

SUMMARY

This summary attempts to provide a guide to the book for the English-speaking reader.

At the beginning, let us note three points.

Firstly, the submitted book *Thomas S. Kuhn (1922-1996) and the Issue of the Copernican Revolution* is an extension of my PhD Thesis (11 December 1997, Instytut Historii Nauki Polskiej Akademii Nauk, Warszawa).

Secondly, this book is, to a large extent, the first of its kind in literature.

Thirdly, in the opinion of the present author, because the details are the substance of the book, its abstract should be treated as only a broad overview of the actual essence of the book.

INTRODUCTION (p. 1-14)

CRUCIAL PROBLEM (p. 1-3)

One of the fundamental paradoxes of the study of the humanistic and social sciences (such as the philosophy and history of science, the sociology of scientific knowledge, the psychology of scientific discovery) over the last four decades lies in the very limited acquaintance with factual substance, and all the more, the genesis of the famous theses proclaimed by Thomas S. Kuhn (1922-1996) [his curriculum vitae is given in Appendix 1] about the mechanism of the progress of science.

AIM AND OUTLINE (p. 3-4)

The aim of the book is to bridge this essential gap of contemporary humanistic and social sciences. However, as the present author's preliminary research has already shown, it is Kuhn's interpretations of the Copernican revolution that have constituted the primary source of his ideas about the mechanism of the progress of science. Thus, the need has arisen to carefully research the genesis and substance of these constructions, to become acquainted with existing critical evaluations of Kuhn's interpretations by earlier scholars, and, if possible, to formulate the present author's own critique of this issue.

METHODOLOGICAL INTRODUCTION (p. 5-9)

For easier reading of the book, the *Methodological Introduction* supplies an explanation of the specific methodological tools applied in the book, i.e. the concept of *the fundamental strategy of the historian*, the idea of *the hermeneutics used by the researcher*, the concept of *the structure of the scientific text*, and *the technique of extended citations*; in this methodological context the term “science” is used in its broadest meaning, referring to all natural sciences and social sciences, including history (in English, this meaning is comparatively young, its origins date back a mere fifty years or so, but compared to German (“Wissenschaft”) and Polish (“nauka”), by contrast, it is an old, even classical concept in its origins).

The fundamental strategy of the historian, summarising the essential frames of the historical method, is (potentially) rather simple.

In order to describe a researched episode (event or process) of History, an historian must become acquainted with the appropriate information contained in historical sources and put them with skill into a suitably constructed text – a certain story (narration) about this episode.

When the researched subject is, for instance, the views of a concrete historical person, it is an historian’s duty to read and enter into the spirit of all the possible writings of this person, both published and unpublished (his correspondence, notes, diaries, and the like). Furthermore, the historian must also know the readings of the researched person, in order to estimate the degree of self-dependence and originality of the views proclaimed by this person.

The term “the hermeneutics used by the researcher” means all the interpretative tools used by the researcher at the stage of his repeated attempts to comprehend the subject under study.

Let us give a simple illustrative example. In order to make out of a scientific article on the general theory of relativity, the researcher must, of course, know this theory. However, this is insufficient. He is also obliged to be acquainted with a great part of theoretical physics, with some branches of mathematics, with some elements of experimental physics and of the philosophy of physics.

If it is possible, the researcher should constantly seek to improve his hermeneutics since the more precise it is, the higher the degree of comprehension by him of the subject under study.

When attempting to describe the views of a thinker, one comes up against a fundamental problem. It is impossible to summarise these views “in one’s own words” without loss of important original information. This is often the case when the considered thinker uses very complicated and highly specialised language or considers very detailed issues. This type of linguistic non-translatibility is one source of fundamental misunderstandings in the compre-

hension of the views of the thinker. To avoid this problem, the present author proposes to use *the technique of extended citations*, that is, to quote widely the thinker's works. This technique is particularly useful for recalling a completely forgotten or wrongly interpreted thought.

In a scientific text (the written "text" is one of the fruits of research), the present author distinguishes three elements: *the form of the text* (the literary type of the text), *the hermeneutics of the text* (all means applied explicitly or implicitly in the text to interpret the subject under study), and *the rhetoric of the text* (all means serving to convince the reader of the expounded theses).

These methodological tools have been used openly in the book in analysing the genesis of Kuhn's views and the substance of his works, and covertly with regard other scholars.

Part One

THE GENESIS AND SUBSTANCE OF THE COPERNICAN REVOLUTION [1957] AND THE STRUCTURE OF SCIENTIFIC REVOLUTIONS [1962]

Part One of the book (p.15-109) deals with the genesis and essence of two of Kuhn's works: *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (1957, 7th, renewed, ed. 1985), and *The Structure of Scientific Revolutions* (1962, 2nd ed. 1970); in the case of the latter book, the only two problems considered are the creation of the *hermeneutics* and the development of the interpretation of the Copernican element.

Acquaintance with the content of Kuhn's autobiographical opinions (given, for instance, in the *CR* [1957a], *SSR* [1962], *ET* [1977], *TSK* [1984], *TSK* [1989], *TSK* [1995]; for the explanation of these and similar abbreviations see *The Index of abbreviations of quoted works* page 5), and with research into his numerous readings (see, for example, the bibliography in the *CR* [1957a] and *SSR* [1962]), reveals the dependence of his various views (on practising the history and philosophy of the exact sciences, and on interpreting the Copernican revolution) on many other scholars' thought.

First of all, however, Thomas S. Kuhn was born as an historian and philosopher of science at Harvard University under James Bryan Conant (1893-1978).

This very important person can be presented as follows: a chemist, the President of Harvard University (1933-1953), during the Second World War an organiser of national scientific research and member of the National Defence Research Committee and the White House Office for Scientific Research; after the Second World War, one of the most important propagators of reform of the

Program of General Education in the USA, and at Harvard University especially; and an historian and philosopher of science.

The two last mentioned points are the focus of our interest. In an attempt to revive knowledge of Conant's now wrongly forgotten thought, a summary has been written of the most significant points of his four most important books on the history and philosophy of science (see Conant [1947], [1951b], [1952], and Conant, Roller (eds.) [1957]; these and similar abbreviations are explained in *The Bibliography*), and of his part in the reform of the program of general education after the Second World War. Within this context, a description of the idea behind the reform of the General Education Program (see The Report of the Harvard Committee on *General Education in a Free Society* [1945]), and its realisation at Harvard University, has been included, in particular concerning the General Education Program in Science (see Cohen, Watson [1952]).

The aim of Conant's group was to deliver lectures on natural sciences and their relations with culture for non-science students at Harvard University. This group consisted of, among others, I. Bernard Cohen, Philippe Le Corbeiller, Philipp Frank, Gerald J. Holton, Edwin C. Kemble, Frederick G. Kilgour, Leonard K. Nash, and T.S. Kuhn himself.

It is common knowledge that Kuhn unanimously criticised the thought of George Sarton (see, for instance, TSK [1984]) but analysis of the works of Conant and all of his group, including those of Kuhn himself, has brought the present author to a completely new conclusion. At the insistence of his teacher and the members of Conant's group, the young Kuhn was in part a continuator of the philosophical core of Sarton's thought that was the basis for the *new humanism*, now forgotten but consistently proclaimed by Sarton from (at least) 1918 to 1956. Firstly, the substance of the *new humanism*, most comprehensively described in Sarton [1931], second enlarged ed. [1937], third ed. [1956] is outlined (see Appendix 2).

Secondly, the reception of the *new humanism*, including its influence on the reform of education in the USA and in particular at Harvard University (see Appendix 2), is revealed. One of the fruits of the *new humanism* has been historically oriented lectures on science at the level of general knowledge for non-science students (i.e. for students not studying natural sciences). This is one of the most basic sources of Conant's reform of the education program of general knowledge in science.

Thirdly, the following argument is expounded. Ideas formulated on the basis of English language: the dichotomy of the "two cultures" ("science" - "the humanities and social studies"), and the "third culture" (that surmounts this dichotomy) were the cornerstones even of Sarton's first works on the *new humanism*. The same terms "two cultures" and "the third culture" were introduced

many years later by Charles Percy Snow (see Snow [1956], [1959]), but without mention of the existence of Sarton's works, already known at that time. Thus the present author's postulation within the context in the discussion is to use "the two Sarton-Snow cultures" and "the third Sarton-Snow culture" instead of the primary terms "the two cultures of Snow" and "the third culture of Snow".

Fourthly, neither Conant nor his group accepted Sarton's idealism, integralism and historical and linguistic perfectionism, but they did accept the core of Sarton's *new humanism* – it was Conant's pragmatic version of the *new humanism*.

Fifthly, it is under the influence of Conant himself and his group that the more pragmatic, moderate approach to the *new humanism* determined the course of Kuhn's interdisciplinary studies in the 1948-1951 period at Harvard and found clear reflection in his later works, including the *CR* and *SSR*.

The aspects of the *new humanism* that the young Kuhn most appreciated were, firstly, the idea of humanising lectures on science (natural sciences) by basing them on the history of science, which acts as a bridge between science and the humanities, and secondly, the demand imposed on every investigator of the history of science to be both a historian and a scientist.

[However, as is known, many years later, Kuhn very severely criticised Sarton's thought, not only rejecting the positivistic style of his chief and most famous works (for instance Sarton [1927-1947]) but also emphasising the unfeasibility of the same idea of the *new humanism* - see TSK [1984]. Regarding the latter point, the present author argues that Kuhn was simply wrong (see *Part one* chap. 1.5, especially fn. 64).

Under the influence of Conant's group, Kuhn appreciated, firstly, the program of the history of ideas by Arthur Lovejoy (the works of Lovejoy [1936] (Introduction), and [1938] on this subject are summarised in Appendix 3), and secondly, the idea of studying the history of different sciences in their own historical conceptual categories, displaying their gradual development. In the latter tasks, Alexandre Koyré [1936] was Kuhn's mentor.

A preliminary examination of Kuhn's *CR* [1957a] might lead one to suppose that the choice of the theme "the Copernican revolution" was one of the fruits of Kuhn's attendance in Conant's group, and this would indeed be an accurate deduction. The basic research of the present author reveals that it was Conant, Frank, Kemble, Holton and Cohen who considered the various aspects of this subject (see, for instance, Conant [1947], [1951], [1952], Frank [1941], [1944], [1946], [1947] (all these works were reprinted in: Frank [1949]), [1952], Kemble [1952], Cohen [1952]). With the help of further, comparative analyses of Kuhn's readings and his own works, it appeared that Kuhn tried to avoid the contradictions existing within the contemporary interpretations of

Copernicus's achievements, i.e. on the one hand the critical view of Dreyer [1906] (repr. [1953]) and Butterfield [1949] that resolves itself finally into the thesis that there was no Copernican revolution at all, and on the other hand the judgement of Koyré [1943b], Struve [1943] and Wightman [1950], confirming the existence of this revolution.

A description is also given here of Kuhn's way of solving these problems by choosing the literary form of a planned work - the historical synthesis that was fashioned to some degree on the book by Butterfield [1949] and was at the same time Kuhn's answer to Lovejoy's program of the history of ideas and to the comprehension of the history and philosophy of science by the members of Conant's group.

Next, Kuhn's readings connected with the Copernican theme are briefly considered, and the *hermeneutics* used by him at this phase of his research are reconstructed.

Finally, analysis of the three elements of the text: *form*, *hermeneutics* and *rhetoric*, has been used to discuss the text structure of the *CR* and *SSR* (in the case of the latter book, the *hermeneutics* and Copernican theme were under discussion).

Part Two

CRITICISM OF KUHN'S INTERPRETATIONS OF THE COPERNICAN REVOLUTION

Chapter I (p. 113-132) and Appendix 4 (p. 238-313) deals with the following problem. To competently discourse on the value of Kuhn's interpretations of the Copernican revolution, it is necessary, first of all, to be well-acquainted with hitherto existing critical assessments. However, in the opinion of the present author, too many scholars have all too little knowledge of this matter. Therefore, to give all readers easy access to the problems connected with the critical assessments of Kuhn's interpretations, selected aspects of almost fifty, the most typical and important works published over the last four decades, are summarised and listed in chronological order - this is the broadest elaboration of this theme in existing literature.

Chapter II (p. 133-143) presents Kuhn's own attempts to answer these critiques. Though strictly speaking, regarding the Copernican subject, this elaboration is short, it is the only one of its kind in literature.

Chapter III (p. 145-158) describes the recent reception of Kuhn's thought by such thinkers as, for example, Buchwald, Smith [1997], Chen, Andersen, Barker [1998], Goddu [1999], [2001], Gottfried, Wilson [1997], Hoyningen-Huene [1997], Heilbron [1998], Kokowski [1996a], [1997], [1998], [2000], Rorty [1997a], [1997b], Sokal, Bricmont [1998], Swerdlow [1997],

[2001], Weinberg [1996], [1998], and Wilson, Barsky [2001a], [2000b], [2000c].

Chapter IV (p. 159-218) shows the present author's own critique of Kuhn's interpretations of the Copernican revolution. It develops a kind of penetrating argumentation of some of the early reviewers of Kuhn's first book - the *CR* [1957a] - who were, in principle, negatively disposed towards it, i.e. Helmann [1957], Dingle [1958], Rosen [1959], Poulle [1960], and Zimansky [1959], and the later followers of this critical attitude such as, for instance, Swerdlow and Neugebauer [1984]. The present author opposes the opinion expressed not only by a large number of early reviewers of the *CR*, including Newman [1957], Swenson [1957], Butterfield [1958], Hoskin [1958], Woolf [1958] and Armitage [1959], but also Westman [1994] - that the *CR* was "tightly written and brilliantly argued" (Westman [1994] p.79).

On the contrary, Kuhn's interpretations of the Copernican revolution, given in the *CR* and similarly in the *SSR*, not only possess some material and formal (interpretative) defects already mentioned by earlier scholars, but are also burdened by many other structural and material faults: too often Kuhn committed errors that the present author determines by the terms "a narrative sophism" and "an incoherentness of narrative returns".

"A narrative sophism" means the intentional, though unexpressed explicitly, way of building a narration so as to persuade the reader at any cost of the theses expounded and doing so without prior sound confirmation of their legitimacy at the level of historical facts and/or of interpretation.

Kuhn uses this rhetorical strategy when he is tackling, among other things, the following issues:

(A) Suggesting strongly that one can adequately understand all the complications of the Copernican revolution without suitable independent detailed research and basing one's study solely on studies by other scholars (p. 160-161);

(B) Suggesting that one can adequately understand works such as the *Almagest* and the *De revolutionibus* without suitable acquaintance with the mathematical language adopted therein and of the empirical problems considered (p. 161-164).

(C) Persuading the reader earnestly of the dichotomous vision of cultivating the sciences known as the exact sciences by contrasting pragmatic-empirical values, those belonging to the core of these sciences, with aesthetic values, those that, at best, are on the peripheries of these sciences (p. 164-166).

(D) In the light of the category of "utility", making the inaccurate, though suggestive, comparison between Copernicus's and Ptolemy's astronomical theories (p. 166-168).

(E) Making too critical an assessment of Copernicus's theory of motion (p. 168-170).

(F) Strongly persuading the reader that the form and time of the Copernican revolution were determined by factors external to astronomy (p. 170-171), including (G) humanism (p. 171-174) and (H) neoplatonism (p. 174-178) - in this point Kuhn claims, among other things, that it was the neoplatonic intuition of harmony that was the deciding factor in the choice of Copernicus's theory by astronomers.

(I) Earnestly attempting to persuade the reader of the soundness of his idea of the mechanism of evolution and revolution in science (p. 178-182).

As an illustration, let us briefly consider case (F): „Were the form and time of the Copernican revolution determined by external factors to astronomy?”

One of the crucial theses of the *CR* [1957a], adopted by Kuhn from Burt [1932] and Butterfield [1949], was a positive answer to the above question. However, Kuhn's argumentation is defective, since his fundamental premise that Copernicus had not possessed any experimental and/or theoretical basis for rejecting Ptolemy's theory is wrong. In fact, one of the main theoretical-empirical issues of Renaissance astronomy was the problem of a theoretical (geometrical) grasping of astronomical observations recorded by competent astronomers over nearly 2000 years from the times of Timocharis, Hipparchus and Ptolemy to the Renaissance. Therefore, the long-period models of certain astronomical phenomena were intensely considered by Renaissance astronomers. The need for reform of the Julian calendar was one of the aspects of this complicated issue.

Kuhn repeatedly makes a mistake that the present author refers to as the error of “an incoherentness of narrative returns”. The structure of this error is as follows. On page p_1 , Kuhn formulates a thesis t_1 about a point pt_1 . Then, on the same page or a number of pages further on, when returning in a new context to the point already raised, pt_1 , he expresses a thesis t_2 that is different to thesis t_1 . The latter either weakens the former or negates it in part or in whole. And he propounds them all with the same rhetoric spiritedness; as a sophist he argues with equal force for the benefit of different and often antagonistic theses, but always invariably wanting to persuade his readers of them.

He commits such errors in the following cases:

(A) Arguing widely for the rational research program on “the Copernican theme” (that is of the genesis, substance and reception of the Copernican theory) and finally assuming an irrational solution to this problem based on mystical neoplatonic philosophy or/and psychology (p. 183-185).

(B) Making an inconsistent analysis of the hierarchy of the fundamental issues of pre-Newtonian astronomy (p. 185-186).

(C) Making an incoherent analysis of the problem of the possession of the same set of theoretical means and observational data by renaissance and ancient astronomers (p. 186-190).

(D) Making an inaccurate analysis of the connections between astronomy, cosmology and physics (p. 190-194).

(E) Making an inaccurate analysis of the dependence of the historical process known as “the Copernican revolution” on neoplatonic philosophy (p. 194-195)

(F) Making an inaccurate analysis of the dependence of Copernicus’ and Copernicans’ thought on scholastic thought (p. 195-200).

As an illustration, let us briefly consider case (C).

The *CR* [1957a] gives two answers to the above question.

According to the first, Copernicus and his contemporaries possessed, in principle, the same sort of observational data and theoretical means (*CR* [1957a] p. 99, 131) as the ancient astronomers:

„Copernicus seems their (that is of Aristotle and Ptolemy) immediate heir, for in the thirteen centuries that separate Ptolemy’s death from Copernicus’s birth no large and enduring modification had been imposed upon their work” (*CR* [1957a] p. 99).

„No fundamental astronomical discovery, no new sort of astronomical observation, persuaded Copernicus of ancient astronomy’s inadequacy or the necessity for change” (*CR* [1957a] p. 131).

However, according to Kuhn’s second interpretation (*CR* [1957a] p.101, 138-140):

„Copernicus and his contemporaries inherited not only *Almagest*, but also the astronomies of many Islamic and a few European astronomers who had criticized and modified Ptolemy’s system”, and who had used new methods and had determined new values of parameters of geometrical models which (both models and parameters) are incompatible with Ptolemy’s system (*CR* [1957a] p.138).

The last of Kuhn’s answers is correct. In the light of today’s knowledge about the history of astronomy, it is certain that Copernicus and ancient astronomers did not possess the same sort of astronomical data and theoretical means. Since, for instance, in contrast to Ptolemy, the medieval and renaissance astronomers considered non-uniform long period models of certain astronomical phenomena that were explained by the motions of an eighth sphere, and rejected the so-called equants used by Ptolemy in his system. In doing so, the astronomers used, among other things, certain mathematical constructions, which are called Tusi’s devices, that were not used by Ptolemy at all. Copernicus continued such interests in his search and construction of his astronomical theory.

Many of Kuhn's errors sprang from inappropriately applied *hermeneutics* insensitive to the mathematical aspects, although there is no doubt that Copernicus's theory is a mathematical work.

Kuhn's interpretations of the Copernican revolution also shows the effect which, by analogy with "the effect of a deficiency of a critical mass of nuclear chain reactions" (that is when one cannot exceed the minimum mass of fission material needed to initiate a nuclear chain reaction), the present author calls "the effect of the deficiency of the critical mass of information for a certain interpretation of the given issue". It is based on the idea that an interpreter, who does not have at his disposal a suitable amount, or "mass", of information (understood both quantitatively and qualitatively) on the analysed issue, cannot cross a specific information threshold and in consequence cannot create a sound interpretation of the information that is nominally available to him about the given issue.

Thus the resulting interpretation, being full of various inconsistencies and sophisms, is simply defective.

In the case of Kuhn's interpretations of the Copernican revolution, the above effect springs both from the insufficiency of Kuhn's acquaintance with, on the one hand, pre- and post-Copernican thought, and of Copernicus's own thought and of his epoch, and on the other hand with the art of scientific investigation in the field of the exact sciences. First of all, however, although the young Kuhn was one of the first of Sarton's new humanists, when writing the *CR* [1957a] and *SSR* [1962] he was not clearly conscious of the principle of the general methodology of the history of science and the exact sciences. These are the reasons for the above-mentioned errors committed by Kuhn. However, after many years of critical considerations, Kuhn radically developed this consciousness on the pages of the *ET* [1977]. This did not exert any degree of influence on subsequent editions of the *CR* (for example on the 7th of 1985, known as "renewed", and the reprints based on it, for instance, 1995, 1997), or the reprints of the *SSR* (2nd ed. 1970).

The present author's research has also revealed the following. For many years, subsequent editors of the *CR* have performed a peculiar manipulation. Firstly, they continue to include on the cover of this still widely read book excerpts taken from very positive reviews by Newman [1957], Woolf [1958] and Butterfield [1958] (published respectively in the "Scientific American", the "Isis" and the "American Historical Review"). Secondly, they persist in neglecting more competent, but critical, reviews by Hellman [1957], Dingle [1958], Rosen [1959], Zimansky [1959] and Poulle [1960] (published respectively in the "Observatory", the "Scripta Mathematica", the "Speculum" and the "Revue d'histoire des sciences et de leurs applications"). Finally and most importantly, they completely ignore the great achievements in the history of sci-

ence in the last four decades! In doing so, they have been strengthening the myth of the great epochal work for which the *CR* [1957a] can in no way be recognised.

**CONCLUSION:
CRITIQUE (p. 202-207)**

In the light of today's knowledge about the history of science, it should be emphasised that Kuhn's interpretations of the Copernican revolution, given both in the *CR* [1957a] and *SSR* [1962], have in many respects only a popular character. This conclusion should shock no-one since Kuhn expressed it himself clearly in the Preface to the *CR*:

“Though my first purpose in writing it [that is the *CR*] was to supply reading for the Harvard course [that is the science General Education course at Harvard College for non-science students] and for others like it, this book, which is not a text, is also addressed to the general reader” (*CR*, p.ix).

But according to the present author, the essayistic style, so characteristic of Kuhn's own widely read writings, though very suitable for popularising, is inappropriate in developing a detailed study of the history and philosophy of science.

It seems too that, albeit Kuhn's unusually famous books are written in lively language, they contain too many very important errors and omissions for the interpretations described in them to be to any degree recognised as paradigmatic for the history and philosophy of the exact sciences.

On the other hand, this univocally negatively sounding conclusion may no doubt surprise many philosophers of science and sociologists of scientific knowledge, and some historians of science, still accepting Kuhn as the eminent expert of the so-called Copernican revolution, or also some famous physicists, such as Weinberg [1998], who think, that Kuhn's idea of scientific revolution puts in principle a good construction on the same beginning of modern science.

But, are we allowed to claim something else, on the grounds given in this book, of the criticism of Kuhn's interpretations of the Copernican revolution? After all, it is Thomas S. Kuhn, who in his book the *ET* [1977], written after many years of critical considerations under the *CR* [1957a] and *SSR* [1962], and dedicated in its great part to the methodology of the history of science, wrote such words:

„The historian at work is not, I think, unlike the child presented with one of those picture puzzles of which the pieces are square; but the historian is given many extra pieces in the box. He has or can get the data, not all of them (what would that be?) but a very considerable collection. His job is to select from them a set that can be juxtaposed to provide the elements of what, in the child's case, would be a picture of recognizable objects plausibly juxtaposed and of what, for

the historian and his reader, is a plausible narrative involving recognizable motives and behaviours. Like the child with the puzzle, the historian at work is governed by rules that may not be violated. There may be no empty spaces in the middle either of the puzzle or of the narrative. Nor may there be any discontinuities. If the puzzle displays a pastoral scene, the legs of a man may not be joined to the body of a sheep. In the narrative a tyrannical monarch may not be transformed by sleep alone to a benevolent despot. For the historian there are additional rules that do not apply to the child. Nothing in the narrative may, for example, do violence to the facts the historian has elected to omit from his story. That story must, in addition, conform to any laws of nature and society the historian knows. Violation of rules like these is ground for rejecting either the assembled puzzle or the historian's narrative" (*ET* [1977] p. 16-17).

However, the methodological consciousness clearly revealed here – we see how it is mature! – did not exert an influence on the subsequent editions of the *CR* (e.g. seventh edition of 1985 called “renewed” and the subsequent reprints based on it, e.g. 1995, 1997) or the subsequent reprints of the *SSR* (second edition of 1970). And though Kuhn knew in some degree the enormous criticism of his interpretations of the Copernican revolution, he in principle simply ignored it.

On the other hand, this methodological consciousness found its full expression as early as 1978 in Kuhn's last book on the history of science: *The Black Body Theory and the Quantum Discontinuity, 1894-1912*. In this monograph, it is vital to remember that Kuhn abandoned all the revolutionary terminology and strategy elaborated in the *SSR*. Moreover, it is the *BBT*, and not his earlier books, *CR* or *SSR*, that Kuhn prized most highly in his achievements in the field of the history of science.

CONCLUSION:

PARTIAL DEFENCE (p. 207-211)

While making such a critical assessment, the present author is far from an absolutely negating of Kuhn's achievements in the field of history and philosophy of science.

In a partial defence of Kuhn's interpretations of the Copernican revolution, there are three arguments that should be noted.

Firstly, these interpretations were constructed at the level of the general reader, and not of specialists.

Secondly, to a large degree, Kuhn's error was to take his material from earlier scholars. Thus, to a great extent, Kuhn's faults reflect the state of contemporary research itself (with the important reservation that Kuhn completely overlooks the works of Ludwik Antoni Birkenmajer ([1900], [1901], [1923],

[1924]) and Aleksander Birkenmajer ([1936], [1953a], [1953b]), and only nominally mentions that of Edward Rosen [1939]).

Thirdly, it is important to remember that the primary tasks undertaken by Kuhn in Conant's group remain feasible, that is (in its genesis) Sarton's project on the one hand to teach non-science students about the spirit (philosophy) of science by applying an historical approach in order to oppose the pseudo-humanistic (literary humanistic) stance, and on the other hand to teach future scientists about the philosophical and historical aspects of their field of knowledge in order to oppose technocratism and scientism. And, as before, there is the issue of the interpretation of the so-called Copernican revolution to be considered, and that of the comprehension of the mechanism, or structure, of scientific revolutions, the problems of the incompleteness of translation and the incommensurability of paradigms and theories.

Furthermore, it is Kuhn's achievement that he called the attention of an enormous number of readers of his books to two things. Firstly, to the existence of the branch of knowledge called "the history of science". Secondly, to the value of research in the field of the history of science for the development of the philosophy of science. It is necessary, however, to notice here that earlier scholars, especially Sarton, Conant and the members of Conant's group had already emphasised this quality of the history of science.

Moreover, the present author, like other researchers of Kuhn's thought, is of the opinion that Kuhn's books, including the *CR* and *SSR* criticised in this monograph, are very interesting. This becomes especially clear when we examine these books against the background of achievements inspired by the leading 20th century philosophies of science different from Kuhn's own philosophy. The *CR* and *SSR* not only have undoubted literary qualities, but also contain certain substantial values, since the view of science that they describe, in spite of many shortcomings, is much closer to the actual practise of research than the views inspired, on the one hand by *the logical neopositivism of the Vienna Circle* and *popperism*, and, on the other hand by *deconstructivism*, *the strong programme of sociology of science* and *social constructivism*.

It was for this reason that some years ago the present author, who is particularly interested in physics and the philosophy and history of the so-called exact sciences, made careful studies of Kuhn's works. His first impression was very similar to Steven Weinberg's [1998] and Noel M. Swerdlow's [1997], [2001], whose achievements he admires very much.

Later, however, two years before Thomas S. Kuhn's death, when the present author undertook, as the subject of his PhD thesis, a critical estimation of Kuhn's interpretations of the Copernican revolution, and made careful observations of these interpretations, his comprehension of Kuhn's thought changed considerably, which finds full expression in this monograph.

In spite of the criticism the work contains, the present author is of the view that Kuhn's *CR* and *SSR* may still be used in academic courses in the field of the history and philosophy of science, and it may even prove very beneficial on the condition that lecturers guard against taking an uncritical approach. And what is more, these books are simply perfect for study at special seminars that aim to teach the critical skill of discussion using works by famous thinkers and masters of rhetoric: i.e. the art of persuading.

Undoubtedly, thanks to his still read and reprinted books, Kuhn has called an enormous number of readers' attention to the same fact of the existence of the branch named the history of science and has shown the value of its research for carrying considerations in the field of the philosophy of science and sociology of scientific knowledge. While developing these lines, one must keep in mind that the literary attractiveness of the produced texts, though they may draw attention of a broad body of readers, needn't always go hand and hand with historico-philosophical genuineness. And the last mentioned point is – according to the present author – the most important in dealing with the history and philosophy of the so-called exact sciences.

With all the above mentioned reasons (that differ, in some degree at least, from the given, on the one hand, by historians of science such as Newman [1957], Swenson [1957], Butterfield [1958], Hoskin [1958], Woolf [1958], Westman [1994] and Swerdlow [1997], [2001], and, on the other hand, by physicists such as Heisenberg [1973b], Ginzburg [1976], Weinberg [1998] and Wilson, Barsky [2001a]) the present author thinks that often controversial views of Thomas S. Kuhn will still stimulate the development of the history and philosophy of science, including especially the same understanding of the historical process named "the Copernican revolution".

However, we should not fall into the Kuhnian-centrism so characteristic of "Kuhnians"; and indeed let us take into account even more seriously the really great achievements of 20th century history and philosophy of the exact sciences, particularly the history of mathematical astronomy of the last fifty years. When we take these achievements into consideration, it will become clear that a great deal of what should be central to detailed, professional interpretations of the Copernican revolution has in fact a limited connection with Kuhn's own interpretations for which were, as he himself stated in the *CR*, intended for the general reader.

Although they see this clearly, contemporary researchers of the so-called Copernican revolution are convinced that the time has now come for new, more thorough, detailed interpretations of this historical process.

Very soon, the present author will be inviting all those interested to read his next book that deals with just such an interpretation.