mechanism of the Universe." He refutes the reproach that his "opinion seemed absurd", listing such honored authorities as Cicero and Plutarchus, and especially the Pythagoreans to whom "had been granted freedom to imagine such circles as they chose to explain the phenomena of the stars." He is not "pleased with [his] own work", but "kept it in store not for nine years only, but to a fourth period of nine years", but then bishops and cardinals urged that he should not refuse any more to contribute the fruits of his labors to the common advantage; they might "contribute even to the Commonwealth of the Church" concerning "the question of correcting the ecclesiastical calendar."[14]

We read in Kuhn (1957: 137, 145, 154, 155, 171) about the inner incongruities in Copernicus' work, especially between its motivations and their realization. The incongruities, however, do not belong to the work itself, they are contradictions between the prefatory letter, and everything else Copernicus has written. Monsters and calendar reforms are mentioned only in preface, nowhere else. In the preface Copernicus lists as his authorities such figures as Plutarchus, Cicero, and Nicetas; a lot which an astronomer finds somewhat comical, but a sixteenth century humanist certainly imposing. A study of Copernicus' work reveals that his real authorities were the Arabs of the Maragha school who had replaced the Ptolemaic equant with a combination of circles, the astronomical text-books of the fifteenth and sixteenth centuries, especially that of Regiomontanus, where the appearance of solar motion as a component of the motions of the planets was thoroughly exposed, and the medieval scholastics who had speculated about the physical possibility of the Earth's rotation.[15] These authors, his real sources, Copernicus never mentions. An incongruity indeed!

The motive of the preface explains why Copernicus there characterized the earlier planetary theory as a monster, and his heliocentric theory as a valiant attempt to escape from that monster. As a matter of fact, nobody saw a monster, neither the community of astronomers, nor Copernicus himself. Copernicus invented the Monster, because in his preface he needed a Monster.

Thomas Kuhn, a deep and keen analyzer of the history of science, knows how misleading prefaces can be as documents. In his article "The Relations between History and the History of Science", reprinted in Kuhn (1977; 127—161), he warns of the dangers of skipping 'the chapters that deal with technical contributions', and relying on programmatic prefaces. He writes (133): "But, ..., the relation of prefaces and programmatic writings to substantive science is seldom literal and always problematic. The former must, of course, be read, for they are frequently the media through which scientific ideas reach a larger public. But they are often decisively misleading with respect to a whole series of issues that the historian ought, and often pretends, to deal with: Where do influential ideas come from? What gives them their special authority and appeal? ..." Again on p. 136 Kuhn warns: "... characteristic infirmities result from what I have previously described as history derived predominantly from prefaces and programmatic works".

In his earlier work, when describing the Copernican Revolution, Kuhn accepted the programmatic preface of De Revolutionibus as a reliable document of 'where influential ideas come from.' There, and only there, Copernicus laments about the crisis of Ptolemaic astronomy which had created a monster. Monsters are useful fictions, both when you are making a Scientific Revolution, and when you are explaining one.

Copernicus' discovery

That Copernicus would have discovered the Earth's motion by making observations of the heavens, is no longer maintained in any realistic presentation of the history of science. Then, how could he make this discovery, or was such a discovery not at all central to his achievement? Shall we agree with Kuhn (1957: 137): "In Copernicus' work the revolutionary conception of the earth's motion is initially an anomalous by-product of a proficient and devoted astronomer's attempt to reform the techniques employed in computing planetary positions". This view attributes the entire Copernican Revolution to the inquirer's psychologically and sociologically determined attitude; the facts of nature have nothing to do with it.

Indeed, which new data could Copernicus have at his disposal? The peculiarity of the Copernican Revolution is that the data were found, not from the study of nature, but from a study of books. Already in the Almagest we find the curious 'solar relations' which join the motion of the Sun as one component to the motion of every planet. As soon as planetary theory began to be taught in medieval European universities, these relations were mentioned, and often commented on, in the texts, beginning with the Theorica Planetarum 'Gerardi', and Campanus of Novara's text-books, and in later renaissance texts, eg. Peurbach's, and in the Epytoma of Regiomontanus.[16]

Copernicus' discovery was an insight into the real geometrical meaning of these relations as signs of the Earth's revolution around the Sun. He was so convinced that arguments relying upon mathematics bring knowledge about the real world that he dared to follow the original geometrical insight to its disturbing physical conclusions. Not all historians accept this simple interpretation of Copernicus' achievement. We have already met — and answered — Kuhn' counterargument that Copernicus in De Revolutionibus was not very explicit; especially, he did not stress the relation his own circles have to the great Ptolemaic epicycles. Maybe he did not care to reveal how exclusively his work meant a reinterpretation of what was given already in the Almagest. Copernicus' knowledgeable followers located quite explicitly and unequivocally his innovation exactly at this point; so do the heliocentrists Kepler and Stevin, and the geocentrist Tycho Brahe.[17]

Hence the decisive role of Ptolemy for Copernicus was not in that the latter rose to opposition against the 'monstrosity' of the former, but that the former offered the details of planetary motions having their natural and logical interpretation in terms of heliocentrism. Copernicus is a true follower of Ptolemy, not a rebel against him. From this point of view we also understand 'the Revolution's timing and the factors that called it forth'. The decisive factor was the clarification and elucidation of the Ptolemaic system, brought about by such renaissance astronomers as Regiomontanus. The timing was determined by the advent of printing which made possible the spread of detailed astronomical knowledge.[18] After the true understanding of the secrets hidden in the Almagest became known widely enough, their solution was bound to became apparent to somebody. That somebody was Nicolaus Copernicus.

Hence the central key of Copernicus' innovation was the discovery, made from existing astronomical literature, that one decisive feature of planetary motions is radically different from what was hitherto supposed. The prominence given to this discovery agrees with the traditional view of the Copernican Revolution. Scarcely anybody would, about 70 years ago, have searched for the significance of Copernicus' work in any other direction than in his discovery that the Earth moves.

Commenting on 'marvelous achievements' in science, Campbell (1987: 103) writes: "However, for the genuinely unanticipatable creative act, our 'ave' and 'wonder' should be directed outward, at the external world, rather than directed toward the antecedents of the discovery". This is against the present trend in the philosophy and sociology of science, which is to search for explanations of the progress of science rather from social and psychological factors than from features of the outer world the scientists are studying. Certainly consciousness of the effect of social factors has enormously deepened our understanding of science as a process, but sometimes they have been overemphasized, and the 'Copernican Revolution' is a case in point. When the fear of 'Whig interpretation' of history, and adoration of irrational paradigms, goes so far as to group Copernicus, Paracelsus, and Robert Fludd to a company characterized by 'Magical tradition', and places them against an 'Organic tradition', represented by Aristotle and, mirabile dictu, Ptolemy, then the development of science has rather been confused than elucidated. (See Kearney 1971: 17—22, 37—37, 96—140). The monsterological interpretation of the Copernican Revolution is widely used to back up such confusions. Therefore, with all respect to Thomas Kuhn as an inspiring analyzer of factors of scientific progress, it seems to be necessary to shoot down the monster.

NOTES

1. Dreyer (1953): 268—272. Copernicus himself both in De Revolutionibus and in the earlier Commentariolus repeatedly critisizes the Ptolemaic equant; see Copernicus (1976): 25, 188, 237—238, and Rosen (1959): 57, 71, 39. The elimination of the equant is often given as a central motivation of Copernicus' innovation; see Cohen (1985): 113—115, 123, Koestler (1964): 205—206, Kuhn (1957): 71, Lakatos-Zahar (1975): 371—373, Zinner (1988): 178, 205. This innovation, however, has as such nothing to do with heliocentrism. The substitution of several uniform rotations to replace the equant was due to the geocentric Maragha astronomers, whose work Copernicus must have known. See Swerdlow-Neugebauer (1984): 46—48, and Lehti (1989): 17—18, 23, 214—215.
2. Dreyer (1953): 275—280, Copernicus (1976): 140—185, Swerdlow-Neugebauer (1984): 127—179.
3. A somewhat more detailed account is given in Lehti (1989): 218—219, in the original Finnish version of this article. For Peurbach and Regiomontanus and their role in spreading astronomical knowledge see Zinner (1988): 90—136.
4. Here we cannot attempt at such an exposition of the astronomical literature of the sixteenth century that this assertion could be properly documented. The readers mastering the Finnish or Swedish language may consult Lehti (1984) and (1989). Some references are given later, e.g. in note 16.
5. Such authors are Burtt (1972), French (1987), Kearney (1971), Yates (1964). For further references see Lehti (1989): 256.
6. See Cumont (1960): 68—76, Lehti (1989): 118—122, Zinner (1988): 21—24, 49—52. For Commentarii in Somnium Scipionis I 20, 1—5, see Macrobius (1970): 78—79.
7. See French (1987): 25, 28—32, 103—108 etc., Clulee (1988): 42—52, 116—121 etc. For a criticism of Dee's mathematics and alleged Copernicanism see Lehti (1989): 149—159. The senselessnes of Dee's mathematics as presented in the Propaedeumata Aphoristica can immediately be seen by looking at that work, available in Shumaker-Heilbron (1978).
8. For Bruno, Fludd, and Paracelsus, see Lehti (1989): 144—149, 159—163, 223—226. Examples of Bruno's incomprehensible or fallacious mathematics can be found for instance in Bruno (1988): 335—337, 384; see also Yates (1964): 241—296.
9. Burtt (1972): 42—44, Kearney (1971): 98, Kuhn (1957): 129. For a more extended analysis of these views see Lehti (1989): 228—229.
10. Kuhn refers to Hall (1956): 16; see Kuhn (1968): 67, Kuhn (1977): 206. The presentation in this article is founded on the analysis in Lehti (1989): 62—65, 94—96, 230—232.
11. Kuhn (1957): 138—139; for the translation used see pp. 280—281. We use, in the following, translations from de Revolutionibus as given by Kuhn; for others see for instance Copernicus (1976), where this text is given on p. 25.
12. Kuhn (1957): 139—141, Kuhn (1968): 83—85. A reader of Copernicus' text recognizes that with 'monster'. Copernicus refers neither to the inaccuracy nor to the general clumsiness of Ptolemy's system, but to an assumed incongruity between the models the system uses for different planets.
13. For Copernicus' physics see Kuhn (1957): 144—155, Copernicus (1976): 36—46. The few sentences generally used to prove that Copernicus was a Hermetist occur in De Revolutionibus I 10 (Copernicus 1976: 50). They mention Trismegistus, and describe the "Sun as if seated on a royal throne", governing the household of stars. For the eagerness with which these words are used, see Burtt (1972): 45, French (1987): 102, Hall (1956): 67, Kearney (1971): 99—100, Kuhn (1957): 131, Shumaker-Heilbron (1978): 44, Yates (1964): 154. For further references see Lehti (1989): 187—191. For an early critic of this surprising and incongruous burst of bad poetry see Stevin (1961): 138, 139.
14. Kuhn (1957): 137—139, 141—143, Copernicus (1976): 23—27. For an analysis of the preface see Lehti (1989): 69—84. Koestler's antipathy against Copernicus has for once led him to an exactly correct estimation that the preface is an "extremely shrewd and calculated document" (Koestler 1964: 177).

15. For details see Lehti (1989) as follows: pp. 17—18, 23, 214—215 for the Maragha astronomers; pp. 103—110 for similarities in the physical argumentation of Oresme and Copernicus; pp. 123—129 for an exposition of the occurrence of the 'solar relations' of the Almagest in European astronomical literature before Copernicus, and pp. 50, 90—91 for the existence of this literature in Copernicus', library. The author has made all efforts to render the present article self-contained in the sense that acquaintance with the book-size Finnish version Lehti (1989) is not necessary. The references to that book are given only to benefit readers capable of reading Finnish language.
16. The solar relations of the Almagest are explicitely mentioned in the earliest text-book on planetary theory written in Latin Europe. This text, Theorica Planetarum 'Gerardi' has erroneously been attributed to the translator Gerhard of Cremona. The references to the solar relations are found in the English translation in Grant (1974) on pp. 452, 456, 458, 461. A very thorough treatment is given in the Theorica Planetarum of Campanus of Novara, written about 1260. After explaining the relation in the case of outer planets (Benjamin-Toomer (1971): 302—305). Campanus comments upon it as follows (pp. 306—307):
"It is clear, then, from what has been said about the individual planets, that all of them are connected with the sun in some way; and the sun seems to be, as it were, a common mirror in which all look and from which all borrow some patterns for their own motion: the moon moves the center of its epicycle toward the east and the apogee of its deferent toward the west in such a way that each on its own side is always equidistant from the sun; Venus and Mercury always move the centers of their epicycles to keep pace with the sun and thus accompany it continuously on one side or the other, ..."
Campanus gives a similar story in his Tractatus de sphera. Benjamin- Toomer (1971): comment: "(Campanus) singles out a feature of the Ptolemaic system which is inexplicable in its own (geocentric) terms, but which immediately makes sense when one substitutes a heliocentric model." This treatise of Campanus was included in an omnibus edition of Sacrobosco's Sphera and related texts, printed in Venedig in 1518 (see Lehti (1984): 27—28); a book which Copernicus had in his library (No 9 in Zinner (1988): 406).
The same collection includes also the Theoricæ Novæ Planetarum of Georg Peurbach. In that standard work on Ptolemaic planetary theory Peurbach mentions, as every writer on planetary theory seems to have done, all the well-known relations the planets have to the Sun in their motions (pp. 760, 767, 770, 772 in the facsimile of the 1472 -edition reproduced in Regiomontanus (1972): 755—793). Regiomontanus did the same in his Epytoma, the work from which Copernicus got most of his knowledge of the Ptolemaic theory (Regiomontanus (1972): 129—131, 192—193, 210, 215—216, 243—244).
A more extended study of the appearance of these relations in the pre-Copernican literature is given in Lehti (1989): 115—118, 123—129. See also Lehti (1984): 40, 45—46, 76, 83, 86.
17. See Kepler (1981): 78—81, Stevin (1961): 116—211. For an analysis of these authors and Tycho Brahe, with references, see Lehti (1989): 164—186, 267—277.
In Lakatos-Zahar (1975): 375—380, the role of these relations is correctly appreciated. It seems, however, far-fetched to interpret these relations as confirmation of "Zahar's New Version of the Methodology of Scientific Research Programs", as both the relations and their importance were well-known to all competent astronomers of Copernicus' time. See the criticism in Krige (1980): 99—100.
Some historians have doubted whether the 'natural' explanation of the solar relations really was a "major feature of Copernicus' system". They justify this doubt by pointing out that also "in Copernicus' system there are features of the motions of these same planets that are related to the earth, even though for Copernicus the earth is a planet just like them" (Cohen 1985: 120). These features, however, are in the case of the planets small 'perturbation terms' (Copernicus 1976: 190, 269, 273). Their existence may worry modern historians, but in the sixteenth century they neither delighted the opponents nor worried the adherents of heliocentrism. See for instance Stevin (1961): 308—309, 313—314, Lehti (1989): 185.
18. For the role of the printing press in the Scientific Revolution see Eisenstein (1983): 185—252. The spread of the astronomical literature laying ground for Copernicus' innovation is illustrated in the catalogue Lehti (1984).

LITERATURE

Benjamin, Francis S. and Toomer, G. J.
1971 Campanus of Novara and Medieval Planetary Theory. Madison etc.: University of Wisconsin Press. (Contains a Latin-English edition of the Theorica Planetarum by Campanus of Novara.)

Bruno, Giordano
1888 Le opere italiane di Giordano Bruno, ristampate da Paolo de Lagarde, Volume primo. Göttingen: Dieterische Universitätsbuchhandlung. (Contains e.g. La cena de le ceneri, 1584, and De l'infinito universo et mondi, 1588.)

Burtt, Edwin Arthur
1972 The Metaphysical Foundations of Modern Physical Science. London: Routledge and Kegan Paul. (First ed. 1924, revised ed. 1932.)

Campbell, Donald T
1987 Blind Variation and Selective Retention in Creative Thought as in Other Knowledge Processes. Pp. 91—114 in Gerard Radnitzky and W. W. Bartley III (ed.): Evolutionary Epistemology, Rationality, and the Sociology of Knowledge. La Salle, Illinois: Open Court. (Originally in The Psychological Review 67, 1960.)

Clavius, Christopher
1612a Christophori Clavii Bambergensis e Societate Jesu Operum Mathematicorum Tomus Qvintus. Moguntiæ: Sumptibus Antonii Hierat.
1612b Novi Calendari Romani Apologia. Moguntiæ: Antonio Hierat.

Clulee, Nicholas H.
1988 John Dee's Natural Philosophy between Science and Religion. London and New York: Routledge.

Cohen, I Bernard
1985 Revolution in Science. Cambridge Mass. and London: The Belknap Press of Harvard University Press.

Copernicus, Nicolaus
1976 On the Revolutions of the Heavenly Spheres: Newton Abbot etc: David & Charles. (English translation by A. M. Duncan of De Revolutionibus Orbium Caelestium, 1543.)

Cumont, Franz
1960 Astrology and Religion among the Greeks and Romans. New York: Dover Publications. (English translation originally 1912.)

Drake, Stillman
1957 Discoveries and Opinions of Galileo. Garden City, N. Y.: Doubleday & Co. (Contains e.g. Galileo's Letter to Madame Christina of Lorraine, Grand Duchess of Tuscany, 1615.)

Dreyer, J. L. E.
1953 A History of Astronomy from Thales to Kepler. New York: Dover Publications. (Original title: History of Planetary Systems from Thales to Kepler, 1905.)

Duhem, Pierre
1973 Le Système du Monde, Tome IV. Paris: Hermann.

Eisenstein, Elizabeth L.
1983 The Printing Revolution in Early Modern Europe. Cambridge: Cambridge University Press.

French, Peter
1987 John Dee, The World of an Elizabethan Magus. London and New York: ARK Paperbacks. (First ed. 1972.)

Galilei, Galileo
1967 Dialogue Concerning the Two Chief World Systems — Ptolemaic and Copernican. Berkeley etc.: University of California Press. (English translation by Stillman Drake of the Dialogo sopra i due Massimi Sistemi del Mondo, Tolemaico, e Copernicano, 1632.)

Grant, Edward
1974 A Source Book in Medieval Science. Cambridge Mass.: Harvard University Press. (Contains e.g. English translations of De Sphera by Johannes de Sacrobosco, the anonymous Theorica Planetarum 'Gerardi', and excerpts of Tabulæ Alphonsinæ.)

Hall, A. R.
1956 The Scientific Revolution 1500—1800. Boston: The Beacon Press. (First ed. 1954.)

Heidelberger, Michael
1980 Some Intertheoretic Relations between Ptolemean and Copernican Astronomy. Pp. 271—283 in Gary Gutting (ed.): Paradigms and Revolutions. Notre Dame and London: Notre Dame University Press.

Kearney, Hugh
1971 Science and Change 1500—1700. London: Weidenfeld & Nicolson.

Kepler, Johannes
1981 The Secret of the Universe. New York: Abaris Books. (Latin-English edition by A. M. Duncan of the Mysterium Cosmographicum, first ed. 1596, second ed. 1621.)

Koestler, Arthur
1964 The Sleepwalkers. Harmondsworth: Penguin Books. (First ed. 1959.)

Krige, John
1980 Science, Revolution, and Discontinuity. Sussex: The Harvester Press; New Jersey: The Humanities Press.

Kuhn, Thomas S.
1957 The Copernican Revolution. Cambridge Mass. and London: Harvard University Press.
1968 The Structure of Scientific Revolutions. Chicago and London: Chicago University Press. (First ed. 1962.)
1977 The Essential Tension. Chicago and London: Chicago University Press.

Lakatos, Imre, and Zahar, Elie
1975 Why did Copernicus' Research Program supersede Ptolemy's? Pp. 354—383 in Robert S. Westman (ed.): The Copernican Achievement. Berkeley etc.: University of California Press.

Lehti, Raimo
1984 Pallojen maailma, Tähtitiedettä ennen Kopernikusta — En värld av sfärer, Astronomi före Copernicus (A World of Spheres, Astronomy before Copernicus). Helsingin Yliopiston kirjasto (Helsinki University Library), Helsinki.
1989 Tanssi Auringon ympäri (Dancing around the Sun). Oulu: Pohjoinen. (Published in fact 1990.)

Macrobius
1970 Ambrosii Theodosii Macrobii Commentarii in Somnium Scipionis, Edidit Jacobus Willis. Leipzig: BSB B. G. Teubner Verlagsgesellschaft.

Pliny
1979 Natural History I. Cambridge Mass. and London: Harvard University Press and William Heinemann Ltd. (Latin-English edition by H. Rackham of Plinius' Historia Naturalis, Praefatio, Libri I, II. Loeb Classical Library, first ed. 1938.)

Ptolemaios, Klaudios
1984 Ptolemy's Almagest, Translated and Annotated by G. J. Toomer. London: Duckworth.

Regiomontanus (Johannes Müller)
1972 Joannis Regiomontani Opera Collectanea (herausgegeben von Felix Schmeidler). Osnabrück: O. Zeller Verlag. (Contains facsimiles of e.g. Epytoma Almagestum Ptolomei, 1496, Ephemerides anni 1475, 1474, and the Theoricæ Novæ Planetarum, 1472, by Georg Peurbach.)

Rosen, Edward
1958 Galileo's Misstatements about Copernicus. ISIS 49.3 pp. 319—330.
1959 Three Copernican Treatises. New York: Dover Publications. (Contains e.g. English translations of the Commentariolus, about 1512?, by Copernicus, and the Narratio Prima, 1540, by Joachim Rheticus. First edition 1939.)

Sacrobosco, Johannes de
1568 Libellus de Sphæra, Accessit eiusdem Autoris Computus Ecclesiasticus. Vitebergae, Excudebat Johannes Crato. (Written about 1250, several printed editions since 1472.)

Shumaker, Wayne, and Heilbron, J. L.
1978 John Dee on Astronomy. Berkeley etc.: University of California Press. (Contains a Latin-English edition of the Propaedeumata Aphoristica, 1568, by John Dee.)

Stevin, Simon
1961 The Principal Works III (ed. by A. Pannekoek and Ernst Crone). Amsterdam: N. V. Swets & Zeitlinger. (Contains e.g. a partial Dutch-English edition of Van den Hemelloop, 1605.)

Swerdlow, N. M. and Neugebauer, O.
1984 Mathematical Astronomy in Copernicus's De Revolutionibus. New York etc.: Springer-Verlag.

Yates, Frances A.
1964 Giordano Bruno and the Hermetic Tradition. Chicago: The University of Chicago Press.

Zinner, Ernst
1988 Entstehung und Ausbreitung der copernicanischen Lehre. (Herausgegeben und ergänzt von Heribert M. Nobis und Felix Schmeidler.) München: C. H. Beck. (Originally 1943.)

Raimo Lehti
Helsinki University of Technology
Institute of Mathematics
SF-02150 Espoo 15
Finland

ESSAYS AND REVIEWS

A defence of Kuhn

Verronen, Veli: *The Growth of Knowledge*. An inquiry into the Kuhnian theory. Publications of the Department of Philosophy, University of Jyväskylä, No. 35, Jyväskylä 1986 (273 p.).

It is largely due to Kuhn that since the 1960s the *history* of science has become one of the focuses of attention in the *philosophy* of science, while at the same time the question of the nature of scientific *rationality* has assumed unprecedented urgency. The discussion continues unabated. In Finland, representatives of the social sciences and the humanities have reacted energetically to Kuhn's conceptions. Scientists in their respective areas would seem on the whole to have no difficulty in recognizing themselves and their particular pursuits in the picture Kuhn presents. Finnish academic philosophy, in contrast, has for the most part looked askance at him. At least for this reason — that is, for the sake of restoring the balance in the debate — it is to be welcomed that Veli Verronen has produced a work in which he seeks on the one hand to refute the most prevalent misconceptions of Kuhn and on the other to interpret and elucidate theses which Kuhn himself on occasion left imprecise.

The structure of Verronen's work is the following. Chapter I (pp. 1—52) outlines Kuhn's historical background, the "traditional" view against which he in his own time reacted. In this view, change/progress in science comprises a steady growth of knowledge, *accumulation*. Verronen distinguishes two separate forms of the accumulation theory. First, in the "positivist" or "justificationist" approach (represented among others by Nagel, Kemeny and Oppenheim) an earlier theory T_1 can always be *reduced* to a later theory T_2 — in other words T_1 is a special case of T_2. Thus, in as far as T_1 contains something that is lacking in T_2, the differences are only ostensible; the parts of T_1 which cannot be reduced to T_2 were not in the first place science at all, they were simply erroneous; hence the development of science does not entail any *genuine losses*. Secondly, the "falsificationist" view of Popper and his successors holds that T1 cannot be reduced to T_2 but that T_2 disproves T_1. Popper himself conceived his falsificationism as differing sharply from the accumulative theory. However, (as Verronen points out pp. 38—40,), falsificationism in fact carries with it the core of that theory: for the passage from T_1 to T_2 to be *progressive* it must be possible to *compare* the two; this comparison must be grounded in a language *common* to the two (termed in Verronen p. 70 SUPER); as a result of the comparison it must be possible to state that the empirical content of T_2 is *greater* than that of T_1 (in other words that T_2 is closer to the truth than its predecessor), and — above all — that T_2 *contains* the empirical content of T_1. What is involved is thus a steady incrementation of empirical content, so that falsificationism is after all a special case of the accumulation theory. Kuhn's (and Verronen's) criticism of this theory is, briefly, that if it were valid a great part of the history of science so far would be incomprehensible.

Chapter II (pp. 53—86) gives a kind of "standard version" of Kuhn's theory. As is well known, Kuhn's view is that science develops in passing from a paradigm P_1 (and the normal science associated with it) by means of a revolution to a paradigm P_2. The essential point now is that according to Kuhn there is *no* language above P_1 and P_2, no SUPER or set of terms within which the two paradigms could be objectively compared. P_1 and P_2 are thus *incommensurable*. On the other hand P_1 and P_2 differ from each other in that they give rise to different predictions; in other words, if S is from the standpoint of P_1 an inexplicable anomaly which P_2 can explain, then P_1 yields "non-S" and P_2 "S". Thus P_1 and P_2 are *incompatible*. But here Kuhn would appear to end up at odds with himself (as for example Scheffler and Watkins have pointed out); prerequisite to the incompatibility of the two paradigms (or the formulation and comparison of "non-S" and "S") is a common language, whose existence the thesis of the incommensurability of P_1 and P_2 specifically denies. One of the tasks Verronen sets himself is to resolve this *prima facie* contradiction. The final section of Chapter II contains twenty subsections in which the standard version of Kuhn's theory is illuminated from practically every