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A Critical Comment on T.S. Kuhn's Views about the So-called Copernican Revolution and Several Current Prejudices – Barriers in Scientific Communities

Abstract

The article is a case study on the views of the famous T.S. Kuhn about the so-called Copernican revolution. Generally, Kuhn is presented as a very successful historian and philosopher of science: an author of world bestsellers. The division among his supporters, i.e. about so-called left-wing and right-wing Kuhnians, is recalled, and the fact that Kuhn himself vehemently dissociated from a large proportion of his adherents. It is also noted here, that in the last 30 years, in addition to abundant hagiographic literature on T. S. Kuhn, there have also been a few critical studies of Kuhn's achievements.

The rest of the article presents the author's critical analysis of Kuhn's views on the so-called Copernican Revolution, which formed the basis of Kuhn's scheme of scientific development presented in *The Structure of Scientific Revolutions* (1962); i.e. the world's most famous monograph in social sciences and humanities so far.

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The criticism encompasses a genesis, content and reception of Kuhn's views and the development of his interpretations. The analysis is carried out by the means of methodology of historical sciences and a scientific method, which the author describes as the hypothetico-deductive method of correspondence thinking.

The criticism is based on the author's current publications (developed here further on), which were sadly unnoticed by the researchers, although presented in the world center for the Copernican research, and are available on the Internet freely.

This fact leads the author to the assumption that international Kuhnian research is underdeveloped seriously and that strong prejudices – barriers may exist in scientific circles, such as, e.g., primacy of number of citations (and other bibliometric indicators) over content analysis, the Matthew effect, the effect of alleged and actual scientific centers and peripheries, some mental remnants of the Cold War, as well as underdevelopment of scientific communication.

Keywords: *Thomas Samuel Kuhn*, *Kuhnian research, Copernican revolution*, *Copernican studies*, *structure of scientific revolutions*, *methodology of historical sciences*, *hypothetico-deductive method of correspondence thinking*, *biography*, *autobiography*, *hypothetico-deductive method of Korespondenzdenken*, *critical analyses, content analysis*, *prejudices and barriers in scientific communities*, *Matthew effect in science*, *intellectual peripheries*, *intellectual centres*, *real and alleged centres*, *linguistic barriers*, *mental remnants of the cold war barriers, underdeveloped scientific communication, case study*

Krytyczny komentarz na temat poglądów T.S. Kuhna o tzw. rewolucji kopernikańskiej i kilka aktualnych uprzedzeń – barier w społecznościach naukowych

Abstrakt

Artykuł jest studium przypadku na temat poglądów słynnego T.S. Kuhna o tzw. rewolucji kopernikańskiej. Początkowa część artykułu w syntetyczny sposób przedstawia go jako bardzo utytułowanego historyka i filozofa nauki, autora światowych bestselerów; przypomniano tu także podział jego zwolenników, na m.in. tzw. lewicowych albo prawicowych Kuhnowców i fakt, że sam Kuhn stanowczo odcinał się od dużej części tych zwolenników; zwrócono również uwagę, że w ciągu ostatnich 30 lat oprócz bardzo obfitej literatury hagiograficznej na temat T.S. Kuhna, pojawiły się także opracowania krytyczne.

Pozostała część artykułu przedstawia autorską krytyczną analizę poglądów Kuhna na temat tzw. rewolucji kopernikańskiej, które to poglądy stanowiły podstawę schematu rozwoju nauki przedstawionego przez Kuhna w *Strukturze rewolucji naukowych* (1962), najsłynniejszej dotąd na świecie monografii nauk społeczno-humanistycznych.

Krytyka obejmuje genezę, treść i recepcję poglądów Kuhna oraz rozwoju jego interpretacji; czyniona jest ona z perspektywy metodologii nauk historycznych i metody naukowej, którą autor określa mianem hipotetyczno-dedukcyjnej metody myślenia korespondencyjnego.

Krytyka oparta jest na nadal aktualnych wcześniejszych publikacjach autora (i ich twórczym rozwinięciu, gdyż nie ogranicza się tylko do omówienia tych publikacji), które z reguły zostały niezauważone przez badaczy myśli T.S. Kuhna, choć powstały w rzeczywistym światowym centrum badań kopernikańskich i są dostępne darmowo w sieci internetowej.

Fakt ten skłania autora do wysunięcia przypuszczenia o poważnym niedorozwoju badań Kuhnowskich w skali międzynarodowej oraz o istnieniu w aktualnych środowiskach naukowych silnych uprzedzeń i barier, takich jak np. prymat liczby cytowań (i innych wskaźników bibliometrycznych) nad analizą

treści publikacji, efekt Mateusza, efekt rzekomych i faktycznych centrów i peryferiów naukowych, mentalne pozostałości barier zimnej wojny oraz niedorozwój komunikacji naukowej.

Słowa kluczowe: *Thomas Samuel Kuhn*, *badania Kuhnowskie*, *rewolucja kopernikańska, badania kopernikańskie, struktura rewolucji naukowych*, *metodologia nauk historycznych, hipotetyczno-dedukcyjna metoda myślenia korespondencyjnego*, *biografia*, *autobiografia, krytyczne analizy, analiza treści publikacji,* efekt *Mateusza w nauce*, *uprzedzenia i bariery w środowiskach naukowych, liczba cytowań*, *intelektualne peryferia*, *intelektualne centra*, *rzekome i rzeczywiste centra*, *bariery językowe*, *mentalne pozostałości barier zimnej wojny, niedorozwój komunikacji naukowej, stadium przypadku*

1. A trivial thesis

> Whether one is pro-Kuhn, anti-Kuhn, or neutral, no one can deny that the work of Thomas Kuhn has been a lightning rod for debates about science, culture, and policy across many academic fields – and even in the political arena and the business world. This is especially true of Kuhn's best-known work, *The Structure of Scientific Revolutions*, originally published in 1962 and expanded in 1970 (Nickles 2002, p. 1).

It is obvious that Thomas Samuel Kuhn (1922–1996) is one of *the most famous* philosophers and historians of science of the 20th century. An elementary proof of this thesis can be shown as follows:

First, he published several world best-sellers, such as:

- *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (Cambridge, Massachusetts, USA: Harvard University Press, 1957, 7th renewed ed. 1985) (hereafter *CR*);
- *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; 2nd enlarged ed. 1970; 3rd ed. 1992; (50th anniversary) 4th ed. 2012 with the introduction by Ian Hacking) (hereafter *SSR*);
- *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago and London: University of Chicago Press, 1977) (hereafter *ET*);
- *The Road Since Structure: Philosophical Essays, 1970–1993, with an Autobiographical Interview* (Chicago: University of Chicago Press, 2000).

Second, in the Western culture (West Europe, North America, Australia), T. S. Kuhn's views became a reason for:

- a vigorous debate in philosophy of science (a real quarrel with the Popperian school), [1]
- increased interest in *logic and psychology of scientific discovery and scientific creativity*, [2]
- a sociological turn in scientific rationality – the development of the sociology of scientific knowledge (among others, *the strong program in the sociology of knowledge* and *radical social constructivism*), and the emergence of the so-called *constructivist history* (Jan Golinski's term),[3]

[1] See, for example, Shapere 1964; 1971; Lakatos, Musgrave (eds.) 1970 [1965]; Lakatos 1970; Musgrave 1971; Gutting (ed.) 1980; Jodkowski 1990; and McMullin 1998.

[2] See, for example, Grmek, Cohen, Cimino (eds.) 1977; Simon 1977; Nickles (ed.) 1980, Pietruska-Madej 1990, De Regt 1993, Simonton 2009, and Feist, Gorman (Eds.) 2012.

[3] See, for example, Whitley (ed.) 1974; Barnes, Shapin (eds.) 1979; Shapin 1980; 1992; Barber 1990; Barnes, Bloor (eds.) 1993; Barnes, Bloor, Henry 1996; and Golinski 1998. For critical views on this approach, see, for example, Brown (ed.) 1984.

- a historical turn in the philosophy of science – the development of historical philosophy of science[4] and historical epistemology,[5]
- development of historical studies of science focused on *scientific revolution*s[6],
- development of *the rhetoric of scientific knowledge*,[7]
- re-interpretations of many branches of culture (not only the history of science and the history of technology, but also the natural sciences, exact sciences, humanities, economics, political sciences, theology, etc.).[8]

This cultural influence is expressed rather well with the following phrases: "Kuhnified times" (culture, all fields, horizons of acritical science studies, etc.) and "Kuhnification of science" (science studies, etc.), both terms coined by Steve Fuller (2000), "Kuhnification of the Humanities" (term coined by David B. Downing (2000; reprint 2004, p. 344), after Steve Fuller).

Third, the reception of his views in other regions of the world, for example, in Europe, Asia and Latin America was more limited, but still absolutely essential as proven, among others, by numerous translations of his main works, especially of *The Structure…*.[9]

Fourth and final, in the years 1954–1995 Kuhn received many important awards and accolades.[10]

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[4] See, for example, Lakatos 1970; 1971; 1978; Feyerabend 1975; Laudan 1977; Hoyningen-Huene 1992; 1993; Kuhn 1993; De Regt 1993; Radder 1997; Nickles 1998; Caneva 1998; Kindi 2006; Friedman 2010; Hoyningen-Huene 2012 and Kuukkanen 2013.

[5] See, for example, Hacking 1982; 1983; Wartofsky 1987; Daston 1991a; 1991b; Daston, Galison 1992; Daston, Park 1998; Poovey 1998; Carl, Daston (eds.) 1999, Hacking 1999a; 1999b; Davidson 2001; Gould (ed.) 2003; Daston, Galison 2007; Gingras 2010.

[6] See, for example, I. Cohen 1985; H.F. Cohen 1994; Weinberg1998.

[7] See, for example, Prelli 1989; Gross 1990; Dear 1991; Pera, Shea (eds.) 1991; Harré 1993; Harris (ed.) 1997; Gross, Keith (eds.) 1997.

[8] See, for example, Gutting (ed.) 1980; Jodkowski 1990; Storage 2012a.

[9] According to Theodore Kisiel and Galen Johnson (1974, 158 fn. 53), during the first ten years almost two hundred thousand copies of various English editions of *The Structure...* (1962) were sold, and the book was translated into six languages (Dutch, French, German, Italian, Japanese, and Polish). According to John Horgan (1991, p. 40), and Lawrence Van Gelder (1996), *The Structure...* was published in "nearly in a million copies in 16 languages". According to Steve Fuller (2000 — as far as I know he was the first to state that), it was published in "nearly a million copies in 20 languages", and according to David Weinberger (2012 — as far as I know he was the first to state that) it was published in over 1.4 million copies.

According to my findings, until 2023 *The Structure...* was translated at least into 31 languages: German (1967, 2nd ed. 1976 with 13. re-edition in 1996; 3rd ed. 2003), Polish (1968, 2nd ed. 2001, re-edition 2013), Italian (1969, 1979, 1991, 2009), Spanish (Mexico 1971, 15th ed. Madrid 2005), Japanese (1971), Dutch (1972, 2nd enlarged ed. 1995, 2003), French (1972, 1983), Serbian (1974), Russian (1975, 2nd ed. 2009; 2001), Hebrew (1977), Swedish (1979, re-editions: 1981, 1997, 2009), Chinese (1980, 1994, 2000, 2003, 2012 with the introduction by Ian Hacking), Korean (1980, 1981), Magyar (1984, 2000, 2002), Taiwanese (1985), Finnish (1994), Bulgarian (1996), Albanian (1997), Czech (1997, 1999), Greek (1997), Slovenian (1998), Ukrainian (2001), Catalon (2002), Croatian (2002), Estonian (2003), Lithuanian (2003), Portuguese (2003), Turkish (2003, 2006), Vietnamese (2005), Norwegian (2007), Romanian (2008). I guess that until 2023 it has been published in over 2.0 million copies.

See also Nakayama 2007; Aronova 2011, pp. 195–198; M. Hanson 2012; Nakajima 2012; Ito 2012; Zhang 2012; Li and Ren 2012; Fu 2012; Gallegos 2013 and the entries "Thomas S. Kuhn" and "Structure of Scientific Revolutions" in different linguistic versions of Internet websites (including *Wikipedia*).

[10] Here is the list: 1954: Fellow of the John Simon Guggenheim Memorial Foundation; 1958–1959: Fellow of the Center of Advanced Study in the Behavioral Sciences; 20/02/1962: Corresponding Member of the International Academy of the History of Science; 1963: Member of the American Academy of Arts and Sciences; 15/10/1966: Effective Member of the International Academy of the History of Science; 1969–1970: President of the History

2. Kuhnians, right- and left-wing Kuhnians and T.S. Kuhn's reading of the Copernican revolution

According to T.S. Kuhn, the advocates of his vision of science called themselves *Kuhnians* (as mentioned in the preface to *The Essential Tensions*, first published on 31 December 1977, and then in 1990s; his statements will be quoted below in section 12).[11]

of Science Society; 1/1972 – 12/1972, 9/1973 – 6/1979: member of Institute for Advanced Studies; 1974: Member of the American Philosophical Society; 1977: The Howard T. Behrman Award for distinguished achievement in the humanities (to be awarded annually to selected faculty members of Princeton's humanities departments, in recognition of research, publication, teaching, or other distinguished service to the University community); 1979: Member of the National Academy of Science, the most prestigious society for U.S. scientists; 1982: The Sarton Medal, the highest honour of the History of Science Society; 1983: John Desmond Bernal Prize, the highest honour of the Society for Social Studies of Science (4S); 1988–1990: President of the Philosophy of Science Association; 1990: A corresponding fellow of the British Academy; 1973–1995: Honorary degrees from 10 academic institutions: University of Notre Dame (20/05/1973), Rider College (1978), Bucknell University (1979), Linköping University (1980), Denison University (1988), Ohio Wesleyan University (1989), Columbia University (15/05/1991), University of Chicago (3/10/1991), University of Padua (07/12/1992), University of Athens (12/10/1995).

[11] To my knowledge, the term "Kuhnians" was coined by Carl R. Kordig in 1971 in order to denote "the Kuhnian philosophers of science", i.e. people who accept Kuhn's vision of science. He states: "On the one hand we are supposed to hold (8): Science is a subjective enterprise (in the sense specified above see, e.g. Feyerabend and Kuhn) whose concepts and domain are theory-laden. But, (8) is indeed put forward as the true, correct, proper, etc., way to view scientific transitions. Thus, on the other hand, in order to claim (8), and on the interpretation of these matters that we are considering, we are supposed to also hold (9): (9) The philosophy of science is an objective enterprise whose concepts and domain are not theory-laden. People who adhere to (8) would, to avoid self-referential problem, *have to* maintain that their own views of scientific change are influenced by the fact that *they are (say) Kuhnians* [emphasis added]; that is they would have to hold (9). (...) The facts scientists deal with are infected by particular theory employed. The facts which *(say) Kuhnian philosophers of science* [emphasis added] deal with are unexplainedly uninfected by their meta-theory, even though this theory is far more *conceptual and theoretical* in nature than any scientific theory" (Kordig 1971, pp. 81–82).

Then (1 January 1977) Larry Laudan applied the term "a Kuhnian": "Kuhn's paradigms, or «disciplinary matrices» are always implicit, never articulated. [...] As a result, it is difficult to understand how we can account for the many theoretical controversies which have occurred in the development of science, since scientists can presumably only debate about assumptions which have been reasonably explicit. When, for instance, *a Kuhnian* [emphasis added] maintains that the ontological and the methodological frameworks for Cartesian and Newtonian physics, for Darwinian biology, or for behavioristic psychology were only implicit and never received overt formulation, he is running squarely in the face of the historical fact that the core assumptions of all these paradigms were explicit even from their inception" (Laudan 1977, p. 75; he also used the term "Kuhnian theory of revolution" – Laudan, 1977, p. 135).

Finally, T. S. Kuhn, in the Preface to *The Essential Tensions...* (published 31 December 1977), was probably the first to use this term referring to sociologists, who were advocates of the strong program of sociology of science. And, it is certain that he popularized this term (see quotations in section 12).

Let us notice also the following observations:

First, four participants of the first conference on T. S. Kuhn's views, organized in 1965 in London by Popperians (John Watkins, Margaret Masterman, I. Lakatos, and T. S. Kuhn himself), applied the adjective "Kuhnian" in regard to Kuhn's views. The adjective was used eighteen times in the monograph edited by Imre Lakatos and Alan Musgrave (1970): (J. Watkins applied it twice, M. Masterman six times, I. Lakatos nine times, and T. S. Kuhn once). But, none of them used the term "Kuhnians".

Second, reading some crucial works by the advocates of *The Strong Program in Sociology of Knowledge*, it seems that they do not use the term "Kuhnians" — see Barnes, Dolby 1970; Barnes 1974; 1977; especially 1982; Bloor 1976; Barnes, Shapin 1979; and Barnes, Bloor, Henry 1996; Barry Barnes and Dolby do not use even the adjective "Kuhnian" but only genetivus possesivus "Kuhn's" (Barnes, Dolby 1970; Barnes 1974; 1977; and especially 1982), but Barnes applies the adjective "Kuhnian" in his earlier article (1972); David Bloor (1976) uses the adjective "Kuhnian" (scientists, accounts, history of mathematics) and genetivus possesivus Kuhn's (view, etc.), and, in Barnes, Shapin (eds.) (1979), and Barnes, Bloor, Henry 1996, respectively, one can find the adjective "Kuhnian" only once.

Since Kuhn's vision of science and Kuhnians's visions of science can be radically different, following T. S. Kuhn, Freeman J. Dyson, Barry Gholson and Peter Barker[12] it is worth making a terminological distinction between the term *Kuhn's ideas* (i.e. the ideas "which can reasonably be attributed to Kuhn himself, on the basis of his original work together with the clarifications that he published") and the term *Kuhnian ideas* (i.e. the ideas "which can be generally associated with Kuhn's name, despite denials on his part").

The Kuhnians include both conservative and radical interpreters of the Kuhn's understanding of science. I call them, respectively, *the right-wing Kuhnians* and *the left-wing Kuhnians* or *the Mertonian Kuhnians* and *the anti-Mertonian Kuhnians* (compare Rorty 1997a; Pinch 1982; 1997).

The terms *the right-wing Kuhnians* or *the Mertonian Kuhnians* denote people who (a) accept and promote the image of science presented in The *Structure of Scientific Revolutions*, and (b) think that T.S. Kuhn's perspective is coherent with the Mertonian vision of science (with a clear separation and distinction of social and cognitive activities of scientists, and a stable system of values – see Merton 1977). I include in this group, among others, sociologists such as Warren O. Hagstrom (1965), Nicholas C. Mullins (1972, 1973) and Henry G. Small (1977), logicians (the representatives of logical *structuralism*) such as Scott A. Kleiner (1970), Joseph D. Sneed (1971, 1983) and Wolfgang Stegmüller (1973)[13], historians of science, such as Theodore Brown (1970),[14] and philosophers of science such as Gary Gutting (1980), Kazimierz Jodkowski (1990) and Paul Hoyningen-Huene (1993).

The terms *the left-wing Kuhnians* or *the anti-Mertonian Kuhnians* denote people who (a) not only accept and promote the image of science given in *The Structure of Scientific Revolutions*, but also think that (b) "*The Structure of Scientific Revolutions* had important implications unintended, and unappreciated, by its author" (this is the original definition of the term "*the left-wing Kuhnian*" introduced by Richard Rorty in 1997)[15], and (c) T.S. Kuhn's image of science is contradictory with the Mertonian vision of science[16].

I include to this group, among others:

- sociologists – the advocates of the *Strong Program in Sociology of Knowledge* such as Barry Barnes, David Bloor, R. G. A. Dolby, Steven Shapin, and John Henry,[17] the advocates of *Social Constructivism* such as Bruno Latour, Steve Woolgar and Karin Knorr-Cetina,[18] and the advocates of *Microsocial Studies of Laboratories and Experiments* and *Social Construction of Technology* such as Harry Collins, Trevor Pinch, Wiebe Bijker and Thomas

[12] See Kuhn 1977, p. xxi; 1992, pp. 1 & 9; Dyson 1999, p. 16; Gholson, Barker 1985, p. 756.

[13] "He described himself as a Carnapian, who was perhaps becoming a proto Kuhnian, or something of the sort" (Kuhn *et al.* 1997).

[14] "T.S. Kuhn, who merely diverted many historians of science when he published his anatomy of scientific revolutions [...] has few followers today and virtually no students who would actually call themselves Kuhnians. Perhaps Theodore Brown, author of a study on iatromechanical movement in England is an exception" (Rousseau 1991, p. 116 fn. 75).

[15] See Rorty 1997a, p. 20.

[16] See Pinch 1982; 1997.

[17] See Barnes, Dolby 1970; Barnes 1974; 1977; 1982, Bloor 1976, Barnes, Shapin (eds.) 1979; Shapin 1980; 1982, Barnes, Bloor (eds.) 1993; Barnes, Bloor, Henry 1996.

[18] See Latour, Woolgar 1976; Knorr-Cetina 1981; 1983; 2012; Latour 1987.

P. Hughes[19], according to whom empirical issues are in fact the result of social interests and agreements;[20]

- philosophers that accept the thesis of so-called *deconstruction of metanarratives* or *epistemological deconstructionism* – e.g. Jacques Derrida, Jean-François Lyotard, and Richard Rorty;[21]
- philosophers of science – perhaps P.K. Feyerabend, with his *anarchic vision of science* (but this is a disputable thesis).[22]

Excluding Paul K. Feyerabend (1975), Stevin Shapin (1996), and John Henry (1997, 2nd ed. 2001, 3rd ed. 2008; 2001), the Kuhnians commented on Copernicus's achievements only marginally. Moreover, their remarks on this issue depended on T.S. Kuhn's and P.K. Feyerabend's interpretations in principle (R. Rorty; B. Barnes).

To illustrate the above, I quote Barry Barnes, a left-wing Kuhnian, who – summarising Kuhn's achievements – repeats Kuhn's opinion expressed in the *CR* and *SSR*:

> Copernicus himself was no revolutionary figure. He has to be understood in the light of tradition of research stemming from Ptolemy's *Almagest*. Copernicus's astronomical concerns were narrowly focused on technical problems; his method, esoteric and mathematical, were those of existing tradition; his innovation of giving motion to the earth there is a sense, as Kuhn says, in which Copernicus was the first modern astronomer; but in considering his own individual contribution Kuhn's book none the less presents him as the last in the great Ptolemaic tradition (Barnes 1982, p. 7).[23]

In addition, works about Copernicus, written by the supporters of *The Social History of Science* and *Constructivist History* (Stevin Shapin, 1996, and John Henry, 1997; 2nd ed. 2001; 3rd ed. 2008; especially) belong to a popular science genre and do not really refer to the program objectives of sociological interpretations of science.

Much more attention was given to Copernicus's thought by researchers in the field called *the Rhetorical History of Science* (with rhetoric as its fundamental research tool). This style stems from the views of Thomas S. Kuhn, Paul K. Feyerabend, and the so-called renaissance of rhetoric at the turn of the 19th century, which has been promoted by the historians of science such as Jean Dietz Moss, Robert S. Westman, William A. Wallace and André Goddu (see Moss 1993; Westman 1990 (reprinted in 1991 and 1994); 2011; Moss, Wallace 2003; Goddu 1996; 2010).

Finally, it is worth noting that the use of the right-wing and the left-wing Kuhnians for interpreting Copernicus's thought did not bring any new discoveries regarding neither

[19] See Collins 1985; Collins, Pinch 1993; 1998; Bijker, Hughes, Pinch (eds.) 1989.

[20] While the sociologists of scientific knowledge share many views, they can differ in many aspects, even radically — see, for example, Bloor 1999 and Latour 1999.

[21] See Derrida 1969; Lyotard 1979; Rorty 1979; 1997a; 1997b. I agree with William Storage that "[Richard] Rorty called himself a «Kuhnian» apart from those Kuhnians for whom *The Structure of Scientific Revolution* justified moral relativism and epistemic nihilism. Despite sharing distance from that sort of Kuhnians, I [William Storage] doubt that Thomas Kuhn ever saw, or would have seen, the conceptual connections that Rorty saw between their doctrines" (Storage 2012b); see Cavagnini 2012.

[22] See Feyerabend 1975. T.S. Kuhn (1962, p. xii) counted Paul K. Feyerabend among the four friends who commented on draft versions of the *SSR* and "whose (that is of four friends) contributions have proved most far-reaching and decisive." On the other hand, perhaps Feyerabend can be described better as a student of Karl Popper. He was not a left-wing Kuhnist at all, and challenged the idea of the scientific method regardless Kuhn's work.

[23] See also Barnes 1974, pp. 93–95 & 107–108.

mathematical aspects (which is not strange), nor sociological and political aspects (which is very strange).[24]

On the contrary, the application of rhetoric as a fundamental tool of research gave such new insights. However, it is debatable whether the advocates of this approach should be counted among the so-called Kuhnians or left-wing Kuhnians at all, because their lack of interest neither in the philosophy of science. nor in sociology of scientific knowledge.

Please note, that I do not include into the Kuhnians such experts on Copernican issues as Edward Rosen, Owen Gingerich, Robert Westman, and Noel M. Swerdlow, though the two latter appreciated Kuhn's essay on Copernican revolution and Kuhn's genius very much (see Westman 1994; Swerdlow 2004; 2013). Since their own approaches were not stimulated by T.S. Kuhn's thought in any way, they applied the classic understanding of the historical method, or the methods of history of exact sciences[25] in their research.

3. The present reservations regarding T. S. Kuhn's legacy and the beginning of a post-Kuhnian era

During the last 30 years, in the Western culture (in the Anglo-American world, especially), apart from writing the extensive hagiographic literature on T.S. Kuhn, serious doubts have grown, regarding the real value of his achievements in recognized circles and centres. This is true for the philosophy of science, the history of science, and the sociology of scientific knowledge. Moreover, there are no researchers at present, who would want to be referred to as Kuhn's true disciples. Following this, a post-Kuhnian era emerges, in which the achievements of T.S. Kuhn are only of historical significance. And there is no point going back to his results, because there are no ideas there, which could be creatively developed today.[26]

In order to illustrate this opinion, I quote four authors: John Heilbron (1996) from California University (Berkeley) and Oxford University,[27] Alexander Bird (2004) from Bristol University,

[24] In this context I must emphasise that emergence of the so-called Social History of Science in the Western culture was not the first socio-political analysis of Nicolaus Copernicus' achievements and his biography. Because these issues had been researched long before by Polish historians of science including, e.g., Jan Czyński (1847), Dominik Szulc (1851; 1855), Ignacy Polkowski (1873), Jeremi Wasiutyński (1938; 2nd ed. 2007), Andrzej Nowicki (1953, 1973), Marian Biskup, Jerzy Dobrzycki (1972) Karol Górski (1968; 1973), and Jerzy Sikorski (1973; 4th ed. 2011). For socio-political analysis belongs to the core of the standard methods in historical sciences.

[25] According to James A. Marcum: "N.M. Swerdlow [1993, p. 166] discussed Regiomontanus' 1464 inaugural oration to a series of lectures on astronomy. Swerdlow was motivated by Kuhn's analysis of the scientific revolution, in which physics was transformed from a classical form in which mathematics was less concerned with quantifying natural phenomena to a more modern form in which mathematics is used to manipulate the quantification of nature. Swerdlow concluded, in terms that echo Kuhn's analysis of Copernicus: What we have in the oration, in the prospectus, and indeed in Regiomontanus' very technical works, is something that belongs to its own time, the Renaissance, with values and virtues of its own that cannot be understood if we regard it only as an early part of the scientific revolution" (Marcum 2005, p. 135). I disagree with Marcum about the real and significant influence of Kuhn's thought on Swerdlow's: Swerdlow was *not* arguing for the Renaissance to be a distinct era from the Scientific Revolution *in terms mirroring Kuhn's analysis of Copernicus*.

[26] See Radder 1997; Caneva 1998; Fuller 2001; Zammito 2004; Bird 2004; Biagioli 2012; Friedman 2010; Barany 2012. However, this kind of reservations was firstly formulated by Barry Barnes in the context of his sociology of scientific knowledge (Barnes 1982, pp. 125–126).

[27] He was T.S. Kuhn's graduate student in the 1960s and the author of footnotes in Kuhn's *The Structure of Scientific Revolutions* (see Barany 2012).

Mario Biagioli (2012) from California University (Davies), and Michael Barany (2012), PhD candidate from Princeton University:

> Although he had few doctoral students in history and none in philosophy, he had an immense readership; no true disciples, but a worldwide congregation. He transformed his contemporaries' understanding of the nature of science and changed the world for those who study the problems that concerned him. His achievement is not easy to explain. He drifted from one academic field to another; his formal equipment for historical research was rudimentary; *Structure* is full of holes; *Black-Body Radiation* is impenetrable; the big book on philosophy has not appeared. What then? Kuhn had the genius to find the words and sketch the concepts that made important old philosophical problems relevant to the public and newly discussable by philosophers. He had the strength of mind and commitment to lead the discussion. He could speak the truly incommensurable languages of physics, philosophy, and history, all necessary to frame and advance his epistemological quest. He wrote, as one of his admirers, Margaret Masterman (1970, 61), put it, in a "quasi-poetic style," sometimes veiled, sometimes with "rhetorical exaggeration," but always after careful and even painful thought. (...) Or, to switch metaphors, he drew the portrait of science in the manner of the Impressionists. At a distance, where most viewers stand, the portrait appears illuminating, persuasive, and inspiring; close in, where historians and philosophers stare, it looks sketchy, puzzling, and richly challenging (Heilbron 1996, pp. 514–515).
>
> Thomas Kuhn was undoubtedly the strongest influence on the philosophy of science in the last third of the twentieth century. Yet today, at the beginning of the twenty-first century it is unclear what his legacy really is. In the philosophy of science there is no characteristically Kuhnian school. This could be because we are all Kuhnians now. But it might also be because Kuhn's thought, although revolutionary in its time, has since been superseded. In a sense both may be true. But it might also be because Kuhn's thought, although revolutionary in its time, has since been superseded. In a sense both may be true. We are all Copernicans –yet almost everything Copernicus believed we now disbelieve (Bird 2004, p. 1).
>
> Self-avowed Kuhnians are more common in the sociology of science (and to a lesser extent, history of science) than in philosophy of science. And even those Kuhnians were repudiated by Kuhn himself (Bird 2004, p. 11).
>
> I do not know who the many readers of *Structure* are these days, but it is safe to assume that historians and philosophers of science or science studies practitioners are not many among them. I believe I am expressing an opinion common in our field by saying that Structure was history-making and, half a century later, has itself become history. In the terminology of Ludwik Fleck (an author whose work inspired the young Kuhn) *Structure* started as an esoteric book studied by few people but quickly became an exoteric one, read, cited, and appropriated by an extraordinarily wider set of audiences, reaching more people and languages than any history and philosophy of science book ever has. Structure no longer frames the research agenda of the field, and yet it is a field it helped create. It is credited for having brought (or having tried to bring) the history and the

> philosophy of science together, and for creating important openings for the sociology of scientific knowledge to join in the action (Biagioli 2012, p. 499).[28]

Finally, after *A workshop in honour of Thomas Kuhn "Structure at 50: Assessing and Reassessing Kuhn and his Legacy"* (Princeton, November 9–10, 2012), at which philosophers of science: Nancy Cartwright and Philip Kitcher, and historians of sciences: Cathryn Carson, Mary Jo Nye, John Heilbron and M. Norton Wise delivered papers, Michael Barany, PhD candidate from Princeton University, summarizes his impressions and states:

> The workshop aimed to bring together a group of historians and philosophers intimately familiar with Kuhn and his work to account, among other things, for the failure of Kuhn's own project for the history and philosophy of science, even as his work so profoundly shaped the respective fields. (Mostly absent at this workshop, as several noted, was the relativist strand of the sociology of science that Kuhn vehemently disowned, though it may have been his greatest legacy.) (...)
>
> One could be forgiven for thinking, in the end, that we have never been Kuhnian. Though *Structure* inspired a great many historians, its mode of history is a far cry from the context-sensitive social and intellectual history that has dominated the field since his time. *Structure* antagonized a great many philosophers, but his propositions were too half-baked and ill-formulated to take on directly and his commitment to incommensurability was (all seemed to agree) at best a red herring.
>
> We heard of Kuhn's formative role in most speakers' biographies (...), but mostly scepticism about Kuhn's influence on the speakers' disciplines. (...) Wise observed that Kuhn worked closely with very few students, and (often bitterly) disagreed with all of them. Who could blame those students? Kuhn's commitment to the intelligibility of history and the reasonableness of historical actors was, in a sense I had not previously appreciated, at bottom both anti-historical and anti-philosophical (Barany 2012).

A brief comment is needed here. Although I am very sceptical about the value of anti-positivist reflection on science and technology in the post-Kuhnian times,[29] I believe that we should not reject our interest in T.S. Kuhn's own views that caused this era. I explain my reasons in the following parts of the present paper.

4. A nontrivial thesis and several author's works on T.S. Kuhn

Although Thomas S. Kuhn (1922–1996) is famous throughout the world and his thought was very influential; the origin, the essence and the criticism of his thought is rather poorly known in academic circles. The limited knowledge about the root of the matter; especially, about genesis of his theses, is, in my opinion, one of the fundamental paradoxes in the humanistic and social sciences (such as philosophy and history of science, sociology of scientific knowledge, and psychology of scientific discovery) in the last seven decades.

[28] An opinion, very similar or identical with Biagoli's sceptical position, was declared by John Heilbron during the Berlin workshop: *Towards a History of History of Science. A Workshop at the Max Planck Institute for the History of Science* (Berlin, October 17–20, 2012).

[29] I agree here with José María López Piñero (1993) and John Heilbron (2002; 2012c), among others.

In 1990s – as an engineer, a solid-state physicist, a philosopher of exact sciences, and a historian of exact sciences, who worked in the Department for History of Science and Technology (Institute for History of Science at the Polish Academy of Sciences) – I started a systematic research on science studies, focusing on the issue described above. Namely, I wanted to research carefully the genesis and substance of Kuhn's concepts, to introduce myself to the existing critical evaluations of his interpretations by earlier scholars, and, if possible, to formulate my own critique of this issue.

To my surprise, I could not find any critical knowledge in commonly cited, and otherwise very interesting, monographs and papers on T.S. Kuhn's views, written by Barry Barnes (1982), Steve Fuller (1992), Paul Hoyningen-Huene (1993), and Robert S. Westman (1994).[30]

In this context, I assumed the following initial hypothesis in my research. *The interpretations of the Copernican revolution by Kuhn himself, and by the previous scholars known to him, were the primary source of his famous ideas about the mechanism of the progress in science*.

In 1993, I published my first paper on philosophy of science, titled *Próba uniknięcia podstawowego błędu filozofii fizyki Kuhna (An attempt to avoid the fundamental mistake of Kuhn's philosophy of physics*) (see Kokowski 1993a). It presents a critical evaluation of T.S. Kuhn's philosophy of science described in *The Structure of Scientific Revolutions* (1962; 2nd ed. 1970). In order to present the intellectual climate of the paper in brief, I quote two excerpts:

> It seems (...) that the *Ethos* of Kuhn's philosophy of physics is alien to the spirit of physics. In Kuhn's thought, I find too much of revolutionary radicalism from the French Revolution; too much competition between research communities, too much natural selection and the struggle for survival of theories, and too little of a characteristic poetry associated with discovering the order of the world (translated from Polish; see Kokowski 1993a, p. 16).
>
> Paraphrasing [a priest and a philosopher Józef] Tischner (1972, p. 917; 1973, p. 18), and referring to [a cultural anthropologist Alfred Luis] Kroeber (1973, p. 13), the point is to speak about physics and physicists, about its and their matters in the language taken directly from the experience of physics and the professional experience and intuition of creative physicists. We should understand physics and physicists through this, what is the most physical and connected with the profession of a physicist. The concern for the autonomy of language is an expression of a belief that physics as a cultural phenomenon cannot be reduced to any other sphere of human activity in its essence. It is therefore necessary, when talking about physics and physicists, to speak in such a language, which expresses and confirms this irreducibility (translated from Polish; see Kokowski 1993a, p. 19).

Then, in a series of works on philosophy of science and history of science published between 1996 and 2005, I developed the nontrivial thesis about the origin, the essence and the criticism of T.S. Kuhn's thought, mentioned at the beginning of this section. In 1996, in the paper *Copernicus and the Hypothetico-Deductive Method of Correspondence Thinking [Korespondezdenken]. An Introduction* (see Kokowski 1996), I described, among other things, my understanding of the

[30] A very good monograph in Polish is worth adding to the list: Kazimierz Jodkowski 1990. *Wspólnoty uczonych, paradygmaty i rewolucje naukowe*, „*Realizm, Racjonalność, Relatywizm*", vol. 22. Lublin: Wydawnictwo UMCS.

scientific method (i.e. the generalisation of *the Hypothetico-Deductive Method* and the idea of hypothesis), and the methodological core of Copernicus' achievements.[31]

On 11th of December 1997, I defended my doctoral dissertation *The critique of Thomas S. Kuhn's interpretations of the Copernican revolution in the light of hypothetico-deductive method of Korespondezdenken (correspondence-oriented thinking)* (in Polish)[32].

Then, during following four years, I elaborated on it and published a monograph *Thomas S. Kuhn (1922–1996) and the issue of the Copernican Revolution* (in Polish, with an English summary). The book was published as part of a series "Studia Copernicana"[33] and has been the first and the only work (as for 2023) that analyses Kuhn's views on the Copernican issue in detail.

5. The so-called Matthew effect and other prejudices: barriers in scientific communities

Because the monograph *Thomas S. Kuhn (1922–1996) and the issue of the Copernican revolution* (written in Polish and partly in English, available online) is the only one of its kind in the literature, it should rather attract the attention of experts focused on understanding the Kuhn's views. Such a fact would be understandable, for it would be dictated by the principle of using a well-chosen bibliography. However, the opposite has happened: it is generally overlooked by thinkers analysing Kuhn's thought, as evidenced in the respective monographs and articles, published in recent years.[34]

Fortunately, there are two exceptions to this rule: a review of my monograph by Michał Heller (2003) in Polish[35], and an article by Pietro Daniel Omodeo (2016) in English, published

[31] See also Kokowski 1997a; 1999c.

[32] Adviser: Prof. Michał Heller (a philosopher of science, cosmologist and theologian at Pontifical Academy of Theology in Cracow and Vatican Observatory; Templeton Prize winner 2008). Reviewers: Assoc. Prof. Grażyna Rosińska, Doctor Habilitatus (a historian of science in the Institute for History of Science of the Polish Academy of Sciences) and Assoc. Professor Alina Motycka, Doctor Habilitatus (a philosopher of science in the Institute for Philosophy and Sociology of the Polish Academy of Sciences).

[33] See Kokowski 2001a. It was the last book edited by professor Paweł Czartoryski (1924–1999), the founder and the editor-in-chief of the "Studia Copernicana" series [Jego tytuł wprowadza tu anegdotę. Anglicy tak nie piszą.] The book is available online, see bibliography. Also, see Kokowski 2001c, the English website of the monograph, with the English summary of Kokowski 2001a (in: Kokowski 2001a, pp. 315–328).

[34] For example, see the works by Thomas Nickles (2002); Wes Sharrock, Rupert Read (2002); Brendan Larvor (2003); Uwe Rose (a very good doctoral dissertation in German, 2004); Noel M. Swerdlow (2004 [1997]; 2013); James A. Marcum (2005); Randy Allen Harris (ed.) (2005); Edwin H.C. Hung (2006); Robert Nola, Howard Sankey (2007); Brian Maricle (2008); Wojciech Sady (2010; 2020); Brad Wray (2011; 2021; (ed.) 2021); Alexander Bird (2012), Benjamin A. Elman (2012); Vasso Kindi, Theodore Arabatzis (eds.) (2012); John Onyekachi Nnaji (a very good doctoral dissertation , 2013); William J. Devlin, Alisa Bokulich (eds.) 2015; Hans-Joachim Dahms 2016: Robert J. Richards, Lorraine Daston (eds.) 2016; Errol Morris 2018; Paweł Jarnicki, Hajo Greif (2022); Thomas S. Kuhn, Bojana Mladenović (2022); Leandro Giri, Pablo Melogno, Hernán Miguel (eds.) (2023).

[35] Professor, priest Michał Heller, the adviser of my doctorate thesis defended in 1997 and then Templeton Prize winner 2008, made a clear summary and reviewed the monograph – see Heller 2003: "One might have some doubts as to whether *CR* book (*Copernican Revolution* …) is so important for Copernican research that it deserves such meticulous criticism. This doubt, however, is mitigated by the fact that Kuhn became one of the central figures in the philosophy of science of the twentieth century, and his concept of the structure of scientific revolutions still plays a very important role in the philosophy of science. It is therefore worth knowing on what basis – historical and substantive – this concept is based. I believe that this very question has prompted M. Kokowski to undertake a huge research, which led to the writing of this book" (translated from Polish). („Można by mieć pewne wątpliwości co do tego, czy książka PK jest tak ważna dla badań kopernikowskich, że zasługuje aż na tak drobiazgową krytykę. Wątpliwość tę jednak łagodzi fakt, że Kuhn stał się jedną z centralnych postaci filozofii nauki XX wieku, a jego koncepcja struktury rewolucji naukowych nadal w filozofii nauki odgrywa bardzo ważną rolę. Warto więc wiedzieć na jakich podstawach — historycznych i merytorycznych — koncepcja ta się opiera. Sądzę, że ten właśnie motyw

in the collective monograph by Alexander Blum, Kostas Gavroglu, Christian Joas and Jürgen Renn (eds.) (2016).[36]

In this context, it is worth to mention *the so-called Matthew effect in science*, introduced by Robert K. Merton[37]. The effect consists in fact that "eminent scientists often get more credit than a comparatively unknown researcher, even if their work is similar" or "the credit is usually given to researchers who are already famous" (in the formulations taken from "Wikipedia" 2013c) or "the credit is usually given to researchers coming from known centres".

Thus, two contradictory explanations of the omission of the monograph *Thomas S. Kuhn (1922–1996) and the issue of the Copernican Revolution* (2001) by the interpreters of T.S. Kuhn's thought are possible.

First, this omission *is not* an example of the so-called Matthew effect in science, because the Institute, where the author is affiliated, is a true centre of Copernican research with the longest uninterrupted tradition in the world, so the results of the representative of this centre cannot be overlooked if we have the benefit of scientific discourse in mind.[38]

skłonił M. Kokowskiego do podjęcia ogromnego wysiłku badawczego, jaki doprowadził do powstania tej książki" (Heller 2008, p. 123)).

[36] "Much has been written about Kuhn's best seller on the history of early modern astronomy. The most exhaustive study on internal and external factors in the conception and reception of Kuhn's Copernican Revolution is a monograph by Michał Kokowski, issued in 2001 as a volume in the series Studia Copernicana" (Omodeo 2016, p. 91).

[37] See Merton 1968; 1988; Strevens 2006; Rigney 2010. Besides, I would like to add that in my opinion the term "Matthew effect" was chosen inappropriately.

First, the primary sense of the excerpt of Matthew's Gospel, cited by R. Merton, is of spiritual and objective nature — we read: "For unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath" (Matthew 25, p. 29, King James Version, which is the English translation of the Christian Bible for the Church of England, begun in 1604 and completed in 1611). In other words, if "the rich" is really rich and "the poor" is really poor, "the rich get richer and the poor get poorer".

Second and final, however, the division into "the rich in science" and "the poor in science" is of sociological, political and subjective nature; and often dominated by socio-political interests, in which the questions of ethics and the priority are ignored. Stephen Stigler's law of eponymy, which in its simplest and strongest form says "No scientific discovery is named after its original discoverer" (Stigler 1980), recognizes only one aspect of this complicated issue.

[38] The Department of History of Science in the Polish Academy of Sciences (Institute for the History of Science in the Polish Academy of Sciences from 1994) was founded in 1954. From the beginning, it was the centre for Copernican studies. One of the fellows there was Aleksander Birkenmajer (1890–1967), Ludwik Birkenmajer's (1855–1929) son, both great experts on Copernican studies. The Institute has published the international edition of Nicolaus Copernicus's collected works, and publishes the "Studia Copernicana" series started in 1970 by Paweł Czartoryski (1924–1999). Many other researchers specializing in Copernican issues worked at the Institute, like Zofia Wardęska (1921–1989), Jerzy Dobrzycki (1927–2004), Grażyna Rosińska (1937–2013) and Jerzy Drewnowski (1941–), among others.

In order to emphasize the importance of Copernican studies in the Institute, it has been called Ludwik and Aleksander Birkenmajer Institute for the History of Science, Polish Academy of Sciences, since 2009.

Ludwik Birkenmajer and his son Aleksander Birkenmajer were the fellows of Academy of Arts and Sciences in Kraków; Polish Academy of Arts and Sciences from 1919). In 1897–1924(?) the Commission for publishing the Copernicus' works operated there and Ludwik Birkenmajer played a key role. In 1900, the Academy published his monograph *Mikołaj Kopernik. Część pierwsza. Studya nad pracami Kopernika oraz materyały biograficzne* (*Nicolaus Copernicus. Part One. Study and research on the works of Copernicus and bibliographic materials*), that is still one of the fundamental monographs of the Copernican studies — see Kokowski (ed.) 2002; Goddu 2016; 2018a; 2018b.

Ludwik Birkenmajer's Copernican research was a continuation of earlier studies by Jan Śniadecki of 1782 and 1802 (see: Śniadecki 1782; 1802). Thus, since 1782, Polish science has the longest uninterrupted tradition of Copernican research in the world. However, the first interest in this topic in Polish culture begins with Jan Brożek /

Second, quite the opposite and much more likely, the omission is a very good example of the so-called Matthew effect in science. The results are overlooked, because this centre, despite being a real research centre with long traditions, is not known by the evaluators.

Whichever explanation is correct, the omission is not accidental and happened due to the following four probable reasons.

First of all, many publications about Kuhn's thought have been published around the world. Many of them go unnoticed, regardless of their substantive value. Why this is so, however, is difficult to explain. Perhaps it is about the linguistic barriers or underdeveloped scientific communication; perhaps it is the negligence of publishers and authors in the field of scientific communication (lack of promotion of works on the publishing market), etc.

Second, the tradition of the critical interdisciplinary research on Kuhn's thought have not developed yet.

Third, in the contemporary scientific culture, the value of a publication is determined with popularity measured by numbers of citations (and related bibliometric indicators), and not with any critical analysis of the content of publications. *So, if we believe in this method, we must conclude: if a work is not cited frequently, it cannot be of value*.

Fourth and final, although the Cold War period ended in 1991,[39] a some mental and political (and only secondarily linguistic) barrier has remained in the minds of Western researchers (and their followers around the world, including Central Europe) until today, which causes that valuable works by scholars from Central Europe to be still neglected. In other words, using more general ideas, the whole of Central Europe is treated as an intellectual periphery of the real intellectual centres from "the Western world".[40] *So, with this attitude, a work written and published in the Central Europe can be neglected safely, because it cannot be original by definition*.

Nevertheless, because the results of my studies in Kuhn's views on the so-called Copernican revolution are still relevant,[41] the benefit of scientific discourse requires that researchers should have access to them. Therefore, still believing in international community of scholars, a critical intellectual dialogue, an openness in thinking, scientific discourse and scientific communication, I synthetically describe these studies in the following parts of the present article. I really hope that these remarks will finally be noticed by the researchers of Kuhn's thought and will be *critically considered* by them.

6. The subsequent stages of the author's interpretation of T.S. Kuhn's views about the Copernican revolution

In order to make a serious assessment of T.S. Kuhn's views of the Copernican revolution, I conducted a multi-stage study:

1) Through my studies in scientific method, I generalized the method of exact sciences (by developing many scholars' views). I call it *the Hypothetico-Deductive Method of Korespondenzdenken* or *the Hypothetico-Deductive Method of Correspondence Thinking*.

Joannes Broscius (1585–1652), who, in 1618, made a scientific expedition to Warmia in order to look for Copernican memorabilia (see Brożek 1956, and Wasiutyński 2007, pp. 503–512, with critical estimation of Brożek's findings).

[39] See *Wikipedia* 2013a; 2013b.

[40] For example, Stefan Zamecki (1991, p. 122) lamented this problem in his review of the book entitled *Recent Development in the History of Chemistry* edited by Colin Archibald Russell (1985).

[41] Since the publication of the monograph in 2001, no new monograph has been published on the subject. See also footnotes 35 and 36, above.

2) I analysed the methodology of historical studies, including the process of research and reporting on the results.
3) I conducted my own research on the so-called Copernican revolution, especially on Nicolaus Copernicus's achievements.
4) I researched the scientific biography of T.S. Kuhn, focusing on his interest in history and philosophy of science, analysed his reading as well as the views of the influential scholars, whom he met in his university milieu, i.e., at Harvard University.
5) I became acquainted with the existing critical evaluations of T.S. Kuhn's interpretations of the Copernican revolution, his own reaction to these evaluations, and the development of his views in different schools of thought.
6) Finally, on the described bases, I formulated my own critique of T.S. Kuhn's interpretations.

In the following parts of this article, I will outline these issues.

7. The Hypothetico-Deductive Method of Korespondenzdenken and the idea of a scientific (r)evolution

While agreeing with the greatest scientists and philosophers such as Albert Einstein, neo-positivists, scholars from Popper's and Marxian schools of philosophy of science, that there exists a scientific method, I differ radically with another group of philosophers: Thomas S. Kuhn, Paul Feyerabend and the *left-wing Kuhnians,* who state that there is no scientific method. However, in opposition to all of them I state that the *Hypothetico-Deductive Method of Korespondenzdenken* (the Hypothetico-Deductive Method of Correspondence Thinking), outlined below, is a general method of exact sciences.[42]

In brief, the Hypothetico-Deductive Method of Korespondenzdenken is composed of two parts, which interpermeate: the Hypothetical-Deductive Method and the Method of Korespondenzdenken (HDMKD = HDM + MKD).

In contrary to, for instance, the Popperian school, the terms "hypothesis" and "deduction" here are descriptive in nature, not normative. That is, the term "hypothesis" means each conjecture, without analysing a mechanism of its arising (it may be generated by a rapid irrational creation of thought, or by a more elaborated and rational way). The term "deduction" has a broad meaning used by scientists in their practice (and revealed by research of the history of exact sciences), that is, it means not only the "deduction" in a narrow sense, but also "induction and "abduction" as defined by logicians.

Let us notice: When we assume a narrow meaning of the hypothesis and the deduction, this Hypothetico-Deductive Method is identical with the understanding of the Hypothetico-Deductive Method assumed by, for instance, the Popperian school.

The Method of Korespondenzdenken is focused on applying an idea of correspondence to different elements of scientific theories (mathematical models, their characteristic constants and variables, and hypothetical entities). The Method considers, among others,

[42] Since I examined the title issues in my earlier papers and monographs, in order not to repeat the narrative once again, I refer the reader to the bibliography given below. As the introduction to the idea of the Hypothetico-Deductive Method of Korespondenzdenken, my paper of 1999 may be of service (see: Kokowski 1999c, available online), and to the idea of a scientific (r)evolution two papers of 2009 and 2012 (see: Kokowski 2009b, pp. 242–244, available online; Kokowski 2012b, pp. 55–58, available online). Many information on this method you can find also in Kokowski 1996b (available online) and 2004.

- the general request of the revision of the theoretical and empirical data, and its implementation by applying the strategy of erasing previously established regularities in extreme cases: in short, the "eraser strategy"[43];
- the postulate of correspondence: the predictions with observations and their particular realisations;
- the correspondence postulate of subsequent theories;
- the generalised correspondence principle linking subsequent theories, which is the realization of the correspondence postulate above;
- a thought experiment including changes in the constant values in a physical or astronomical model – this helps to understand better the features of the model (George Gamow's idea)[44].

This part of the method of exact sciences was underestimated or neglected by the previous philosophers of exact sciences, though it is crucial to understand well the development of the so-called exact sciences.

Keeping in mind the history of the so-called exact sciences, I think that that the idea of a scientific evolution and the idea of a scientific revolution are nothing more than idealisations of a real process, and I treat these ideas as the Max Weber's ideal types,[45] the limit cases of a more general idea of scientific (r)evolution: in some aspects, the great scientific changes introduced new views (ideas, methods, etc.) {discontinuity$_1$}, or made their imperfect translations {discontinuity$_2$} but also many old views remained {continuity}.

Viewing through the prism of the Hypothetico-Deductive Method of Korespondenzdenken, it is clear, for example, that:

(1) T.S. Kuhn (and P.K. Feyerabend) understood the problem of the incommensurability [46] of the fundamental theories linked by certain generalized correspondence principles, but did not

[43] From the history of science, we know that from time to time, *for the legitimate theoretical or empirical reasons*, representatives of the so-called exact sciences make corrections of theoretical and empirical data by erasing previously established regularities of nature. It is a standard strategy used, for example, by Copernicus, Brahe, Galileo, Kepler, and Newton.

[44] See Gamow 1940.

[45] See Weber (2002; first published 1962), pp. 29 & 32; Secher 2002, pp. 14 & 20; Coser 1977b.

[46] For the fundamental work about the genesis of the idea of incommensurability in Feyerabend's and Kuhn's thoughts, see Oberheim 2005. In addition to the works by Kuhn and Feyerabend, he analyzes many other works, among others, Whewell 1860, Appendix H; Duhem [1906] 1908, 1954, pp. 159, 190–196; 209; Fleck 1927; 1935; Ajdukiewicz [1934a] 1978a; [1934b] 1978b; Köhler 1938, p. 17; Polanyi 1951, p. 100; 1958, p. 174, p. 294 fn. 3; Popper [1957] 1972, pp. 190–205; Nagel [1949] 1960, p. 309; [1961] 1966, pp. 380–397; Agassi 2002, p. 409.

It was Ludwik Fleck ([1927] 1986a; [1935a] 1979; [1935b] 1986b; [1936] 1986c; [1947] 1986d), who, many years before T.S. Kuhn and P.K. Feyerabend, introduced the concept of incommensurability in the history of medicine. He applied it, while describing different styles of thinking, terms, theories, changes in formulating problems and standards, conceptual changes or replacements and theory-ladenness of observation – see Feyerabend 1951; 1954; 1955; 1958a; 1958b; 1958c; 1960; 1962; 1975; Kuhn 1962; 2000 [1970]; Harwood 1986; Sankey 1991; Sankey, Hoyningen-Huene 2001; Babich 2003a; 2003b; Oberheim 2005; Bird 2007; Wolf 2007; Demir 2008; Moreno 2009; Psillos 2008; Soler, Sankey, Hoyningen-Huene 2008; Peine, Alexander 2011; Pinto de Oliveira 2017; and finally Oberheim, Hoyningen-Huene 2018, section 2.2.2.

From my point of view, Kuhn's and Feyerebend's considerations about the incommensurability in the so-called exact sciences were secondary to Fleck's considerations about the incommensurability in the history of medicine. But Kuhn and Feyerabend did not mention this relationship and priority. Fleck's conceptual grids are not well suited to analyse mathematical and physical issues, since Fleck – as later Kuhn and Feyerabend – missed the key issue of generalized correspondence principles. However, in the case of Fleck, who studied history of medicine, this

notice the importance of the strategy of the applying the Korrespondezdenken to the development of exact sciences.[47]

(2) T.S. Kuhn's (and P.K. Feyerabend's) interpretations of scientific revolutions were dominated by the model of socio-political revolution, such as the French Revolution and the Soviet Revolution, as well as by the Gestalt psychologism.[48] However, this model, which includes two sides: parties fighting for power and Gestalt switches do not reflect correctly the development of the exact sciences (because the ideas mentioned above are not well suited for mathematical and physical issues).[49]

(3) In consequence, T.S. Kuhn's model of developing science described in *SSR* is not valid.

8. The methodology of historical sciences (including the history of science)

In order to understand best both T.S. Kuhn's views and the dependence of these views on other scholars, it was necessary to analyse the same bases of researching on the historical sciences, including the history of science[50] and history of exact sciences, and writing down the results. Thus, the method of historical studies should be considered, and the specific features of the method of studies in history of exact sciences should be shown. I explain the results of these considerations below.

When we make a methodological analysis of a research process in the historical sciences, it is worth focusing our attention on the following list of correlated matters:

1) An ultimate perfect basis of the historical sciences are primary historical data; that is, the actual traces of existence and activities of a person X in a particular historical space-time (when the person lived). However, such data are very often not available, since the person is no longer alive and we have no opportunity to learn from the person's report on any events, ideas, thoughts, etc., that we are interested in.
2) Thus, the factual basis of the historical sciences consists in secondary historical data, i.e., historical sources proving the existence and activities of the person X: the primary sources – manuscripts, copies, prints, interviews, photos, films (from the era, in which person X lived) and the secondary sources (studies based on the primary sources).
3) In order to understand historical facts, we must accept an appropriately chosen hermeneutics of research (explained below).
4) We need to select an appropriate list of historical facts (as the equivalent of empirical data in the natural sciences).
5) We need to interpret the list of the selected historical facts in the light of a chosen hermeneutics of esearch.

omission is not strange, since medicine is much less mathematic than exact sciences interpreted by Kuhn and Feyerabend.

[47] Previous authors, representatives of exact sciences or of social sciences, who commented on the views of T.S. Kuhn, overlooked these issues. See Kokowski 1993; 1996; 2001; 2004; 2015e.

[48] In this context, we should remember that the idea of Gestalt switches was applied already by L. Fleck (1935) to explain changes of thinking styles and thinking collectives in medicine – see Cedarbaum 1983, pp. 199–200; Harwood 1986, p. 177; Oberheim, Hoyningen-Huene 2018, section 2.2.2.

[49] See fn. 46 (my point of view).

[50] In this 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 relatively new, its origins date back to a mere fifty years ago or so, but compared to German "Wissenschaft" and Polish "nauka", by contrast, it is an old, even classical — in its origins — concept.

6) There is a key requirement: our interpretation must be consistent with the historical facts (that is, „it must save phenomena"), and must be consistent (that is, it must provide coherent explanations in the light of the hermeneutics applied).

"A term "research hermeneutics" (= a hermeneutics used by a researcher) denotes all interpretative tools used by a researcher at the stage of his repeated attempts to comprehend the subject under study" (see Kokowski 2001b, p. 316). And, let us give a list of five fundamental properties of a research hermeneutics:

1) Each research hermeneutics is a decoder of a potential source of information and a cognitive filter.
2) This type of a decoder and filter can have different qualities. It can decode and filter information with a different precision!
3) A research hermeneutics may be composed of different number of elements, and be adjusted well or adjusted badly, in respect to the aim of the research.
4) If a research hermeneutics is composed of different elements, there is a hierarchy of importance of its elements.
5) The more integrated, detailed, and subtle the hermeneutics of research is, the more easily one can understand the problem. Therefore, we must always try to improve our research hermeneutics.

It is worth adding here an illustrative commentary. Suppose, that we are interested in understanding the issue of formulating the theory of general relativity. Now, there is a basic question: what elements should our hermeneutics consist in and what should the hierarchy of these elements be, so that we achieve our aim successfully?

A supporter of an internalist approach would say:

> First of all, we must know the theory. However, this is not sufficient. We are also obliged to be well acquainted with a great part of theoretical physics, with some branches of mathematics, with some elements of experimental physics and philosophy of physics, including its methodology, and the elements of logics.

A supporter of an externalist approach would say:

> We must know the historical contexts, in which we should understand the psychological, social, political, economic, and similar matters. Therefore, we must know general psychology (especially psychoanalysis), psychology of scientific discovery, sociology of scientific knowledge, political and economic doctrines (for example Marxism), etc.

Keeping in mind the development of the history of science, the philosophy of science (including scientific methodology and scientific rhetoric), and the sociology of scientific knowledge during the last century (both in the so-called Western culture and in the Soviet bloc) referred to in my earlier works (see: bibliography), and the development of my own research experience, enquiries and works, I postulate to treat the internalist and externalist approaches as only the limit cases (Max Weber's ideal types) of one, more general and more integrated attitude, which I call here *the Method of the Complementary Explanations* (MCEs). The core of this more general attitude are the following three ideas:

1) the idea of the *integrated* hermeneutics of research (outlined above),

2) the idea of an art of *rational* persuading,
3) the idea of a *hierarchy of the importance* of the different possible aspects of the problem considered (firstly we must look for the internal explanations; then, if we cannot find them, we should seek the external explanations).

Furthermore, in my attempts to understand T.S. Kuhn's views, I applied the additional specific methodological tools, i.e. a concept of *the fundamental strategy of the historian*, a concept of *the structure of the scientific text,* and *a technique of extended citations*.

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 it with skill into a suitably constructed text – a certain story (narration) about the episode. When the researched subject is, for instance, the views of a particular historical person, it is an historian's duty to read and learn a spirit of all the available 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 the person.

While attempting to describe the views of the thinker, one meets a fundamental problem. It is impossible to summarise someones' views "in one's own words" without losing important original information. This is often the case when the considered thinker uses very complicated and highly specialised language or examines very detailed issues. This type of linguistic non-translatability is one source of fundamental misunderstandings in the comprehension 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.

Furthermore, in order to understand the essence of a scientific text better (the written "text" is one of the fruits of research), I distinguish three elements in every scientific text: *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 in study), and *the rhetoric of the text* (all means used to convince the reader to the theses explained).

The mentioned above methodological tools I have used openly in my works on the genesis of T.S. Kuhn's views and the substance of his works, and secretly regarding the works of the other scholars that were important to him.[51]

9.Nicolaus Copernicus' achievements in focus

Imre Lakatos (1970, p. 131 fn. 110) observed rightly that none of the critics of T.S. Kuhn's vision of the development of science described in *The Structure*... (1962, 2nd extended ed. 1970) – he mentioned by name philosophers of science only: David Shapere, Israel Scheffler, Karl Popper, John Watkins, Stephen E. Toulmin, Paul K. Feyerabend, Alan Musgrave, and himself – applied a systematic *historiographical* criticism to Kuhn's work.[52]

[51] For further details see Kokowski 2001a.

[52] I noticed this fact after reading a very good monograph about T. S. Kuhn's philosophy of science by a Polish philosopher of science Kazimierz Jodkowski — see Jodkowski 1990, p. 193.

My aim was to complete this task in respect to, first of all, Nicolaus Copernicus's views: their essence, genesis and reception,[53] and then in respect to Kuhn's views.

Therefore, I did my own research on these views in the light of my understanding of the methodology of historical studies and of the scientific method outlined above. I focused on the history of Copernican studies.[54] I shall mention only four most important findings regarding the genesis, the essence and the reception of Copernicus's thought. These matters were not well known or not known at all to T.S. Kuhn (and many other scholars):

1) Since Copernicus studied at the best universities of his time – in Cracow (mathematical sciences and Buridanistic thought), Bologne (canon and civil law), Padua (medicine) – he knew the thought of Buridanists very well (*this influenced the genesis of Copernicus' thought*).
2) Copernicus was a very good methodologist of mathematics (in the ancient, medieval and renaissance meaning, i.e. mathematico-physical sciences, or the so-called exact sciences of our times; he knew all elements of the Hypothetico-Deductive Method of Korespondenzdenken and deliberately applied them, while constructing his two distinct theories described in the *Commentariolus* (ca. 1508)[55] and the *De revolutionibus* (1543). Owing to that, he constructed his theories in accordance with the postulate of the correspondence of theories. His theories are linked to Ptolemy's theory by certain principles of generalised correspondence (similarly to the way, in which relativistic mechanics is linked to the classical mechanics, or quantum mechanics is linked to the classical mechanics);[56] but he was not the first scholar, who applied these methodological tools *(this influenced the essence of Copernicus's thought and its genesis*).
3) The form and time of the Copernican revolution was determined both by internal and external factors in astronomy and physics (*this influenced the genesis and reception of Copernicus's thought*):
    3a) The following five issues were very problematic in medieval and renaissance astronomy: the issue of long-period motions (rotations, revolutions) of the eighth sphere; the problem of equant and Tusi's devices; the problem of incompatibility of predictions of astronomical models with astronomical observations; the problem of calendar delay; and the problem of physical mechanisms (explanations) of astronomical theories, that is, physics of astronomical phenomena. I classify them as the internal factors of astronomy (*this influenced the genesis and reception of Copernicus' thought*).

[53] I chose this topic, because I work at the Instytut Historii Nauki Polskiej Akademii Nauk (Institute for the History of Science, Polish Academy of Sciences), where Copernican research has been conducted continuously since 1954. I am also associated with the Commission for the History of Science of the Polish Academy of Arts and Sciences (established in 1999). The Commission continues the activities of the Commission for the Publishing of Copernicus's Works (founded in 1897), the Bibliographical Commission of the Academy of Arts and Sciences (founded in 1901) and the Commission for the History of Mathematics and Natural Sciences of the Academy of Arts and Sciences (founded in 1910), of which Ludwik Birkenmajer was an active member. See also fn. 37, above.

[54] The results of these detailed interdisciplinary inquiries are given in several works – see especially Kokowski 1996b; 1997a; 1999c; 2001a; 2004; 2006a/2007a (or 2007b); 2009c; 2012a; 2012b; (ed.) 2012e.

[55] The date 1508–1514, was determined by Ludwik Birkenmajer (1900, pp. 70–88) based on historical analyses. Recently, George Borski and Michał Kokowski (2022, pp. 391–393, 408–411) suggest the date 1503–1504, based on the Latin style of Copernicus.

[56] It is a standard strategy used, for example, by Copernicus, Brahe, Galileo, Kepler, and Newton.

3b) The two following ideologies: *Modern Christian Platonico-Aristotelian syncretism* (starting ca. 1450) and *Biblical literalism regarding cosmological matters* (starting ca. 1542) influenced, to the great extent, the reception of his cosmological views in the culture from the 16th to 19th century. I classify them as the external factors in astronomy (*this influenced the reception of Copernicus' thought*).

4) However, no ideologies were able to stop the development of the heliocentric astronomy and physics (Kepler, Galileo, Newton, …), and then rising of general relativity theory (Einstein). All of these issues were constructed using the Hypothetico-Deductive Method of Korespondenzdenken (*this influenced the reception of Copernicus' thought*).

10. The beginnings of T.S. Kuhn's career and his interest in history and philosophy of science

Searching the scientific biography of T.S. Kuhn, I focused my attention on his interest in history and philosophy of science. Therefore, I analysed views of the main scholars, whom he met in his university milieu, that is at Harvard University, including especially James Bryan Conant (1893–1978), the promoter of the Program of General Education in Science at Harvard University, and his collaborators (to whom T.S. Kuhn also belonged), such as I. Bernard Cohen, Philippe Le Corbeiller, Philipp Frank, Gerald J. Holton, Edwin C. Kemble, Frederick G. Kilgour, Leonard Kollender Nash, and also George Sarton (the promoter of new humanism, who was outside this group). I studied Kuhn's readings of many authors, like the members of Conant's group, and other scholars, such as Arthur Ocean Lovejoy, John L. E. Dreyer, Herbert Butterfield, Alexandre Koyré, Otto Struve and William P. D. Wightman, etc., among others.

In consequence, I made many important findings. Let me list here five key ones.

1) The basis for Conant's reform of general education was a moderate version of *new humanism* propagated by George Sarton. (T.S. Kuhn accepted this idea, at least while writing *The Copernicus revolution* and *The Structure of scientific revolutions*.)

2) Taking into account the value (depth and extent) of Sarton's new humanism (propagated by this author from (at least) 1918 to 1956) and its priority, we should replace the terms: "the two cultures of Snow" and "the third culture of Snow" introduced by Charles Percy Snow in 1956–1959 (he did not cite Sarton's works) with the terms "the two Sarton-Snow cultures" and "the third Sarton-Snow culture".

3) Under the influence of James Bryant Conant's group, T.S. Kuhn appreciated, first, the program of the history of ideas by Arthur Lovejoy (Lovejoy 1936 "Introduction", and 1938), and second, the idea of studying history of different sciences in their own historical conceptual categories, displaying their gradual development (in this Alexandre Koyré was T.S. Kuhn's mentor, see Koyré 1936).

4) The topic "the Copernican revolution" was one of the results of T.S. Kuhn's involvement in James Bryant Conant's group. The various aspects of this subject were considered by James Bryant Conant, Philipp Frank, Edwin C. Kemble, Gerald J. Holton and I. Bernard Cohen (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). In his interpretation of Copernican revolution, T.S. 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 John Louis Emil Dreyer (1906, reprinted 1953), and Herbert 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 Alexandre Koyré (1943), Otto Struve (1943) and William Persehouse Delisle Wightman (1950), confirming the existence of this revolution.

5) Answering to these problems, T.S. Kuhn choose a historical synthesis as the literary form of his planned work (since he did not make his own research on Copernican issues). It was fashioned, to some degree, on a book by Herbert Butterfield (1949). At the same time, he wanted to answer to Arthur O. Lovejoy's program of history of ideas, and to the history and philosophy of science, as postulated by the members of James Bryant Conant's group.[57]

11. The critical evaluations of T.S. Kuhn's interpretations of the Copernican revolution by earlier scholars

In order to discourse on the value of Kuhn's interpretations of the Copernican revolution competently, it is necessary to be well acquainted with existing critical assessments made by other scholars hitherto. Fulfilling this requirement, I conducted careful studies in this matter and discovered, with great surprise, that many researchers, who wrote on Kuhn's views (especially his advocates) had all too little knowledge of this topic. Therefore, to give all readers easy access to the problems related to the critical assessments of Kuhn's interpretations, I summarised and listed in chronological order selected aspects of almost fifty most important works published between 1957 and 2001 (this is the broadest elaboration on this subject in existing literature).[58]

12. T.S. Kuhn's own reaction to the evaluations of his understanding of the Copernican revolution and of the structure of scientific revolutions

In order to enter into a rational discussion with T.S. Kuhn's views, it is necessary to be well acquainted with his reaction to the outline of his views, made by the critics and advocates of his views.

Therefore, I summarized his attempts to answer these critiques.[59] The most important fact is that T.S. Kuhn did not value highly his own essays on *The Copernican Revolution*: *Planetary Astronomy in the Development of Western Thought* (1957) and *The Structure of Scientific Revolutions* (1962; 2nd ed. 1970). On contrary, as his greatest scholar achievement he recognized *The Black-Body Theory and the Quantum Discontinuity, 1894–1912* (1987). However, this

[57] For further details see Kokowski 2001a, pp. 15–109, and its English summary (Kokowski 2001a, pp. 316–317; available also online: Kokowski 2001b).

[58] It appears that many scholars criticized competently T.S. Kuhn's views on the Copernican revolution, among others, Doris Hellman (1957), Herbert Dingle (1958), Philip P. Wiener (1958), Curt A. Zimansky (1959), Edward Rosen (1959; 1960; 1965; 1983; 1984), Emmanuel Poulle (1960), Norwood R. Hanson (1961; 1964), Jerome R. Ravetz (1965), Dudley Shapere (1973), Owen Gingerich (1975b) [1973b], Robert S. Westman (1973a; 1973c; 1990, reprinted in 1991 and 1994; 1994), Imre Lakatos, Elie Zahar (1975 [1973]), Fritz Kraft (1975 [1973]), Michael Heilderberger (1976), I. Bernard Cohen (1985), Hans Blumenberg (1987), Peter Barker, Bernard R. Goldstein (1988), Robert S. Westman, David C. Lindberg (1990, reprinted in 1991, 1994), Noel M. Swerdlow (1990; 1997; 2001), Howard Margolis (1993), Ernan McMullin 1993 [1990], Maarten Franssen (1993), Michał Kokowski (1996b; 1997; 1998; 2000), Jed Z. Buchwald, George E. Smith (1997), Kurt Gottfried, Kenneth G. Wilson (1997), Paul Hoyningen-Huene (1997), Richard Rorty (1997a; 1997b), Xiang Chen, Hanne Andersen, Peter Barker (1998), John L. Heilbron (1998), André Goddu (1996; 2001), Alan D. Sokal, Jean Bricmont (1997/1998), Noel M. Swerdlow (1997; 2001), Steven Weinberg (1996; 1998) and Kenneth G. Wilson, Constance Barsky (2001a [2000a]; 2001b [2000b]; 2001c [2000c]).

For further details see Kokowski 2001a, pp. 113–132, Appendix 4 (Kokowski 2001a, pp. 238–313) and Chapter III (Kokowski 2001a, pp. 145–158).

[59] See Kokowski 2001a, pp. 133–143. Though strictly speaking, regarding the Copernican subject, this elaboration is short, yet it is the only one of its kind in the literature.

monograph was much less popular than other Kuhn's books because it dealt with the history of quantum mechanics and the author abandoned his attractive jargon used in the *SSR* (*paradigms*, etc.).[60]

Furthermore, it is necessary to remember that T.S. Kuhn disagrees with Kuhnians (mainly sociologists) who, according to him, use his work 'uncritically' to argue against the Mertonian approach:

> In the literature of sociology of science, the value system has been especially discussed by R. K. Merton and his followers. Recently that group has been repeatedly and sometimes stridently criticized by sociologists who, drawing on my work and sometimes informally describing themselves as 'Kuhnians,' emphasize that values vary from community to community and from time to time. In addition, these critics point out that, whatever the values of a given community may be, one or another of them is repeatedly violated by its members. Under these circumstances, they think it absurd to conceive the analysis of values as a significant means of illuminating scientific behavior (Kuhn 1977, p. xxi).[61]

> (...) the [radical] transformation [of the image of science during the last quarter century] has had a by-product-centrally philosophical, but with implications also for the historical and sociological study of science-that frequently troubles me, not least because it was initially emphasized and developed by people who often called themselves Kuhnians. I think their viewpoint damagingly mistaken, have been pained to be associated with it, and have for years attributed that association to misunderstanding (Kuhn 1992, p. 1).[62]

> I am among those who have found the claims of the strong program absurd: an example of deconstruction gone mad. And the more qualified sociological and historical formulations that currently strive to replace it are, in my view, scarcely more satisfactory. These newer formulations freely acknowledge that observations of nature do play a role in scientific development. But they remain almost totally uninformative about that role - about the way, that is, in which nature enters the negotiations that produce beliefs about it (Kuhn 1992, p. 9).[63]

> A few years ago I [Freeman J. Dyson] happened to meet Kuhn at a scientific meeting and complained to him about the nonsense that had been attached to his name. He reacted angrily. In a voice loud enough to be heard by everyone in the hall, he shouted, "One thing you have to understand. I am not a Kuhnian" (Dyson 1999, p. 16).

13. Author's critique of T.S. Kuhn's interpretations

Finally, I formulated my own critique of T.S. Kuhn's interpretations of the Copernican revolution. I followed subtle argumentation by some of the early reviewers of Kuhn's first book (*CR*), who

[60] See Klein, Shimony, Pinch 1979; Kuhn *et al.* 1996; Marcum 2005, p. 23; Nakayama 2007.

[61] According to T. S. Kuhn (1977, p. xxi), the *locus classicus* of this kind of critique is the work by S. B. Barnes and R. G. A. Dolby (1970).

[62] See very good discussion about the emergence of anti-Mertonian sociology of science by John H. Zammito, especially chapter 5 "How Kuhn became a sociologist" (Zammito 2004, 123–150), and chapter 6 "All the Way Down: Social Constructivism and the Turn to Microsociological Studies" (Zammito 2004, 151–182).

[63] See also Kuhn *et al.* 1997.

were, in principle, negatively disposed towards it, namely L. Doris Helmann (1957), Herbert Dingle (1958), Edward Rosen (1959), Curt A. Zimansky (1959) and Emmanuel Poulle (1960), and the later adherents of the critical attitude such as, for instance, Noel M. Swerdlow and Otto Neugebauer (1984).

Speaking briefly, I oppose the opinion expressed not only by a large number of early reviewers of the *CR*, including James R. Newman (1957), Hugo N. Swenson (1957), Herbert Butterfield (1958), Michael A. Hoskin (1958), Harry Woolf (1958) and Angus Armitage (1959), but also Robert S. Westman (1994) – that the *CR* was "tightly written and brilliantly argued" (Westman 1994, p. 79). Why is that? Since I have shown that Kuhn's interpretations of the Copernican revolution, given in the *CR* and similarly in the *SSR*, not only have some substantive and formal (interpretative) defects already mentioned by earlier scholars, but are also burdened by many other serious structural and substantive faults. Namely, too often T.S. Kuhn committed serious errors that I define by the terms:

1) "a narrative sophism",
2) "an incoherentness of narrative returns",
3) "an inappropriately applied *hermeneutics* insensitive to the mathematical aspects",
4) "the critical mass deficiency effect (not enough information for a certain interpretation of the given issue".

Below I explain these terms and link them with issues.[64]

First, "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 applies this rhetorical strategy by, among other things:

(A) suggesting strongly that one can adequately understand all the complications of the Copernican revolution without profound independent research and based solely on the studies by other scholars (Kokowski 2001a, pp. 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 and empirical problems considered therein (Kokowski 2001a, pp. 161–164);

(C) persuading the reader earnestly to accept a dichotomous vision of cultivating the exact sciences by the means of opposing pragmatic empirical values (belonging to the core of these sciences) with aesthetic values (those that, at best, are on the peripheries of these sciences) (Kokowski 2001a, pp. 164–166).

(D) making the inaccurate, though suggestive, comparison between Copernicus's and Ptolemy's astronomical theories in the light of the category of "utility" (Kokowski 2001a, pp. 166–168);

(E) making a too critical assessment of Copernicus's theory of motion (Kokowski 2001a, pp. 168–170);

(F) strongly persuading the reader that the form and timing of the Copernican revolution were determined by factors external to astronomy (see Kokowski 2001a, pp. 170–171),

[64] I use here the extensive excerpt (with slight changes) of the English summary of my monograph — see Kokowski 2001b, pp. 321–325 (available online).

including (G) humanism (Kokowski 2001a, pp. 171–174) and (H) neoplatonism (Kokowski 2001a, pp. 174–178) – at this point Kuhn claims, among other things, that it was the neoplatonic intuition of harmony that was a 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 mechanisms of evolution and revolution in science (Kokowski 2001a, pp. 178–182).

As an illustration, I briefly consider the case (F): „Were the form and timing of the Copernican revolution determined by the factors external to astronomy?" One of the crucial theses of the *CR* (1957a), adopted by T.S. Kuhn from Burtt (1932) and Butterfield (1949), was a positive answer to the above question. However, T.S. 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 that had been recorded by competent astronomers for over nearly 2000 years since the times of Timocharis, Hipparchus and Ptolemy until the Renaissance. Therefore, the long-period models of certain astronomical phenomena were intensely considered by the Renaissance astronomers. The need for reform of the Julian calendar was one of the aspects of this complicated issue.

Second, the structure of the error labelled "an incoherentness of narrative returns" 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 to the point already raised, pt_1 in a new context, he expresses a thesis t_2 that is different to the thesis t_1. The latter thesis either weakens the former thesis or negates it in part, or in whole. And, he propounds them all with the same rhetoric fervour; being a sophist he argues for the different and often antagonistic theses with equal force, however always wanting to persuade his readers of them, invariably. He makes such errors in the following cases:

(A) while arguing widely for the rational research program on "the Copernican topic" (that is of the genesis, substance and reception of the Copernican theory) and finally finding an irrational solution to this problem based on mystical neoplatonic philosophy or/and psychology (Kokowski 2001a, pp. 183–185);

(B) while making an inconsistent analysis of the hierarchy of the fundamental issues of pre-Newtonian astronomy (Kokowski 2001a, pp. 185–186);

(C) while making an incoherent analysis of the problem whether renaissance and ancient astronomers had access to the same set of theoretical means and observational data (Kokowski 2001a, pp. 186–190);

(D) while making an inaccurate analysis of the connections between astronomy, cosmology and physics (Kokowski 2001a, pp. 190–194);

(E) while making an inaccurate analysis of the dependence of the historical process known as "the Copernican revolution" on the neoplatonic philosophy (Kokowski 2001a, pp. 194–195);

(F) while making an inaccurate analysis of the dependence of Copernicus's and Copernicans' thought on scholastic concepts (Kokowski 2001a, pp. 195–200).

As an illustration, I briefly present the case (C). The *CR* gives two answers to the question mentioned.

According to the first, Copernicus and his contemporaries possessed, in principle, the same sort of observational data and theoretical means (Kuhn 1957a, pp. 99 & 131) as the ancient astronomers used to had:

> 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 (Kuhn 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 (Kuhn 1957a, p. 131).

However, according to T.S. Kuhn's second interpretation (Kuhn 1957a, pp. 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] (Kuhn 1957a, p. 138).

The last of T.S. 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. For instance, the medieval and renaissance astronomers, as opposed to Ptolemy, considered non-uniform long period models of certain astronomical phenomena that were explained by the motions of an eighth sphere to be true, 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 had not been used by Ptolemy at all. Copernicus continued his interests in them, searching for his astronomical theory.

Third, many of T.S. Kuhn's errors sprang from an inappropriately applied *hermeneutics* insensitive to the mathematical aspects, although it is obvious that Copernicus's theory is profoundly mathematical. The above is clear just from a careful reading of Nicolaus Copernicus' *De revolutionibus orbium coelestium libri VI* (Nuremberg 1543), since the motto of this work is the following phrase: "Ageômetrètos mèdeis eisitô" ("Let no one ignorant of geometry enter"). Also, in the preface addressed to the Pope Paul III, to whom Copernicus' *opus magnum* is dedicated, we read: "Mathemata mathematicis scribuntur" ("Mathematics is written to mathematicians") – compare also above points (B), (C), (D).

Fourth, Kuhn's interpretations of the Copernican revolution show the effect which, by analogy with "the effect of a deficiency of a critical mass for a nuclear chain reaction" (that is when one cannot exceed the minimum mass of fission material needed to initiate a nuclear chain reaction), I call "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. 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, Copernicus's own thought and the thought of his time, and, on the other hand, with the art of scientific investigation in the field of the exact sciences. Besides, although the young Kuhn was one of the first among Sarton's new humanists, he was not clearly conscious of the principle of the general methodology of the history of science and the exact sciences, while writing the *CR* (1957) and *SSR* (1962). These are the reasons for the above-mentioned errors made by Kuhn. However, after many years of critical considerations, Kuhn radically developed this consciousness on the pages of the *ET* (1977). Unfortunately, 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).

My research has also revealed the following peculiar strategy assumed by the subsequent editors of the *CR*. Namely, for many years they continue to include (on the cover) excerpts taken from very positive reviews by James R. Newman (1957), Harry Woolf (1958) and Herbert Butterfield (1958) (published in the "Scientific American", the "Isis" and the "American Historical Review", respectively) and persist in neglecting more competent, but critical, reviews by L. Doris Hellman (1957), Herbert Dingle (1958), Edward Rosen (1959), Curt A. Zimansky (1959) and Emmanuel Poulle (1960) (published in the "Observatory", the "Scripta Mathematica", the "Speculum" and the "Revue d'histoire des sciences et de leurs applications", respectively). Finally and most importantly, they completely ignore the achievements in the history of science of the last decades! In doing so, they have been strengthening the myth of the great epochal work, for which the *CR* (1957) cannot be recognised in any way.[65]

In summary: I have shown that *CR* is full of exaggerated accents and inconsistencies, omissions and errors. First of all, it is impossible to understand the Copernican revolution well without detailed knowledge about history of astronomy and physics. I mean not scientific ideas only but also mathematical language. Kuhn did not care enough about such details. For example, in describing the geocentric cosmology, Kuhn, in contrary to historical knowledge, introduced the idea of two spheres, instead of eight spheres at least. He also believed – in line with many previous historians and philosophers of science – that Copernicus's theory and Ptolemy's theory were geometrically and observationally equivalent with the exception of some details, e.g., the stellar parallax; and, that Aristarchus of Samos had already developed a heliocentric theory. Also, Kuhn claimed that Copernicus formulated his astronomical theory in response to a supposed crisis of astronomy in his times, measured, among other things, by the increasing number of epicycles in Ptolemy's theory, which did not improve the prediction accuracy anyway. Thus, Kuhn was wrong on these crucial points, which relate to the question of removal of equants and the question of the so-called long-term movements of the eighth sphere studied since Ptolemy, as well as the problem of the empirical accuracy in Ptolemy's theory and in Copernicus's theory. Such specialized issues aside, Kuhn was compelled to explain the changes introduced by Copernicus in terms of external factors in the context of empirical philosophy, such as values of esthetical harmonies and the philosophy of Neoplatonism.

In the *SSR* (1962, 2nd ed. 1970) Kuhn developed this picture: He formulated a general scheme for development of science: a scientific community, that is a group of scientists, agrees on accepting one paradigm (a complex entity "consisting of theoretical framework, a set of exemplary problems-and-solutions, puzzling phenomena awaiting solution, instruments, standards for gauging whether

[65] For further details see Kokowski 2001a, pp. 159–201 (in Polish).

puzzles have been solved, and cognitive values for researchers to seek after")[66]. And the paradigm defines regular science, which is being developed evolutionary and solves mere scientific puzzles; description of normal science is provided in textbooks; solving scientific puzzles breeds scientific anomalies (in the context of the paradigm) and then a scientific crisis (a situation when minor adjustments do not solve the problem); then a scientific revolution occurs when the crisis is being solved through a new paradigm, which defines a new regular science solving new scientific puzzles; the new paradigm is incommensurable with the previous one in terms of ideas, concepts, theories and methods; so, science thrives on paradigm shifts.[67]

The history of Copernican revolution was a paradigmatic example of Kuhn's scheme of science development. In this way, the weaknesses and errors of the *CR* were transferred to the *SSR*.

Moreover, these weaknesses have been further magnified: Kuhn openly denied the existence of the correspondence principle linking the competing theories (for example relativity theories with classical mechanics). This negation was a basis of Kuhn's thesis about the incommensurability of competing theories, impossibility of their mutual translation and application of uniform evaluation standards. Additionally, Kuhn linked this thesis with the inability to reach agreement between adherents of competing theories, and believed that they became such adherents not because of logical, rational arguments, but because of aesthetic arguments. And this radical change of views happened due to the sudden irreversible psychological mechanism called the Gestalt switch.[68]

For Kuhn, the paradigmatic example of the concept of incommensurability was the idea of a planet, understood differently in geocentric and heliocentric theories. Kuhn, however, knew

[66] Jacobs 2006, p. 164.

[67] Hence Kuhn's image of the development of science is a derivative of Fleck's image of the development of medicine. For similarities and differences between these images, see Jacobs 1987; 2002; 2006; Brorson, Andersen 2001; Oberheim, Hoyningen-Huene 2018, section 2.2.2–3; Jarnicki, Greif 2022 (including important references mentioned on p. 5, fn. 2).

From my point of view (Kokowski 2001, pp. 106–107, fn. 9), Kuhn was, to a large extent, a continuator of Fleck's thought, although he sometimes diminished this relationship, see Kuhn 1970, pp. vi-vii; (1976) 1979, pp. vii-viii; Kuhn, Sigurdsson 1990/2016; Kuhn, Baltas, Gavroglu, Kindi 1997. For example: "[…] It was I think in Reichenbach's *Experience and Prediction (…)* that I found a reference to a book called *Entstehung und Entwicklung einer Wissenschaftlichen Tatsache*. (...) I said, my God, if somebody wrote a book with that title – I have to read it! These are not the things that are supposed to have... they may have an *Entstehung* but they are not supposed to have an *Entwicklung*. I don't think I *learned* much from reading that book, I might have learned more if the Polish German [sic! – M.K.] hadn't been so very difficult. But I certainly got a lot of important reinforcement. There was somebody who was, in a number of respects, thinking about things the way I was, thinking about the historical material the way I was. I never felt at all comfortable and I still don't with «thought collective.» It was clear it was a group, since it was collective, but the model was the mind and the individual. I just was bothered by it, I could not make use of it. I could not put myself into it and found it somewhat repugnant. That helped keep me at somewhat at arm's length, but it was very important that I read that book because it made me feel, all right, I'm not the only one who's seeing things this way" (Kuhn, Baltas, Gavroglu, Kindi 1997).

However, the alleged Polish-German of the author of *Entstehung und Entwicklung einer Wissenschaftlichen Tatsache* (the book published in Basel, Switzerland, and the author lived then in Austrian Monarchy where he had constant contact with the German language) or/and, as pointed out by Daniel Goldman Cedarbaum (1983, p. 199), Kuhn's poor knowledge of German biochemical terminology, did not prevent Kuhn from placing many competent comments in the margins of the copy he read.

[68] See fn. 48, above.

nothing about the general principles of correspondence linking Copernicus's theories with Ptolemy's theory.

Summing up: from the perspective of the knowledge of the general reader, the Copernican revolution took place, as was explained by T.S. Kuhn with the help of his conceptual grids and of the correlated factography selected by him. However, from the perspective of expert knowledge, there was the Copernican (r)evolution, explained by M. Kokowski with the help of his conceptual grids, including the hypothetico-deductive method of Korespondezdenken, and of the correlated factography selected by him.[69]

14a. Conclusion: Critique

In the light of today's knowledge about the history of science, it should be emphasised that T.S. Kuhn's interpretations of the Copernican revolution, given both in the *CR* (1957) and *SSR* (1962), have, in many respects, a popular character only.[70] This conclusion should shock no-one since T.S. 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 (Kuhn 1957a, p. ix).

However, according to me, the essayistic style, so characteristic for T.S. Kuhn's own writings, though very suitable for popularising, is inappropriate in developing a detailed study of the history and philosophy of science. Also, it seems that albeit his unusually famous books are written in lively language, they contain too many major errors and omissions. Thus, the interpretations described in the books cannot be 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 as well, who still accept T.S. Kuhn as the eminent expert on the so-called Copernican revolution. The same is true for famous physicists, such as Steven Weinberg (1998), who think, that T.S. Kuhn's idea of scientific revolution puts in principle a good construction on the same beginning of modern science.

But, on the basis of my monograph about T.S. Kuhn and his interpretations of the Copernican revolution, which present the results of my research, I cannot say anything else. Thus, I suggest that the advocates of T.S. Kuhn's views read carefully my book and reflect on it with appropriately. After all, it is Thomas S. Kuhn, who in his book *ET* (1977), written after many years of critical reflections about *CR* (1957) and *SSR* (1962), and dedicated, in its great part, to the methodology of 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

[69] See Kokowski 1993; 1996; 2001; 2004; 2015e. Note, a part of this method is the "eraser strategy" – see section 7, above.

[70] I use here the extensive excerpt (with slight changes) of the English summary of my monograph — see Kokowski 2001b, pp. 325–326 (available online).

> 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 (Kuhn 1977, pp. 16–17).

However, the methodological mind that was clearly revealed here – we see how mature it is! – did not influence the subsequent editions of the *CR* (e.g. the seventh edition of 1985 called "renewed" and the subsequent reprints based on it, e.g. 1995; 1997) or the subsequent reprints of the *SSR* (the second edition of 1970). Although Kuhn knew, to some degree, the enormous criticism of his interpretations of the Copernican revolution, he simply ignored it.

On the other hand, his methodological mind 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*.[71] It is vital to remember that in this monograph Kuhn abandoned all his revolutionary terminology and strategy elaborated on in the *SSR*. Moreover, it is the *BBT*, and not his earlier books, *CR* or *SSR*, that Kuhn prized most highly among his works in the field of history of science.[72]

Furthermore, though I am not an advocate of the Kuhn's and Kuhnian visions of science, I am not surprised that many authors (especially sociologists of scientific knowledge) could find in T.S. Kuhn's writings a ground to express many too radical or simply absurd views. This was caused by the actual incoherence in his views (see above "a narrative sophism", "an incoherentness of narrative returns", "an inappropriately applied *hermeneutics* insensitive to the mathematical aspects", and "the effect of the deficiency of the critical mass of information for a certain interpretation of the given issue").

14b. Conclusion: Partial defence

While making such critical assessment, I am far from negating T.S. Kuhn's achievements in the field of history and philosophy of science absolutely. In a partial defence of Kuhn's interpretations of the Copernican revolution, I would like to present three arguments.[73]

First, these interpretations were developed at the level of a general reader, and not of for specialists.

[71] However, as early as 1954, according to Stephen G. Brush, who was then attending Kuhn's "History of 19th century thermodynamics" seminar at Harvard University, Kuhn had established himself as a experienced historian and methodologist of the history of thermodynamics and chemistry, see Brush 2000, especially pp. 39–46.

[72] For further details see Kokowski 2001a, pp. 202–207.

[73] I use here the extensive excerpt (with slight changes) of the English summary of my monograph — see Kokowski 2001b, pp. 326–328 (available online).

Second, to a large degree, T.S. Kuhn's error was to take his data from earlier scholars. Thus, to a great extent, his faults reflect the state of contemporary research, with the important reservation that he completely overlooks the works by Ludwik Antoni Birkenmajer and Aleksander Birkenmajer, and only nominally mentions that by Edward Rosen. [74]

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

Furthermore, it is T.S. Kuhn's achievement that he attracted the attention of an enormous number of readers and focused it on two things. First, on the existence of a branch of knowledge called "the history of science". Second, on the value of research in the field for the development of philosophy of science. It is necessary, however, to notice that earlier scholars, George Sarton, James Bryant Conant and the members of J. B. Conant's group especially, had already emphasised this quality of the history of science. Moreover, like other researchers on T.S. Kuhn's thought, I am of the opinion that his books, including the *CR* and *SSR* so criticised by me, 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, excluding Kuhn's own philosophy. The *CR* and *SSR* not only have undoubted literary qualities, but also contain certain substantial values. For 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 *deconstruction*, *the strong programme of sociology of knowledge*, *ethnology*, and *social constructivism*.

It was for this reason that in 1992, I made careful studies of T.S. Kuhn's works, being particularly interested in physics, and the philosophy and history of the so-called exact sciences. My first impression was very similar to Steven Weinberg's (1998) and Noel M. Swerdlow's (2004) [1997], whose achievements I admire very much. Later, however, in 1994, two years before Thomas S. Kuhn's death, when I undertook, as the subject of my PhD thesis, a critical estimation of his interpretations of the Copernican revolution, and made careful observations of these interpretations, my comprehension of his thought changed considerably, which finds full expression in my papers and monographs (including Kokowski 2001a).

However, in spite of the criticism, I am of the view that T.S. 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 caution against uncritical approach. Moreover, these books are simply perfect for studying 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. [75]

[74] See L. A. Birkenmajer 1900; 1901; 1914; 1917; 1923; 1924; A. Birkenmajer 1936a; 1936b; 1953; 1954; and Rosen 1939.

[75] In this point, I definitively differ from Mario Biagoli (2012, p. 499) and John Heilbron (2012), see section 3, above.

15. An epilogue: A postulate for a revival of *New Humanism*

Undoubtedly, owing to his still read and reprinted books, T.S. Kuhn has called the attention of an enormous number of readers to the mere fact of the existence of the discipline named the history of science and has shown the value of it for research on the field of the philosophy of science and sociology of scientific knowledge.[76]

However, while pursuing this line, one must keep in mind that the literary attractiveness of the produced texts, though they may draw attention of a broad body of readers, and historico-philosophical genuineness does not need to go hand in hand. And the latter is – in my opinion – the most important in dealing with the history and philosophy of the so-called exact sciences.

With all the above-mentioned reasons that differ, to some degree, from those given by historians of science such as James R. Newman (1957), Hugo N. Swenson (1957), Herbert Butterfield (1958), Michael A. Hoskin (1958), Harry Woolf (1958), Rober S. Westman (1994) and Noel M. Swerdlow (2004) [1997] on the one hand, and, by physicists such as Werner Heisenberg (1973b), Виталий Лазаревич Гинзбург (Witalij Lazarewicz Ginzburg) (1976), Steven Weinberg (1998) and Kenneth G. Wilson, Constance Barsky (2001a [2000a]) on the other hand – I think that the Thomas S. Kuhn's controversial views will continue to stimulate the development of the history and philosophy of science, including the mere understanding of the historical process named "the Copernican revolution", especially. Nevertheless, we should not fall into the Kuhnian-centrism, so characteristic of all "Kuhnians". Indeed, let us consider seriously the great achievements of the 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 is clear that a great deal of what should be central to detailed professional interpretations of the Copernican revolution bears in fact a limited connection to T.S. Kuhn's interpretations, which were, as he himself stated in the *CR*, intended for the general reader.

Seeing this clearly, the contemporary researchers of the so-called Copernican revolution formulated more thorough, detailed interpretations of this historical process.[77]

Furthermore, from the perspective described in the present paper, it is obvious that sociological interpretations can be a useful tool in explaining the genesis and the reception of scientific views, including those of Copernicus. However, the choice of a hermeneutics of research, based only on sociological grounds, causes this hermeneutics to be blind to other important, or much more important, aspects of the development of science. Therefore, it should be clear that *purely sociological interpretations* are not able to grasp the essence of scientific views.[78]

Finally, the case of *the Copernican (r)evolution* shows that, on the one hand, science is not a mere text, facts are not mere social constructs, but, on the other hand, science is always a profound historical and social process. In consequence, in order to write about the history of science, and more generally, on science studies, competently, one should merge the workshops both of the humanists (historians, philosophers, sociologists, etc.) and the scientists.

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[76] See section 1, above.

[77] See, for example, Kokowski 2004; 2009; De Pace 2009; Goddu 2010; Westman 2011; Vesel 2014.

[78] This is the case, for example, of the alleged finding of the grave of Nicolaus Copernicus in Frombork's Cathedral (Warmia, Poland) in 2005–2006 (see Bogdanowicz *et. al.* 2009). In fact, it is only a sociological and journalistic myth caused by the use of the hermeneutics of research which is too primitive to solve the problem — see Kokowski 2015c; 2015d; Kokowski (ed.) 2015b.

Let us notice, however, that the above thesis is not new, since it was, at the very least, proclaimed by George Sarton from 1918 to 1956, in the context of *New Humanism*. And it was this idea that was alive in James Bryant Conant's group for the general education reform at Harvard University, where one of members was no-one else but Thomas S. Kuhn.

Unfortunately, the era of the amazing career of the left-Kuhnian views in the academia during last sixty years was, in the same time, the era of gradual decay of the ideals of Sarton's *New Humanism*. In consequence, these ideals are nearly entirely forgotten now. I am of opinion that it is high time to make a true revival of these ideals in our university curriculums, since our societies need humanists, who will not be ignorant in natural sciences, exact sciences and technology, and scientists who will not be technocrats.[79]

Hence, a good familiarity with T.S. Kuhn's thoughts (their genesis, contents and reception) creates a solid foundation for avoiding that sort of arrogance and naivety in science and technology studies (including the sociology of scientific knowledge especially) that fuels the fire of *Science Wars*.[80]

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[79] Following this lead, I formulated the model of the university of new humanism, according to which the key role in the structure of the university should be played by an interfaculty institute or department of the history of science and science-of-science; such a unit should promote interdisciplinary thinking, Sarton's new humanism and a critical attitude towards the tyranny of scientometrics and bibliometrics – see Kokowski 2015a.

[80] See Slezak 1994a; 1994b; Matthews (ed.) 1998; Sokal 1996a; 1996b; Sokal, Bricmont 1997/1998; Sokal 2008. In this context, it is still worth to remember the classical considerations of Florian Znaniecki of 1940 about "Sociology of Scientific Knowledge" (in Znaniecki 1986, pp. 1–22, especially pp. 1–6), which are free of these faults.

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