Chapter 5
Kuhn's Paradigm of Paradigms: Historical and Epistemological Coordinates of *The Copernican Revolution*

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> *I shall not try to explain here the reasons and causes that produced the spiritual revolution of the sixteenth century. It is for our purpose sufficient to describe it, to describe the mental or intellectual attitude of modern science.*
>
> Alexandre Koyré (1943)

> *It was a revolution beside which the French Revolution was a child's play, a world struggle beside which the struggles of the Diadochi appear insignificant. Principles ousted one another, intellectual heroes overthrew each other with unheard-of rapidity [...] All this is supposed to have taken place in the realm of pure thought.*
>
> Karl Marx and Friedrich Engels (1846)

The Historical and Epistemological Centrality of Copernicus for Kuhn

The Renaissance astronomer Nicholas Copernicus, his scientific achievement, its impact and the reception of the heliocentric planetary theory occupied a special place in Thomas Kuhn's reflections on science, both historical and philosophical. Kuhn often referred to Copernicus as the first of a progeny of genial scientists; modern heroes whom he deemed to have produced major shifts in epistemic developments.

> [T]he major turning points in scientific development [are] associated with the names of Copernicus, Newton, Lavoisier, and Einstein. (SR, 6)[1]

In his classic of historical epistemology, *The Structure of Scientific Revolutions*, Kuhn constantly referred to Copernican astronomy as an insightful case apt to illustrate his basic notions of 'paradigm' and 'scientific revolution.'

As a matter of fact, *Structure* was preceded by a monograph on this crucial historical case, *The Copernican Revolution* (1957). Kuhn probably composed the two works in parallel.[2] At least he had conceived them together. In fact, as early as 1952 he had successfully applied for a Guggenheim fellowship, which he wanted to use to complete a monograph on the Copernican issue along with another one on scientific revolutions in general for the *International Encyclopedia of Scientific Revolutions* (Marcum 2005, 13). Evidently, *Copernican Revolution* and *Structure* are the two sides of one and the same endeavor. The historical side was a preparation and a support for philosophical speculations while the theoretical one guided the historical inquiry and was implemented on the latter's basis. On this purpose, Noel M. Swerdlow remarked that

> *The Copernican Revolution*, Kuhn's first published attempt at an answer [to the problems of methodology of scientific research], may be understood as a great case history of one of the monumental changes in the history of science in order to provide an explanation of how so great a revolution happens. In this sense, it is his first scientific revolution. (Swerdlow 2004, 75)

The Copernican turn in planetary astronomy served for Kuhn as a lense through which later intellectual breaks in the history of science could be understood. In this sense Copernicus was Kuhn's παράδειγμα παραδειγμάτων, the paradigmatic case of all paradigms. "What he [Kuhn] needed was a historical exemplar—so John Heilbron—He found it in the Copernican revolution. The story of the shift from geo- to heliocentrism as he simplified it for pedagogical purposes, from Ptolemy and Aristotle to Copernicus and Newton, made a superb play for his theater of warring paradigms. The two opposite systems straightforwardly made two different worlds" (Heilbron 1998, 508).

[1] I will refer to *The Structure of Scientific Revolutions*, Kuhn (1996), hereafter cited as SR followed by the page number in this third edition. Similarly, I will use CR as an abbreviation for *The Copernican Revolution*, Kuhn (1959).

[2] Cf. Kuhn, "Preface" to the 1962 edition of *The Structure of Scientific Revolutions*. SR (vii): "The essay that follows is the first full published report on a project originally conceived almost fifteen years ago."

Copernicus was not just the protagonist of one among many revolutions. Rather, he became *the symbol* of the Scientific Revolution. As a consequence, Kuhn's first book cannot be read, understood and criticized *solely* from the viewpoint of history. *The Copernican Revolution* is a point of departure for a correct assessment of his philosophy of science.

Kuhn stated that history of science and epistemology are two entangled genres, albeit separated. They are closely inter-related although historians and philosophers belong to two different disciplinary fields and have different goals. Indeed, the former construct plausible narratives while the latter seek something that is "true at all times and places."[3] In Kuhn's *curriculum vitae* the two professions coexisted, as he himself observed in a biographical note, in the talk "The Relations Between the History and the Philosophy of Science," delivered in 1968:

> To say that history of science and philosophy of science have different goals is to suggest that no one can practice them both at the same time. But it does not suggest that there are also great difficulties about practicing them, alternately, working from time to time on historical problems and attacking philosophical issues in between. Since I obviously aim at a pattern of that sort myself, I am committed to the belief that it can be achieved. (Kuhn 1977, 5)

Surprisingly, in this passage Kuhn downplayed the dependency of the historical moment on the epistemological or *vice versa*. He presented the relation between the two fields of investigation as a thematic overlapping, as an "interdisciplinary" instead of "intra-disciplinary" relation. Copernicus was the author of one scientific upheaval, if seen from a historical perspective, but also *the model* revelatory of the structure *of any scientific revolution*, from the universalizing viewpoint of philosophy.

Kuhn was not the first who allotted to Copernicus the role of a founding father of modern science. On this account, he mostly relied on Alexander Koyré, one of the *innovators* of the history of science whom he openly acknowledged in the preface to *Structure*.

It should be immediately remarked that the reception of Koyré's historiography played an important role in the ideological confrontations of the Forties and the Fifties dividing the world into two camps, west and east of the *Iron Curtain*. In influential publications on the history of early modern science, such as *Études galiléennes* (1939) or the later *From the Closed World to the Infinite Universe* (1957), Koyré explicitly offered a *spiritual* conception of the Scientific Revolution as descending from the *heavens* (both literally and symbolically), which he

[3] Kuhn (1977, 6), from "The Relations Between the History and the Philosophy of Science."

explicitly opposed to the socio-economical and technological accounts of scientific advance proposed by Marxist scholars. As Yehuda Elkana put it in a colorful way, "[Koyré's] studies became a paradigm for history of science as history of disembodied ideas."[4] Since Kuhn's theory of paradigms and of revolutions represented in many respects a generalization (an *epistemologization*) of Koyré's conception of the Scientific Revolution,[5] *Structure* was enhanced by participating of the Koyréan symbolic capital. Therefore, before discussing Kuhn's Copernicus, it will be expedient to consider some key elements of the Cold War mentality affecting the history of science of those years. I will especially point out the Marxist challenge that made the Koyréan approach appear as a viable counter-program in the Anglo-Saxon West.

Koyréan Commitment

By employing historical notions such as 'Scientific Revolution' and 'Copernican Revolution,' Kuhn revealed himself as a 'son of his age,' a reader and follower of Koyré, whom he acknowledged in *Structure* alongside others like Anneliese Maier and Arthur O. Lovejoy (SR, *Preface*, viii). The choice of these authors is by no means casual. All of them were *historians of ideas* investigating the abstract entities of theory as independently as possible from material aspects. In an entry on "The History of Science" for the *International Encyclopedia of the Social Sciences* (1968), Kuhn pitted the Duhemian school, in which he included Koyré, against historiography exposed to Marxist influences. The asymmetry of his treatment of (and judgment on) the two schools is striking.

On the one hand, he extolled the French conservative historian of science Pierre Duhem as capable of "disclosing" new prospects, namely the historical singularity of medieval and Renaissance science. Duhem's reconstructions of medieval history, so Kuhn, shed light on the ground out of which "the *new science* sprang." Kuhn added that, "more than any other, that [Duhemian] challenge has shaped the modern historiography of science. The writings which it has evoked since 1920, particularly those of E. J. Diksterhuis, Anneliese Maier, and especially Alexandre Koyré, are the models which many contemporaries aim to emulate" (Kuhn 1977, 108). One should not be deceived by the apparent facticity of the statement. The reference to the alleged success of Duhem's school is prescriptive. Kuhn counted himself as one of the "contemporary emulators" of

[4] Elkana (1987, 115). Yet, according to Elkana, the contextual awareness of Koyré's historiography was the indirect source of post-Kuhnian historical sociology of science. Elkana (1987, 144): "Koyré *genuit* Kuhn; Kuhn (and Merton and a few others) *genuerunt* the Historical Sociology of Scientific Knowledge." I will discuss Kuhn's sociology *without society* later.

[5] For a brief overview of Koyré's idea of the Scientific Revolution, see Hall (1987).

the medievalist. In an article appearing in the *Études d'épistémologie génétique* (1971), Kuhn explicitly committed himself to that French legacy by mentioning Koyré as "the man, who, more than any other historian, has been my *maître*."[6]

On the other hand, as the 1968 encyclopedia entry goes on,

> Still more recently, one other set of influences has begun to shape contemporary work in the history of science. Its result is an increased concern, deriving partly from general history and partly from German sociology and Marxist historiography, with the role of nonintellectual, particularly institutional and socioeconomic factors in scientific development. Unlike the ones discussed above, however, these influences and the works responsive to them have to date scarcely been assimilated by the emerging profession [of the historian of science]. For all its novelties, the new historiography is still directed predominantly to the evolution of scientific ideas and of the tools (mathematical, observational, and experimental) through which these interact with each other and with nature. Its best practitioners have, like Koyré, usually minimized the importance of nonintellectual aspects of culture to the historical developments they consider. […] As a result, there seems at times to be two distinct sorts of history of science, occasionally appearing between the same covers but rarely making firm or fruitful contacts. (Kuhn 1977, 109–110)

Not only does Kuhn side with Koyré's critique of the materialist excesses of the Marxist historians of science, but also treats the two approaches, the intellectual and the socio-economical, as "incommensurable paradigms," making virtually no contacts. Again a descriptive-sounding statement has a prescriptive intention.

It seems appropriate to quote here Roy Porter's comment on the ideological divisions of Cold War history of science:

> As part of the rejection of everything Marxist in the years of the Cold War, Anglo-American history of science was to distance itself from all such concerns with the social roots and even the social fruits of science. Instead, from the 1950s it became profoundly fascinated with the internal intellectual challenges posed by science. (Porter 1990, 35)

[6]Kuhn (1977, 21). The article is entitled "Concepts of Cause in the Development of Physics."

Kuhn makes no exception. In this regard his work, especially "*Structure* does not so much transcend the Cold War mentality as expresses it in a more abstract, and hence more portable, form" (Fuller 2000, 6).

The Other Side of the Ideological Divide: Marxist HPS

In the history of science, Cold War ideological confrontations famously began at the *International Congress of the History of Science and Technology*, held in London, in 1931.[7] On that occasion, the Russian leader Nikolai Bukharin led a Soviet delegation of historians presenting their Marxist viewpoint on the history and philosophy of science. As the British X-ray crystallographer John Desmond Bernal, himself a Marxist, reported in a picturesque manner,

> The Russians came in a phalanx uniformly armed with Marxian dialectic, but they met no ordered opposition, but instead an undisciplined host, unprepared and armed with ill-assorted individual philosophies. There was no defense but the victory was unreal. […] Their appeal to dialectic, to the writings of Marx and Engels, instead of impressing their audience, disposed them not to listen to the arguments which followed. (Bernal 1949, 338)

Bernal immediately perceived the ideological dimension of the confrontation he had witnessed. Bukharin and his group tried to extend the political struggle of the Russian Revolution from immediate political confrontation to cultural production in general and the history and philosophy of science in particular. British academics were quite unprepared for such challenges. As Bernal stressed, the Soviets defended "a point of view, right or wrong; the others had never thought it necessary to acquire one" (Bernal 1949, 336). The Soviet viewpoint on science was the opposite of that which Koyré was to become a champion of. It focused on the material and the socio-economical factors of scientific progress. It was aimed to contrast a widespread historiography dealing with *internal* theoretical developments and technicalities or with the biographies of "Romantic geniuses," such as Copernicus, Galileo and Newton.

In order to grasp the leading ideas shared by the Soviet delegates, it is useful to isolate a few crucial theses expressed by their leader. In London, Bukharin

[7] This and the following two sections are a partial reworking of the talk "Reflections on History of Science and Cultural Hegemony at the Threshold of the Cold War," delivered at the 2013 Moscow conference *Social and Human Sciences on Both Sides of the 'Iron Curtain'* (Poletayev Institute for Theoretical and Historical Studies in the Humanities – National Research University "Higher School of Economics," October 17–19).

delivered the talk, "Theory and Practice from the Standpoint of Dialectical Materialism," which began with an address to the "fundamental questions of philosophy: the question of the *objective reality of the external world*, independent of the subject perceiving it, and the question of its *cognisability*" (Bukharin 1931, 11ff.). He asserted that objective material reality is the necessary presupposition of science that cannot be renounced: "Epistemology which is praxeology must have its point of departure in the reality of the external world: not as a fiction, not as an illusion, not as a *hypothesis*, but as a basic fact" (Bukharin 1931, 16).

Already in his very successful elementary introduction to Marxist philosophy, *Historical Materialism: A System of Sociology* (first issued in Russian in 1921, and soon translated into French in 1921, German in 1922 and English in 1925), Bukharin supported philosophical materialism, and reflected on the conditions for science and its aims. Scientists, he wrote, seek for general laws, either natural or social: "In nature and society there is a *definite* regularity, a *fixed* natural law. The definition of this natural law is the first task of science. This causality in nature and society is objective" (Bukharin 1921, 20). Thus, in principle, history is predictable:

> An eclipse of the Sun does not depend either directly or indirectly on human desires […] The case with social phenomena is entirely different, for they are accomplished *through* the will of men. […] Socialism will come inevitably because it is inevitable that men, definite classes of men, will stand for its realization, and they will make so under circumstances that will make their victory certain. Marxism does not *deny the will but explains it*. (Bukharin 1921, 51)

Note that, although human will counts as a factor of social transformation, still it is not free because it is determined just as natural phenomena. The developmental law regulating both nature as well as society is one and the same. In Marxist jargon it is called dialectics.

This naturalization of society and historical processes fostered Bukharin's lively interest in the natural sciences and in epistemology. Many pages of *Historical Materialism* were dedicated to science, philosophy and their mutual relations, especially chapter VI, "The Equilibrium between the Elements of Society." Bukharin's theory rested on the Marxist distinction between an economic *structure* and a political and cultural *superstructure*. The latter comprises "the social and political system of society […]; manners, customs and morals […]; science and philosophy; religion, art and finally, language" (Bukharin 1921, 150). According to Bukharin, science belongs to the realm of *ideology*, a concept coextensive with culture. "Science"—he wrote—"is a unified coordinated system of thoughts, embracing any subject of knowledge in its harmony"

(Bukharin 1921, 208). Its *fundamental epistemological principle* is that "every science is born from practice" (Bukharin 1921, 161). As Bukharin affirmed at the London conference, "science or theory is the continuation of practice." Its social function is "orientation in the external world and in society, the function of extending and deepening practice, increasing its effectiveness, the function of a *peculiar* struggle with nature" (Bukharin 1931, 20). Accordingly, science is so closely connected with technological advance that it is completely dependent on it. This emerges from two basic theses of epistemological import: "1. that the content of science is given by the content of technology and economy; 2. that its development was determined among other things by the tools of scientific knowledge" (Bukharin 1921, 169). As to the former point, Bukharin remarked that the technical-economical basis of scientific advance is witnessed by many historical instances, in which different scholars carried out discoveries simultaneously and independently from each other:

> The content of science is determined in the last analysis by the technical and economic phase of society; these are the 'practical roots,' which explain why an identical scientific discovery, invention or study, may be achieved simultaneously in different places, perhaps quite 'independently.' (Bukharin 1921, 164)

Note that, according to Bukharin, the practical determination of science does not mean an utilitarian (say, Baconian) conception: "It is not a question of the *direct* practical importance of *any* individual principle […] It is a question of system as a whole" (Bukharin 1931, 20). It is also a question, from a historical viewpoint, of recognizing "that *genetically* theory grew up out of practice" (Bukharin 1931, 19). For historical examples of science emerging from practice, Bukharin often mentioned Ernst Mach's reconstructions. Although he did not follow the latter's *empiriocriticist* epistemology, whose principle of economy had been severely criticized by Lenin, Bukharin derived historical examples and perspectives from Mach, whose philosophy had been widely received in Russia between the XIX and the XX centuries (Steila 1996).

A famous case study resulting from the application of the general principles so far outlined was the presentation of the Soviet delegate Boris Hessen. His paper, "The Social and Economic Roots of Newton's *Principia*," is generally seen as the most significant historical essay presented at the 1931 conference. Gideon Freudenthal and Peter McLaughlin have synthesized his concept in the following three points:

1. Theoretical mechanics developed in the study of machine technology;

2. Conversely, in those areas where seventeenth-century scientists could not draw on existing technology the corresponding disciplines of physics did not develop;
3. Ideological (theological) constraints descending from the political constellation affected crucial philosophical concepts of Newton's physics (such as matter).
(Freudenthal and McLaughlin 2009, 2–3)

The fact that the unity of theory and practice is obscure to most scientists and philosophers of science is a social-historical byproduct of labor division. In capitalist society, specialization and abstraction go hand in hand. The connection between *theory* and *praxis*, between science and its social roots and aims are only mediated in a world in which intellectual labor and physical labor are kept apart, and the latter is subordinated to the former. One aim of socialist society, so Bukharin, was to blur progressively the distinction between intellectuals and physical workers. In his vision of the future, theory and practice were destined to merge. The connection of science and economy should be established on a new basis expanding the model of economical planning to scientific production.

Reception of Marxist Historical Epistemology and Reaction to It

After World War II, a small but visible group of historians, especially in the West, continued the line traced by the Soviet delegates and produced significant social and material accounts on the history of science (Young 1990). As to the *Wirkungsgeschichte* of the Soviet challenge, Joseph Needham, in the second edition of *Science at the Cross Roads: Papers Presented to the International Congress of the History of Science and Technology [...] by the Delegates of the U.S.S.R.* (1971), claimed that a flourishing externalist tradition of studies in the history of science had emerged in the wake of the 1931 conference. According to him, "[Hessen's] essay, with all its unsophisticated bluntness, had a great influence during the subsequent forty years" (Needam 1971, viii). Furthermore, Needham acknowledged that his own multi-volume *Science and Civilisation in China* (1954, 1st vol.) was a result of stimuli from Bukharin, Hessen and the other Soviet delegates.

In a sense, the sociology of science launched by Robert Merton was linked with the same legacy. In *Science, Technology and Society in Seventeenth-Century England* (1938), he stated that he derived from Hessen important insight concerning the relation between science, technology and society in the age of Isaac Newton:

> In the discussion of the technical and scientific problems raised by certain economic developments, I follow closely the technical anal-

> ysis of Professor B. Hessen in his provocative essay, "The Social and Economic Roots of Newton's Principia," in *Science at the Cross Roads* […] Professor Hessen's procedure, if carefully checked, provides a very useful basis for determining empirically the relations between economic and scientific development. These relations are probably different in an other than capitalistic economy since the rationalization which permeates capitalism stimulates the development of scientific technology. (Merton 1938, 501–502, n. 24)

Furthermore, a long critical assessment of Hessen's theses is to be found in George Clark's *Science and Social Welfare in the Age of Newton* (1937), a book devoted to the investigation of "the cooperation of science and economic life" (Clark 1970, 2), Chapter III "Social and Economic Aspects of Science," was almost in its entirety a detailed discussion of Hessen's 1931 article, in a rather critical but respectful way (Clark 1970, 61–86).

Bukharin and his group's provocation influenced significant developments of the history of science after World War II not only in a positive manner but also in the form of negative reactions. *Science at the Cross Roads* marked the beginning of an ideological bifurcation documented, in the 1940s, by the theoretical opposition between Edgar Zilsel's materialist approach to early-modern science and Koyré's intellectual historiography, and by the clash between 'internalist' and 'externalist' historians of science. Just as *externalist* history of science emerged from the Marxist camp, the *internalist line* had an *ideological* character, too. As a matter of fact, the majority of Anglo-American historians of science responded to the Soviet challenge by wiping out from their considerations all elements *external* to pure theory. Earlier authors, who did not belong to Marxist historiography and philosophy of science but stressed material or social aspects in the genesis and development of science, were also marginalized from the prevailing narrative, as was the case with Ernst Mach and Leonhard Olschki.

As indicated by Wolfgang Lefèvre, Koyré's 1943 essay "Galileo and Plato" can be seen as a manifesto of the anti-materialistic, anti-communist line (Lefèvre 2001, 11–13). Koyré's article begins with a brief overview of the adversaries' theses:

> This revolution [the Scientific Revolution] is sometimes characterized, and at the same time explained, as a kind of spiritual upheaval, an utter transformation of the whole fundamental attitude of the human mind; the active life, the *vita activa* [i.e., the πράξις] taking the place of the θεωρία, the *vita contemplativa*, which until then had been considered its highest form. […] [According to this perspective,] the science of Descartes—and a fortiori that of Galileo—is nothing else

> than (as has been said) the science of the craftsman or of the engineer. (Koyré 1943, 400)

Koyré is quick to add: "I must confess that I do not believe this explanation to be entirely correct." Actually, his intention is to show that this explanation is *completely wrong*. He reduces the practical and social reconstruction of early-modern science to a form of Baconianism: "The attitude we have just described is much more that of Bacon […] than that of Galileo or Descartes" (Koyré 1943, 400–401). This is the typical misunderstanding of Bukharin's and Hessen's positions.[8] Koyré confuses individual intentions with social functions, and immediately perceived application of knowledge with practical factors at a socio-economical level supporting certain practices and lines of natural investigation.

Koyré maintained that Galileo's and Descartes's "science is not made by engineers or craftsman, but by men who seldom built or made anything more real than a theory" (Koyré 1943, 401). This is his main point. Galileo's *mathematized physics* was a form of Platonic contemplation of the numbers and geometries hidden behind the natural phenomena. Just as Galileo was a Platonist (Koyré 1943, 424), the Scientific Revolution was a "spiritual revolution" (Koyré 1943, 403). This matches with the cliché image of Koyré, whose view has been often reduced to the following three points: mathematical Platonism, rejection of the sociology of sciences and rational idealization of the scientific process (Redondi 1987, 2–3). To these, Georges Canguilhem added a fourth pont, a proto-Kuhnian "anticontinuisme résolu" leading to a "théorie des révolutions scientifiques [laquelle] a bien été le moteur d'une révolution dans l'histoire des sciences et confère aux travaux de Koyré leur unité originale" (Canguilhem 1987, 9).

Kuhn was well aware of the polemical meaning of Koyré's disembodied approach. As he observed in the footnote of the article "Mathematical versus Experimental Traditions in the Development of Physical Science," appeared in 1976 on the *Journal of Interdisciplinary History*:

> Note also the way in which distinguishing between [a pluralistic approach to science or a unitarian] […] deepens and obscures the now far better known distinction between internalist and externalist approaches to the history of science. Virtually all the authors now regarded as internalists address themselves to the evolution of a single science or of a closely related set of scientific ideas; the externalists fall almost invariably into the group that has treated the sciences as one. But the labels 'internalist' and 'externalist' then no longer

[8] Cf. Freudenthal and McLaughlin (2009, 9).

> quite fit. Those who have concentrated primarily on individual sciences, e.g. Alexandre Koyré, have not hesitated to attribute a significant role in scientific development to extrascientific *ideas*. What they have resisted primarily is attention to socioeconomic and institutional factors as treated by such writers as B. Hessen, G.N. Clark and R.K. Merton. But these nonintellectual factors have not always been much valued by those who took the sciences to be one. The 'internalist-externalist debate' is thus frequently about issues different from the ones its name suggests, and the resulting confusion is sometimes damaging. (Kuhn 1977, 32, n. 1)

Kuhn does not contribute to illuminate the reader about the implicit issues at stake he hints at. Koyré is more explicit. In fact, in a dense footnote in the Galileo-and-Plato article he makes his anti-Marxist intention clear. Koyré contrasts there his own views with those of Marxist exponents (Koyré 1943, 401, n. 6). In particular, he indicates two works stemming from the Frankfurter Schule: Franz Borkenau's *Der Übergang vom feudalen zum bürgerlichen Weltbild* (Paris, 1934) and Henryk Grossmann's rectification, *Die gesellschaftlichen Grundlagen der mechanistischen Philosophie und die Manufaktur* (Paris, 1935). Whereas Borkenau's image of "Descartes artisan" is quickly dismissed as an "absurdity," Grossmann's writing is referred only for its criticism of Borkenau's too simplistic economicism, and not for its counter-proposal, which in many respects coincides with that of Hessen.[9] After them, Koyré turns to Leonhard Olschki, treating his interpretation of Renaissance science as the outcome of the technological culture of the late Middle Ages, as if it was just the same interpretative line of Borkenau, Grossmann and Zilsel. For that 'socialist sin,' Olschki has to be banned from historiography of science, as well. Koyré mentions also Zilsel's essay "The Sociological Roots of Science" (*The American Journal of Sociology* 47, 1942) for its stress on "the role played by the 'superior artisans' of the Renaissance in the development of the modern scientific mentality." For Koyré science cannot be anything else than a *mental* issue, even when presenting the viewpoints of those who contend this assumption and stress its extra-mental origins. Remarkably, Koyré makes no mention to the Soviet papers of the 1931 conference. In general, he avoided mentioning even the name of Marx apart from one *lapsus*, in a postscriptum of 1961 to an essay of 1930, "Les études hégeliennes en France," which is revelatory of his profound aversion to Marx and his followers:

> [E]nfin—*last but not least*—l'émergence de la Russie soviétique comme puissance mondiale et les victoires des armées et de

[9] Cf. Freudenthal and McLaughlin (2009).

> l'idéologie communiste […] Hegel *genuit* Marx; Marx *genuit* Lenine; Lenine *genuit* Staline.[10]

To sum up Koyré's perspective, he intentionally construed an immaterial and spiritualist alternative to the *dangerous* social and material historiography of science. Thereby he inaugurated a politically-correct historiography that was to be embraced by influential US-American scholars. Among them, Thomas Kuhn, who was a close collaborator of the anti-communist designer of education policies, James B. Conant, praised Koyré as one of the most important recent scholars in his field. In another essay, "Alexandre Koyré and the History of Science: On an Intellectual Revolution," Kuhn extolled the merits of the former, in particular of his *Études galiléennes* (Paris, 1939), as a work inaugurating a novel approach:

> Within a decade of their appearance, they [the *Études galiléennes*] and his subsequent work provided the model which historians of science increasingly aimed to emulate. More than any other scholar, Koyré was responsible for […] the historiographical revolution. (Kuhn 1970, 67)

Does not this claim for a *historiographical revolution* in a *historical* discipline sound bizarre? Probably, as Kuhn observed relative to the 'internalist-externalist debate,' also this *Koyréan Historiographical Revolution* was "about issues different from the ones its name suggests, and the resulting confusion is sometimes damaging." The immateriality, or "Platonism," of Koyré's proposal offered an alternative to out-dated positivism which, at the same time, avoided the pitfalls of socio-economical historiography. This conferred Koyréan history of science all the characteristics needed for the construction of an anti-communist History and Philosophy of Science.

The Harvard Entwurf of a HPS for a "Free Society"

Kuhn's academic formation was affected by the militant and anti-communist cultural climate of Harvard in the 1940s and 1950s. His mentor, Harvard president Conant, occupied crucial political positions. During the Second World War, in 1940, he became a member of the National Defense Research Committee and one year later he became its chair. He then entered the cabinet supervising the atomic bomb project and had direct responsibility for the uranium fission. As reported in a biographical memoir issued by the American Academy of Sciences, "on Conant's recommendation in the spring of 1942, this project was expedited

[10] I quote from Elkana (1987, 141).

by direct, industrial-scale plant construction carried forth simultaneously on four different ways of preparing fissionable material for atomic weapons. Three of the four methods were successful, and all contributed to the successful [sic!] bomb of 1945" (Bartlett 1983, 100). In the 1950s, Conant became chairman of the Anti-Communist Committee on the Present Danger.

Concerning Kuhn's student years, they were marked by the World War. "After Pearl Harbor—so Conant in his autobiography—and until V-J Day in August 1945, Harvard was primarily a university at war. Before the academic year 1941–1942 was over, a gradual exodus had begun. Some took commissions in the armed forces, some in civilian war agencies; almost without exception, the physical scientists were enrolling in one or another of the government-supported secret laboratories located in various institutions of higher learning" (Conant 1970, 363). During the wartime Kuhn made himself visible with public declarations in favor of the president's policy. He authored an editorial in the daily student newspaper, The *Harvard Crimson*, in which "he supported Conant's effort to militarize the universities in the United States. The editorial, of course, came to the attention of the administration, and eventually Conant and Kuhn met" (Marcum 2005, 6). Conant had also organized a committee whose task was to outline the program for "a General Education in a Free Society," whose ideological commitment is clear.

Kuhn greatly benefited from the power and visibility of his mentor. It has been remarked that "Kuhn's intellectual gestation at Harvard (1940–1956) enabled him to acquire, with little effort of his own, […] 'the strength of the weak ties.' […] Kuhn had a singularly strong tie to Conant, who in turn had many weak ties to opinion leaders in American society" (Fuller 2000, xiv).

Part of Conant's educational project was to disseminate scientific knowledge among the general public, in an age when scientific-technological programs required the support of a wide public opinion. At Harvard he planned classes of history of science for upper-level undergraduates, merging humanities and sciences. In 1947, he appointed Kuhn as an assistant and, in the fall of the following year, sponsored him a Harvard Junior Fellowship, which Kuhn spent to initiate his investigation of the history and philosophy of science. One of the first fruits of this research was his textbook on early modern astronomy, *The Copernican Revolution* (1957), which appeared in Conant's series of *Case Histories in Experimental Science* (Swerdlow 2004, 71–76). In the preface Kuhn cherished his benefactor:

> Many friends and colleagues, by their advice and criticism, have helped to shape this book, but none has left so large or significant a mark as Ambassador James B. Conant. (CR, xi)[11]

In return, Conant endowed Kuhn's book with a foreword, which began with a reference to the ideological curtain that was being built in the aftermath of the Second World War:

> In Europe west of the Iron Curtain, the literary tradition in education still prevails. An educated man or woman is a person who has acquired a mastery of several tongues and retained a working knowledge of the art and literature of Europe. By a working knowledge I do not refer to a scholarly command of the ancient and modern classics or a sensitive critical judgment of style or form; rather, I have in mind a knowledge, which can be readily worked into a conversation at a suitable social gathering. An education based on a carefully circumscribed literary tradition has several obvious advantages: the distinction between the 5 to 10 percent of the population who are thus educated and the others makes itself evident almost automatically when ladies and gentlemen converse.

Conant's words imply that east of the Iron Curtain the humanistic tradition had been interrupted along with the abolishment of "ladies and gentlemen." He implicitly excluded the communist camp from the "Western culture," which he celebrated as the educational basis for the *free society* he considered himself a "Social Inventor" of (Conant 1970). Note, in the abovementioned passage, the elitist understanding of culture as the privilege of a small group of gentle conversant people, not to be confused with populace. Note moreover the Eurocentric viewpoint.

Koyré was not less exclusive and Eurocentric than Conant.[12] *From the Closed World to the Infinite Universe* was affected by acute hellenophilia: "The conception of the infinity of the universe, like everything else or nearly everything else, originates, of course, with the Greeks."[13] In this cultural context, Koyré became the paradigm for an elitist-rational, Eurocentric and *spiritualized* history of science, to be opposed to the economy-and-technology narrative of those sympathizing with socialist ideas. On his part, Kuhn did not limit himself to continue the Koyréan program for the history of science. He also implemented on its basis a politically-correct philosophy.

[11] At that time Conant was US Ambassador in Western Germany.
[12] A "hardcore elitist" according to Elkana (1987, 129).
[13] Cf. Conner (2005, 117).

The resulting epistemology, that of the *Structure*, was irreconcilable with the most important theses of the Marxist program outlined by the Soviet scholars. To notice this opposition, it is sufficient to consider the following crucial epistemological assumptions of Kuhnian epistemology:

1. *Irrelevance of the economic structure*—In the *Structure* no technical or practical aspects significantly account for the historical development of science. The economical basis is completely absent. Thus, the *structure* underlying science has nothing to do with the socio-economical basis. It is rather a conceptual framework. Science is a cumulative but discontinuous intellectual process, framed in conceptual structures and punctuated by revolutions of thought.
2. *Individualism of discovery*—Second, Kuhn's scientists are not creative as a collectivity but only, rarely, as individuals. The community of those practicing "normal science" is rather a conservative majority. Accordingly, Kuhn assumes that scientific discovery is individual.
3. *Mysticism of discovery*—Kuhn does not dismiss or explain the *mystery* of discovery, in one word, *geniality*, which is the inexplicable element in intellectual history: "The new paradigm […] emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis" (SR, 89–90).
4. *Contingency of historical development*—Fourth, the development of science is contingent. Kuhn, even more than Koyré, was convinced of this. A historiography centered on technology and economy menaced to foster deterministic views. This, at least, was Bukharin's idea of Marxist historiography, on which also his program in HPS rested. This could be seen as the weak point of Marxist historiography, namely determinism. What Koyré and Kuhn were probably unaware of (or rather not interested in) was the fact that Bukharin's naturalization of social processes was lively debated and even criticized also within the Marxist camp. György Lukács and Antonio Gramsci, to mention two influential Marxist thinkers, wrote harsh criticisms of Bukharin's deterministic viewpoint, which they regarded as misled and fatalistic.[14] Moreover, in the Soviet Union scholars prone to a *scientist* Marxism and a determinist understanding of historical development (the so-called 'mechanists') and Bukharin were involved in heated philosophical polemics and were strongly reprimanded in the early 1930s.[15] During this quarrel, scientism and scientist Marxism, labeled as "mechanistic materialism," were condemned in Soviet Union by the Communist Academy,

[14] See Sochor (1980, 707–712); Omodeo (2010).
[15] Cf. Kolakowski (2005, 841).

in 1929, and this condemnation invested also Bukharin's philosophy, seen as "anti-dialectical pseudo-Marxism" (Mirsky 1931, 652).

The Copernican Question: a Material or a Celestial Question?

In the cultural climate of the Cold War, Kuhn's anti-determinist, anti-economicist and intellectualistic historical epistemology represented a politically correct alternative to approaches suspected of being too close to the ideology of the Soviet camp. Regarding the *Copernican Revolution* the question to be addressed concerns the role Copernicus played from the point of view of the historical and epistemological debates of the time. For this assessment, I would like to briefly recount the meaning ascribed to the heliocentric astronomer in earlier accounts on early modern science.

According to a reputed history of physics, Ernst Mach's *Die Mechanik in ihrer Entwickelung: historisch-kritisch dargestellt* (1883), cosmology played a subaltern role in the development of physics, since the connection between mechanics and planetary theory occurred at a late stage in the history of mathematical physics (thanks to Kepler and especially Newton) (Mach 1942, 231 ff.). Mechanics evolved independently of astronomy, regarding its theoretical premises (e.g. the Archimedean legacy) as well as its material, technical and social roots. The development of statics from ancient times to the Renaissance, one reads, "illustrates in an excellent manner the process of the formation of science generally. […] These beginnings point unmistakably to their origin in the experiences of the manual arts" (Mach 1942, 89). As far as dynamics is concerned, celestial physics was an extension of terrestrial mechanics to astronomy. Quite naturally, this stress on the practical roots of science met with the approbation of Bukharin and his like. By contrast, Koyré's later narrative of the Scientific Revolution as originating from a cosmological turn was fit to immaterialist conceptions about the emergence of early modern science. As such, it offered a counter-history to the history of physics propounded by Mach and his direct or indirect followers.

A critical point in Mach's narrative lies in neglecting the import of the physical questions that arouse from post-Copernican astronomy. In particular, the thesis of terrestrial motion had to be accompanied by a new explanation of gravity and motion accounting for the vertical fall of heavy bodies in a dynamic system. (The geocentric and geostatic model of Aristotle and Ptolemy just took for granted that heavy bodies have a simple tendency toward the cosmological center of the world *quo* center of gravity). Mach seemed to regard as relevant for the history of physics only the mechanical treatment of the solar system as a whole. Koyré took the opposite direction. Contrary to Mach, he emphasized the role of astronomy in order to construe a foundational myth of modern science.

He depicted the Scientific Revolution as a break with the past that began with a cosmological transformation. That Copernican transformation was continued by scientific-philosophical thinkers (only *pure* thinkers!) like Giordano Bruno, Galileo Galilei and René Descartes, and concluded with Newton's new synthesis of terrestrial and celestial physics. Furthermore, Koyré reduced this process to a few central ideas. In physics he allotted primary importance to the principle of inertia, first conceived by Galileo as a solution to the open problems of the heliocentric theory (Koyré 1978, 131). Koyré thus assumed that the scientific revolution had a *prologue* and an *epilogue* in the heavens, with Copernicus's system at its outset and Newton's unification as its destination. Hence, contrary to Mach's opinion, modern dynamics originated from celestial concerns. For Koyré celestial mechanics was not the extension of a science stemming from practice but, rather, it was a discipline much closer to theology than to technology. Plato was for him the philosopher who inspired in Renaissance authors an almost religious reverence for mathematical abstraction. Kuhn's image of Copernicus "the Platonist" fits well in this scheme:

> Neoplatonism is explicit in Copernicus's attitude toward both the Sun and mathematical simplicity. It is an essential element of the intellectual climate that gave birth to his vision of the universe. (CR, 131)

As far as the epistemological premises are concerned, Mach did not consider science a pure and disinterested endeavor. Rather, he postulated a *principle of economy* for both epistemology (theory choice as dependent on thought economy) and historiography (science as rooted in practical needs of the human species). This "vulgar" perspective met severe oppositions. Among others, Max Planck began a philosophical polemic with him as early as in 1908. On the occasion of a public talk at the University of Leiden, he attacked Mach's principle of economy, which he deemed to be philosophically undesirable because it led to relativism. Moreover, Planck claimed that such an explanatory principle was in disagreement with the history of science. As he declared, the most important modern scientists—*in primis* Copernicus, Kepler and Newton—were motivated in their inquiries by their desire to reach the truth, that is, by their aspiration toward objective knowledge, and not for the idle affirmation of their own "*Illusionen*." Here the relevant passage from Planck's talk follows:

> Zum Schluß noch ein Argument, das vielleicht auf diejenigen, welche trotz alledem den menschlich-ökonomischen Gesichtspunkt als den eigentlich ausschlaggebenden hinzustellen geneigt sind, mehr Eindruck macht als alle bisherigen sachlichen Überlegungen. Als die großen Meister der exakten Naturforschung ihre Ideen in

> die Wissenschaft warfen: als Nikolaus Kopernikus die Erde aus dem Zentrum der Welt entfernte, als Johannes Kepler die nach ihm benannten Gesetzte formulierte, als Isaac Newton die allgemeine Gravitation entdeckte, als Ihr großer Landsmann Christian Huygens seine Undulationstheorie des Lichtes aufstellte, als Michael Faraday die Grundlagen der Elektrodynamik schuf […], da waren ökonomische Gesichtspunkte sicherlich die allerletzten, welche diese Männer in ihrem Kampfe gegen überlieferte Anschauungen und gegen überragende Autoritäten stählten. Nein – es war ihr felsenfester, sei es auf künstlerischer, sei es auf religiöser Basis ruhender Glaube an die Realität ihres Weltbildes. (Planck 1958, 28)

Planck's narrative, akin to later ones by Koyré and Kuhn, supposed titanic efforts on the part of individual epistemic warriors aiming to besiege the bulwarks of tradition. No dirty interests animated their efforts, nor could biological or social-economical drives account for the origins of science in its highest form. Planck assumed that science was born out of a disinterested desire for truth. Yet, Planck was not Duhem. Correspondingly, his philosophical outlook was different also from the later Koyréan. In fact, Planck did not look for a spiritualized conception of science. Rather, he sided with the positivistic ideal of science and a materialistic view of nature. Scientific advance, so Planck, is a de-anthropomorphizing process moving away from subjectivity and striving for objectivity.

> Worauf es hier einzig und allein ankommt, ist die Anerkennung eines solchen festen, wenn auch niemals ganz zu erreichenden Zieles, und dieses Ziel ist […] die vollständige Loslösung des physikalischen Weltbildes von der Individualität des bildenden Geistes. Es ist dies eine etwas genauere Umschreibung dessen, was ich oben die Emanzipierung von anthropomorphen Elementen genannt habe, um das Mißverständnis auszuschließen, als ob das Weltbild von dem bildenden Geist überhaupt losgelöst werden sollte; denn das wäre ein widersinniges Beginnen. (Planck 1958, 27–28)

By contrast, Koyré's historiography focuses precisely on the relation between *Weltbild* and *bildender Geist*. There is no space for "objectivity" in his historical reconstructions. He never looks at the relation between scientific investigation and the 'world.' As has been noted, he "wrote in terms of the interaction of what his heroes thought about the world and what they thought about their knowledge" (Elkana 1987, 116). Accordingly, "the ideas to which Koyré attributes the greatest importance as factors of change are all ideas about knowledge and not about the world" (Elkana 1987, 118). In the first place, in his eyes,

scientific changes are epistemological and metaphysical. One can say, that they are "shifts of paradigm" in the Kuhnian sense. Even more than in the *Études galiléennes*, in *From the Closed World to the Infinite Universe*, Koyré made his epistemological-metaphysical focus noticeable. In this work, he pointed to two pillars of the cosmological revolution, namely mathematization and infinity, which are epistemological and metaphysical, respectively. Both pillars had *spiritual* import for him. Koyré had already argued for the Platonic flavor of the Galilean use of mathematical abstractions in physics in other writings. As far as infinity is concerned, in the *Closed World* boundlessness was seen as the application of a theological-metaphysical concept to nature. In fact, the idea of the *infinite universe* had theological origins. For the *economy* of this story, Koyré predated the beginning of the modern revolution of thought known as Scientific Revolution and traced it back to the speculations of a theologian and metaphysician, Cardinal Nicholas Cusanus.

However concerned about the spiritual dimension of science, Koyré never renounced the idea of a *rationale* underlying the history of science, perhaps in the form of a teleological drive. Accordingly, the image of the infinite universe *had to* triumph over that of a closed world just as the heliocentric system *had to* triumph over the Aristotelian-Ptolemaic cosmos. Kuhn radicalized the spiritual element of Koyré's narrative by radicalizing the contingency of the paradigm shift from geocentrism to heliocentrism. It is curious but symptomatic the manner in which he constantly resorted to metaphors and termini stemming from religion to depict the early dissemination of the Copernican system. Here a list of significant passages follows:

> The state of Ptolemaic astronomy was a scandal before Copernicus' *announcement*. (SR, 67)
>
> Copernicanism made few *converts* for almost a century after Copernicus' death […] This difficulties of *conversion* have often been noted by scientists themselves. (SR, 150–151)
>
> Those who Copernicus *converted* to the concept of a moving earth […] (CR, 183)
>
> [H]e could embrace a cosmological *heresy*, the earth's motion. (CR, 184)
>
> Maestlin […] *gained a few converts*, including Kepler, for the new astronomy. (CR, 187)
>
> The group of *avowed Copernicans* […] (CR, 187)

> [W]hatever their *beliefs* about the position and motion of the Earth. (CR, 187)

> Copernicus's innovation seemed absurd and *impious*. (CR, 188)

The image here suggested is that of a faith dealer and his apostles preaching a new gospel. This idea of the affirmation of the Copernican theory is indeed very far from Galileo's call for *sensate esperienze* and *certe dimostrazioni*. Rather, it matches with Kepler's account of his discovery of the Platonic *Mysterium cosmographicum*, the secret harmony underlying the Creation, in terms of divine enlightenment. It is not without reason that Kuhn assigned to Kepler the decisive role to make "the Copernican system work" (CR, 131). As one reads in *Copernican Revolution*, Kepler had "the decisive revolutionary role" of completing the heliocentric theory (planetary laws and *Rudolphine Tables*) and making it endure.[16]

In the following, I shall consider the historical and epistemological implications of the religious metaphor.

Copernican versus Ptolemaic Faith

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*.[17] Another significant assessment is Robert S. Westman's "Second Look at Kuhn's *The Copernican Revolution*." In this paper, Westman points out the imagery of warfare used by Kuhn to depict the reception of Copernicus but also remarked that "the notion of conversion is an important corollary of the incommensurability thesis in *The Stucture the Scientific Revolutions*" (Westman 1994, 93–94). It is from the religious metaphor, although it is perhaps "not well developed in *Copernican Revolution*" (Westman 1994, 94), that I would like to start a historical-epistemological assessment and argue that Kuhn's recourse to it is revealing of theoretical difficulties entailed in his approach.

The first difficulty in Kuhn's narrative concerns the relationship between Copernican and Ptolemaic astronomies regarded as the opposition between

[16]Cf. Westman (1994, 104).

[17]Kokowski (2001). I would like to stress the relevance of Part 1, section I.4, also dealing with the Conant-Kuhn connection; of Part 2, section I,2, providing an overview of the first reactions to Kuhn's *Copernican Revolution*; and Part 2, chap. 4, where Kokowski critically discusses its limits (see also Kokowski (1993)). I shall like to thank Prof. Kokowski for sharing with me several of his views on Kuhn, his philosophy and work.

two *incommensurable paradigms*. Kuhn maintained that, since the heliocentric planetary system was the essential aspect of Copernicus's achievement, this was the only issue at stake in the dissemination of his major work, *De revolutionibus orbium coelestium* (Nuremberg, 1543).[18] Additionally, he conceived of "Copernican astronomy" as a paradigm with a coherent deductive-like structure. This means that, according to him, all elements of Copernicus's work and post-Copernican astronomy were systematically interconnected.

This premises led Kuhn to dubious conclusions, for instance that the Copernican background was essential for every advance in Renaissance astronomy, for instance, for those determining the supra-lunar nature of comets and *novae* during the sixteenth century.

> Late sixteenth-century astronomers repeatedly discovered that comets wandered at will thorough the space previously reserved for immutable planets and stars. The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world. (SR, 116)

That the observation of comets and supernovas in the second half of the sixteenth century undermined certain Aristotelian assumptions about the nature of the heavens is historically true (Tessicini and Boner 2013, "Introduction"). That this fact directly or indirectly stemmed from Copernicus is false, as can be easily argued considering the overview of sources on the comet of 1577–1578 included in Tycho Brahe's *De mundi aetherei recentioribus phaenomenis* (1588).[19] This is famously the work in which the Danish astronomer described his own geo-heliocentric system for the first time. How many of the authors of cometary tracts reviewed by Brahe conceived of comets' observation as relevant for the heliocentric cause? Probably only a couple among them, for instance Michael Mästlin and Thomas Digges, and perhaps Cornelius Gemma. But they were a minority.[20]

Besides, Kuhn's viewpoint neglects the variety of early interpretations of Copernicus's work depending on the different interests and motivations of its readers.[21] Renaissance scholars confronting *De revolutionibus* did not mainly focus on the so-called hypotheses, that is, terrestrial motion and solar centrality and immobility. Many of them regarded *De revolutionibus* as the basis for new astronomical tables, such as Erasmus Reinhold's *Prutenicae tabulae* (1551);[22]

[18] Since then it has become a sort of challenge for Copernicus's scholars to count the early "Copernicans." See, for instance, Tredwell and Barker (2004).
[19] The standard reference is Hellman (1944).
[20] See Westman (2011, chap. IX).
[21] I have reconstructed various thematic lines of reception of Copernicus in Omodeo (2014).
[22] Cf. Gingerich (1993).

others, from Kaspar Peucer to Brahe, appreciated Copernicus's geometrical models renouncing Ptolemy's equant; others, like Rheticus, were enthusiastic about the substitution of a Ptolemaic 'anomaly' for the terrestrial circle about the Sun, which also provided a yardstick (the Earth-Sun distance) to establish planetary distances.[23] Only for the last issue the heliocentric theory was central. Thus, one might legitimately doubt whether adherence to heliocentrism is sufficient to define a Renaissance follower of Copernicus, as Kuhn did. Furthermore, is there anything more striking than the epistemological and philosophical differences among 'realist Copernicans' in the late sixteenth century? Take the cases of Giordano Bruno, Galileo Galilei and Johannes Kepler. Can one say that they worked within the same 'paradigm,' that they shared common ideas about science and nature, only due to the fact that they accepted the rotation of the Earth about its axis and around the Sun?[24]

Contrary to the incommensurability thesis, Kuhn himself had to notice that Copernicus worked in the wake of Ptolemy, from whom he derived his methodology, his conceptual tools and the structure of his major work. For those reasons, Kuhn called him "radical" and "conservative" at the same time (CR, 148), and his book "revolution-making" rather than "revolutionary" (CR, 135).

A further evidence against incommensurability showing the permeability of geocentric and heliocentric systems is documented by the exchange of arguments in favor of and contrary to terrestrial motion and the heliocentric system between famous scholars such as Brahe and Rothmann in their correspondence and, later, Galileo's indirect response to Brahe's criticism of Copernicus.[25] However, the proliferation of heliocentric and hybrid or geo-heliocentric planetary systems during the sixteenth and seventeenth centuries is the clearest evidence that the choice between different options concerning cosmological order was not just that between two major systems. The Jesuit astronomer Giovanni Battista Riccioli, in his *Almagestum novum* (1651), even enlisted—perhaps with some exaggeration—eight different geocentric options (including geo-heliocentric and homocentric world systems) and two geokinetic variants (one, in which only the daily rotation is admitted, and heliocentrism).

[23]Cf. CR (174–177). For a brief overview of the advantages of Copernicus's heliocentric theory in relation to the geocentric, see Swerdlow (2004, 88–90).

[24]For an assessment of the epistemological differences among "realist Copernicans," see Omodeo (2011).

[25]On the geo-heliocentric debates, see Granada (1996); on Galileo's reactions to Brahe, see Buccianti ni (2003, 23–48, chap. 2, "Padova: Pinelli, Tycho, Galileo").

Experimentum Crucis?

A second historical-theoretical difficulty in Kuhn's account lays in the fact that the incommensurability thesis and the reduction of the historical meaning of Ptolemy and Copernicus to planetary hypotheses make *the choice between* those *two paradigms* extremely elusive, *almost inexplicable*, or even fortuitous.

> Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute. [For instance…] Ptolemy's computations of planetary positions. (SR, 23)

This is Kuhn's starting point. Yet he has difficulties to apply it to Copernicus's case, due to the impossibility to indicate a decisive experiment capable of establishing the superiority of the heliocentric system over the geocentric during the Renaissance:

> "Crucial experiments"—those able to discriminate particularly sharply between two paradigms—have been recognized and attested before the new paradigm was even invented. Copernicus thus claimed that he had solved the long-vexing problem of the length of the calendar year. (SR, 153)

It should be remarked *en passant* that Copernicus was not able to provide the solution to the calendar reform which was carried out by the Jesuit mathematician Christopher Clavius for Pope Gregory XIII, in 1582, relying both on the *Alfonsine Tables* as well as on the "Copernican" tables of Reinhold. The determination of the length of the year and its application for the calendar reform, to which Copernicus referred in his book, cannot be regarded as "crucial experiments" testing two competing sets of planetary hypotheses.

Kuhn hints at "special telescopes to demonstrate the Copernican prediction of annual parallax" as an example of "predictions from the paradigm theory" (SR, 26). Indeed, the absence of any observable stars' parallax and the fact that the starry heaven appeared to be a sphere always bisected by any horizon for any observer on Earth, even after Galileo's inauguration of telescopic astronomy, was one of the main astronomical arguments against the circumsolar revolution of the Earth.

> Even Copernicus' more elaborate proposal was neither simpler nor more accurate than Ptolemy's system. Available observational tests […] provided no basis for a choice. […] Ptolemaic astronomy had failed to solve its problems; the time had come to give a competitor a chance. (SR, 75–76)

Kuhn was thus forced by historical evidence to acknowledge that the assumption that any criteria could univocally determine the choice between two competing paradigms does not fit the Copernican-Ptolemaic divide.

These difficulties can also be remarked by the contradiction between Kuhn's idea of a 'switch,' introduced in order to account for scientific revolutions, and the timing of the Copernican reception as it occurred in fact. On the one hand, in fact, he admitted that Copernicanism might count as an exception to the all-at-once-emergence thesis:

> In other cases, however—those of Copernicus, Einstein, and contemporary nuclear theory, for example—considerable time elapses between the first consciousness of breakdown and the emergence of a new paradigm. (SR, 86)

On the other hand, he deemed *paradigms' transition* to happen like a *sudden gestaltic switch* (SR, 111 and ff.).

> The new paradigm […] emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis. (SR, 89–90)

The religious metaphor about the Copernican conversion maintains the pathos of a sudden revelation.

Plurality and Unity in Science

I have so far argued that the hypotheses-centered interpretation of Copernican and Ptolemaic astronomies as paradigms entails several difficulties linked to the Kuhnian theory of scientific revolutions. In fact, it can be demonstrably objected that,

1. *Ptolemaic and Copernican planetary approaches were permeable and commensurable*;
2. *There was no experimentum crucis* that could be used to establish the superiority of the heliocentric alternative in all respects;
3. *The transition from geocentrism to heliocentrism* did *not* happen as *a sudden conversion-like event*.

Kuhn was not willing to take into account the theoretical consequences of these historical statements. In particular he neglected evidence of commensurability through controversy and hybridization because he assumed that plurality is a clear symptom of crisis. He contrasted in fact the incertitude of theories'

proliferation as a crisis state to scientific advancement within a well-established theoretical framework (what he referred to as "normal science"), for instance the Copernican planetary theory.

> Copernicus' […] famous preface still provides one of the classic descriptions of a crisis state. (SR, 69)

> Proliferation of versions of a theory is a very usual symptom of crisis. In his preface, Copernicus complained of it as well. (SR, 71)

> There was no longer one Ptolemaic system but a dozen or more, and the number was multiplying rapidly with the multiplication of technically proficient astronomers. All these systems were modeled on the system of the *Almagest*, and all were therefore 'Ptolemaic.' But because there were so many variant systems, the adjective 'Ptolemaic' had lost much of its meaning. The astronomical tradition had become diffuse. (CR, 139–40)

Contrary to historical evidence, Kuhn assumed that Copernican astronomy was different from Ptolemaic and was able of substituting plurality for unity. Apart from the fact that this interpretation is at odds with the proliferation of cosmological and planetary models after Copernicus, one might legitimately ask: why should unity be superior to variety since the history of Renaissance astronomy witnesses rather to the contrary?

This remark can be extended to other periods and intellectual shifts. For instance, in an article on Kuhn's employment of his epistemological categories to the emergence of quantum theory ("a scientific revolution *par excellence*"), Jochen Büttner, Jürgen Renn and Matthias Schemmel argued, against the gestaltic-switch thesis, that "crisis" was the outcome rather than the source of theory discontinuity and that crisis might even count as a feature of "normal science" (Büttner, Renn, and Schemmel 2003, 56).

Controversy versus Linguistic / Conceptual Misunderstanding

Kuhn's conversion-like treatment of the Copernican paradigm shift downplays the argumentative strategies employed in the controversy over the heliocentric and geokinetic theories. Scholars' choice between terrestrial mobility and immobility was indeed complex and depended on the weight they attached to special aspects at the expenses of others, as well as on their philosophical and cultural choices, and their political and religious bias: e.g. the lack of observable stellar parallax and the physical and scriptural difficulties were enough for Brahe to

reject terrestrial motion but not for Bruno, Galileo and Kepler who developed very different counterarguments depending on their philosophical backgrounds and convictions.

By contrast, Kuhn tended to treat controversies as mere misunderstandings. For instance, he linked Copernicus's preference for mathematical-astronomical harmony, as opposed to Aristotelian natural philosophy, to his lack of comprehension of the reasons against terrestrial motion resting on terrestrial physics:

> But an excessive concern with the heavens and a distorted sense of values may be essential characteristics of the man who inaugurated the revolution in astronomy and cosmology. The blinders that restricted Copernicus' gaze to the heavens may have been functional. They made him so perturbed by discrepancies of a few degrees in astronomical prediction that in an attempt to resolve them he could embrace a cosmological heresy. (CR, 184)

Kuhn's statement has no historical evidence since Copernicus's education, his concerns about scholastic and theological opposition to his hypotheses as well as his first-hand knowledge of Aristotle bear witness to the contrary.[26] Thus, Copernicus's writings document his awareness and commitment in favor of precise epistemological and philosophical views instead of blindness depending on his disciplinary affiliation as a mathematician. Let us consider also this quotation:

> Since paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and the apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed. […] Consider, for an example, the man who called Copernicus mad because he proclaimed that the earth moved. […] Part of what they meant by 'earth' was fixed position. […] Correspondingly, Copernicus' innovation was not simply to move the earth. Rather, it was a whole new way of regarding the problems of physics and astronomy, one that necessarily changed the meaning both of 'earth' and of 'motion.' (SR, 149–50)

It might be true that the meaning of many traditional concepts changed alongside the geokinetic perspective. Yet, the fact that concepts had to be defined anew does not imply that "Copernicans" and "anti-Copernicans" did not understand each other, as Kuhn's incommensurability thesis suggests. Bruno, for one, explicitly referred in his writings to the fact that the expression "world" (Latin, *mundus*, and Italian, *mondo*) had a different meaning according to the

[26] See for instance Goddu (2010).

Aristotelian definition of "cosmos" than according to the Epicurean one as "celestial body," which he preferred.[27] He moreover added a third definition of *mundus*, now obsolete, as star-centered planetary system. This example testifies that Renaissance and early modern intellectuals were not incapable of discussing definitions and understand others' philosophical approaches in spite of the fact that they could rebut certain definitions and approaches as undesirable or wrong. This is also clear from the most celebrated *Dialogue Concerning the Two Chief World Systems* by Galileo, which is in fact a discussion of arguments and counterarguments in favor of geocentrism and heliocentrism. Furthermore, Kuhn's example of "the man who called Copernicus mad" is out of purpose to illustrate the alleged linguistic and conceptual changes depending on "paradigm shifts." That man was in fact Martin Luther who is reported to have rejected the heliocentric system on the basis of scriptural passages. I really doubt that, on this point, Luther could miss the Copernican meaning of 'earth' and 'terrestrial motion.' He simply dismissed this opinion knowing what he was rejecting. Furthermore, *pace* Kuhn, Luther barely referred to the Bible and not to Aristotle or Ptolemy, and did not really care about the Aristotelian definition of earth as a heavy and fixed element but only about the literal meaning of certain scriptural passages.

Concluding Remarks

The issue of paradigms and paradigms' shifts has been crucial in the reception and discussion of Kuhn's epistemology from the very beginning. In particular, incommensurability, connected with the thesis of the gestaltic switch, seemed to many commentators to downplay or even neglect the centrality of rational argumentation in the development and discussion of scientific theories. Kuhn faced the criticism of irrationality on several occasions, beginning with his "Reflections on my Critics" that was included in the proceedings of the 1965 *International Colloquium in the Philosophy of Science* (Lakatos and Musgrave 1970, 231–278). In a postscript to the 1969 edition of the *Structure*, Kuhn answered to his critics' objections to the non-argumentative character of the choice between two paradigms—brief, of a "scientific revolution."[28] In the section entitled "Exemplars, Incommensurability, and Revolutions," he just reaffirmed his point of view stressing the difference between a scientific controversy that takes place within a given framework of accepted rules and premises (i.e., "normal science") and discussion over the premises of the scientific discourse themselves. Con-

[27] See Omodeo (in press).

[28] Cf. Kokowski (2001, 136–138). For critics of Kuhn's concepts of paradigm and paradigm shift, see Lakatos and Musgrave (1970).

troversies over foundational aspects ultimately rest on persuading colleagues and new generations within the scientific community. To corroborate his thesis, Kuhn thus introduced a sociological element into epistemology. Still, this shall not obscure the profound difference between such minimalist sociologization of science and Marxist historical materialism. Kuhn's perspective did not abandon the intellectualistic understanding of scientific advance and never embraced in his treatment socio-economical and political factors. "Sociology" for him never meant anything more general than academic interactions and exchanges at the level of the scientific community. Nor did Kuhn ever try to overcome the individualist characterization of discovery. By contrast, in his theory the moment of discovery remained the inexplicable moment of paradigm shift—notwithstanding the fact that "awareness" could precede the full unfolding of a "paradigm."[29] In this sense, the epiphany-and-conversion metaphor is revealing of Kuhn's radicalization of contemporary claims for the intrinsic intellectualism of science. On this account, he went much further than his *maître à penser*, Koyré.

Steven Fuller argued for the structural correspondence between the Kuhnian paradigms and incommensurable Cold-War worldviews (Fuller 2000, 175). As we have seen, there are passages in Kuhn's writings documenting that his epistemology echoes political constellations. This is for instance evident in the manner he contrasted the 'French school' of Duhem and Koyré against the historiography affected by social preoccupations. Apart from this, I deem the attempt at intellectualization / spiritualization of science to be not less dependent on Cold-War and post-World-War cultural 'paradigms.' The crucial problem was the propagandistic necessity, within American democracy, to foster the wide support on the part of public opinion for scientific investments aimed to warranty the military superiority of the United States, even after the horrors of technological war had cast irredeemable doubts on the linearity and irreversibility of scientific progress. Kuhn offered an understanding of science restoring the 'innocence' of its public image. As Westman put it,

> What Kuhn neglected to say in *Copernican Revolution*, however, was that postwar science no longer gained its legitimacy in a sociopolitical order dominated by ecclesiastical universities but from an alliance amongst secular disciplines and secret agreements between the military, science, and bureaucratized universities. Science no longer earned its authority by showing its harmony with the Book of Genesis but by using radar technology to control the invisible realm across which airplanes were guided to their targets. (Westman 1994, 114)

[29] SR (86), where Kuhn pits the "consciousness of breakdown" to the "emergence of a new paradigm."

Conant's program of scientific popularization was closely connected with these political issues and Kuhn's *Copernican Revolution* proved the most successful textbook in his *Case Histories in Experimental Science*. As Kostas Gavroglu argued in a recent conference on *Science as Cultural Hegemony* (Barcelona, 22–24 January 2014), "scientific popularization and the various forms of knowledge in circulation are involved in the process of continuous rearticulations of the dominant hegemonic ideology."[30] The political program behind Conant's popularization efforts was precisely directed toward the US civil society aiming to create a public opinion supportive of the tremendous costs of war and post-war science.[31]

The religious vocabulary employed by Kuhn to describe the emergence of heliocentric astronomy is not just a matter of words (elsewhere Kuhn also employs military metaphors like "battle" and "victory").[32] Rather, it is symptomatic of certain difficulties entailed in his notions of paradigm and paradigms' shift which, in turn, were rooted in Cold-War mentality. In the postscript to the 1969 edition of the *Structure*, Kuhn explained the persuasive character of paradigm choice through religious imagery:

> The conversion experience that I have likened to a gestalt switch remains […] at the heart of the revolutionary process. Good reasons for choice provide motives for conversion and a climate in which it is more likely to occur. Translation may, in addition, provide points of entry for the neural reprogramming that, however inscrutable at this time, must underlie conversion. But neither good reason nor translation constitute conversion, and it is that process we must explicate in order to understand an essential sort of scientific change. (SR, 204)

As I argued on the basis of the Copernican case, there are some major difficulties concealed under the announcement-and-conversion metaphor. To the first class of difficulties belong the incommensurability thesis and its gestaltic-switch corollary accounting for the (alleged) lack of decisive experiments or arguments in favor of one of the two irreconcilable paradigms. A further issue is the one-idea-centered concept of paradigm, according to which intellectual history deals

[30] I am quoting from the conference pre-circulating paper.

[31] Cf. Nieto-Galan (2011, 453): "As chairman of the Anti-Communist Committee in the 1950s, and designer of science education policies, James B. Conant, Kuhn's mentor, strongly supported an uncontroversial, neutral science, which was to be transmitted to the younger generations as a taken-for-granted worldview far from any critical reflection on the material conditions of thought. The Structure reinforced the idea that the scientific process remains essentially the same whenever and however it occurs." As standard references on popularization and cultural hegemony, see Shapin and Barnes (1977) and Cooter and Pumfrey (1994).

[32] For a treatment of Kuhn's rhetoric strategies in support of his narratives, see Kokowski (2001, 160–199).

with the production and effects of single ideal entities (say, the *heliocentric system, inertia*, or the *great chain of being*, to mention some of Kuhn's sources of inspiration)[33] instead of the constant combination and reorganization of clusters of ideas. In order to account for the historical development of science, the Copernican case suggests to recognize the dialogical-argumentative character of the natural discourse, the permeability of different worldviews and approaches to nature, as well as the composite character of natural and scientific conceptions. The latter are ideas' clusters marked by plurality and variety, rather than total systems hinged on one idea or a small set of ideas.

Still, as I have argued at length, the Kuhnian problematic cannot be reduced to the modeling of science and scientific processes. My main point has been to show that Cold-War mentality (if one prefers, "Cold-War *ideology*") significantly pervaded Kuhn's epistemological premises and conclusions. The historical axioms looming behind the thoughts and conceptions of Kuhn and of his contemporaries or immediate forerunners shall be investigated, questioned and reassessed, taking into consideration the material context out of which they emerged. After the end of the Cold-War Era and of its the ideological divides, we can better detect the political-cultural concerns and limitations lying behind the epistemological discourse of those years. Economic determinism and disembodied narratives seem to be the two opposite pitfalls that the exponents of the opposite camps were not always able to avoid in their role as intellectuals belonging to one of the *Two Chief World Systems* of the Cold-War Era. As for Kuhn, his understanding and practicing of historiography and philosophy was inscribed within these geo-cultural coordinates. As I said, the influence of Harvard president Conant, as an *organic intellectual* of McCarthyist US should not be underestimated. Kuhn's historiography, epistemology and even *popularization of science* represent a clever and successful unfolding of the cultural agenda of his time. Thus, notwithstanding the author's claims for *structural meta-historicity*, one can consider the Koyréan legacy and his account of Copernicanism to be deeply rooted in the political climate of the time. In conclusion, not only did the *Copernican Revolution* anticipate the epistemology of the *Structure* but, more importantly, political-theoretical assumptions guided and even distorted the historical reconstruction of Copernican astronomy.

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[33]Cf. Koyré (1939) and Maier (1951) for inertia-centered interpretations of classical mechanics. Cf. Lovejoy (1936) for the history of the idea of "the Great Chain of Being."

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