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Steve FULLER[1]

KUHN AND THE MYSTERY OF CONSTITUTING THOUGHT, WORD AND DEED INTO A WORLD

Abstract. Ever since my first book, *Social Epistemology,* I have argued that Thomas Kuhn's philosophy of science presupposes a version of 'many worlds realism'. This paper continues that line of argument by situating Kuhn's thinking about language and science in the context of shifting philosophical developments in the 1950s-1970s. Kuhn's view is related to others exposed to the same developments, especially Willard Quine, Donald Davidson, Wolfgang Stegmüller and Karl Popper. Notably, Quine and Davidson were not tempted to go down the 'many worlds' route, largely due to a background commitment to a behaviorist understanding of language that precluded any role for 'world-making'. However, Alfred Tarski's 'semantic' theory of truth made a notable impression on the logical positivists and Popper, inclining the latter towards his own version of many worlds realism. As Kuhn astutely observed in his later writings, whether one adopted a monist or pluralist approach to the world depended on whether translation or meaning was the key to making sense of language. The paper ends by suggesting that the German historiographical concept of *Sonderweg* ('special way') might provide an interesting, more normatively charged understanding of the sort of many worlds realism promoted by Kuhn.

Keywords: Kuhn, Quine, Popper, realism, translation, meaning, *Sonderweg*

Many mysteries surround the massive reception of *The Structure of Scientific Revolutions* (Kuhn 1970), which made it the most influential work on science in the second half of the twentieth century (Fuller 2000). One such mystery is the relative ease with which philosophers who had

[1] "Auguste Comte" Chair in Social Epistemology, Department of Sociology, University of Warwick, U.K, https://orcid.org/0000-0002-2190-6552. Email: <S.W.Fuller@warwick.ac.uk>

previously discussed science almost exclusively in terms of its language accommodated Kuhn's perspective, in which language occupies a *prima facie* subordinate position. As Science and Technology Studies researchers have rightly observed, pre-Kuhn philosophy of science was largely about the logical structure of scientific theories, even when their dynamics were considered (*à la* Popper and Lakatos), while after Kuhn greater attention was paid to the 'practices' of science in a broad sense that seemed designed to include anything *but* the language of science – at least as it appears in academic journal articles.

One explanation for this curious situation is that notwithstanding the subtleties of the debates in the philosophy of language in the 1960s and '70s, most philosophers of science still had a looser attitude to language than their philosophy of language colleagues. For philosophers of science, language was simply the expression of thought in a privileged form. This certainly explains the logical positivists' original attraction to Gottlob Frege's *Begriffschrift* ('thought writing') approach to logical notation (Sluga 1980, chap. 3). Against this backdrop, Kuhn's apparent demotion of scientific language still left the 'thought' informing the language very much intact, now understood in terms of scientists' beliefs or the intended objects of their inquiries. Kuhn was interpreted to have simply distributed this 'thought' across the variety of artefacts and behaviors that constitute 'scientific practice'.

This idea had already become familiar through the 'structural anthropology' of Claude Lévi-Strauss, whose bricoleur-based image of the 'savage mind' suggested that thinking could occur just as much outside as inside the head. This thesis has been developed further by cognitive scientists and anthropologists as the 'extended mind' and 'cognition in the wild' (Levi-Strauss 1966; cf. Clark 1997, Hutchins 1995). Among philosophers of science themselves, such a loose attitude to language helped to boost the fortunes of the 'language of thought' thesis in cognitive science debates in the 1980s, as championed by Jerry Fodor (1975). Fodor went on to refocus Kuhn's 'incommensurability' between scientific paradigms as 'cognitive impenetrability' within a single mind, whereby a scientist might continue to respond to something as if it were real even if after they have been told that it is not real. This routinely happens to non-scientists in the case of optical illusions (de Gelder 1989).

Nevertheless, Kuhn's later work clearly shows that, appearances to the contrary, he took those earlier debates in the philosophy of language very seriously (Kuhn 2000, chaps. 2-3). It resulted in an irony that only Kuhn might have appreciated: What philosophers liked about Kuhn was 'incommensurable' with what Kuhn liked about philosophers. This perhaps explains why Kuhn's discussions of the major contributors to the philosophy of language of the period – Willard Quine, Hilary Putnam and Saul Kripke – were never reciprocated. Only Kuhn's Princeton student Philip Kitcher (1983) responded to him dutifully on matters at the interface between the philosophy of language and the philosophy of science. Yet, there is no doubt that while writing *Structure*, Kuhn was subject to many of the same influences as Quine (1960) when he was writing *Word and Object*, both in the late 1950s.

At that time, the dominant presence in psychology at Harvard was B.F. Skinner, who was writing the definitive account of language as the operant conditioning of 'verbal behavior', which he understood as an indirect but economical means to get things done in the world – namely, by using people as intermediaries. This arguably made human language the most efficient of all animal communication systems, at least as measured by the relationship between signifying effort and material consequences (Skinner 1957; cf. Fuller 1988, chap. 2). For Skinner, as for Quine, the key is people's responsiveness to what is said to them rather than any intrinsic connection between word and object. Thus, if you hand over a knife when asked, it does not matter to the success of the transaction whether you are providing a kitchen utensil or a lethal weapon. Indeed, to worry about such matters constitutes what Quine (1974) called the 'original sin' of language, whose fallen users science ultimately 'redeems'.

Quine's appeal to Augustinian imagery here was intended as a call for scientifically minded philosophers – so-called 'naturalized epistemologists' – to observe the conditions in the world that regularly make communication involving a given set of words successful. These need not correspond to what the communicating parties themselves think they mean when speaking and responding to those words. Indeed, the parties may mean different things when they use the same words without affecting the efficacy of the exchange. Moreover, there need not

even be a fact of the matter that settles which party knows the 'meaning' of their communication. All that is required is that the exchange sufficiently satisfies the parties that they continue in productive engagement.

Quine characterized this feature of language – again, understood as verbal behavior – as the 'indeterminacy of translation', whereby the same set of utterances can be subject to indefinitely many different interpretations, each of which accounts for the linguistic phenomena equally well. A special case applies to science, whereby theory choice is 'underdetermined' by the available evidence. Here one needs to imagine – as arguably Francis Bacon did – that a laboratory experiment is a transaction between the scientist and nature, the ultimate significance of which is secondary to its ongoing reliability. In this context, what matters is that the 'evidence', understood as the result of a controlled observation, can be routinely generated. Without that baseline condition, the various theories on offer to explain the evidence lose their salience.

The historic precedent for this way of thinking – beyond Pierre Duhem, who is often cited – is Leibniz's idea of *phenomena bene fundata* ('well-grounded phenomena'). This becomes especially important in twentieth century quantum mechanics, whereby the pattern of microphysical observations conform to a range of mathematical formulae (*e.g.*, Heisenberg's uncertainty principle, Schrödinger's wave equation, Dirac's delta function), the meanings of which remain highly contested by both philosophers and physicists – but without disturbing the normal conduct of scientific research in terms of testing hypotheses whose outcomes can be agreed (cf. Hesse 1963, chap. 1). However, here's the twist, which reveals the power of Skinner's operant conditioning: Because a phenomenon is 'well-grounded' only if it can be demonstrated on a reliable basis, it must be producible on demand, somewhat like a dramatic effect or even a magic trick. The history of modern science is largely about the management of these demonstrations (cf. Shapin 1994).

Kuhn shrewdly observed that for Quine, the indeterminacy of translation implies that the quest for universal translation and common meaning are mutually exclusive projects (Kuhn 2000, 61). Whereas for Quine common meaning must go, for Kuhn universal translation must go. What is at stake here? Consider what Quine famously called 'referential opacity' (Quine 1960, chap. 4). It is another way of expressing what he

regarded as the original sin of language, namely, to think that talking about things in radically different ways implies talking about radically different things. On the contrary, the same thing may be talked about in radically different ways. Moreover, following Frege, Quine believed that by regularly demonstrating an underlying identity to disparate appearances, science has over the centuries brought order to the world in a way that can be most perspicuously captured in mathematical logic. Thus, an important moment in astronomy's ontological consolidation came when Pythagoras discovered that the 'Morning Star' and the 'Evening Star' refer to the planet Venus under different conditions of observation.

However, it is not clear that this neat episode is representative of science as a whole, since it involves tracing back two functionally differentiated descriptions ('morning star' and 'evening start') to a common physical source prior to its functional differentiation (Venus). But when the physical object is known from the outset – as in the case of the knife discussed above – over time it may become functionally differentiated (*e.g.*, into a kitchen utensil and a lethal weapon) to such an extent that the object is no longer significant in itself but only as a placeholder for other things that could perform the same function. Such a situation is reasonably understood as one in which the original object has acquired multiple meanings, which may easily result in a breakdown in the established response patterns that in the past had stabilized the exchange of words and deeds. In short, we may come to talk about radically different things when using a word such as 'knife'. This is how Kuhnian incommensurability gets a foothold in our understanding of language and science. Thus, the shift from Quine to Kuhn in terms of the idea of 'language as tool' amounts to an evolution from navigating the one world to constructing alternative worlds.

Quine is often portrayed as viewing science as a continually evolving 'web of belief' that aims for internal coherence as it incorporates new data. The phrase 'web of belief', taken from the title of a popular book on reasoning that Quine published later in his career (Quine and Ullian 1970), is somewhat unfortunate, since a Quinean 'belief' is a non-psychological state closer to Gilbert Ryle's characterization of concepts as 'inference tickets', a verbal rite of passage in making one's way in the world. In this context, a 'theory' is simply an account of the world's

coherence at various moments in science's evolution that allows one to make further progress. It reflected Quine's nominalist approach to logic and metaphysics, which retained the logical positivist view of 'meaning' as a purely private matter lacking 'cognitive significance'.

The positivists had applied this phrase to the public character of language as normally demonstrated in 'translation' in that broad positivist sense that includes not only a successful exchange of words but also the conversion of a string of such words into a prediction (aka 'operationalization'), say, during the test of a hypothesis in a laboratory experiment. In any case, what Quine meant by 'cognitive significance' was most certainly *not* a Kuhn-style 'paradigm', with its implication of a world-picture whose vision defines a problem horizon. Indeed, Kuhn's semantically rich conception of paradigm – or 'model', in the jargon of more recent philosophy of science – ultimately threatened to commit the original sin of language by letting the words determine the world, rather than vice versa.

Donald Davidson (1986) carried this line of thought to its logical conclusion, effectively outperforming Quine at his own game. Davidson argued that what linguists and logicians call 'semantics' is nothing but the moment-to-moment reconciliation of prior expectations and passing responses. This was a more fine-grained articulation of Davidson's widely cited but often misunderstood 1973 presidential address to the American Philosophical Association, 'The Very Idea of a Conceptual Scheme' (Davidson 1974). There he had argued that the sort of incommensurability that Kuhn associated with paradigm differences amounted to what, in homage to Quine, Davidson called the 'third dogma of empiricism' – namely, that one can draw a neat distinction between 'theory' and 'data', with the latter somehow captured and extended by the former. Put bluntly, Davidson followed Quine in denying the existence of either separate languages or separate theories, let alone *Weltbilder* that might be projected from them, as Kuhn's conception of paradigm seemed to suggest. Both believed in a 'flat ontology', in which organized strings of words function as relatively durable but ultimately makeshift tools for navigating the only world in which we all live, regardless of whatever private views one might hold about the 'meanings' of those verbal tools at a given time and place.

A good way to see the stakes here is to consider *symbolism*. When religion, literature and art are said to be 'symbolic' media, the implication is that the words and images used to convey certain ideas participate in those ideas to such an extent that they may come to be treated like the realities to which the words and images refer. For example, the Bible is 'sacred' because its readers treat the biblical text as a privileged portal to a highly valued sense of reality. This 'privilege' is evident from the seriousness – sometimes misleadingly called 'literalness' – with which each verbal formulation in the sacred text is taken as an invitation to imagine an alternative reality. And because the text is sacred, it is taken as normative over the actual world, a potential prompt to innovative and even violent performance.

Freud spoke of this approach to symbols as 'fetishism', and it subsequently became the target of the logical positivists, who took a different, more demystified approach to symbolism. For example, the positivist Otto Neurath championed the 'ISOTYPE' ('International System of Typographic Picture Education'), whose advertised virtue was that its pictographic character could trigger a certain range of actionable responses. He envisaged that such symbols might inform societal transformation, and they were valuable only insofar as they enabled the desired transformation. In short, the symbols had no intrinsic value and merited replacement if they failed to do their intended work. Quine and Skinner would be pleased.

For his part, Kuhn treated the mathematical formulae that constitute a paradigm's 'symbolic generalizations' as a framework for identifying patterns in the data generated by normal science research. In this respect, they are not so different from Leibniz's *phenomena bene fundata*. However, through repeated application, these symbols can acquire the sort of larger meanings associated with a more robust conception of symbolism, as the formulae are integrated with the experience of researchers who think about other (philosophical, political, etc.) matters similarly and interact with each other regularly. Over time they may become schools of thought, or 'thought collectives', to recall the expression used by the Polish medical researcher Ludwik Fleck (1979), who may or may not have influenced Kuhn's idea of paradigm. In effect, these collectives spontaneously generate 'metalanguages' (more about

which below), which are interpretations of the paradigm's symbolic generalizations that channel and circumscribe their application.

The social psychology of this situation is Janus-faced. While thought collective members are thereby motivated to do more focused work, their ability to understand the work generated from alternative thought collectives is impeded. The significance of this point can be seen in the case of persistent anomalous results that confound all those working in a field, who in response draw on other resources to interpret the findings. It is here that 'incommensurability' arises, as meanings of the formulae that were previously privately circulated within a given thought collective start to be discussed openly among all the field's thought collectives, which bring to the surface submerged 'philosophical' differences about the original spirit of their common inquiry. This situation threatens to destabilize the paradigm, as language is increasingly deployed to partition the one reality that the scientists had heretofore presumed that they shared. It amounts to Quine's and Skinner's worst nightmare.

There is one sense of the notoriously protean term 'paradigm' on which Kuhn and Quine could find common ground. Arguably, it is the point from which they subsequently diverged, as Kuhn traveled down the path of referential opacity (*i.e.*, incommensurable meanings) and Quine of referential transparency (*i.e.* translatable languages). It is the sense of 'paradigm' as template or exemplar, what analytic philosophers used to call a 'paradigm case'. For Kuhn, this is the heart of puzzle solving in 'normal science'. The phenomena of nature are disciplined by the artifice of laboratory, according to a recipe for constructing problems in a way that affords solutions by applying the normal methods of science. The recipe is anchored in an original episode that proved especially efficacious, stylized versions of which continue to be presented in scientific textbooks. I say 'recipe' to convey the extent of staging and scripting required for the paradigm to work. Whereas Skinner had talked about the reinforcement of such 'operants' according to an appropriate 'schedule', Quine believed in a general 'predilection for conformity' that underwrote any schedule of reinforcement (Quine 1960, 75). Perhaps here he was influenced by Charles Sanders Peirce, who believed that 'habit' was built into the emergent structure of the cosmos,

such that 'evolution' amounts to a gradual lessening of the role of chance in the universe over time.

Kuhn did not weigh in on whatever metaphysical differences may have divided Skinner and Quine. Instead, he focused on the potential unintended consequences of applying a paradigm to new cases, which he called 'anomalies'. For example, the Newtonian paradigm was designed to account for motion in all its material forms, yet the motion of light remained stubbornly anomalous for two centuries. To be sure, Kuhn shared Quine's general 'conservative' approach to these matters, namely, that the default response should be to assimilate each anomalous episode to the existing paradigm as much as possible, and whatever cannot be assimilated should result in a minimal alteration of the paradigm, with an eye to accommodating other similar cases in the future. However, unlike Quine, Kuhn believed that the history of science has demonstrated the limited feasibility of this strategy; hence, the need for 'scientific revolutions' that periodically reset the focus of the templates governing the scientist's transactions with nature. But how might such anomalies in the application of the paradigm persist and accumulate to the point that they can no longer be contained by conservative adjustments, such that what Kuhn called a 'crisis' develops, which in turn precipitates a radical paradigm shift?

Both Quine (1960) and Kuhn (1970) cite fellow Harvardian Eugene Nida (1964) as a primary authority on translation, perhaps because at the time Nida was developing a theory of translation based on the most widely translated book, the Bible. He stressed two radically different functions that translation might serve: on the one hand, it may seek to create greater distance between the original text and the readers of the translated text to introduce them to an alternative way of seeing the world; on the other hand, it may seek to minimize the distance by encouraging readers to think and act along lines that they have already been at least implicitly pursuing. The Bible's reception history can be easily understood through these opposing lenses. The former, more alienating translation has often functioned as part of a strategy to deploy the Bible as a metalanguage for critiquing certain beliefs and practices of the text's readers. It is favored by Biblical scholars, starting with rabbis, whose expertise in the original meaning of the sacred text qualified them

to function as judges over the Jewish community. The latter, more familiarizing translation has often served to reinforce certain existing beliefs and practices of the text's readers by suggesting that they enact a version of what the Bible intended. From the onset of Christianity, it has been favored by evangelists, as exemplified by the centrality of the Gospels (generic 'good news') and the Epistles (targeted messaging) in the New Testament, both intended to present the Biblical faith as something very much within the reader's reach.

The second part of my first book, *Social Epistemology*, was largely devoted to working through the implications of Nida's Janus-faced view of translation in relation to the historiography of science, as well as recent French, German and Anglo-American work in the philosophy of language (Fuller 1988, chaps. 3-6). Nida himself cast the contrast in translation strategies in terms of 'formal' versus 'dynamic' equivalence, which looks like the difference between the semantic and pragmatic dimensions of language. However, in linguistics, these two dimensions are normally seen as complementary rather than opposing. In other words, semantics is supposed to be enriched by pragmatics, rather than 'semantic' and 'pragmatic' being alternative modes of translation, as Nida seemed to suggest.

Here I would argue that Quine and Kuhn understood the matter very much as Nida did, but they chose alternative translation strategies: Quine favoring dynamic equivalence and Kuhn formal equivalence. This explains Quine's (and Davidson's) notably demystified view of semantics. For them, the translated text is not designed to stand judge over readers in the translating language; rather, it is to be incorporated as equipment in the readers' repertoire of tools for dealing with the world as they already find it. In contrast, by stressing the radical otherness of the translated text, Kuhn presupposed a limit to meaningfulness, whereby the significance of some things can only be fully understood by inhabiting the world from which their meaning derives. In effect, Kuhn treated language not as a toolkit but an infrastructure. Thus, a text originally written before the reader was born and in a language that they do not speak implies a limit to the reader's world.

In this respect, Kuhn relativized the view of language found in Wittgenstein's *Tractatus*, which originally led the logical positivists,

notably Rudolf Carnap (1937), to develop the idea of semantics as metalanguage, especially after his encounters with Alfred Tarski. They treated Proposition 5.6 of the *Tractatus* ("The limits of my language mean the limits of my world.") as a philosophical challenge to overcome. Kuhn agreed with Carnap that the solution was not the one 'flat' world that Quine and Davidson would later advocate, but in a vision of 'many worlds'. However, Carnap and Kuhn differed over the arrangement of these worlds. Carnap's worlds were organized hierarchically, with one, so-called 'meta' language setting the truth conditions for another, so-called 'object' language. Such an arrangement suited a progressive view of scientific inquiry, whereby later theories comprehend and extend earlier ones, while identifying and removing their falsehoods. Indeed, this formed Wolfgang Stegmüller's (1976) attempt to reconcile Kuhn and logical positivism, effectively supplying the 'logic' to update Auguste Comte's original nineteenth century positivist program.

In contrast, Kuhn believed that these 'many worlds' existed not in a hierarchy but 'in parallel', so to speak (Kuhn 2000, 76). He literally held that the past is a foreign country, separated from the present in time as if in space. It led him to advocate the now academically popular opinion that a science and its history are 'separate but equal' fields of inquiry, each requiring its own kind of specialist. The position is perhaps most noticeable in its negative effects on the public communication of science, as incommensurable disciplines engage in a dialogue of the deaf. Thus, historians routinely declare scientists to be ignorant of the history of their own field, to which scientists respond that historians are irrelevant to their cutting-edge research. On this matter, Quine sided squarely with the scientists, treating the 'history of science' as something that scientists leave behind, a bit like the husks that seeds discard as they mature (Rorty 1982).

Kuhn thought that such crosstalk between scientists and their historians was ultimately futile and missed the point of incommensurability. A notorious feature of Kuhn's account of scientific change is that each new paradigm seals itself off from the past of its science by an 'Orwellian' rewrite of the science's history, which portrays all its past achievements as contributions to the new paradigm, as if other paradigm contenders had never existed. Such historiography involves enormous cherry-picking

and airbrushing, resulting in a history of science that is often virtually unrecognizable to professional historians of science. Nevertheless, Kuhn was comfortable with that arrangement, advising that professional history of science should only be done of 'closed sciences', a phrase originating with Werner Heisenberg, whom Kuhn interviewed for the US National Science Foundation's history of quantum mechanics project in the 1960s. An interesting difference between Kuhn and Heisenberg was that Heisenberg saw the closure of the past paradigm as reflecting open horizons in the new paradigm, whereas Kuhn stressed, rather Quine-like, the capacity of scientists in the new paradigm to focus more effectively on their inquiries once they treat their predecessors as dim signals dominated by noise (Bokulich 2006).

In this discussion of the dynamics of scientific change, Karl Popper proves to be an interesting witness, since he was at least as much influenced by Tarski as Carnap and the positivists were. However, Popper was attracted by a logical sensibility that Tarski shared with Kurt Gödel, namely, that no consistent language can determine the truth of all its propositions: It requires another language with greater expressive capacity than the original language. Over the years, Popper nurtured the insight to conclude that this 'greater expressive capacity' required the generation of new 'objects' that amounted to problems that needed solving once the metalanguage projecting these new objects resolved the truth conditions of the object language (Popper 1972, chap. 9). While one might be tempted to interpret Popper in the manner of Stegmüller, their projects were radically opposed in spirit: whereas Stegmüller sought epistemic closure in science, Popper embraced science's ontological openness.

This point is perhaps best illustrated by the suspension of axioms of Euclidean geometry, which in turn opened the door to the world of non-Euclidean geometries, which ushered in the relativity revolution in early twentieth century physics. For Stegmüller, Einstein's breakthrough was simply about explaining Newton as a special case of relativity theory, while for Popper it showed that by retrieving the conditions of the possibility for Newton being as correct as he was (*i.e.*, the set of coherent geometries of which Euclid's is a member), Einstein could find an alternative geometry that could encompass more of the physical universe. In terms of scientific method, Stegmüller treated Einstein as

proceeding *deductively*, whereas Popper treated him as proceeding *abductively*, which meant a recognition of the capacity of mathematics to extend our sense of physical reality beyond what had been empirically allowed (cf. Wigner 1960). In this respect, the English translation of the title of Popper's *Logik der Forschung* – *The Logic of Scientific Discovery* – may not have been so bad, after all.

Put in Marxist terms, metalanguages produce 'surplus value': They not only decide which truth claims live or die but also provide the conditions for new truth claims to thrive. In the history of science, physics has been best positioned to exploit the surplus value of mathematics, whereby an epistemological excess is converted into ontological profit. What start as mathematical innovations of use only to fellow mathematicians turn out to prefigure a new sense of reality that physics and the other sciences capitalize on. In short, scientists discover what mathematicians can only imagine. This may be the best way to think about the reality of what Popper (1972) called 'World Three'. Randall Collins has provided an interesting sociological account of how mathematics has performed this function, namely, by reflexively taking itself as the subject of matter of its inquiries, with the aid of standard notation that functions as a scaffolding to increase the levels of abstract thought (Collins 1998, chaps. 10, 13).

At a deeper level, the productive capacity of metalanguages – be they articulated in words, numbers or symbols – speaks to what the Greeks originally called *poiesis*, a quasi-divine power to conjure up worlds in speech that was possessed by those who were adept in the arts of poetry, drama and rhetoric. For Plato, the free deployment of these arts threatened social order, as rapt audiences are induced to entertain alternative normative regimes to the ruling one (Fuller 2018, chap. 2). In this regard, Kuhn is a latter-day descendant of Plato in believing that the unchecked proliferation of multiple worlds – aka 'paradigms' – would undermine the purposefulness of scientific inquiry. This helps to explain Kuhn's antipathy to sociologists who in his day claimed that theirs is a 'multiple paradigm science' (Ritzer 1975). Yet, again more like Plato and less like Quine, Kuhn did not quite wish to discard lines of inquiry that had been abandoned by scientists; he simply wanted to restrict – if not outright prevent – their access to the main business of science. While

Kuhn did not see the history of science as *progressing to* the ultimate true account of reality, he did see it as *progressing from* its past (Kuhn 2000, chap. 5). In that sense, the history of science has an increasingly fictional ('artefactual', euphemistically put) standing in relation to the dominant scientific paradigm of the day. This perhaps explains why the historians of science following in Kuhn's footsteps have tended towards the methods of art history to interpret past science (*e.g.*, Daston and Galison 2010).

But of course, one might share Kuhn's belief in the existence of simultaneously existing multiple worlds without privileging the frame of reference of present-day science. In other words, one might treat the dominant paradigm and its various alternative pasts, presents and futures in both ontologically and *normatively* symmetrical terms. This would be Plato's worst nightmare, but he understood exactly how it could – and did, in his day – happen. The alternative world is not only presented more attractively than the world as normally experienced, but also in that world the actual world appears somehow deficient in ways that might motivate actions to realize the alternative world. In this context, the great mid-twentieth century avant-gardist Antonin Artaud (1958) spoke about the 'theatre and its double', whereby the thoughts and feelings that a dramatic performance induces in audiences might not simply end in the theatre but spillover into the streets. But this state of 'critical alterity', so to speak, need not be so dramatically expressed. For the nearly half-century of the Cold War, Marxist historical materialism coexisted with liberal bourgeois social science as parallel universes, each portraying the other as extreme deviations from objective reality, aka 'ideology'. But perhaps worthier of further pursuit is the historiographical concept of *Sonderweg* ('special way'), which has been deployed both positively and negatively to characterize the distinctive path that modern German history has followed (Wehler 1985).

Sonderweg is ambiguous because while 'special way' clearly implies a path that has branched off from the dominant trajectory, it is unclear whether that 'specialness' lies in its preserving and fully realizing an original spirit that the dominant trajectory has lost or even corrupted over time or, on the contrary, its having transformed or even perverted that original spirit as the distinct character of the alternative path has unfolded. In either case, the facts and order of events need not be under

dispute. Rather, the focus is squarely on the potential for normative reversal in the narrative holding them together, effectively flipping the positive and negative evaluative poles. While the appeal of *Sonderweg* is easily appreciated as a way to understand Germany's dramatic rise and fall over the nineteenth and twentieth centuries, it might also be fruitfully deployed to address the increasing visibility of 'alternative paradigms' in science (*e.g.*, ecological, creationist, homoeopathic, etc.) that presuppose not a 'separate but equal' approach to science and its history *à la* Kuhn, but rather an approach to science where the drive for dominance consists in a struggle between rival ways of incorporating contemporary research into a common history.

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Thodoris DIMITRAKOS[1]

KUHN'S PHILOSOPHY OF HISTORY OF SCIENCE AND THE DEFENSE OF SCIENTIFIC RATIONALITY

Abstract. In the present paper, I provide a reconstruction of Kuhn's philosophy of history of science based mainly on Kuhn's criticism of Lakatos. My goal is to examine the compatibility of the Kuhnian philosophy of history with his explicit aspiration to defend scientific rationality. I argue that the Kuhnian philosophy of history is essentially formed by three tenets: (a) contextualism, (b) radical anti-presentism, and (c) naturalism. I conclude that the combination of those three tenets is incompatible with the logical distinguishability between being-justified and being-taken-to-be-justified, which is a prerequisite for the proper defense of scientific rationality.

Keywords: Thomas Kuhn, philosophy of history, history of science, scientific rationality, Imre Lakatos

1. Introduction

Thomas Kuhn is the central figure of the historical turn in the philosophy of science which, according to the standard narrative, overthrew the so-called 'received view' established by the logical positivists and Popperian falsificationism[2]. Kuhn's historicism is explicitly stated in the very first

[1] Department of Philosophy, University of Patras, https://orcid.org/0000-0001-6990-1980. Email: <thdimitrakos@upatras.gr>.

[2] However, against the oversimplification of the standard narrative, we can note that Kuhn's *Structure of Scientific Revolutions* has been published in the book series for which Rudolf Carnap, one of the leading figures of the Vienna Circle, served as editor. For a challenge of the standard narrative with regard to the relationship

lines of *The Structure of Scientific Revolutions*: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed" (Kuhn 1996, 1). Despite Kuhn's (2000b, 2000c, 2022) later regret regarding his early view of history as providing empirical evidence for the philosophy of science, he never ceased to defend a historical – that is, history-informed – philosophy of science. The interpretation of Kuhn's historicist philosophy of science is far from uncontroversial. For instance, there is tension between interpretations that emphasize the naturalistic aspects[3] and those that underline the neo-Kantian elements[4] of Kuhnian historicism. Despite reasonable exegetical divergences, this aspect of Kuhn's thought is well-discussed. What is less discussed is Kuhn's philosophy of the history of science, for which Kuhn himself bears partial responsibility, given that he only provided sporadic comments on the issue. The aim of the present paper is to reconstruct Kuhn's philosophical conception about the history of science. I suggest that the importance of this reconstruction lies in the implicit tension with Kuhn's ambition, to defend a (historicized) version of scientific rationality. Therefore, I will briefly present Kuhn's mature view on scientific rationality and set it against the reconstructed theses concerning the philosophy of history of science. The reconstruction will be provided by focusing on two texts (Kuhn 1971, 1980) in which Kuhn criticizes Imre Lakatos's view on history[5]. Focusing on the criticism of a fellow historicist who "expresses opinions so closely paralleling [Kuhn's] own" (Kuhn 1971, 137) can reveal, I argue, the elements of the Kuhnian philosophy of history that are at odds with his attempt to defend scientific rationality.

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between Kuhn and logical positivism, see (Irzik 2012; Friedman 2003, 1999, 2001, 2002). For a rejection of this challenge, see (Tsou 2015).

3 See (Bird 2004, 2005, 2012a; Shapin 2015). However, there are disagreements even within the camp of the naturalist Kuhnians. Alexander Bird stresses the internalist features of Kuhn's view, while Shapin takes Kuhn as a predecessor of sociological externalism.

4 See (Friedman 2011, 2002, 2001).

5 Further textual evidence will be provided only as complementary.

My argumentation will be developed through the following steps. In the next section I will present Kuhn's late view with regard to scientific rationality. In the third section, I will present Kuhn's criticism of the Lakatosian conception of the history of science. This criticism, I contend, relies on two components: (a) the internal/external distinction and (b) the dilemma of case studies. In the fourth section, I reconstruct Kuhn's philosophy of history arguing that it is fundamentally formed by three tenets: (a) contextualism, (b) radical-antipresentism, and (c) naturalism. Finally, I conclude that contextualism combined with radical anti-presentism and naturalism makes Kuhn's attempt to defend scientific rationality inconsistent.

2. Scientific Rationality Contextualized

Kuhn was well aware of the accusation that his view "proclaim[s] the irrationality of theory choice" (Kuhn 2000a, 208). The accusation is first and foremost concerned with the role that incommensurability plays in the possibility of evaluating successive paradigms. But Kuhn explicitly and repeatedly denied that the notions of incommensurability and rational evaluation are totally incompatible:

> "Properly understood – something I've by no means always managed myself – incommensurability is far from being the threat to rational evaluation of truth claims that it has frequently seemed. Rather, it's what is needed, within a developmental perspective, to restore some badly needed bite to the whole notion of cognitive evaluation. It is needed, that is, to defend notions like truth and knowledge from, for example, the excesses of postmodernist movements like the strong program." (Kuhn 2000b, 91)

Respectively, he "never accepted the description of [his] views as a defense of irrationality in science" (Kuhn 1971, 139). Science is not only rational but also our role model of rationality, for it can drastically shape our conception of rationality.

> "Scientific behavior, taken as a whole, is the best example we have of rationality. Our view of what it is to be rational depends in significant ways, though of course not exclusively, on what we take to be the essential aspects of scientific behavior. […] [I]f history or any other empirical discipline leads us to believe that the development of science depends essentially on behavior that we have previously thought to be irrational, then we should conclude not that science is irrational but that our notion of rationality needs adjustment here and there." (Kuhn 1971, 144)

This kind of adjustment is exactly what Kuhn attempted to provide during the last period of his career. And while he never completed this attempt,[6] he provided sketchy remarks on the issue.

Those remarks reveal a clear intention to form an intermediate position between ahistorical absolutism and historical relativism with regard to scientific rationality. Ahistorical absolutism is the view that the evolution of scientific knowledge takes place by conforming to unchanging rational standards. Those standards secure the progressive character of the evolution in question. This is the perspective, at least according to Kuhn, of the received view (logical positivism and Popperian falsificationism) in the philosophy of science. Historical relativism is the view that there are no rational standards that dictate scientific development. This is the perspective of Paul Feyerabend[7] and of the Strong Programme in the

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[6] In 1990, he wrote: "Clearly, I can't hope to make all that out here: it's a project for a book. But I shall try, however sketchily, to describe the main elements of the position the book develops. I begin by saying something about what I now take incommensurability to be, and then attempt to sketch its relationship to questions of relativism, truth, and realism. In the book, the issue of rationality will figure, too, but there is no space here even to sketch its role" (Kuhn 2000b, 91). By the time of his death in 1996, he never delivered this book. We can only have access to some drafted chapters of this book that were published recently (Kuhn 2022). While they can be valuable for the interpretation of the Kuhnian work in general, I don't think that those drafts can drastically alter the perception of this work, at least with regard to the topic I discuss here.

[7] The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other (Feyerabend 1993, 14).

sociology of scientific knowledge (Bloor 1991), among other 'postmodernist' views. The intermediate position consists in a contextualized perspective which takes rational standards as inevitably operating within a concrete historical framework. Let me provide a passage that is quite long but also the most indicative of this contextualized perspective.

> "On the developmental [*i.e.* in his own] view, scientific knowledge claims are necessarily evaluated from a moving, historically situated, Archimedean platform. What requires evaluation cannot be an individual proposition embodying a knowledge claim in isolation: embracing a new knowledge claim typically requires adjustment of other beliefs as well. Nor is it the entire body of knowledge claims that would result if that proposition were accepted. Rather, what's to be evaluated is the desirability of a particular change-of-belief, a change which would alter the existing body of knowledge claims so as to incorporate, with minimum disruption, the new claim as well. Judgments of this sort are necessarily comparative: which of two bodies of knowledge – the original or the proposed alternative – is better for doing whatever it is that scientists do. And that is the case whether what scientists do is solve puzzles (my view), improve empirical adequacy (Bas van Frassen's), or increase the dominance of the ruling elite (in parody, the strong program's). I do, of course, have my own preference among these alternatives, and it makes a difference. But no choice between them is relevant to what's presently at stake.
> In comparative judgments of the kind just sketched, shared beliefs are left in place: they serve as the given for purposes of the current evaluation; they provide a replacement for the traditional Archimedean platform. The fact that they may – indeed probably will – later be at risk in some other evaluation is simply irrelevant. Nothing about the rationality of the outcome of the current evaluation depends upon their, in fact, being true or false. They are simply in place, part of the historical situation within which this evaluation is made. But if the actual truth value of the shared presumptions required for the evaluation is irrelevant, then the question of the truth or falsity of the changes made or rejected on the basis of that evaluation cannot

> arise either. A number of classic problems in philosophy of science – most obviously Duhemian holism – turn out on this view to be due not to the nature of scientific knowledge but to a misperception of what justification of belief is all about. Justification does not aim at a goal external to the historical situation but simply, in that situation, at improving the tools available for the job at hand." (Kuhn 2000b, 95-96)

In short, despite the incommensurability between two successive systems of beliefs, an evaluation between them can take place based on the shared body of beliefs and according to their problem- or puzzle-solving capacity. Kuhn's developmental perspective leaves room for neither the traditional correspondence theory of truth[8] nor the traditional ahistorical theories of rationality. In other words, it leaves no room for a fixed or absolute framework of evaluation or what he calls an 'Archimedean platform'. But it aims to defend the rational character of scientific knowledge by claiming that each succession of incommensurable paradigms can be seen as justified (and hence rational) according to the criterion of puzzle-solving and in light of the body of shared beliefs by the competitive paradigms.

3. The Critique of Lakatos's Conception of History of Science

At this point, I would like to focus on Kuhn's philosophy of history. I will do that in two steps. First, I will examine Kuhn's critique of Lakatos' philosophical views on the history of science. Then I will attempt to reconstruct the main tenets of the Kuhnian philosophy of history of science.

Kuhn, despite that he agreed with Lakatos that "failure to fit historical data provides grounds for criticizing a current methodological [*i.e.* philosophical] position" (Kuhn 1971, 138), famously objected that "what Lakatos conceives as history is not history at all but philosophy fabricating examples" (Kuhn 1971, 143). The objection is directed against

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[8] For a detailed critical presentation of Kuhn's conception of scientific realism, see (Dimitrakos 2023).

Lakatos' project to use the history of science as an arbiter for the competing methodologies of science, *i.e.* the competing philosophical theories of scientific rationality (Lakatos 1978). Briefly put,[9] this project suggests that every attempt to provide historical understanding of past science is based on an explicit or implicit philosophical theory of scientific rationality. Hence, the history of science is split into an internal history, which consists in the rational episodes, and an external history, which includes the irrational episodes, according to the theory of scientific rationality at hand. The competing theories of rationality, then, can be evaluated both on the grounds of their consistency (*e.g.* whether falsificationism is falsifiable or actually falsified) but also on the ground of their ability to reconstruct the history of science as more rational in comparison to the rival theories[10]. Kuhn's objections against this project can be categorized into two components. The first component has to do with the internal/external distinction. The second has to do with the ability of rationally reconstructed history to provide a test for the philosophical theories of scientific rationality.

3.1. Internal and external history or normative **vs** *empirical scientific explanations*

Kuhn stresses that Lakatos uses the distinction between internal and external history in a quite different way than it is usually employed by the historians of science: "Lakatos' internal history is far narrower than that of the historian" (Kuhn 1971, 140). The common use of the term 'internal' in the history of science includes whatever is concerned with 'internal' relations between the members of the scientific community, while the term external refers to the influences that come from the wider social, economic, political, or more generally, cultural milieu. On the other hand, Lakatos uses the term internal as synonymous with rational reconstruction and the term external as synonymous with an empirical

[9] For a detailed critical presentation of Lakatos' project, see (Dimitrakos 2020b).

[10] *"[P]rogress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational"* (Lakatos 1978, 133, emphasis in the original).

understanding of the irrational episodes of the history of science. Kuhn is correct. Lakatos did change the standard terminology. But he did that for good, I think, philosophical reasons. The standard use of the internal/external terminology is philosophically insignificant. It demarcates what is taken as institutionally internal to scientific practice in each epoch as opposed to the wider cultural milieu. The Lakatosian distinction, on the other hand, is philosophically significant, for it demarcates the normative from the empirical-scientific explanations.

The distinction between the normative and the empirical-scientific explanations – and I cannot but be very sketchy here[11] – is a logical one. Normative explanations make a belief change intelligible by showing how this change conforms to an epistemic norm or set of norms. For instance, one may explain the transition from the Ptolemaic geocentric to the Copernican heliocentric model in terms of showing how the latter is simpler than the former. In this case, the explanation of the belief change is performed by revealing how this change conforms to the norm of simplicity. Empirical-scientific explanations, in opposition, explain a belief change by showing how it is placed in the causal order provided by one or more empirical sciences (psychology, sociology, etc.). One can explain, for example, the emergence of the early interpretations of Quantum Mechanics by showing how the emergence in question is placed within the wider social environment of the post-World War I German-speaking world as described by historical sociology. This distinction is logical because normative explanations of belief changes are at the same time justifications for these changes while empirical-scientific explanations are not. As justifications, normative explanations are characterized by some kind of necessity.[12] If someone asks me why X believes Q, and if I reply by saying that X believes that 'if P then Q' and also 'that P', I have provided a normative explanation of X's belief as it conforms to *modus ponens*. Now, if modus ponens is considered a genuine epistemic norm, and if X is justified to hold 'that P', X is also justified to believe 'that Q'. In other words, X holding Q is a rational

[11] For an extensive presentation of the distinction, see (Dimitrakos 2021).

[12] I don't want to examine what kind of necessity this is. For the purposes of the present paper, my account can remain neutral with regard to the different metaphysical perspectives on the source or ground of necessity.

episode. On the contrary, if I explain X's holding 'that P' by saying that believing P makes X feel less insecure, *i.e.*, by placing X's belief into the causal order assumed by empirical psychology, I have provided an empirical-scientific explanation. It goes without saying that in this case X is not considered justified for holding 'that P'.

Lakatos equates internal history with the sum total of normative explanations and external history with the sum total of empirical-scientific explanations of scientific belief changes. He does that because he rejects what he calls 'historiographical positivism': "the position that history can be written as a completely external history. For historiographical positivists history is a purely empirical discipline. They deny the existence of objective standards as opposed to mere beliefs about standards" (Lakatos 1978, 135 fn4). What Lakatos calls historiographical positivism is radical naturalism in philosophy of history. He rejected this view, arguing that historians cannot identify what is science and what is not, let alone make historical sense of it, without a set of normative standards of scientific rationality at hand (Lakatos 1978, 114). And this is why, according to Lakatos, the historical understanding of past science is not possible without an implicit or explicit philosophical theory of rationality at hand. Therefore, internal history for Lakatos is the history of belief change that takes place according to, and also is understood through, the rational standards (or the epistemic norms) that constitute the essence of scientific practice.

Kuhn (1971, 138) acknowledges that doing history presupposes some preconceptions about what is essentially scientific and what is not. However, he aims to restrict those preconceptions to the minimum possible level. He says,

> "[…] [T]he historian is usually well-advised to set expectations aside before beginning research. If science and method, for example, are the subjects, then both should be learned from the people under study not from later scientific and philosophical texts. That advice is, of course, a council of perfection: no one can entirely set aside thought patterns induced by prior experience and training; such patterns do influence research, which in any case could scarcely begin without them. But it is nonetheless essential that the attempt to unlearn them be made." (Kuhn 1980, 183)

Thus, Kuhn seems to understand that some kind of implicit or explicit theory of scientific rationality should be at hand before attempting to interpret or reconstruct the historical course of science. However, this is, let me say, a historiographic necessary evil. In the ideal state of historiography, which is, of course, untenable, we could get rid of those philosophical preconceptions. We could "learn from the people under study". In other words, the ideal state of historiography looks like Lakatosian historiographical positivism.

Furthermore, despite that Kuhn is sympathetic to Lakatos' suggestion that historical narratives which present the history of science as less irrational are generally preferable, he denies that this conclusion should be drawn by adopting the Lakatosian internal/external distinction.

> "That point can be made, however, without recourse either to a concept of 'actual history' or, more significantly, to an internal/ external distinction governed by prior standards of rationality. For the historian, actual history is simply history that has actually been written or a selected subset thereof. One way of improving on it is to improve its fit to the range of facts already made accessible by interpretation. Others involve reinterpreting the existing data base or else extending it. All of these processes result in narratives that aim to say what occurred and to make it plausible, aims that require no prior decisions about what part of what occurred was rational, what not. Once the historian has provided such a narrative, the philosopher (sometimes the same person, but wearing a different hat) may examine it, asking about its significance for current doctrines concerning scientific method. If history is to have a chance of influencing those doctrines, however, such questions should be withheld, insofar as possible, until the task of the historian is complete." (Kuhn 1980, 185)

Philosophy of science cannot play the crucial role assumed by Lakatos in historiographical research[13]. On the contrary, history of science can play

[13] Kuhn takes philosophy to be relevant to the historiography of science only because philosophy and science used to be inseparable until a few centuries ago. "Historians of science need philosophy for reasons that are, at once, apparent and well known. For them it is a basic tool, like knowledge of science. Until the end of the seventeenth

a crucial role in the refinement of the philosophical accounts of science,[14] but only if the historical work is kept separate from philosophy until its final conclusions are reached.

With regard to the rejection of the Lakatosian internal/external distinction, Kuhn is totally in line with his younger self. In the *Structure* he wrote:

> "History, we too often say, is a purely descriptive discipline. The theses suggested above are, however, often interpretive and sometimes normative. Again, many of my generalizations are about the sociology or social psychology of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology. In the preceding paragraph I may even seem to have violated the very influential contemporary distinction between 'the context of discovery' and 'the context of justification.' Can anything more than profound confusion be indicated by this admixture of diverse fields and concerns? Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge, and I still suppose that, appropriately recast, they have something important to tell us. *Yet my attempts to apply them, even grosso modo, to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic*." (Kuhn 1996, 8-9, emphasis added)

In short, every attempt to use the distinction between internal (*i.e.* 'context of justification' or normative explanations) and external ('context of discovery' or empirical-scientific explanations) in the actual historical work has proven problematic. The historical study provides narratives of the past that transgress and violate this distinction systematically.

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century, much of science was philosophy. After the disciplines separated, they continued to interact in often consequential ways" (Kuhn 1977, 10).

[14] "Though I do not think current philosophy of science has much relevance for the historian of science, I deeply believe that much writing on philosophy of science would be improved if history played a larger background role in its preparation" (Kuhn 1977, 12).

Therefore, the internal/external distinction needs to be rejected in the philosophy of history of science.

3.2. The 'Dilemma of case studies'

On another level, Kuhn rejects Lakatos's aim to make history of science a source of tests for philosophical accounts of scientific rationality by articulating a version of what later was called the "dilemma of case studies" (Pitt 2001). Pitt uses the dilemma to support a radical skeptical view or, in the terms I used above, a radical version of historical relativism. But there are various versions of the argument from the dilemma of case studies with different aspirations (*e.g.* Schickore 2011; Nickles 1986, 1995). What all these versions share is the intention to block the possibility of employing historical case studies as empirical evidence for testing philosophical theories of scientific rationality.

In Kuhn's version of the argument, it is impossible to use historical cases studies as a source of empirical tests for philosophical theories, in Lakatos' way, because a philosophy-laden historical narrative will be taken either as a manipulation of the historical record or as irrelevant to the proponents of rival theories of scientific rationality. With regard to the first horn of the dilemma, he stresses:

> "[Lakatos'] point is not simply that the historian selects and interprets, but that prior philosophy supplies the whole set of criteria by which he does so. If that were the case, however, there would be no way at all in which the selected and interpreted data could react back on a methodological position to change it." (Kuhn 1971, 141)

and

> "Data can, and must be permitted to, react back on expectations, make trouble for them, play a role in their transformation." (Kuhn 1980, 182)

In short, doing history with a very specific theory of rationality in mind results in the manipulation of the historical record. As it is obvious, a rigged

or manipulated historical record cannot serve as an arbiter for the theories of rationality. With regard to the second horn of the dilemma, he states:

> "When Lakatos provides an historical case to illustrate the comparative merits of the methodology of research programmes, he is not selecting the elements of his internalist narrative from 'actual history' but creating them from often distant data or else choosing from the similar creations of earlier historians. Under those circumstances, it is not surprising that the story he tells makes essential use of elements that other methodologies would relegate to external history. It is by no means clear, however, that proponents of those methodologies would accept the elements of his narrative as simply factual, and it is upon that agreement that his demonstration depends. History is interpretative throughout." (Kuhn 1980, 184)

The point is that what counts as rational for the proponents of one methodology (*i.e.* a theory of scientific rationality) does not count as rational for the proponents of rival methodologies. Therefore, suppose that there is a methodology A and a respective reconstructed history which presents the actual history of science as more rational[15] than rival methodologies B and C. This cannot lead to the conclusion that A is better than B and C because the proponents of B and C would not be compelled to accept that the excess part of the internal history provided by A is indeed rational. They can still argue that those episodes are actually irrational and need to be understood by external history. The case studies which serve as corroboration for A are irrelevant for the proponents of B and C.

4. The Main Tenets of Kuhn's Philosophy of History of Science Reconstructed

Let us now examine what we can learn from Kuhn's late defense of scientific rationality, and from his critique of Lakatos, about his view on the

[15] This means that internal history is larger than the internal history which occurs when rival methodologies are at hand.

philosophy of the history of science as presented in the last two sections. I suggest that this view is fundamentally formed by three interconnected tenets. I will call them (a) contextualism, (b) radical anti-presentism, and (c) naturalism.

4.1. Contextualism

The first tenet that springs naturally from what we have already said is that the historical comprehension of past science presupposes taking into consideration the historical context in which the scientific changes took place. It has to be stressed that belief change is the central issue that requires explanation according to Kuhn.

> "For the philosopher who adopts the historical perspective, the problem is the same: understanding small incremental changes of belief. When questions about rationality, objectivity, or evidence arise in that context, they are addressed not to the beliefs that were current either before or after the change, but simply to the change itself. Why, that is, given the body of belief with which they began, do the members of a scientific group elect to alter it, a process that is seldom a mere addition but ordinarily calls for the adjustment or abandonment of a few beliefs already in place? From the philosophical point of view, the difference between those two formulations – the rationality of belief versus the rationality of incremental change of belief – is vast." (Kuhn 2000c, 112)[16]

The main task is to explain the belief change and this task is untenable without taking into consideration the historical context.

Alexander Bird (Bird 2012b) calls this tenet 'the conservative strand[17] of [Kuhn's] historicism' and he defines it as follows: "In the broadest terms,

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[16] See also (Kuhn 2022, 7).

[17] The other strand, according to Bird, is determinism. Given the so-called cyclical model of scientific change (normal science → crisis → scientific revolution → new normal science), Bird thinks that Kuhn's philosophy of history is deterministic. I think that this conclusion is at least at odds with Kuhn's explicit rejection of the idea that

this is the claim that there is an intimate relationship between the evaluation of an idea (or indeed any other human product) and its historical context" (Bird 2012b, 167-168)[18]. Given that science is a puzzle-solving activity which always takes place within a framework of shared beliefs – even among the proponents of incommensurable paradigms – that serves as a historically changeable Archimedean platform, any change in belief cannot be made intelligible without taking into account the framework in question. This requires a tenacious effort by historians to understand the historical connotations. The effort presupposes, among other things, to 'unlearn' or get rid of any preconceptions that are related to the present science which are an inevitable part of the intellectual constitution of the historians. And this takes us to the second tenet.

4.2. Radical anti-presentism

Kuhn was a strong advocate of what we may call anti-presentism in the history of science. This is the methodological imperative to look at the past without the glasses of the present.

> "Insofar as possible (it is never entirely so, nor could history be written if it were), the historian should set aside the science that he knows. His science should be learned from the textbooks and journals of the period he studies, and he should master these and the indigenous traditions they display before grappling with innovators whose discoveries or inventions changed the direction of scientific advance. Dealing with innovators, the historian should try to think as they did." (Kuhn 1977a, 110)

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history of science can make predictions: "From it I conclude, among other things, that an ability to predict the future is no part of the historian's arsenal" (Kuhn 1977, 16). However, the topic of determinism is not immediately related with the issue of scientific rationality and for that reason I am not going to discuss it further.

[18] It has to be clear that contextualism here refers to the philosophy of history of science. It shouldn't be conflated with alethic relativism. I don't want to ascribe alethic relativism to Kuhn. He explicitly rejected this view. See (Kuhn 2000b, 91; 2022, 53).

Kuhn belongs to the generation that saw the history of science becoming a mature discipline in strong opposition to any kind of anachronism. As Hasok Chang (2021, 98) notes, it is Herbert Butterfield (1931) who gave us the derogatory term 'Whig history' as synonymous with anachronism and, at the same time, bad historical endeavor. "[I]n the 1960s and '70s, the period of consolidation of the history of science as an academic discipline, the attacks on 'Whiggishness' (which sometimes appears as 'Whiggism' in this era of isms), 'triumphalism' and 'hagiography' were of a piece with a general repudiation, in favour of more professional and disinterested approaches, of the didactic and often moralistic writings that had dominated the field right up to the 1960s" (Jardine 2003, 127). Therefore, as Michael Gordin (2014, 421) stresses, anti-whiggism is wired "into the central core of [history of science] as a *discipline*".

The Kuhnian philosophy of history of science is first and foremost anti-whiggish,[19] that is, anti-presentist[20]. For Kuhn, the historian of science is like the ethnographer who studies cultural phenomena from the point of view of the subject under study. Both need to learn a foreign language in order to make their subject matter intelligible and both need to 'forget', where possible, the connotations of their own language.

> "Finding and disseminating a vocabulary that permits description and understanding of older times or of other cultures is central to what historians and anthropologists do. Anthropologists who refuse the challenge are called 'ethnocentric'; historians who refuse it are called 'Whig'." (Kuhn 2000a, 213)[21]

Of course, Kuhn understands that it is practically impossible for anyone to get outside of their skin and totally forget what they know about present science. But, theoretically speaking, presentism is always a source of historiographical mistakes that cause a distorted picture of the past. As Adam Tuboly puts it, according to Kuhn, "[t]he historian of science starts

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[19] Whig history is useful only for the education of scientists. It helps them form the identity of their community and practice normal science. See (Kuhn 2022, 87-88).

[20] For Kuhn, whiggism is synonymous with presentism. As Chang (2021) shows, there are also other forms of presentism.

[21] See also (Kuhn 2022, 29 & 47).

from the fact that 'intelligent people' of the past have accepted strange, outdated, and obviously false theories as fundamental truths about the world, which raises the question of why and how. We should not assume, though we often do from our ethnocentric viewpoint, that past scientists were plainly wrong and their strange and unacceptable beliefs come from ignorance" (Tubloy 2023, X). Thus, anti-presentism is a sort of regulative ideal of historical research and its prior task is to discover and reconstitute "the integrity of an out-of-date scientific tradition" (Kuhn 2022, 8).

4.3. Naturalism

The third tenet is more complicated and needs several distinctions. Philosophical naturalism is a fundamental feature of Kuhnian thought from the *Structure* until the end of his life (Mayoral 2023; Mladenović 2022, xvi-xix). But naturalism in different subareas of philosophy means different things. It is quite clear, for instance, that Kuhn was a naturalist with regard to the philosophy of mind and the philosophy of language. His sustained engagement with experiments in cognitive and developmental psychology, in order to explain concept acquisition and the pursuit for an empirical theory of meaning, respectively, reflects these forms of naturalism. Furthermore, his rejection of foundationalism and of the relevant overarching role of first philosophy is also an expression of philosophical naturalism. While the complete mapping of Kuhn's naturalistic insights is beyond the scope of the present paper, I nonetheless want to focus on naturalism with regard to the philosophy of history[22]. Despite the fact that Kuhn never explicitly discussed the issue on these terms, I argue that we can reconstruct an obvious naturalistic stance in his philosophical conception of history.

First of all, according to this conception, history is an explanatory enterprise which consists in narratives.

> "The final product of most historical research is a narrative, a story, about particulars of the past. In part it is a description of what occurred

[22] Or, put more broadly, the philosophy of social sciences.

> (philosophers and scientists often say, a mere description). Its success, however, depends not only on accuracy but also on structure. The historical narrative must render plausible and comprehensible the events it describes. In a sense to which I shall later return, history is an explanatory enterprise; yet its explanatory functions are achieved with almost no recourse to explicit generalizations." (Kuhn 1977b, 5)

This passage alone does not necessarily reveal a naturalistic tendency. The naturalistic implications come up in Kuhn's comparison between the history of science and other subareas of history:

> "The history of science is not in principle a narrower specialty than, say, political, diplomatic, social, or intellectual history. Nor are its methods radically distinct from the ones employed in those fields. But it is a specialty of a different sort, for it is concerned in the first instance with the activity of a special group – the scientists – rather than with a set of phenomena which must at the start be abstracted from the totality of activities within a geographically defined community. In this respect its natural kin are the history of literature, of philosophy, of music, and of the plastic arts.
> […]
> I have been considering the suggestion that the relations between history and the history of science differ only in intensity, not in kind, from the relations between history and the study of the development of other disciplines." (Kuhn 1977a, 151 & 154, respectively).

The equation of the history of science with the history of plastic arts, to cite one example, is characteristically naturalistic for it neglects the normative character of scientific knowledge. I don't claim that art is necessarily a non-normative enterprise. But even if it is, its normativity is completely different from the normativity that dictates scientific knowledge. Only by neglecting the special normative status of scientific knowledge, which is a characteristically naturalistic attitude, can the history of science be presented as akin to the philosophy of literature. Furthermore, only a naturalistic attitude which neglects the normative character of scientific knowledge would conclude that the history of science shares the same methods as political, diplomatic,

or social history. The only difference that Kuhn detects between them is that the former but not the latter are concerned with a special group.

At this point, one may object that Kuhn rejected some central positions of the naturalistic philosophy of history. First and foremost, he rejected (Kuhn 1977c, 15-18) the so-called covering law model in history (Hempel 1965). He also refers to history as a hermeneutic enterprise. But by rejecting the covering law model Kuhn rejects only a version of naturalism, not naturalism *per se*. He rejects the positivist version of naturalism which takes scientific explanation as essentially nomological. Moreover, the term 'hermeneutic', as it is used by Kuhn, does not refer to the anti-naturalist tradition of *Hermeneutics* which has its origin in the 19th century German-speaking world and contends that there is a methodological gap between the human and the natural sciences, because the latter provide explanations while the former interpretations[23]. The adjective 'hermeneutic' in the Kuhnian context is synonymous with 'ethnographic' and this is by no means anti-naturalistic.

I suggest that it would be fruitful to think about Kuhn's historiographic naturalism in terms of the distinction between normative and empirical-scientific explanations, despite that Kuhn himself never used these terms and rarely discussed the issue of naturalism in general. Historiographical naturalists (or positivists in Lakatos' terminology) reject the domain of normative explanations altogether. They deny that rationality is a genuinely explanatory notion and consequently claim that all normative explanations can and should be reduced to empirical-scientific ones. They are eliminativists with regard to normative vocabulary[24]. Showing how a belief modification conforms to a set of norms is not enough. Providing a thorough understanding of the

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[23] There is one important affinity between Kuhn and most proponents of the Hermeneutics tradition. Both suggest that historical knowledge is knowledge of particulars of the past (Collingwood, Taylor, and Schiller 1922, 433). The similarities between Kuhn's view on history and Hermeneutics is an interesting topic. Sometimes Kuhn flirts with the Hermeneutical methodology (see Kuhn 2002, 133-134). However, in an explicit comparing of his view with the Hermeneutic conception of history, Kuhn does not seem to agree with its main tenet, *i.e.* that the difference between explanations (*Erklären*) provided by the human sciences and interpretations (Verstehen) relies on the metaphysical specialty of human behavior, which is characterized by intentionality. See (Kuhn 2000b).

[24] It is true that there are several varieties of naturalism. They are not all eliminativist. See (Dimitrakos 2020a). For sake of brevity, I am going to use the term historiographic naturalism as equated with its eliminativist version.

modification requires an empirical account which explains why the bearers of the beliefs (*i.e.* the scientific community in our case) thought that the modifications conform to this set of norms. Historiographical anti-naturalists reject this eliminativist aspiration on the grounds that rationality is an explanatory term. For instance, Lakatos thinks that normative explanations are indispensable for making rational episodes intelligible.

Kuhn is not exactly an eliminativist. But neither does he take rationality as a genuinely explanatory notion. Only instrumental rationality is explanatory, according to the Kuhnian historiography. The main task of the Kuhnian historian is to show how past beliefs, which seem absurd in the light of contemporary science, are reasonable in the light of the historical context within which they were actually held. "*If* we understand Aristotle's physics as an integrated whole, with concepts different from ours, we will understand why Aristotle *had* to think that void is impossible" (Kuhn 2022, 91, emphasis added)[25]. As we can see, there is a kind of necessity here. If we accept the rest of Aristotelian physics, we *have* to reject the existence of the void. But this kind of necessity is conditional. It is associated with instrumental rationality. It is very common for naturalists, especially of the Humean variety, to limit rationality to its instrumental form. Kuhn seems to endorse this view with regard to the philosophy of history. We can only explain why someone is compelled to adopt a belief given that they have already adopted a set of beliefs. As he used to repeatedly stress, "evidence functions only in the evaluation of change of belief, not of belief itself" (Kuhn 2022, 131). Therefore, the rational character of a belief can be revealed only with respect to an already given framework of beliefs, and hence the only kind of rationality that has an explanatory role in history is instrumental rationality.

5. On the Defense of Scientific Rationality

Let me now return to scientific rationality and its defense. I take for granted that a proper defense of scientific rationality should be coupled

[25] In fact, this is Bojana Mladenović's recapitulation of the first section of chapter two of the first part of Kuhn's last unfinished text.

with a philosophy of history of science which is sensitive to the logical distinction between what is actually justified and what is seemingly justified (or what is taken-to-be-justified). For instance, the philosophy of history of the Strong Programmers is blind to this distinction. As Psillos and Shaw (2020, 407) argue in commenting on David Bloor's recapitulation of the Strong Programme, "justification is replaced by being confidently held to and lived by. The relativist crux then is that there is no distinction between being-taken-to-be-justified (by a community) and being-justified. Whatever justification-conferring properties are taken by a community to confer justification on a belief are the 'right' properties." Kuhn, of course, rejected the Strong Programme's view. This is well-known. But my question is whether his philosophy of history of science is sensitive to the logical distinction between being-justified and being-taken-to-be-justified, and hence whether it can be coupled with a proper defense of scientific rationality.

I claim that contextualism alone does not cause problems for the defense of scientific rationality. However, contextualism combined with radical anti-presentism and naturalism leads inevitably to the indistinguishability between being-justified and being-taken-to-be-justified. The Kuhnian historian can only tell us whether Aristotle was justified to reject the concept of void given Aristotle's entire body of beliefs but he cannot tell us whether Aristotle was justified *per se*. As Kuhn suggests,

> "From the historical perspective, however, where change of belief is what's at issue, the rationality of the conclusions requires only that the observations invoked be neutral for, or shared by, *the members of the group making the decision*, and for them only at the time the decision is being made." (Kuhn 2000d, 113, emphasis added)

The indistinguishability for the Kuhnian historian does not spring from the same reasons as the indistinguishability for the Strong Programmer, but it is still indistinguishability and as such it turns Kuhn's aspiration to defend scientific rationality inconsistent.

I suggest that Kuhn, by the end of his career, somehow felt the incompatibility between his philosophy of history and the proper defense of scientific rationality. For that reason, I think, he was forced to concede

that "anachronistic or Whig history of science should not be abandoned. Its goal is to explain the success of present-day scientific theories, and so it produces anachronistic narratives in which past science appears as constituted by a series of rationally warranted conclusions and choices, leading to our present scientific theories" (Kuhn 2022, 102)[26]. However, I can't see how the proper defense of scientific rationality could rely on "a lie" – even if "a noble one" – as Kuhn (2022, 88) characterizes Whig history.

6. Conclusions

My pivotal aim in the present paper is to reconstruct Kuhn's philosophy of history in order to examine whether it can be coupled with a proper defense of scientific rationality. I claimed that three fundamental tenets form essentially the Kuhnian philosophy of history: contextualism, radical anti-presentism, and naturalism. I also argued that those three tenets combined lead to the indistinguishability between being-justified and being-taken-to-be-justified and therefore makes Kuhn's aspiration to defend scientific rationality inconsistent. As I said, I think that Kuhn had a sense of this inconsistency. What he lacked was a proper diagnosis of its source. If I am right, the problem springs from radical anti-presentism and naturalism. For what it's worth, in my view, the rejection of these tenets in the philosophy of history does not necessarily distort Kuhn's perspective on science. But a positive account of the philosophy of history of science is a topic for another text.

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[26] Again, this is Mladenović's formulation based on Kuhn's unfinished manuscripts.

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Leandro GIRI[1],
Matias GIRI[2]

A *STRUCTURE* FOR HISTORY: REFLECTIONS FROM KUHN'S HISTORIOGRAPHIC STUDIES[3]

Abstract. In the present work, we aim to analyze Lorraine Daston's critiques of the historiographical value of Thomas Kuhn's *The Structure of Scientific Revolutions*: we will defend its relevance from the attacks of "the history of contingencies". Daston's proposal asserts that the Kuhnian historiographical programme of professionalizing the history of science (moving it towards history departments to the detriment of science departments) has been fulfilled but has resulted in the abandonment of the Hegelian spirit from Kuhn's historiography, *i.e.* the search for "a structure" of the history of science has been abandoned. We will analyze and incorporate the recent responses from K. Brad Wray and Pablo Melogno. Finally, through a thorough analysis of the relationships between philosophy and the history of science, particularly in Kuhn's work, we will propose a defense of the systematic and explicit use of metatheoretical structures for historiographical endeavors.

Keywords: historiography of science, historical philosophy of science, Thomas Kuhn, historicism

1. Introduction

The perennial importance of Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) remains perplexing (Green 2016; Hacking 2012; Giri &

1 Assistant Researcher, Universidad Nacional de Tres de Febrero, Consejo Nacional de Investigaciones Científica y Técnicas, Argentina, https://orcid.org/0000-0002-7068-9750. Email: <lgiri@untref.edu.ar>.

2 Professor, Centro de Estudios en Filosofía e Historia de la Ciencia – Universidad Nacional de Quilmes, Consejo Nacional de Investigaciones Científicas y Técnicas, Argentina. Email: <matiasgiri@outlook.com>.

3 This paper is dedicated to our great friend Dr. Pablo Melogno (1979-2023), a sharp Uruguayan philosopher and a profound connoisseur of Thomas Kuhn's thought.

Giri 2020). The topics Kuhn worked on in his classic are still contested, and this author remains a central interlocutor, despite the obvious advances that more than 60 years of development in the philosophy and history of science produced after *Structure* (Richards & Daston 2016; Giri, Melogno, & Miguel 2023). One of the recent debates (though clearly not entirely novel, since it was constitutive of the rise of the so-called "new philosophy of science" or, better still to abandon the anachronistic label, "historicist philosophy of science") was triggered by Lorraine Daston (2016) in a volume in homage to the 50th anniversary of *Structure*. In the historian's opinion, the term "structure" in reference to history (in general, and of science in particular) has become outdated as a guide for historiographical work.

Thus, proposals to conduct history of science from rational reconstructions (*i.e.* to search for "structures" that highlight certain dynamic patterns of theoretical change in order to develop relevant historical narratives) have, in Daston's opinion, become obsolete. The history of science, as it moved from the majority aegis of scientists to history departments, took on a contextualist turn that distanced it from philosophy and sociology. The history of science becomes a history of contingencies, and each historical episode becomes a unique episode of its kind (see also Kindi 2023).

In this paper we will start from Daston's analysis and the recent responses that her work received from two important exegetes of Thomas Kuhn's work. On the one hand, the response of K. Brad Wray (2023), and on the other, the comments to Wray (and thus also to Daston) by Pablo Melogno (2023). We will then propose our interpretation of the controversy and our position in it, for which we will defend the relevance of a history of science based on systematic metatheoretical frameworks, and the dangers of not having an explicit philosophical framework to support the historiographic narratives. This way of working has a fundamental antecedent in Kuhn, although, to his regret, since he himself defended (as, we will see, not without serious contradictions) that philosophy and the history of science are separate enterprises. Finally, we will present our conclusions.

2. The controversy

2.1. Unstructured history: Lorraine Daston

The central idea of *History of Science without Structure* (Daston 2016) is perfectly consistent with its title, and is also a categorical diagnosis: historians of science have abandoned the search for structures in the history of science, not because the one proposed by Kuhn (1962) has necessarily been refuted or because a better one has replaced it: simply, historians do not consider useful for historical enquiry to take into account more or less constant patterns or models in the historical facts to be analyzed. Why?

> "Most historians of science no longer believe that any kind of structure could possibly do justice to their subject matter. The very idea of looking for overarching regularities in the history of science seems bizarre, a kind of leftover Hegelianism seeking a hidden, inexorable logic in the apparent vagaries of history – in Kuhn's case, the last attempt to give Reason (now incarnate in science) a rational history." (Daston 2016, 117)

Thus, according to Daston, there is a tendency, especially since the 1990s, towards a kind of academic baroque that favours works rich in complex information over those that seek simplifying patterns, which, in the history of science, translates into a contextualist history. There, each particular fact is unique, incapable of being fitted into any natural class.

Daston's diagnosis does not seem to be supported by any extensive bibliographical analysis of the works on the history of science of the last 30 years, but a brief analysis of the curricula of the history of science seminars offered in the history courses of some important Latin American universities seems to support the author, and it is very plausible that the same tendency is also registered in the American and European academy: the works of philosophers belonging to the "new philosophy of science" (nowadays better called "historicist philosophy of science", *i.e.* those who defended the relevance of the use of philosophically grounded structures for a fertile history of science) are mostly conspicuous by their absence. The only work by Kuhn that seems to be read in most majors in introductory philosophy of science courses

is, of course, *Structure* (see Giri & Giri 2020), but the interest seems to be more in his model of the dynamics of science than in a heuristic for a history of science through rational reconstructions.

Thus, in the absence of refuting instances, we can take Daston's diagnosis as true: historians of science (to be cautious, let us say "the majority") who come from history departments do not use "structures" in their research, but have a contingentist conception of history. Daston then proposes a dichotomy: either one researches with "structures" or one is "historicist", on the understanding that one form of research implies the negation of the other:

> "The historicist program in the history of science has fractured the once- monolithic 'science' into the sciences and raised serious questions as to whether the term can be applied at all to the premodern epoch. Yet the historicism Kuhn prophesied and welcomed has ultimately dismantled the structures he sought: an essential tension at the heart of his own still riveting vision for the history of science." (Daston 2016, 118)

In part, the break with structures has to do with the fact that this historicism (so called presumably because it is the method of historians coming from history departments) uses the same working strategies for the history of science as for the history of any other topic: basically an exhaustive treatment of contextualized sources, whereas structural methods, in seeking to isolate a structure for science, do not require much of the external (or non-immanent) other than as decorations in the narrative (in the manner of Lakatos 1970). The contingentist style dissolves the internalism-externalism discussion by intermingling them and denying that science has an "inside and outside". After all, the focus would be on practices, whose description will necessarily include components of both types of historiographies.

A corollary of the abandonment of structures is the severing of the links between the history of science and the philosophy and sociology (of science). However, it may be possible, according to Daston, to recover these links by changing what we understand by "structure". Structure in *Structure*, as Daston understands it, involves

> "how disciplines became 'mature sciences' with the advent of their first paradigm; how paradigms 'provide scientists not only with a map but also with some of the directions for map-making'; how cumulative progress is only possible within a paradigm; why it was impossible for paradigms to peacefully coexist, for 'proponents of competing paradigms practice their trades in different worlds.' Only 'orthodox theology' could compete with the 'narrowness and rigidity' of scientific education into the reigning paradigm. The history of science was nothing more or less than the history of paradigms as they rose and fell like empires. According to Kuhn, paradigms could even mold perceptions, encoding knowledge into the very neural processes of scientists." (Daston 2016, 24)

However, if we take by "structure" not these elements of paradigms (understood as a "disciplinary matrix") but instead paradigms understood as "exemplars", then interdisciplinarity can still be achieved. An exemplar is for Daston (and for the Kuhn of the Postscript, see 1996) a much richer unit of analysis, as it points out, not by algorithmic rules, but by implicit characteristics, the correct ways of operating in scientific practice (in a given socio-historical and cultural context). The adoption of exemplars would allow "historicist" historians of science to work with a sufficiently concrete unit of analysis without committing themselves to the idea of a "special" science, esoteric and more rational than any other practice or mode of knowledge, and it is also a "context-sensitive" unit (Daston 2016, 128), and of interest also to sociologists, philosophers and even anthropologists of science. Thus, paradigmatic exemplars incarnate Daston's compromise solution for a history of science that is simultaneously contingentist but also interdisciplinary.

2.2. Structure *as a strictly philosophical book: Brad Wray*

Brad Wray's paper, "A defense of structure in *Structure of Scientific Revolutions*" (2023) constitutes Wray's vigorous response to Daston and can be read as a plea for the perennial relevance of *Structure* in the philosophy of science. The central core of Wray's proposition is the following: Daston's

critique of *Structure* is ungrounded because her argument rests on the claim that Kuhn attempted to provide therein a methodology for the practice of the history of science (namely, by making rational reconstructions of science using paradigmatic structures as the unit of analysis). Daston's argument is unsound, basically because Kuhn's intention in *Structure* would be for Wray entirely philosophical and not historiographical (in other words, the target reader is a philosopher and not a historian, and so, *Structure*'s ideas are not meant to be applied in doing history of science). As additional premises, Wray adds that Kuhn's notion of "structure" is respectable and that the author's analysis of science is essentially correct.

Wray admits that it is not easy to classify *Structure* (or Kuhn himself, who has done both purely philosophical and purely historiographical work) disciplinarily. However, he emphatically defends the classification he chooses for *Structure* on the following grounds:

1. Kuhn (1962) claims that if we study the history of science (in an anti-whig way) it will transform our view of science, but "transforming our view of science" is a philosophical and not a historiographical goal.
2. Kuhn (1977a) recognizes that there are essential differences between history and philosophy of science (developing narratives about the particular and pursuing general theories, respectively).
3. Contemporary historians of science generally do not attach much importance to the impact of structure on their practice (in this respect there is clearly agreement with Daston).
4. Some epistemic authorities such as Peter Galison, Joel Isaac, Peter Dear and David Kaiser and even his disciple John Heilbron deny the value of *Structure* for the history of science.
5. Perhaps the strongest argument: Kuhn himself claimed in his famous interview in *The Road Since Structure* that his aims were philosophical, notably by stating: "...my ambitions were always philosophical. And I thought of *Structure*... as being a book for philosophers" (2000, 206).
6. He also states in the preface to *Structure*: "[had a shift in career plans] from physics to history of science and then, gradually,

> from relatively straightforward historical problems back to the more philosophical concerns that had initially led to history" (1962, xxxix-xl).

Once Wray leaves his thesis on the philosophical essence of *Structure*, he proceeds to elucidate the notion of "structure" presupposed therein:

> "…Kuhn wants to show that scientific revolutions do not happen in some random, chaotic, or unpatterned way. Rather, they take on a particular form. In fact, according to Kuhn, it is not only scientific revolutions that have a structure... The development of a scientific field as whole has a pattern or form (...). Roughly, the structure is as follows. [After emerging out of the preparadigmatic state] A period of normal science, in which scientists take the fundamentals of a field for granted, leads to a period of crises, caused by persistent anomalies that resist solutions. The crisis causes a slackening of the disciplinary norms and standards, which leads to the generation of new theories (...). Finally, a new theory proves to meet the challenges the field faced and it becomes the dominant theory, which leads to a new normal scientific research tradition." (Wray 2023, 31-32)

This notion of structure may certainly not be of interest to historians, but it is still plausible, and for Wray essentially correct: theories are born, grow, go into crisis and then are replaced in a way that, while it may have exceptions, more or less represents actual processes of theoretical change.

Finally, Wray embarks on denying that *Structure* (in particular) or Kuhn (in general) has a "historicist" view of the history of science in Popper's (1991; 2011) sense, *i.e.* that it is believed to possess an identifiable trajectory comprehensible by the social sciences (and hence predictable at some point). The kind of pattern that Kuhn identifies in science and its progress is not teleological, it does not allow a forward-looking historical trajectory to be established more than vaguely. In other words, it is not the same thing to posit that a current paradigm will go into crisis as it is to make a specific prediction of the future. In fact, the most bitter enemy of Popperian historicism (Popper himself), although a fierce critic of Kuhnian metatheory, did not attack Kuhn for

imposing a teleology, but especially for defending the existence and progressivity of the stages of normal science (see especially Popper, 1970). Thus, Wray considers that Kuhn adheres to the so-called "contingency thesis" (Hacking 1999), which states that the conceptual development of science is by no means predetermined.

In short, for Wray, Daston is fundamentally wrong in her critique of Kuhn because *Structure* is a philosophical text, not a historiographical one, and because, although historians prefer to ignore it, this philosophically grounded structure is prolific for the analysis of science.

2.3. Pablo Melogno vindicates the historiographical role of Structure

The paper "A vindication of *Structure in Structure of Scientific Revolutions*: A comment to K. Brad Wray" (Melogno, 2023) is not only a response to Wray's analysis, but also to Daston's. The main reason for the disagreement between Melogno and Wray will be over the scope of *Structure*, which Melogno will understand not only as philosophical but also as historiographical. However, he will differ with Daston about the character of Kuhnian historiography being teleological, and hence, that it is "a kind of leftover Hegelianism" (Daston 2016, 117).

First, Melogno recognizes Wray's analysis of the term "structure" as good, but defends its historiographical value by stating that "if we consider that Kuhn claims that scientific revolutions do not occur randomly but follow a pattern, and if this pattern is repeated throughout history, we are attributing to Kuhn a historiographical thesis about scientific revolutions" (2023, 45). This statement means that the admission of certain patterns in science sustained over time has an impact on the history of science, and since the search for these patterns has been a constant in the philosophy of science, it is clear that philosophical activity has a historiographical corollary.

Philosophical activity being inherently legitimate, Daston should accept that its corollary is legitimate too, regardless of whether historians (with history majors) like to use those patterns or not. The decoupling of historians and philosophers is neither new nor necessarily problematic

according to Melogno, given that both have been extremely prolific in their respective endeavours, and so was Kuhn himself:

> "Kuhn's historiographical theses about the dynamics of scientific revolutions have turned out to be extremely fertile for shedding light on the historical nature of science. *Structure* enabled the development of a philosophy of science with a better historical sensibility and introduced game rules followed even by Kuhn's staunchest detractors. When considered in detail, the specific thesis of the kuhnian historiographical frame can be more or less acceptable. However, after Kuhn, it is no longer possible to address the problems of philosophy of science without considering the historical dimension of science." (Melongo 2023, 49)

Thus, for Melogno, Daston's interpretation of the historiographical (but also philosophical) character of *Structure* is plausible, but not his accusation of historicism à la Hegel, as

> "The historical structures posited by Kuhn are more formal, less ambitious, and clearly weaker than those predominant among historians at the beginning of the twentieth century-and among some historians of science during the nineteenth century [*i.e.* historians who advocated the existence of ineluctable historical laws, like Hegel]. Proposing structures and introducing historical regularities are elements of a project that can adopt different expressions, showing different degrees of formality and rigidity. (...). In other words, Daston's rejection of the historiographical use of the notion of structure requires a specific criticism of the historical patterns assumed by Kuhn, rather than a generic delegitimization of the notion as he used it." (Melongo 2023, 48)

Melogno also agrees with Wray on the philosophical (and, he adds, also historiographical) fertility of the notion of "structure".

3. Giri & Giri in the Daston–Wray–Melogno controversy: What is "historicist philosophy of science"?

The Daston-Wray-Melogno controversy is intrinsically interesting and also far from settled. It is not our intention to close it, but to enter into it with our own standpoint. Our position, in a nutshell, attempts to recover and vindicate the tradition of the so-called "historicist philosophy of science" (*i.e.* "new" philosophy of science), a tradition that for mainstream philosophy is inaugurated by the publication of *Structure* (although virtuous and necessary antecedents can be named, in particular Ludwik Fleck, see especially 1979; see also Hoyningen-Huene 1993; Hacking 2012). From this tradition we defend the following strong thesis: every history of science implies an idea about science. This thesis is a specialized version of a more general thesis that could be made explicit in this way: all metascience implies an idea about science. Other specializations of this thesis could then be on the anthropology of science, the sociology of science, the psychology of science, the rhetoric of science, and even the politics of science (this last point was emphasized by Mario Bunge, 1988). Our first step will then be to justify this thesis and to point out in what sense its support implies a collision with Daston's position. We will then conclude by stating our position on the difference between philosophy and history of science, in order to contrast our position with that of Wray and Melogno.

3.1. Metascience and science

Daston's statement, "The very idea of looking for overarching regularities in the history of science seems bizarre, a kind of leftover Hegelianism seeking a hidden, inexorable logic in the apparent vagaries of history" (2016, 117) is, in our view, the *locus* of the polemic. However, the accusation is not entirely novel. Feyerabend accused Kuhn in *Structure* of proposing a dialectical and rigid theory of science in the Hegelian sense (see Hoyningen-Huene, 1995), while Graham directly asserted that the Kuhnian proposal is a "philosophical history in the Hegelian style" (1997, 127), Reynolds (1999) asserted that Kuhn's thesis of scientific revolutions

could be classified as a form of Popperian historicism (*i.e.*, the type Popper criticized, including the Hegelian variety), and Bird (2015) identified two "historicist strands" in Kuhn's historiography of science (a "conservative" and a "determinist"), both of which he claims are coincident with Hegel's historiography, and claimed that the determinist is the one attacked by Popper.[4]

Now, to establish that science has a dynamic structure with certain regularities does not necessarily imply a Hegelian remora. Everything that is not permanent, after all, is born, develops and disappears. Philosophers of science have analyzed and polemicized about the way in which this happens, and from reading their works it is more or less clear, implicitly or explicitly, that time is a variable to be taken into account. It is trivial but it seems necessary to state it: the birth, development and disappearance of scientific knowledge does not occur in a chronologically infinitesimal temporal instant, but during heterogeneous but never negligible periods of time. What is not trivial is undoubtedly the mechanism (or mechanisms) by which such changes occur, and it can be affirmed either that they always occur in the same way or that they never occur in the same way, or, maybe, some intermediate position (where, according to certain factors, change occurs in one way or another, but a taxonomy of types of change can be established). Which position is held depends on a particular idea of science, *i.e.* on a philosophy of science.

If a philosophically informed historian wishes to make a history of science, she may legitimately use whatever idea of science she sees fit as a hypothesis or model of how the events she cares to historicize happened when seeking sources and developing a narrative, and it can hardly be argued that this is a matter of debate. However, if the historian believes

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[4] Wray (2023) subscribes to Bird's idea of the two historicist strands in Kuhnian historiography, although Bird clearly states that, according to his thesis, Hegelian historiography also carries both strands, and that the determinist strand is the one attacked by Popper. Wray notes, "Significantly, Bird's characterization of the two dimensions of Kuhn's historicism are not the same as the historicism which Popper objected" (2023, 37), a thesis that, unfortunately, he does not justify beyond the fact that Popper himself did not accuse Kuhn of Hegelianism. In our opinion, as will be seen, the determinism implied by the Kuhnian model of theoretical change is too weak to warrant such an accusation.

that her theory of science implies a teleology such that her knowledge allows her to predict a concrete and objective goal or rational end of the history of science, she could be rightfully accused of Hegelianism. On the other hand, if metatheory points out that at some moment a paradigm will enter into crisis due to an accumulation of recalcitrant anomalies and will be replaced in a process called "scientific revolution", or a research programme will become degenerative and will be gradually abandoned in order to focus resources on more progressive programmes, the accusation would be exaggerated and unfair. The charge does not fit either Kuhn or Lakatos, since none of them claim that their metatheory provides a general law of the course of history, they do not even have any pretensions to predictability other than at a very abstract level.

A relevant analogy would be the following: we all know that people eventually die; a historian working on the biography of a figure from, say, the Roman Empire, knows, whatever the sources say, that her character died at some point, and that that point must be somewhere between his birth and hardly more than 100 years later (much more likely, less than 80). It is also possible to infer that he died either by disease, accident, or murder. These trivial patterns are not enough to accuse a historian of historicism à la Popper, and, we argue, neither are the patterns of historicist philosophers like Kuhn, but also Imre Lakatos, or Larry Laudan: their patterns are sufficiently abstract to prevent any form of general sense of history from being derived from them. Of course, one may suppose that the "structure" ascribed to science or its parts by some of these philosophers is more fertile or less fertile for historiographical work because of its suitability to the sources or the kind of narrative desired,[5] but that would not constitute an attack on the idea that science possesses something like a structure, but on particular structures.

[5] It is true that the data yielded by historical sources can be accommodated to some extent to match the constraints provided by metatheoretical structures. On the other hand, it is also true that structures can (and should) be made more flexible to better accommodate such data. However, if a philosophical structure must be twisted too much to accommodate the data, or if the data must be heavily altered to fit the structure, we have reason to doubt the fertility of the structure for historiographical work, and hence also for philosophical research (see Nickles 1986; Moulines 1986).

Let us look at another situation. Suppose a radical historicist historian à la Daston (*i.e.*, a historian of contingencies), who explicitly assumes that science has no structures or patterns, and who does not believe that the work of philosophers of science is anything more than an intellectual challenge on the same level of importance as solving crossword puzzles. This historian works in the manner indicated by Daston: she pays attention to the infinite contextual details of the facts of the selected time snippet and deliberately ignores the patterns that science may have. This type of historian, as Wray and Melogno acknowledge, not only exists but is even in the majority, especially within Kuhn's number one profile of the historian of science (see 1977a, b, c; e.g. historians coming from history majors, see also Giri & Giri 2020), as we will discuss later. Daston is absolutely right in her sociological analysis of the community of historians coming from history majors. However, denying that there are patterns in science that are useful for historiographical work implies affirming that science is a practice where contingencies are the most relevant thing, and that is a strong thesis about science.

In other words, according to this thesis, each historical event in the history of science is singular in such a way that it is not possible (or interesting) to group it with other events in order to obtain diachronic patterns. However, it turns out that scientists do things like propose hypotheses and test them experimentally, publish results and engage in controversy. It can be argued that the way these things happen is never the same, but to hold that they happen already establishes the existence of a kind of structure,[6] which may be admittedly ephemeral and contingent, but by no means non-existent. To hold such a thesis does not, of course, amount to having no thesis about science, but rather the opposite: it amounts to holding a thesis about science (*i.e.* a philosophy of science) whose structure is ephemeral, variable and asystematic to such a degree that comparison is impossible or uninteresting because of

[6] We are understanding here the notion of "structure of science" as it is understood by Wray and Melogno, *i.e.* as the form or sequence of the process in which the scientific dynamic occurs: in its Popperian form it would occur, for example, as a process of hypothesis formulation and bold attempts to falsify them (which at first the hypothesis stubbornly resist by showing their mettle, and finally fall down during the conduct of the crucial experiment).

its triviality. It is clear that such a historian is not a Hegelian, but neither is she exempt from a thesis about science, namely that science is a practice whose events are so unique that they cannot be fruitfully compared.

There is also another type of historian, much less sophisticated, who simply has no philosophical idea about science, and merely narrates it by more or less arbitrarily selecting facts to accommodate a certain narrative idea. Philosophical analysis of her work, however, would allow us to reconstruct an implicit idea of science, which might be asystematic and contradictory but by no means non-existent: this kind of hypothetical historian would presumably be dangerous in conveying asystematic and contradictory ideas about science, and we believe that education in philosophy of science should combat such approaches.

Returning to our sophisticated contingentist historian at the beginning of our argument, while we admit the legitimacy of such a position (without sharing it), we deny that it implies a denial of any structure in science. Indeed, we deny from our initial thesis that it is possible to do any kind of meta-scientific study without an idea of what science is and what its component parts are (which is itself a minimal notion of what a structure of science is). Thus, the contingentist should not accuse someone who uses some structure as a framework or model for her historiographical work of being a Hegelian, but reserve, like Popper (1991; 2011), such a label for those who think they can understand History's overall meaning.

Having said all this, unlike the criticisms of Wray and Melogno, we think very interesting to incorporate into our analysis Daston's proposal to reunite the history of science with philosophy and sociology through the "soft" (and spongy) core of *Structure*, namely, the paradigmatic exemplars (already present in the original edition but baptized in the postscript of '69 (Kuhn 1996) following Masterman's (1970) criticisms of the ambiguity of the term "paradigm"). As stated above, Daston argues that this unit of analysis is richer than the paradigm as a disciplinary matrix because it better reflects the nature of scientific practice and how aspiring scientists learn their craft. This is not the space to discuss this proposal, but we do not find it dismissible. In fact, in Giri & Giri (2020), it is argued that the most prolific version of Kuhnian historiography does not occur in *Structure* but in an previously unpublished work recently recovered and published only in Spanish, *Scientific Development and Lexical Change*

(Kuhn 2017),[7] a work that draws from his Thalheimer Lectures of 1984. However, we do want to highlight the following point: if the "soft" structure of science (built on paradigmatic exemplars) does not imply for Daston a carrier of Hegelianism, her concern would not be with all structures *per se*, but with the particular structure upheld in the idea of paradigmatic cycles in *Structure*. It is a legitimate part of philosophical and historiographical work to consider which structures are most suitable for designing narratives in the history of science, but it is not legitimate to accuse any work explicitly supporting a specific structure as being Hegelian.

3.2. Once again, the differences between philosophy and history of science

As we saw in 2.2, Wray (2023) denies the presence of a presupposed historiography in *Structure*, providing a range of arguments to support the purely philosophical nature of the treatise.[8] We won't deny that Kuhn's intention may have been purely philosophical. Nor will we refute Daston's claims regarding the limited influence of *Structure* on history majors. Our disagreement lies at a more conceptual level, regarding the possibility of making sharp distinctions between philosophy and the history of science. Certainly, the philosophy of science, as a professional discipline, differs in scope and method from the history of science, although they clearly have intimate and profound relationships.

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[7] There, Kuhn systematically considers a philosophy (and, we add, consequently a historiography) of science focused on the analysis of theoretical change through the alteration of taxonomies (which influences phenomena of local incommensurability). The novelty of the work lies in its systematic and comprehensive presentation, but Kuhn's semantic concerns do not emerge only in his Thalheimer Lectures; in fact, they go much further back (see Mayoral, 2023; Melogno and Giri, 2023).

[8] Although this is not entirely evident, given that, according to the argument, Kuhn presented himself either as a philosopher or as a historian, the citations provided by Wray can be contrasted with others, such as his introduction to the Isenberg Conference in 1968, six years after the publication of *Structure* and a year before drafting his postscript: "I stand before you as a historian of science. My students, for the most part, wish to be historians, not philosophers. And I am a member of the American Association of History, not philosophy" (1977a, 3). For numerous citations and analyses of the intention of Kuhn's project, and a bold transcendental interpretation of it, see Kindi (2005).

The issue of the relationship between history and philosophy of science has been addressed in many works, some of which are already considered classic, especially by historicist philosophers of science. It is enough to recall Lakatos's (1970) famous Kantian paraphrase: "Philosophy of science without history of science is empty; history of science without philosophy of science is blind" (91). Perhaps Lakatos's work most clearly outlines the methodological relationship between the two, emphasizing the importance of historiographical work for the philosopher by providing the sources to be rationally reconstructed and the role of philosophical work for the historian by indicating units of analysis and dynamic patterns to guide the construction of historical narratives. It is clear that certain types of historians (numerically the majority) seem to doubt the fertility of such units of analysis and dynamic patterns for their work. Lakatos would accuse them of creating "blind" historical narratives, but in many cases, what seems to be happening is that such units and patterns are not explicit in the narratives and will only emerge with reconstructive philosophical effort.

Certainly, in discussions regarding the relationships between history and philosophy, as we said before, it is also asserted that, for these relationships to be virtuous, they should not be overly restrictive. In other words, it would be challenging for historical facts, as revealed by sources, to fit into a specific philosophical framework without some degree of flexibility concerning certain parameters (see Nickles 1986; Moulines 1986). However, according to our thesis, some philosophical premises will always be present in historiographical work. In fact, Paul Hoyningen-Huene (2012) terms a set of presuppositions that historians necessarily use to select material to guide the narrative as "philosophical elements of historiographical work," and these include:

> "…the usually implicit assumptions about history itself, or about proper historical research and presentation which influence historical work. It is clear that, for example, decisions about the general aims of historiography of science (...), or convictions about the influence of social factors on the content of science, qualify both as criteria of historical relevance and as philosophical elements of the respective historiography." (Hoyningen-Huene 2012, 283).

Having said all this, we find ourselves asking whether *Structure* is a book about philosophy or historiography, but the answer at this point seems trivial: it is both of them. It aims to reveal certain aspects of the nature of science (its structure, dynamics, etc.) and also aims to assert that such structure and dynamics fruitfully model real historical processes of science as revealed by sources[9]. In this regard, our disagreement with Wray (and agreement with Daston and Melogno) regarding the historiographical (but not exclusively historiographical) nature of Thomas Kuhn's classic is evident. We clarify that we are asserting that *Structure* is a book of philosophy and historiography, but not a history book. The historical cases, as interpreted by, among others, Sharrock & Read offer "precious little" (2002), serving as illustrative and persuasive examples without any claim to exhaustiveness. Thus, Kuhn's proposed model of how science progresses in his classic is simultaneously "philosophical" and "historiographical," as it can be used to describe and explain phenomena of theoretical change but also to support certain narratives about specific events in the history of science.

Having stated that, we would like to go beyond the question of the nature of *Structure* to further analyze the relationship between philosophy and the history of science. It is clear that it is possible to engage in philosophy of science without historical sensitivity. Much of the philosophy of the Received View possessed this nature, as it inquired about the abstract structures of certain scientific processes without concern for their adequacy to real historiographical sources, being more normative than descriptive in character (see Kuhn 1996). This philosophy may be accused (and has been) of being "empty," but in any case, it is evident that it can be done. On the other hand, doing the history of science without the philosophy of science seems, after our reflection, much more difficult. We insist, of course, that the scope and method of each discipline are different, but still, we, along with Hoyningen-Huene (2012), affirm that historiographical work requires criteria in its methodology of source selection and narrative construction that are of a philosophical nature

[9] Without aiming for exhaustiveness, examples of the history of science carried out through a fairly systematic use of the Kuhnian metatheoretical tool can be seen in the approach to the chemical revolution by Chang (2012) or the history of the emergence of the theory of Jay Wright Forrester's *Theory of Dynamic Systems* by Giri (2021).

(although also involving others that are not, such as narrative and factual criteria). Again, it can be acknowledged that many historians may not be aware of the philosophical presuppositions they are considering when doing their work, and they may remain implicit, but that does not mean they do not exist.

Let's look at an example of a statement extracted from *Structure*:

> "Just because the emergence of a new theory breaks with one tradition of scientific practice and introduces a new one conducted under different rules and within a different universe of discourse, it is likely to occur only when the first tradition is felt to have gone badly astray." (Kuhn 1996, 85-86)

Is it a philosophical or historiographical statement? It seems more like the former, as it looks like a generalization drawn from historical cases in which the resistance of scientists immersed in a theory prevented the adoption of a new one until the old theory entered a terminal crisis. Let's rewrite the previous quote to 'historicize' it:

> *Just because the emergence of Newtonian Mechanics broke with the Aristotelian tradition of scientific practice and introduced a new one conducted under different rules and within a different universe of discourse, it was likely to occur only when Aristotelian Mechanics was felt to had gone badly astray.*

Our 'historicizing' method consisted solely of instantiating the previous generalization in a particular case, and indeed, it would not be complex to perform an inverse 'philosophizing' function to go from the second to the first, merely generalizing the particular. However, the fact that our method was successful should not be interpreted as a statement that the difference between the two disciplines is merely a difference between the general and the particular, or between the normative and the descriptive. What should be understood, in our view, is that the premises of the historicist philosophy of science, of which the previous statement is just a small example, although trivially philosophical, have clear and intentional historiographical implications. Therefore, it becomes an unfruitful task

to separate the philosophical from the historiographical in the works of authors like Kuhn, Lakatos, Laudan, Kitcher, and others. It only makes sense in obvious cases, but the most interesting aspects lie in the unclassifiable intermediate gray areas.

However, it is worth to note that the argument we have presented here not only sets us apart from Daston but also diverges from Kuhn himself. While he excelled as both a philosopher and a historian, Kuhn argued that these two activities should be sharply separated (see Kindi 2005, 496), even if there might be some kind of inter-fertilization. We believe that Kuhn, by fluidly switching roles, inadvertently pointed the way to undermine the strict separation between the two disciplines. An example of this is found in his work *Black-Body Theory and Quantum Discontinuity*, where the philosophical concepts of *Structure* are conspicuously absent. In fact, in his Afterword, he himself recommends avoiding philosophical terminology in historical work (Kuhn 1987), and scholars like Klein, Shimony & Pinch (1979) had acknowledged conceptual incongruities between the two works. However, Hacking asserts, "Note, however, that he often said in conversation that '*Black-Body and the Quantum Discontinuity*,' a study of the first quantum revolution launched by Max Planck at the end of the nineteenth century, is an exact example of what *Structure* is all about" (Hacking 2012, 6).

On the other hand, Kuhn himself also suggested that, "Often I do not know for some time after my historical work is completed the respects in which it does and does not fit *Structure*. Nevertheless, when I do look back, I have generally been well satisfied by the extent to which my narrative fits the developmental schema that *Structure* provides" (Kuhn 1987, 363).[10] Considering these quotes, although *Black-Body* may not have been explicitly done using *a priori* the *Structure* model as a

[10] We do not quote this passage extensively, crucial as it is, for synthesis, but here Kuhn summarizes how the concepts of *Structure* apply to the narrative of *Black-Body*, and after that states "These illustrations of the substantive applicability of *Structure* can be extended, but, for this paper, it is the book's historiographic applications that are relevant" (1987, 364). Kuhn's statements in the Afterword of *Black-Body* not only support Hacking's claim that Kuhn regarded it as a narrative strongly compatible with the philosophical model of theoretical change in *Structure* but, as we assert here, Kuhn also considers his model applicable to historiography.

historiographical tool, the fit was sufficient for Kuhn to be satisfied, to the point that it is deemed "an exact example of what *Structure* is all about", at least *a posteriori.* We won't attempt to assert how Kuhn was able to unintentionally achieve an exemplary instantiation of the Kuhnian model but will limit ourselves to affirming that this integration between history and philosophy exemplified by *Black-Body* is what we defended in these pages.

This issue has an interesting interpretation by appealing to Kuhn's taxonomy of profiles of historians of science, which we have already referenced (Kuhn 1977a, b, c). Kuhn distinguished two profiles of historians of science: type I had been trained as historians and dedicated themselves to science as a subdiscipline. It is clear that this is the type of historian, a historicist, described in detail by Daston (2016) in her anti-structure proclamation. Additionally, Kuhn identified a type II profile: those trained as scientists who later worked on the history of the disciplines in which they were experts (these concerned Kuhn due to their Whig tendency, although he also recognized the utility of this profile for the training of scientists, see 1963).

In Giri & Giri (2020), a third profile of historian is described, one of "individuals trained in the philosophy of science who, based on some epistemological profile preference, have begun to delve into the past, generating fruitful works in the history of science" (2020, 79). This philosopher/historian is undoubtedly chameleonic, making it not worth classifying as one or the other. What is worth emphasizing, however, is the legitimacy and fruitfulness of their work. Kuhn is undoubtedly one of the most emblematic scholars belonging to this profile, even though he denied its fertility; we assert that he embodied it in an exemplary manner.

4. Final remarks

This work has aimed to recover the most relevant notes from the fascinating Daston-Wray-Melogno controversy, particularly regarding the relevance of abstracting a structure for science in historiographical work. We have argued that it is not possible to conduct the history of science without an idea of what science is, and any conceptualization of science involves a

description of its parts, relationships, and dynamics, ultimately constituting a minimal notion of "structure." As a corollary, every historian presupposes a structure of science in their historiographical work, even if not necessarily made explicit.

We find Daston's accusation of a Hegelian remnant in *Structure*, as well as Wray's disdain for the historiographical value assigned to Kuhn's classic work (in agreement with Melogno on both criticisms), unfair. At the same time, we celebrate the controversy as an opportunity to re-explore classic themes that emerged with the popularization of historicist philosophy, especially regarding the methodological value of explicating structures during historiographical analyses and the relationship between history and philosophy of science.

We have defended the relevance of using philosophical rational reconstructions for the history of science as a typical method of a third profile of historian coming from the disciplinary area of the philosophy of science. We have also asserted that, while history and philosophy of science are different disciplines, their relationship is so close, especially in historicist philosophy of science, that certain statements can be read as belonging to either discipline or easily lead to equivalent statements in the opposing discipline.

As a corollary to all of this, it is cause for celebration to revive these classic discussions and seek dialogue, albeit critical, among different profiles of historians, which will ultimately contribute to better historiographical work. At the centenary of his birth, it is also valuable to reclaim the legacy of one of the great authors of our time through the critical discussion of his perpetually relevant contributions.

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Constantin STOENESCU[1]

BEFORE *STRUCTURE*. THE RISE OF KUHN'S CONCEPTUAL SCHEME IN *THE COPERNICAN REVOLUTION*

Abstract. Thomas S. Kuhn's intellectual development could be summed up in a two-stage course, first, the transition from physics to the history of science (primarily physics) and then from the history of science to the philosophy of science, a field in which he achieved consecration with *The Structure of Scientific Revolutions* (*SSR)* published in 1962. In the 1950s, before *SSR*, Kuhn dealt with the history of science and, finally, developed a detailed research on the case of the Copernican Revolution, publishing a book with the same name. *The Copernican Revolution. Planetary Astronomy in the Development of Western Thought* (*CR*). My aim in this paper is to argue that in this case study Kuhn identified all those situations that he will later describe in the terms of the *SSR*'s vocabulary, from "paradigm" and "incommensurability", to "normal science" and "scientific revolution." I think that although the terminological options in *CR* differ, such as, for example, the use of the expression "conceptual scheme" for what will later be called "paradigm", a simple conceptual archaeology directs us to claim that *CR* is the immediate predecessor of *SSR*.

Keywords: Thomas S. Kuhn, history of science, Copernican Revolution, scientific revolution, scientific belief, cultural and intellectual contexts

1. The road to the history of sciences

After completing his studies at Harvard University with a Ph.D, in Physics, the young Kuhn continued with several scientific studies to deepen the research in his own field of specialization. The Ph.D. thesis was published

[1] Professor at the University of Bucharest, Faculty of Philosophy, https://orcid.org/0000-0002-3235-7771. Email: <constantin.stoenescu@filosofie.unibuc.ro>.

by Harvard University under the title "The Cohesive Energy of Monovalent Metals as a Function of Their Atomic Quantum Defects" in 1949. Two studies published one year later in *Physical Review*, "A Simplified Method of Computing the Cohesive Energies of Monovalent Metals" and "An Application of the W.K.B. Method to the Cohesive Energy of Monovalent Metals," seemed to announce a researcher devoted to his very narrow field of scientific interest and by no means concerned with the history of science and, even less, with any philosophical aspects. Eventually, the only admissible extensions of the research lead beyond the borders of Physics in the immediate neighbouring in the area of mathematical physics, a fact confirmed by the article "A Convenient General Solution of the Confluent Hypergeometric Equation. Analytic and Numerical Developments", published in *Quarterly of Applied Mathematics* in 1951.

But such an image of his career beginnings become apparent and deceptive since it considers this one-sided interest for professionalization in the academic field of Physics. In fact, Kuhn's first public appearance as an author in the Harvard University environment was the early publication in 1945, in the magazine of the alumni society, of some declared subjective notes regarding the characteristics that education should have in a free society (Kuhn 1945a; 1945b).

The decisive event that directed him towards the history of science happened, as he testifies in the "Preface" to *The Essential Tension* (Kuhn 1977, xi), in 1947 when, as a result of an institutional request, he interrupted his physics studies to prepare a series of lectures on the history of science, especially on the origin of mechanics of the 17th century. These lectures were a kind of introductory science lessons that he gave to students from the humanities field, not interested in a career in science, but whose general culture had to be built up by including some elementary scientific knowledge. The lectures were part of an innovative curriculum designed by James B. Conant, president of Harvard University, who proposed a comprehensive educational model, based on achieving a balance between humanistic culture and scientific training[2]. Conant considers that an intellectual history of science involves placing science in a cultural context, which leads to a new image of science and which allows us to understand that

[2] For details about this project see Swedlow, 2004.

"science has been an enterprise full of mistakes and errors as well as brilliant triumphs; science has been an undertaking carried out by very fallible and often highly emotional human beings; science is but one phase of the Western world which have given us art, literature, and music" (Conant 1995, xviii).

Kuhn later described the consequences of assuming this institutional task which gradually turned itself into an exploratory and formative experience. The course was focused on case studies from the history of science which gave him the opportunity to study ancient scientific texts, including Aristotle's *Physics.* If he was initially disturbed by the Aristotelian theories, so different from those of modern physics, he tried to understand them correctly in their own context and was thus able to find an explanation regarding the acceptance of Aristotelian physics by the ancient thinkers. Kuhn realized that it is a mistake to read the Aristotelian texts from the perspective of Newtonian vocabulary and tried to enter into the conceptual network of Aristotelian way to put the questions and give the answers. He understood the for Aristotle, unlike Newton and his successors, the main subject

> "was change-of-quality in general, including both the fall of a stone and the growth of a child to adulthood. In his physics, the subject that was to become mechanics was at best a still-not-quite-isolable special case. More consequential was my recognition that the permanent ingredients of Aristotle's universe, its ontologically primary and indestructible elements, were not material bodies but rather the qualities which, when imposed on some portion of omnipresent neutral matter, constituted an individual material body or substance. The position itself was, however, a quality in Aristotle's physics, and a body that changed its position therefore remained the same body only in the problematic sense that the child is the individual it becomes. In a universe where qualities were primary, motion was necessarily a change-of-state rather than a state." (Kuhn 1977, xi-xii)

Moreover, as he will mention in the "Preface" to The *Copernican Revolution* when he combined the strictly technical aspects with those of intellectual history, he realized that although "scientific materials are essential, they

scarcely begin to function until placed in a historical or philosophical framework where they illuminate the way in which science develops, the nature of science's authority, and the manner in which science affects human life" (Kuhn 1995, ix).

As a result of this teaching activity, Kuhn also reconsidered the priorities of his scientific research and he turned form physics to the history of science, the main themes approached in this context being the theory of matter from the 18th century and then the early history of thermodynamics. He begins to publish a series of short articles and reviews on these topics in the prestigious journal *Isis* edited by the University of Chicago, founded and directed by George Sarton between 1913 and 1952, taken over from 1953 by I. Bernard Cohen, professor of the history of science at Harvard University. *Isis* journal promotes both studies on fundamental theories in the history of science, as well as on applied developments in medicine and technology, or on contextual economic, social and cultural influences. Kuhn first publishes, in 1951 and 1952, several studies and a reply on topics regarding the theory of matter developed by Newton and Boyle. These are: "Newton's 31st Query and the Degradation of Gold" (Kuhn 1951b), "Robert Boyle and Structural Chemistry in the Seventeenth Century" (Kuhn 1952a), "Reply to Marie Boas: Newton and the Theory of Chemical Solution" (Kuhn 1952b), "The Independence of Density and Pore-Size in Newton's Theory of Matter" (Kuhn 1952c). The next two years were intensively used for a historical research of Descartes and Galilei works. Kuhn has published two reviews, one in *Isis* about "*The Scientific Work of René Descartes* (1596-1650), by Joseph F. Scott", and of "*Descartes and the Modern Mind*, by Albert G.A. Balz" (Kuhn 1953a), another in *Science* about "Galileo Galilei: *Dialogue on the Great World Systems*, revised and annotated by Giorgio de Santillana" and of "Galileo Galilei: *Dialogue Concerning the Two Chief World Systems – Ptolemaic and Copernican*, translated by Stillman Drake" (Kuhn 1954b). Kuhn also reads and reviews in *Isis* works on the Cartesian philosophy such as "*New Studies in the Philosophy of Descartes: Descartes as Pioneer and Descartes' Philosophical Writings*, edited by Norman K. Smith," and "*The Method of Descartes: A Study of the Regulae*, by Leslie J. Beck" (Kuhn 1955c). Finally, paying attention to new appearances in the field, Kuhn becomes a critical reader of *Ballistics in the Seventeenth Century: A Study in the*

Relations of Science and War with Reference Principally to England, by A. Rupert Hall (Kuhn 1953a), *The Scientific Adventure: Essays in the History and Philosophy of Science*, by Herbert Dingle (Kuhn 1953c) and *Main Currents of Western Thought: Readings in Western European Intellectual History from the Middle Ages to the Present*, edited by Franklin L. Baumer (Kuhn 1954a), published alternatively in *Isis* and *Speculum.* If the first works of this kind proposed an externalist perspective that considered the relationship between science and technology, the latter two were the expression of the increasing concerns for the intellectual history of science. All these concerns are the evidence of Thomas Kuhn's effort to synchronize with the current state of research in the field of the history of science and to assert himself as a researcher who belongs to this field and is part of this community of researchers.

Some of the case studies on which the series of lectures was based attract his attention by their relevance for a new understanding of the history of science. Kuhn begins to work on the project of a larger research on the Copernican Revolution and is concerned with the beginnings of thermodynamics. On this last topic, he publishes two notes about the so-called "Carnot cycle", "Carnot's Version of Carnot's Cycle" (Kuhn 1955a) and "La Mer's Version of Carnot's Cycle" (Kuhn 1955b). The fact that between 1955 and 1957 he did not publish much anymore, is an indirect proof of directing his efforts towards the completion of the book about the Copernican Revolution. In the same years he is involved in the activity of the Society for the History of Science, founded in 1924 by the same George Sarton, publishing the minutes of the council meetings and one report (Kuhn 1956a; 1956b).

In short, starting from 1947, when he begins to prepare the series of lectures on the history of science and until the publication of the book *The Copernican Revolution. Planetary Astronomy in the Development of Western Thought* in 1957, Thomas Kuhn went through a complete metamorphosis process from a specialist in physics into a historian of science. But even more important is the way in which Kuhn rethinks the traditional history of science as intellectual history.

2. A new approach to the history of science

The history of science was traditionally divided into the external (or externalist) history of science, which implied a historical vision of the facts by correlating the process of science development with the evolution of society, primarily from an economic and technological standpoint, and the internal (or internalist) history of science, which implied a perspective on the history of science as a history of ideas or an intellectual history. This second perspective had gained more prestige and relevance even in the years when the young Kuhn was asked to deal with the history of science in the form of significant case studies. The debate on how the history of science should be done was opened by Sarton with a famous article (Sarton 1916) in which he identifies the dilemma between the prioritization of the external conditions of science development and, respectively, the highlighting of the ideational relations between the various particular sciences within science as an intellectual practice.

The research carried out in order to complete the work on the Copernican Revolution will give Kuhn the opportunity to think about the way in which the history of science is traditionally done. Thus, starting from the case of the Copernican Revolution, Kuhn finds that history was told from a multitude of points of view, without capturing the characteristics that transcend all these unilateral interpretations, and that each researcher looked in isolation at those aspects towards which he directed his attention from the beginning. Or, according to Kuhn, the Copernican Revolution, although interpreted pluralistically, has a common core and an interdisciplinary character:

> "Though the Revolution's name is singular, the event was plural. Its core was a transformation of mathematical astronomy, but it embraced conceptual changes in cosmology, physics, philosophy and religion as well." (Kuhn 1995, vii)

This plurality of the Copernican Revolution allows the researcher to become aware of how different disciplinary fields provide concepts and ideas that "are woven into a single fabric of thought." (Kuhn 1995, vii)

As a result, a correct and complete history of science must take into account the various external conditions, from the economic ones to the cultural ones, and contextually reveal the relationships between ideas in their succession. Thus, although Copernicus himself was a narrow specialist, concerned with an esoteric problem of mathematical astronomy, that of calculating the position of the planets in the sky, the direction of his research was determined by conditions external to astronomy, as were the exploratory researches in medieval physics on the fall bodies, or the Renaissance resurrection of the old mystical philosophy according to which the sun was considered an image of divinity, or the geographical discoveries of navigators that widened the horizon of knowledge.

Together with Kuhn, we can distinguish between three dimensions of the Copernican Revolution: one strictly astronomical, one generally scientific, another philosophical. In the strict astronomical sense, the Copernican Revolution is a reform of the fundamental concepts of this field: through his work, *De revolutionibus orbium coelestium*, published in 1543, Nicolaus Copernicus aimed at nothing more than to increase the accuracy and precision of the theory about the movement of celestial bodies on their orbits by transferring to the Sun those astronomical functions that until then were attributed to the Earth, resulting the Earth losing its unique position as the astronomical center of the universe.

In a general scientific sense, the Copernican Revolution is important for the somewhat unintended consequences it produced in understanding the nature and role of science in society. Copernicus' attempt to improve the predictive power of the theory regarding the positions of the heavenly bodies generated debates about the compatibility of this theory with the traditional view of the universe and became the intellectual ferment of the Scientific Revolution of the 17th century that culminated with Newton's theory. From this perspective, the Copernican Revolution led to a radical change of the conceptions about the universe.

Thirdly, the Copernican Revolution also has a deep philosophical significance. His astronomical theory was a tool able to assure the transition from medieval to modern thought because it influenced the changing image of the relationship between man, the universe and God, as well as, along with this, it produced revaluations and re-significations of the meaning of human existence. Therefore, Kuhn concludes:

> "Initiated as a narrowly technical, highly mathematical revision of classical astronomy, the Copernican theory became one focus for the tremendous controversies in religion, in philosophy, and in social theory which, during the two centuries following the discovery of America, set the tenor of the modern mind." (Kuhn 1995, 2)

The traditional histories of science artificially separated in the mind what was in reality united and thereby lost the authenticity of the process of science development. They either limited themselves to the investigation of certain economic or other external conditions, or followed internally a succession of ideas, or described the cultural diffusion of a scientific invention or idea. Kuhn's proposal is a historical reconstruction that connects all these aspects:

> "We need more than an understanding of the internal development of science. We must also understand how a scientist's solution of an apparently petty, high technical problem can on occasion fundamentally alter men's attitudes toward basic problems of everyday life." (Kuhn 1995, 4)

The main problem raised by the young Kuhn involves an understanding of the old texts in their own intellectual framework of their time starting from their explicit claims and implicit assumptions and commitments. As a result, the historian of science must offer a historical reconstruction of a scientific episode that involves placing it in a context, and by no means an evaluation of the conceptual schemes of the past from the perspective of the concepts and theories accepted in the present. Kuhn resorted to this historical plunge to understand Aristotle and will do the same in the case of the Copernican Revolution. Otherwise, it would mean that we do not understand anything from the old theories, that we wonder how they were accepted, and we consider them as strange or irrational products of the human mind.

3. Why do we accept theories that are later discarded?

Kuhn's problem is rather that of identifying and explaining the grounds that underlie the acceptance of a theory at a given time. Researching the Copernican Revolution allows Kuhn to have some insights into the development of science as a process and the relations between science and society at different stages of science development. Through such historical research, we grasp both the common problematic area and the radical differences between Copernicus' theory and the previous ones, but at the same time, we find that the previous ones were equally credible for those who supported them. However, why were they accepted? The reason for their acceptance was the same for which we later accepted Copernicus' theory: "they provided plausible answers to the questions that seemed important." (Kuhn 1995, 3)

So, theories change, but a theory has its heyday when it is accepted and taken for granted. Kuhn believes that the history of science is an important source to have "a perspective from which to examine the scientific beliefs which it takes so much for granted" (Kuhn 1995, 3-4). Kuhn is surprised by several aspects that were against the traditional image of scientific progress:

- scientific theories do not follow each other cumulatively, but replace each other;
- theories in science are not definitive, they are temporary and they can be revised or abandoned;
- but the old theories were trusted by the members of the scientific community because they fulfilled some explanatory functions specific to science.

Kuhn concludes in an evolutionist vocabulary that anticipates further developments in the "New philosophy of science" and not only that:

> "If we can discover the origins of some modern scientific concepts, and the way in which they supplanted the concepts of an earlier age, we are more likely to evaluate intelligently their chances for survival." (Kuhn 1995, 4)

Starting from the case study of the Copernican Revolution, Kuhn believes that we can obtain conclusions that are valuable for science in general and that we can thus give answers to questions such as "What is a scientific theory? On what should be based to command our respect? What is its function, its use? What is its staying power?" (Kuhn 1995, 4). Even if historical analysis does not provide complete and conclusive theoretical answers to these questions, it can help us to understand them better and it guides us in our theoretical research.

Let's consider the Copernican Revolution as a case study that allows us to understand the mechanism by which a theory is accepted. It is obvious that astronomical observations and theories have an impact on cosmological thinking, that is, on the set of concepts regarding the structure of the universe. Seen in their historical sequence, the cosmologies went further and further from a scientific, technical and systematic point of view, but each one, at its moment of glory, received the consensus of the intellectual community and society as a whole. This consensus is ensured by the fulfilment by each cosmology of two requirements, namely, that of providing an image of the world that satisfies certain psychological needs and that of giving a coherent explanation of the observed phenomena (See Kuhn 1995, 7). Thus, primitive cosmologies are shaped by everyday experiences and by the need to offer for each person the comfort of integration in a universe that they feel like their home. In Kuhn's terms, these cosmologies or cosmological sketches give meaning to everyday, practical or spiritual activities.

Gradually, the second requirement of a coherent explanation of the observations became more important and, finally, it was reached a bifurcation: scientific observations began to play the role of empirical validation for the various images of the universe that were accepted precisely for that they ensured psychological comfort. But things are not quite simple because, according to Kuhn, observations are not neutral or pure, but loaded with theoretical expectations. A first level of these expectations is given by the observational habits that we acquire over time as a result of observing various astronomical regularities, for example, the configuration of the constellations. These act on our mind like a familiar star map and their acceptance is explained by Kuhn with the help of *gestalt* psychology, through the universal need to identify

certain familiar patterns in the chaotic flow of experience. Moreover, we can use the star map to make predictions about the position of the sun in the sky in the future. This ability to make such astronomical predictions becomes a mark of scientific knowledge.

We discover here, in a still rudimentary form, two of the theses that will become redoubtable later in Kuhn's philosophy, that of the image of the world as a *gestalt* with a certain structure and that of the theoretical loudness of observation. Both ideas will become key elements in the explanation of the paradigm shift. The theories invented by the astronomers are tentative solutions based on interpretations of observations that are incorporated into the vocabularies used. From here we can reach the paradoxical situation in which "two astronomers can agree perfectly about the results of observation and yet disagree sharply about question like the reality of the motion of the stars" (Kuhn 1995, 26).

Therefore, we accept a theory to the extent that it fulfilled explanatory and utilitarian functions, namely, logical and psychological functions that intertwine and that ensure the theory's resistance over time once we start to believe in it. In short, generalizing, we will say that we fully accept the theories we believe in and ensure a coherent and comfortable perspective on the world.

4. The idea of a conceptual scheme

Kuhn introduces the idea of a conceptual scheme starting from the case study of the astronomical model of "the two sphere-universe". The ancient Greeks were the first to describe the structure of the universe through a conceptual scheme in the form of the two sphere-universe, the inner or terrestrial sphere and the outer or celestial sphere. This image that enjoys the consensus of astronomers and philosophers looks like this: the Earth is a tiny sphere suspended stationary in the geometric center of a much larger sphere that rotates and carries the stars. The sun moves in the vast space between the earth and the sphere of the stars. Beyond the stars sphere there is nothing, neither space nor matter. The sources of this astronomical model are Egyptian and Babylonian, and it corresponds to their observations and their cosmological vision. The

ancient Greeks develop the conceptual scheme by articulating the model within a philosophical framework.

Kuhn identifies the main elements that make up this conceptual scheme. First of all, we must mention the Platonic philosophical sources that question a perfect universe, from where it follows that, because it is perfect, it must be symmetrical. This argument based on symmetry is very strong and coherent in ancient thought, although some of its consequences seem strange to a modern thinker. Anyway, the important thing is that this model, a product of the imagination, corresponded with the observations that had been made.

The second element, perhaps even more striking, is that the model of the two sphere-universe achieves a "conceptual economy" (Kuhn 1995, 37) in relation to the complexity and abundance of observations. The two-sphere model compactly summarizes a huge number of observations and is quite useful. It remains valid today, for example, for navigation on the earth, that is, we do not need to take anything else into account; it is enough to assume that the earth is at the center of a rotating sphere. The model is useful to navigators, regardless of whether it represents reality or not. In this sense, from the perspective of conceptual economy, the two-sphere model remains a successful theory.

Symmetry and conceptual economy are logical functions, but the model of the two spheres also has psychological functions that depend on the and beliefs of the scientist. For example, the desire to feel at home can only be satisfied if the scheme offers more than a conceptual economy. The ancients and early moderns even believed that the universe of the two spheres was a real one, and the adjacent cosmology offered an image of the world, established man's place in the universe and provided a meaning to the relationship with God. Therefore, it is quite obvious that a conceptual scheme that functions as part of a cosmology has more than a strictly scientific significance.

Beliefs affect how conceptual schemes work in science. We have a spectrum that has at one end conceptual economy as a purely logical function and at the other intellectual and emotional satisfaction as a purely psychological function. But we have to add other intermediaries. A good example is that of an astronomer who believes in the validity of the two-spheres universe because it provides a synthesis of the observed

appearances, but also because that model explains them, leading us to understand them as they are. These two terms, "to explain" and "to understand", seem to refer simultaneously to both logical and psychological aspects. Logically, the two-spheres model explains the motion of the stars as it is deduced reductively from the model. Psychologically, however, the universe of the two stars offers an explanation only if we believe in it. We recognize in this distinction a theme of subsequent debates regarding the distinction between the logic of research and the psychology of discovery as it was drawn in the famous dispute between Popper and Kuhn.

Moreover, the scientist's adherence to a conceptual scheme has a psychological nature: "A scientist's willingness to use a conceptual scheme in explanations is an index of his commitment to the scheme, a token of his belief that his model is the only valid one" (Kuhn 1995, 39). Kuhn warns that such a commitment is always imprudent and hasty because conceptual economy (the logical criterion) and cosmological satisfaction (the psychological criterion) cannot guarantee truth, whatever we mean by "truth".

Given all these theoretical ingredients, Kuhn describes the process of science development as a competition and succession of conceptual schemes, so that we can suppose that all that is missing from this vocabulary previous to *SSR* is the term "paradigm". The history of science is full of the relics of conceptual schemes which "were once fervently believed and that have since been replaced by incompatible theories. There is no way of proving that a conceptual scheme is final" (Kuhn 1995, 39). But these conceptual schemes have another function that consists in their comprehension, namely, their capacity to transcend the known, "becoming first and foremost a powerful tool for predicting and exploring the unknown" (Kuhn 1995, 39). As a result, based on a conceptual scheme accepted at a given moment, we can not only interpret the entire history of a scientific field, but we also have a guide to the future that limits our theoretical choices and exploratory preferences. However, these constraints are weak enough to allow revisions and extensions:

> "Typically, a conceptual scheme provides hints for the organization of research rather than explicit directives, and the pursuit of these

> hints usually requires extension or modification of the conceptual scheme that provided them." (Kuhn 1995, 40)

Thus, the two sphere-universe proved a fertile conceptual scheme that was able to solve some problems of planets motion and that effectively guided the research and was the framework for organizing it.

5. "The anatomy of a scientific belief"

How do we explain this strong resilience of the belief in the central position of the earth, although the problem of the planets revealed serious inadvertences? Kuhn becomes aware of the fact that a scientific community hardly gives up the conceptual scheme that its members share in common. That is why it is not hazardous to say that perhaps even modern man would believe in the universe of the two spheres if the only celestial bodies visible to the naked eye were the sun and the stars. But the planets were also visible. The logical form of Kuhn's argument is one of a reasoning by *reductio ad absurdum*. Kuhn adopts a methodological strategy in which the observations, as a tribunal of experience, are those from which the interpretations derive:

> "Once again we consider observations before dealing with interpretive explanations. And once again the discussion of interpretations will confront us with a new and fundamental problem about the anatomy of scientific belief." (Kuhn 1995, 45)

But he finds that, in fact, already established beliefs were the ones that guided the observations and made them appear as we expected. However, in the case of the problem of the planets, the observations could no longer be adjusted according to our expectations and, therefore, this problem became the source of the Copernican Revolution.

Kuhn considered that the big problem of the two-sphere universe model was to reconcile the irregularities observed in the movement of the planets with a rigorous mathematical theory.

Astronomers believed in their model because it was consistent with all other cosmological and philosophical beliefs and their goal was to create a mathematical tool that would allow a more precise calculation of planets position. The problem of the irregular movement of the planets was an old one that came from Plato and became a great challenge for astronomers, still being "the big question" in Copernicus' time. Ptolemeus was the first to match observations and mathematics through the theory of epicycles, so he offered an astronomical archetype that justifies the statement that Ptolemaic astronomy refers rather to the traditional approach of the planets problem.

Ptolemaic astronomy, in its developed mathematical form, as a system of compound circles, based on epicycles and deferents, was a brilliant achievement, "but it never quite worked" (Kuhn 1995, 73). The greater accuracy was obtained with the price of increased complexity, that is, the addition of new epicycles and other instruments. None of the new more complex versions of the Ptolemaic system stand up to increasingly sophisticated observational tests, and these failures, coupled with the total disappearance of the conceptual economy, that supported the original versions, led to the Copernican Revolution, but it took about 1800 years, an enigmatic longevity that leads to questions:

> "How did the two-sphere universe and the associated epicycle-deferent planetary theory gain so tight a grip upon the imagination of the astronomers? And, once gained, how was the psychological grip of this traditional approach to a traditional problem released? Or to put the same question more directly: Why was the Copernican Revolution so delayed? And how did it come to pass at all?" (Kuhn 1995, 74-75)

According to Kuhn, we have here not only a problem of the history of science, but also one concerning "the nature and structure of conceptual schemes and with the process by which one conceptual scheme replaces another" (Kuhn 1995, 75). From a logical point of view, Kuhn admits in a Popperian style of falsificationism, that we have here a lot of alternatives and the observations should ensure the choice of one of them. But it doesn't happen like that. To explain due to what reasons such thing is

possible, Kuhn develops an alternative to Popperian falsificationism. In fact, we never have such observations absolutely incompatible with a conceptual scheme. On the other hand, Copernicus felt that the behavior of the planets is incompatible with the universe of the two spheres.

How to understand this historical fact in which logical incompatibility and psychological constraints mix contradictory? How can a conceptual scheme that one generation finds subtle, flexible, and complex become obscure, ambiguous, and unwieldy for the next generation? Why do scientists insist on supporting theories despite the discrepancies, and why, after having supported them, do they abandon them? How do we explain the strength of a tradition? Here are all the questions that will lead Kuhn to the theoretical developments from *SSR*.

In *CR* Kuhn explains at length how the astronomical model of the two spheres was incorporated into a complex fabric of non-astronomical beliefs. Likewise, the Copernican model will be part of such a complex system of beliefs. Therefore, *The Copernican Revolution* should not be reduced to a simple change regarding the position of the earth and the sun, but, viewed in its multiplicity of relations with fields external to astronomy, as a change in our worldview (Kuhn 1995, 94). This does not mean that nothing happened between Aristotle and Copernicus. On the contrary, intense work was done, immense intellectual energies were expended, but the Ptolemaic conceptual scheme was not questioned. And when this happened, it was produced not only by the internal problem of the planets, but also by the fact that the external, non-astronomical intellectual environment, had prepared such a change. The processes described here will later be named by Kuhn using the terms of the *SSR* vocabulary, from "normal science" to "disciplinary matrix".

6. The lessons given by *The Copernican Revolution*

According to Kuhn[3] we may distinguish between two aspects that a historical research of *The Copernican Revolution* reveals:

[3] It is interesting to mention that Kuhn returned to and revised his conception of science development outlined in *SSR*, but he did not return to the case study from

1. Considered as a typical scientific theory, its history is illustrative for the processes by which scientific concepts evolve and by which new concepts replace old ones.
2. Considered from the perspective of its extra-scientific consequence and of all its influences outside science, the Copernican theory is exemplary for the case of some theories, few in number, which produced large-scale changes in the external intellectual environment and determined reorientations of Western thought, such as Darwin's theory, Einstein's theory of relativity, and Freud's psychoanalytic theory.

If we look at the sequence of the two sphere-universe model and of the Copernican model, then we will conclude that the two are different, but the second was possible just because the first was developed till is the last consequences. Kuhn does not yet introduce the thesis of incommensurability, but accepts a dynamic based both on continuity in solving certain problems, such as the calculation of the planet's positions, and on a break at the basic level, such as the admission by the Copernican model of the hypothesis of a planetary earth:

> "The Copernican universe is itself the product of a series of investigations that the two-sphere universe made possible: the conception of a planetary earth is the most forceful illustration of the effective guidance given to science by the incompatible conception of a unique central earth. (...) The two-sphere universe is the parent of the Copernican; no conceptual scheme is born from nothing." (Kuhn 1995, 41)

It is obvious that incompatibility does not mean incommensurability here yet and that this second concept will be one of the novelties in SSR together with all its radical theoretical consequences for the understanding of science history.

CR. Such a reconstruction of the case study from the perspective of changes in Kuhn's conception of science development is proposed by Westman (1994).

Instead, although he does not use the concepts of paradigm and pre-paradigmatic phase, Kuhn describes in *The Copernican Revolution*, when he talks about the competitors of two sphere-universe, a state of scientific knowledge that has all the attributes of a pre-paradigmatic phase.

The model of two sphere- universe was not the only one proposed by the ancient Greeks, there were alternative models. It was eventually accepted from many alternatives, although some of the cosmological contenders looked more like the Copernican model of modernity than the two-sphere model. It is enough to mention the model of infinite worlds proposed by Leucippus and Democritus before Aristotle, or the model proposed by Heraclides Ponticus, contemporary with Aristotle, who suggested that there is a diurnal movement of the Earth, and not a rotation of the celestial sphere, or the model more later, from the 3rd century, proposed by Aristarchus of Samos, also called "the Copernicus of antiquity", which assumed that the Earth revolves around the Sun. However, most ancient philosophers and astronomers rejected these alternatives because they lacked the arguments that later supported the Copernican model. The main reasons to reject them were these:

> "These alternative cosmologies violate the first and most fundamental suggestions provided by the senses about the structure of the universe. Furthermore, this violation of common sense is not compensated for by any increase in the effectiveness with which they account for the appearances. At best they are no more economical, fruitful or precise than the two-sphere universe, and they are a great deal harder to believe. It was difficult to take them seriously as explanations." (Kuhn 1995, 43)

The observations suggested that the first astronomical distinction we must make is that between the earth and the heaven and that it would be absurd to believe, based on these observations, that the earth moves. Therefore, if we take these observations into account, then the difficult problem would not be to explain why the model of the two sphere-universe was derived from them, but why this model was abandoned.

Again, in a way that anticipates the ideas form *SSR*, Kuhn identifies that problem that gradually became an anomaly in relation to the model

of the two sphere-universe and generated efforts to solve it. Kuhn does not yet use a vocabulary that contains the term "anomaly" and the expression "extraordinary research", but the situations he describes in *CR* are similar to those to which the two expressions will refer. Kuhn also mentions the problem of the ingredients of a disciplinary matrix and highlights the role of various philosophical beliefs, in the case of Copernican Revolution the rediscovery of Platonism by the Renaissance.

Indisputably, the conceptual scheme developed by Kuhn in *CR* is based on his new approach to the intellectual history of science which consists of historical reconstructions as case studies. Kuhn derives from the case of the Copernican Revolution many of his theoretical theses that will then be coherently assembled in a new vision of the development of science presented in *SSR*.

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Peter DAN[1]

PARADIGM AND SYMBOLIC UNIVERSE: THE ENDURING SIGNIFICANCE OF THOMAS KUHN

Abstract. The importance of Kuhn's theory is examined from the perspective of its epistemological impact and contrasted with Popper's concept of falsification; the theory of paradigm shift is analyzed as a general model of change. Its concepts are applied to Berger and Luckmann's socially constructed reality and to the Symbolic Universe which sustains it. The theory of paradigm shift is used to analyze the process of changing the Symbolic Universe.

Keywords: Paradigm, anomaly, incommensurability, Symbolic Universe, conspiracy theories, apophenia, crank magnetism, information bubble

Before the publication of Kuhn's *The Structure of Scientific Revolutions*, the accepted criterion for establishing the validity of a theory in the natural and social sciences was Popper's (1934, 1959) concept of "falsifiability". While logically rigorous, it also resulted in an awkward view of the evolution of science advancing by denying the validity of hypotheses rather than affirming them. This view is contradicted by the scarcity of scientific journal articles containing negative findings, although, according to Popper, those are the only ones that should be considered. It also proved counterintuitive, implying that each experiment that verified a theory only increased its plausibility, in effect making "truth value" a probabilistic variable, asymptotically approaching verifiability but never reaching it. In effect, it meant that proof based on statistics was meaningless. It reminded

[1] Professor, Long Island University, NY, USA. Email: <peterdan13@hotmail.com>.

of Boltzmann's statistical interpretation of entropy, in which the possibility that all the molecules in a gas, rather than colliding randomly, will move simultaneously in the same direction exists, however infinitesimally small. This created a conundrum, since in my field of study, Psychology, and in Social Sciences in general, studies use statistical verification to prove their validity. A rather convoluted artifact, namely the "null hypothesis" was employed: proving your hypothesis by disproving the opposite of what you are trying to prove. Kuhn's proposes that

> "during normal science scientists neither test nor seek to confirm the guiding theories of their disciplinary matrix. Nor do they regard anomalous results as falsifying those theories… Rather, anomalies are ignored or explained away if at all possible. It is only the accumulation of particularly troublesome anomalies that poses a serious problem for the existing disciplinary matrix." (Bird 2004)

Gergen (1982) argued that Popper's model is not appropriate for the study of phenomena in social sciences. He stated that "the chief criterion for theoretical evaluation" (by traditional standards) namely empirical validity (or its close associates, "truth value", "empirical content", and "resistance to falsification"), is inappropriately applied to "theories of human conduct". Gergen proposed as a replacement the "generative capacity" – the capacity to raise fundamental questions, to challenge the basic assumptions of a culture concerning social life, to provide alternatives for social action.

> "After reviewing the major theoretical orientations: Popper, Habermas, the phenomenological and the dialectical method, Gergen (1982) believed that there is a commonality of ideas underlying them which may constitute the basis for a unified alternative: the emergence of a new, 'sociorationalist' metatheory: the generation of rationality through social interchange." (Dan 2011, 34)

Popper did not believe that Psychoanalysis and Individual Psychology are sciences, because they cannot be falsified. Kuhn, on the other hand, believed that because they lack a common methodology and interpretive

framework, social sciences are immature, falling in the "prescience" category. Note though the overlap between Gergen's "generative capacity" and Kuhn's "puzzle solving." The view of knowledge progressing and being accumulated by sciences solving essential "puzzles," rather by constantly disproving emerging hypotheses, proved to have a strong stimulating effect. After all, some of the most influential psycho-social experiments of our times: Asch's social illusion experiment, Milgram's obedience studies, Zimbardo's Stanford prison experiment and Elliot's "Blue eyes–Brown eyes" experiments were published without any formal statistic validation.

> "Furthermore, the sources of some of the most influential psychological theories such as those of Freud, Piaget, and Erickson are based on observations and very small – sometimes a single subject – case studies, the weakest of all experimental designs. Nonetheless, generativity and postdiction override the simplistic methodological objections and grant these theories the place they deserve." (Dan 2011, 35)

I believe this is due to the fact that these studies solved essential "puzzles," are significant contributors to the paradigm of social sciences, and in some cases created revolutionary science.

Paradigm shift as a model for change

Another aspect of the enduring significance of Kuhn's theory is that it provided a common framework for conceptualizing change in widely different domains. For example, in psychology, there are numerous theories of development taking place in stages, for example Freud's psychosexual stages, Piaget's stages of the development of intelligence, Erikson's stages of psychosocial development, Kohlberg's stages of moral development. In each of these theories, stages are distinct from each other, yet each stage continues the previous one and prepares the next one. Applying Kuhn's theory of paradigm shift to developmental stages clarifies how the dynamic between the forces for stability and forces for change allows for conceptualizations of development and change that can be both continuous and discontinuous. The mechanisms described by Piaget: centering and decentering, assimilation and accommodation, the dialectic of "quantitative

accumulations leading to qualitative jumps," even models in different fields such as Eldredge and Gould's (1972) "punctuated equilibria" in evolutionary biology, can be easily translated and conceptualized in the terms of Kuhn's theory.

Below is a general model of change I developed in the mid 1980's following an exchange with Stanley Milgram on the transition from autonomy to obedience to authority. The model assumes that the system is evolving, becoming more complex over time.

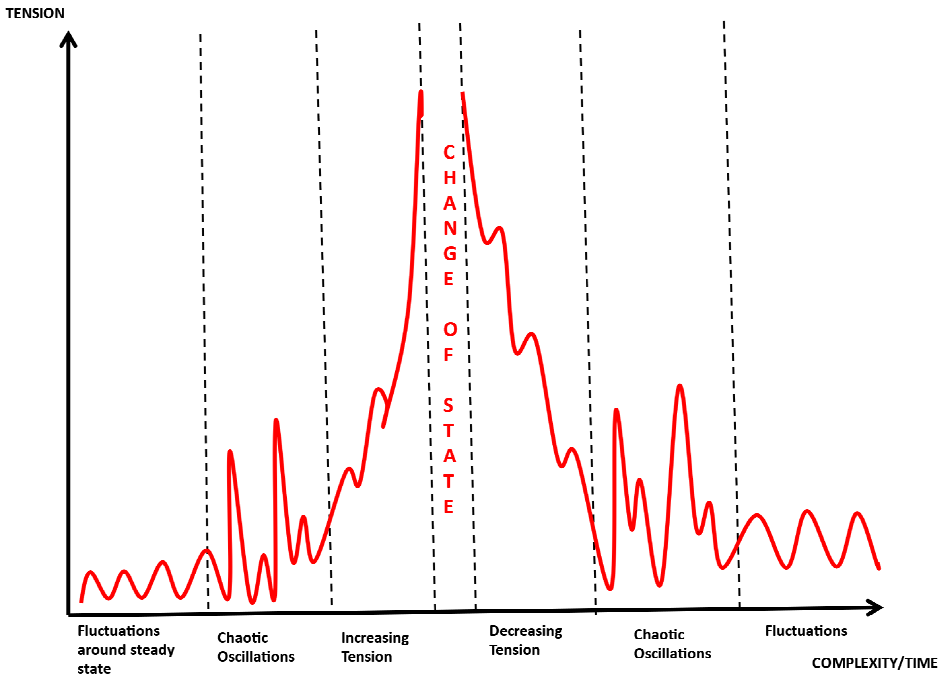

Figure 1. A general model of change

Equation of Increasing tension: T1= FC2/K1FS1 – (FC2/FS1-R),
where T1-tension in system at moment; K1- Inertia or resistance to change;
FS1-Forces of status quo R-relief from strain defenses can provide;
FC2- force trying to change the system to state 2.
If forces of Status Quo (FS1) and Inertia (K1) are high T1 will decrease, and the system will remain stable.
If FC2 increases T1 increases and the system enters chaotic oscillations, then changes to state 2;
T2a= FC1/K2FS2 – (FC1/FS2 –R) Equation of decreasing tension,
then: T2b= FC3/K2FS2 – (FC3/FS2 –R) Equation of Increasing tension possibly leading to state 3.

In retrospect, Kuhn's influence seems obvious, but to the best of my recollection, I was not aware of it at the time. I had read Kuhn's work, and I had internalized it, to the degree that it had become implicit to my way of thinking; I suspect this is the case with many researchers in various fields. I believe this to be the ultimate measure of success for a theory.

Paradigm and Symbolic Universe

In 1967, five years after the publication of *The Structure of Scientific Revolutions*, Berger and Luckmann published *The Social Construction of Reality: A Treatise in the Sociology of Knowledge*, which proposed an entirely new perspective on the development and ubiquity of shared frames of reference.

We evolved in small groups of hunter-gatherers and eusociality (Wilson 2012) is the key to our evolutionary success. The necessary social cohesion within the group was enhanced by the emergence of a common frame of reference. The development of language allowed the sharing the mental imagery, which in turn lead to the emergence of storytelling and to the invention of myths of creation. Myths played an essential role in structuring the universe into realms, (this world and the spirit world, this realm and the one beyond) and in the emergence of ideas about transcendence, mortality, and immortality.

The different aspects of reality were integrated by incorporation in the same overarching universe of meaning, which Berger and Luckmann (1967) named the Symbolic Universe – "which constitutes the universe in the literal sense of the word because all human experience can now be conceived as taking place within it...the entire historic society and the entire biography of the individual are seen as taking place within this universe" (Berger and Luckmann 1967, 62). This socially constructed reality is subjectively experienced as objective reality. "The reality of everyday life is taken for granted *as* reality. It does not require additional verification over and beyond its simple presence. It is simply *there*, as self-evident and compelling facticity." (Berger and Luckmann 1967, 23)

The Symbolic Universe is sustained by universe maintenance mechanisms, which act as safeguards against dissonance and ensure its internal consistency and continuity. Culture, theology, philosophy and

science are all universe maintenance mechanisms. The figure below illustrates "the relationship between the Symbolic Universe and different levels of social organization from individual to national. At each level, the Symbolic Universe is the interpretive framework and the conveyor of meaning for all individual or collective actions" (Dan 2015, 5).

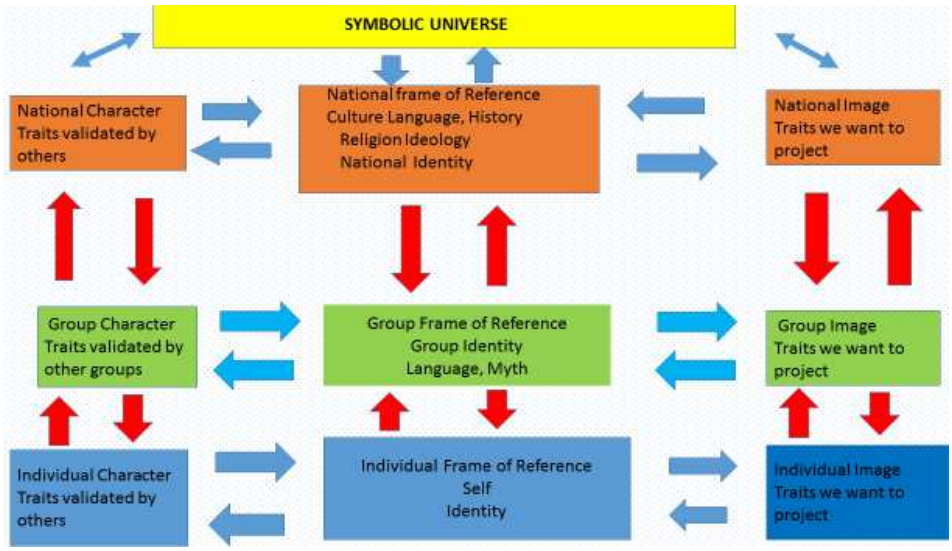

Figure 2. Individual, group and national frames of reference and the Symbolic Universe (Dan 2015)

The similarities and differences between Kuhn's concept of paradigm and Berger and Luckmann's Symbolic Universe were noted and debated in the literature. For example, Schutz (1973) and Vogel (2011) adopted an institutionalist-phenomenological framework for their critiques based on the "basic distinction between the external world and constructed reality. The external world is the world of objects which exists independently from human perceptions, and which is the ultimate ground of all experiences" (Vogel 2011, 87).

I intend to follow a different approach, trying to apply Kuhn's theory of paradigm shift to the changing of the Symbolic Universe. Both Kuhn's paradigm and Berger and Luckmann's Symbolic Universe are overarching interpretive structures which provide context and meaning to the events taking place within, and which are themselves changing as a result of

the outcome of those events. The Symbolic Universe contains the paradigms of the universe maintenance mechanisms, any of which could undergo a shift. A paradigm shift in one science, for instance the transition from the Ptolemaic geocentric model of the solar system to the Copernican heliocentric one in cosmology, Darwin's theory of evolution. Einstein's Theory of Relativity or Quantum Physics, which have all changed the paradigms of their respective fields, may have limited impact on the paradigms in other fields. This is not true for the changes of the Symbolic Universe. Science, religion, culture, philosophy are universe maintenance mechanisms, each containing several fields, each with their own paradigms. The system has some tolerance for the contradictions between or within universe maintenance mechanisms. For example, in the Symbolic Universe of the Western world different religions coexist without much tension. Within narrower geographical and cultural boundaries, Mungiu-Pippidi (1999) has shown that ethnic Romanian and Hungarian inhabitants of Transylvania hold distinctly different collective memories about their history, and Morar (2011) found that Saxons and Romanians from the same Transylvanian village had different approaches to morals: deontological and teleological respectively.

The periods of stability and the periods of change have widely differing dynamics. The periods of stability are characterized by the maintenance of the status quo:

> "the members of a scientific community, once they are committed to the paradigm, are unburdened from the need to justify which problems they select and how they solve them. What counts for a scientific problem and for its adequate solution is predefined by the paradigm. The reality scientists work in appears to them as self-evident and, in this sense, as unproblematic… With their solution, scientists engage in the confirmation of the paradigm and thus in its reproduction. These self-legitimizing forces of social reality signify the institutionalist elements in Kuhn's approach." (Vogel 2011, 85)

Likewise, during periods of stability, most of the Symbolic Universe is not even in awareness and is subjectively experienced as weltanschauung and the individual perception of the social contract. The maintenance

mechanisms of the Symbolic Universe, namely Culture, Religion, Law, Ethics, History etc., are getting reflected at the national, group and individual levels, resulting in a shared perception of the social contract and of moral expectations. At each level, morality is the filter through which permissible or unacceptable actions are judged. There are multiple interactions between levels. For example, as Morar notes (2023, personal communication) the highest level of Kohlberg's moral development at the individual level, corresponds to Kant's moral imperative at the universe maintenance mechanism level. The perception of the social contract, mediated by morality, is organized into "partial equivalency structures" (Wallace 1970) in which behaviors are connected in a predictable sequence. The potential actions are modified by an Overton Window, trying to decrease cognitive dissonance and to increase stability (See Figure 3).

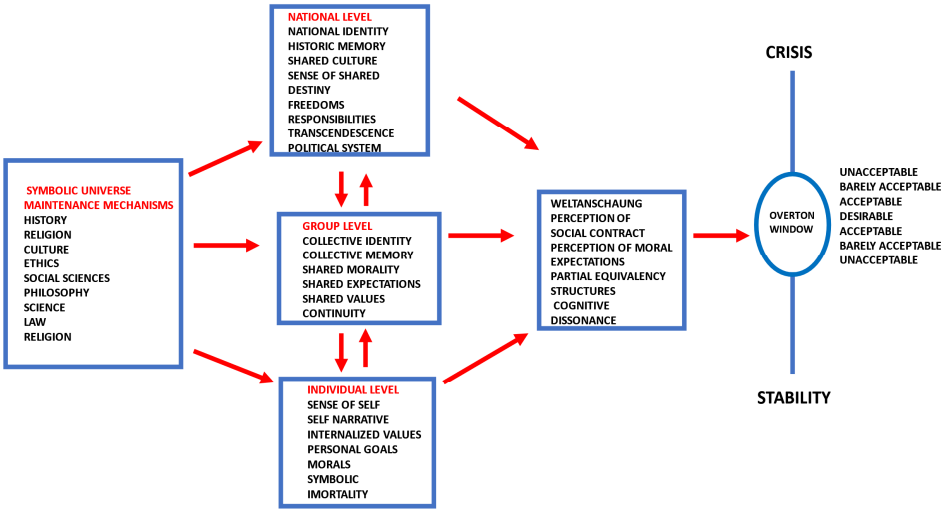

Figure 3. The role of morality in mediating the perception of the social contract

However, during periods of crisis or scientific revolution, the paradigm and the Symbolic Universe function very differently. Paradigm shift may be accompanied by scholarly disputes, most of them taking place outside of public awareness due to their esoteric nature, while changes in the Symbolic Universe, caused by historical events such as the spread

of Christianity, the discovery and colonization of the Americas, the Meiji period in Japan, the rise of Communism and Fascism, the advent of the internet and the phenomenon of the social media are usually periods of great upheaval. Kuhn's paradigm shift is triggered by the fact that solutions to the essential "puzzles" solved by scientists challenge the paradigm rather than reinforcing it, forcing changes in the "explanatory matrix". On the other hand, changes in the Symbolic Universe are preceded and precipitated by historical events and followed by prolonged periods of transformations and instability. For example, the conquest and colonization of North America brought Europeans in contact with native Americans. A number of treatises on the "noble savage," on "primitive naifs leaving in hunter-gatherer societies," and on "the role of working the land as a basis for ownership" were produced as a demonstration of European superiority and as a justification for the displacement and the stealing of the property of indigenous people. After the publication in 1703 of the popular "Curious Dialogues with a Savage of Good Sense Who Has Traveled" describing the discussions between the author, Baron de Lahontan and Wendat Chief Kondiaronk, European thinkers were confronted with the reality that they were not dealing with savages, but with an egalitarian society of sophisticated individuals, and started focusing on egalitarianism. Kondiaronk's penetrating critique of the materialistic European society influenced the thinking of Rousseau, which, in turn, had a major influence on the French Revolution, triggering another significant change of the Symbolic Universe.

While during paradigm shift the disputes between scientists are mostly civil, conflicts from other universe maintenance mechanisms may create distortions. The classic example would be the conflict between science and religion, and between politics and science. For example, relativistic physics were repudiated in Nazi Germany as "Jewish physics", and Lysenko's pseudoscience was elevated in the Soviet Union to the level of state scientific position, to be contradicted at one's own risk.

The Symbolic Universe guards against instability by using defense mechanisms. "Deviants" – those whose definitions of reality do not fit the Symbolic Universe – are dealt with by either inclusion or nihilation using conceptual machinery to "liquidate conceptually everything outside… the (symbolic) universe." (Berger and Luckmann 1967, 96)

Nihilation is most often used against individuals or groups that no longer belong to society which assigns them an "inferior ontological status." This leads to a chilling conclusion: "whether one… goes on to liquidate physically what one has liquidated conceptually is a practical question of policy" (Berger and Luckmann 1966, 97). Changes in the Symbolic Universe contain the implicit threat of violence.

I believe that the current period has the hallmarks of a period of instability of the Symbolic Universe. The social and moral explanatory frameworks seem unable to provide an adequate context for the integration of events. The causal factors of the instability are the rise of (mostly right) populist politics, the advent of post-truth society, the rise of distrust in institutions, the social effects of the pandemic, the proliferation of conspiracy theories, with their ensuing effects of increased nationalism, xenophobia, fragmentation, tribalism, and increased polarization.

The "controlling idea" (Lifton 1989) of right-wing populism is not economics but identity. When identity is seen as being under attack, a psychological state of "totalism" (Lifton 1989) ensues, leading to the rigid emphasizing of differences and the diminution of perceived similarities. The language of the totalist environment is characterized by the thought-terminating cliché.

> "The most far-reaching and complex of human problems are compressed into brief, highly reductive, definitive-sounding phrases, easily memorized and easily expressed. These become the start and finish of any ideological analysis." (Lifton 1989, 429)

The "Great Replacement Theory, the "invasion" by outsiders who will "replace us" and the "destruction of our culture" by them are such thought stopping clichés. Tajfel & Turner (1986) defined social identity threat as a reaction to the perception that one's group is evaluated negatively. Such threats induce "resentment and cognitive dissonance." (Tajfel & Turner 1986). The unpopular immigration policies and refugee crises, the terror attacks, the economic disenfranchisement of the middle class, especially in areas subject to postindustrial desertification (Guilluy 2014) and the effects of the current pandemic have resulted in "social fragility" which generates nativism, xenophobia and racism.

The fact that we live in post-truth societies greatly facilitates the dissemination of the populist right's message. Post truth society is an almost fact free environment. In the absence of a criterion for truth, there is no moral penalty for lying. One of the effects is the defusing of the mechanisms that inhibit social behavior, namely shame and guilt. Widespread toxic shamelessness allows for the open proclaiming of blatant untruth.

A consequence of the increased use of social media is the creation of self-reinforcing targeted information, the result of the algorithms used to keep users connected. Pariser (2011) defined the "filter bubble": a personalized web search algorithm which exposes the user only to information consistent with the previous search history. In Pariser's view this makes people more vulnerable to "propaganda and manipulation" since people are iteratively subjected to information that they have selected and that they already know. In effect, it is "invisible auto-propaganda, indoctrinating us with our own ideas" (Pariser 2011). A second, psychological bubble is created by our tendency to seek out confirmation rather than information, once we decide that we are in favor of a given position. This process is iterative: the previous output-the change in attitudes and beliefs – becomes the input for the next cycle. This psychological bubble complements the filter bubble created by search algorithms: the two processes reinforce each other.

Kahan, Jenkins-Smith and Braham (2010) in their discussion of "Cultural Cognition" have identified the process of "narrative framing": "Individuals tend to assimilate information by fitting it to preexisting narrative templates or schemes that invest the information with meaning" (Kahan et al 2010, 3). This means that once one has accepted the premises of populist ideology, a permanent narrative framing bias will distort the way new facts are integrated, force-fitting them into the existing storylines. In turn, this facilitates the reinforcement of conspiracy theories.

Several mechanisms contribute to the crystallization of a self-consistent alternative worldview: confirmation bias, narrative framing, willful ignorance (Proctor 2008), crank magnetism (the tendency to hold simultaneously, without cognitive dissonance, several irrational, absurd, unrelated beliefs) obsessive apophenia (the tendency to find patterns where none exist) and collective narcissism (De Zavala 2009).

The above distortion devices allow the person to "reality shop" – to select the version of reality which confirms pre-existing biases. In addition, the emotional state of acedia (restlessness, boredom, numbing, alienation) amplified by the social isolation due to the pandemic, increased the need for subscribing to an explanatory and motivating ideology. The internal consistency of this shared reality construct is enhanced by the creation of a filter bubble, by use of cognitive framing, by modifying the internalized moral code, and by the manipulation of the collective identity and memory to reduce cognitive dissonance.

Westen *et. al* (2006) using neuroimaging compared the functioning of subjects asked to make a decision after being confronted with facts contrary to their political beliefs, and concluded that they manipulated the data in order to get a confirmation of their pre-existing beliefs rather than analyzing the facts. In addition, apophenia provides an additional impetus. The act of discovering a hidden pattern is empowering and gives those "in the know" an illusion of control and superiority. It matters little if the discovered facts are true; in a post truth society dominated by information bubbles, the emotional factor dominates. Confirmation of one's beliefs is rewarded by the pleasure center of the brain with doses of endorphins. This is what makes apophenia addictive, and a significant factor in the development of conspiracy theories.

After being generated, the conspiracy theories are spreading similarly to an epidemic: the memes embedded in social values and in cultural messages are the equivalent of viruses, and, once received, create in the host the distorted thought patterns which form the conspiracy theory.

Crank magnetism provides an illusion of internal consistency by facilitating the merging of several unrelated conspiracy theories, and allowing people who subscribe to them to "buy in". For example, conspiracy theories about the origins of the pandemic, anti-vaccination theories and paranoid fears about government control merge into the single theory that the virus was created on purpose by China and the vaccine is a way of spreading the disease. At the same time, Bill Gates included microchips in the vaccine, which can be activated via the G5 networks, allowing for government mind control. The degree to which our acceptance of deviancy has changed is illustrated by the fact that if someone had asserted the above theory 10 years ago, they would have been referred for a mental status examination. The same is true for the

Qanon conspiracy theory which holds that the top Democrats are cannibalistic pedophiles, killing children to extract an elixir of youth. The parallels with blood libel are obvious.

Conspiracy theories are impervious to logical or moral arguments, having become a defining factor of the identity of those who hold them. They are "corrosive facts." A corrosive fact is not only fake. It has a destructive effect on the truth. Corrosive facts cause stress but once amalgamated into conspiracy theories, can also alleviate stress, for example by scapegoating. The facts organized in conspiracy theories play the role of myths of origin and are the basis of generating a new Collective Memory and Identity. Once the individual finds a group that shares his beliefs the process becomes irreversible. Collective Memory and Identity are consolidated by Narrative Framing, willful ignorance (Proctor 2008), Crank Magnetism, Apophenia, into a belief system similar to a religion or an ideology. An alternate reality is created. Collective Narcissism (De Zavala 2009) accentuates the polarization, increasing the distance between groups.

I believe that these conspiracy theories, which continue to proliferate, are the equivalent of "anomalies" in Kuhn's theory of paradigm shift. An anomaly is defined as "a violation of the paradigm-induced expectations that govern normal science" (Kuhn 1970, 52-65) Conspiracy theories are also "incommensurable" (in Kuhn's sense) with the symbolic universe, because a common frame of reference cannot exist. Their versions of reality are mutually exclusive, resulting in divergent and conflicting worldviews. The incommensurability is at the same time semantic – "(it is) possible for scientists to make and understand certain new statements *only after* a particular theory had been introduced (in the older vocabulary the new sentences are nonsensical)" (Oberheim & Hoyningen-Huene 2018, 2), taxonomical – "it only becomes possible for historians to understand certain older statements by setting aside current conceptions that otherwise cause distortion" (2018, 2) – and methodological – "there is no common measure between successive scientific theories, in the sense that theory comparison is sometimes a matter of weighing historically developing values" (2018, 2). The process is iterative: the incoming information if filtered according to pre-existing biases, pushing the system towards change. The filtering process is performed by a "Swiss Cheese Filter" (Reason 1990) as illustrated below:

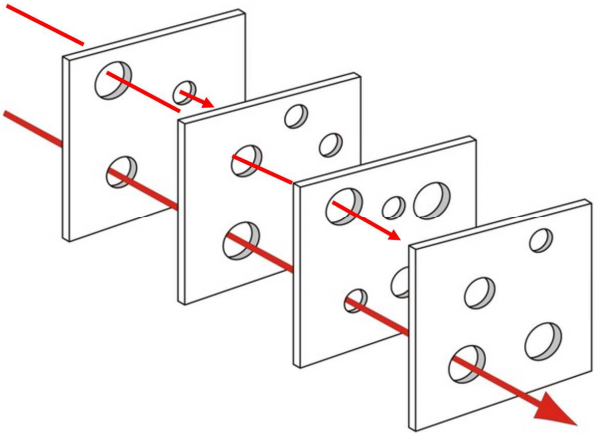

Figure 4. Swiss Cheese Filter (Reason 1990)
Source: Swiss cheese model.svg

The holes can represent rational analysis gates or cognitive bias gates. The filter can be used to eliminate fake and corrosive facts or to eliminate facts and select fake and corrosive facts consistent with one's biases, diminishing cognitive dissonance. In both situations, the illusion of objectivity and rationality are maintained. The cumulative effect of systematic filtering bias is shown below:

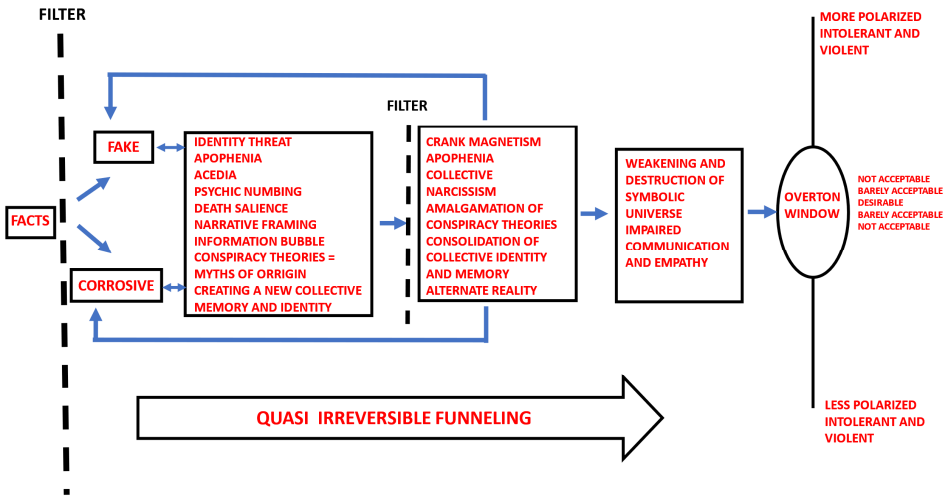

Figure 5. The cumulative effect of fake and corrosive facts

When the Symbolic Universe's maintenance mechanism such as science, theology, philosophy, become unable to provide a satisfactory explanation of the events taking place, the Symbolic Universe is forced to change. The issue we are trying to answer is whether the present trend to tribalism and fragmentation results in the de facto disintegration of the Symbolic Universe and its replacement by a vague, diffuse social media-based worldview. The acceptance of this creed/ideology implies a surrender of autonomy, similar to Milgram's (1969) "agentic state," freeing the individual from the restraints of personal responsibility. The diffuse, internet-based nature of the emerging belief system makes it accessible anywhere and facilitates acts of stochastic terrorism (Dan 2020). The social consequences of these developments are represented in *Figure 7*.

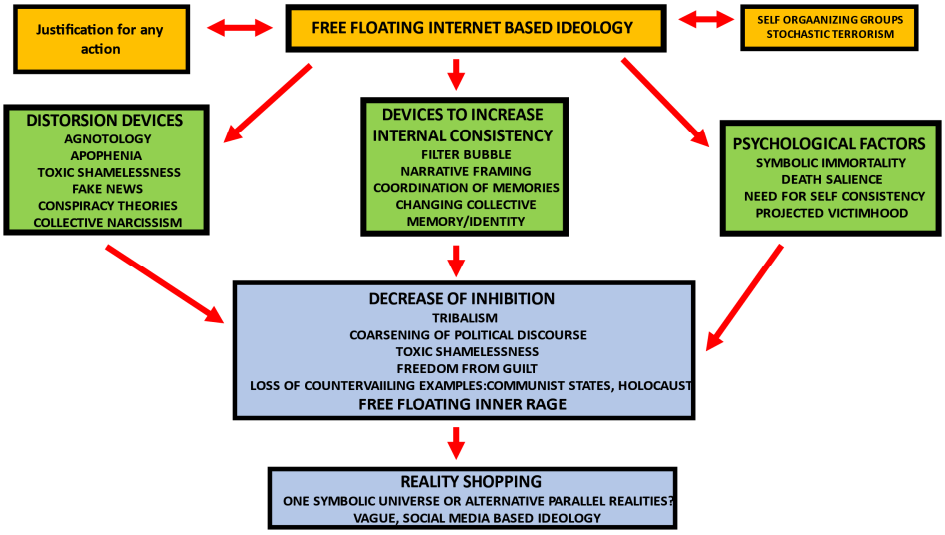

Figure 6. The decay of the Symbolic Universe

Reality shopping leads to fragmentation and the emergence of subuniverses. There is a degree of overlap with the all-encompassing Symbolic Universe in some areas (physics, chemistry, mechanics etc.) but not in areas which are important to the maintenance of each sub-universe (history, religion, morality, personal freedoms, societal restrictions).

As Michta (2017) notes, the decline of Western values is not due to the rise of an alternative civilization or to economic decline, but to "a failure to reach consensus on shared goals and interests… the problem, rather, is the West's growing inability to agree on how it should be defined as a civilization. At the core of the deepening dysfunction in the West is the self-induced deconstruction of Western culture" (Michta 2017, 1).

Seen from a Kuhnian perspective, the deteriorating status of the present Symbolic Universe resembles an autoimmune disease. Certain universe maintenance mechanisms produce their own anomalies. The effect of the proliferation of conspiracy theories and memes on the Symbolic Universe is similar to that of infection by computer viruses. They permeate the universe's maintenance mechanisms such as science, morality, philosophy and alter them, weakening the social contract and fraying the support structure of underlying conventions by generating radically different versions of reality which are impervious to logical arguments – in other words, incommensurable – with the prevailing one. They work by mimicking the forces of paradigm change by creating false contradictions, generating disturbances (anomalies) that imply that the present paradigm is no longer able to provide an explanation of the changes and conflicts contained within it. This results in increased fragmentation and communication difficulties. In turn, this disrupts the "partial equivalency structures" which make actions unpredictable. As the sub-universes diverge, we are living more and more in separate realities. The only question is whether an emergent Symbolic Universe will foster a new, unifying sense of community or continued fragmentation. (An illustration of this process can be found in the *Appendix*.)

I believe that using Kuhn's concept of paradigm and his theory of paradigm shift proved useful, providing insights into the processes of the transformation and changing of the Symbolic Universe. The interactions between the paradigms of different universe maintenance mechanisms, as well as their relationship to the Symbolic Universe seems a fascinating subject which requires further analysis.

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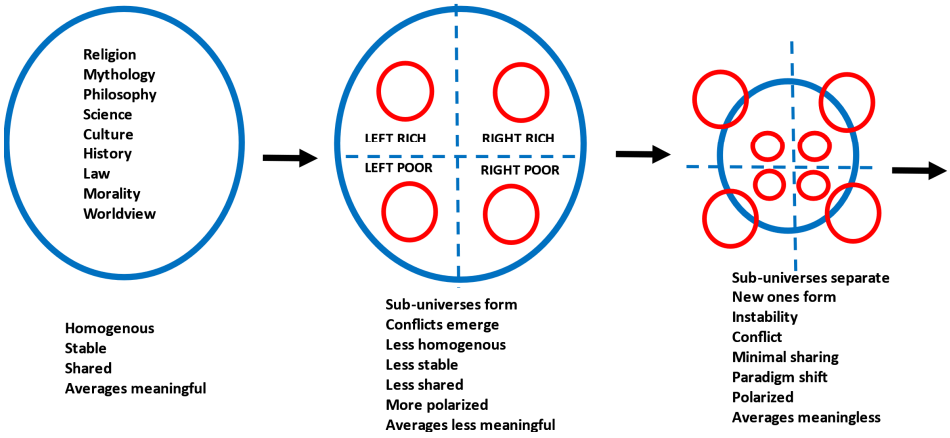
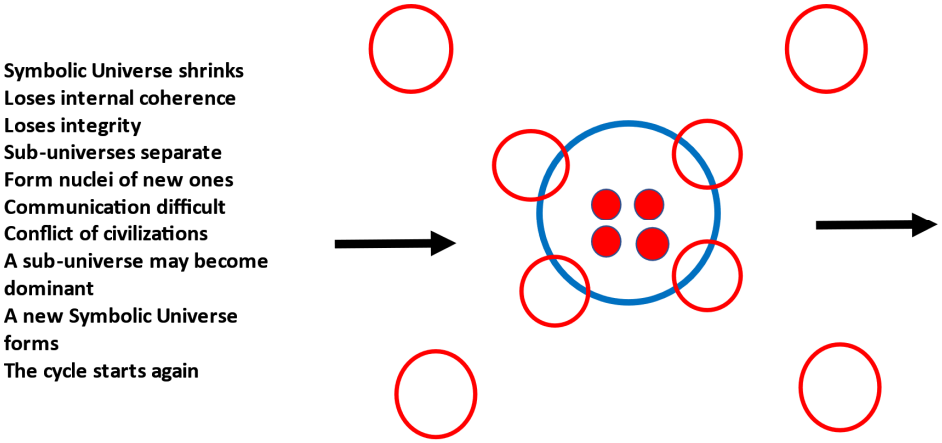
Appendix



Figures 7 and 8. The life cycle of the Symbolic Universe
(Dan 2023)



Figures 7 and 8. The life cycle of the Symbolic Universe Continued
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Oana ȘERBAN[1]

THE UNCOMFORTABLE KUHN. A REVOLUTIONARY READING OF DISBELONGING: TO WHAT DOMAIN SHOULD WE LEAVE THE KUHNIAN INHERITANCE?

K. BRADY WRAY, 2021, ***Kuhn's Intellectual Path. Charting "The Structure of Scientific Revolutions"***, Cambridge, Cambridge University Press, ISBN-10: 1316512177, ISBN-13: 978-1316512173

About Kuhn we have already read, in 60 years since the release of the *Structure of Scientific Revolutions*, critiques portraying him as a transgressor, a visionary, a reformist of the history and philosophy of science. But to achieve all this capital of notoriety and to raise a tradition by itself – for which many turned the partisanship for his convictions into a title of nobility, becoming "Kuhnians" – having a touch of genius is not enough: one's education is as important as one's innate talent. *Understanding Kuhn's Intellectual Path* is not only a curiosity, but also an exotic epistemic travel to different philosophical openings of his education, which influenced – contingently or decisively – his unique theory on the change of paradigms in the history of science. K. Brad Wray offers us intriguing insights on Kuhn's intellectual becoming in one of his recent volumes published by Cambridge University Press (2021).

[1] University of Bucharest, Faculty of Philosophy & CCIIF – The Research Center for the History and Circulation of Philosophical Ideas, https://orcid.org/0000-0003-1071-9222. E-mail: <oana.serban@filosofie.unibuc.ro>.

This is not a classical monography and has nothing to do with a biographical reconstruction. Wray's book fills in the gaps between the Harvard period of Kuhn (1947-1955), his influence on championing different movements from the sociology of scientific knowledge and his return to the history of science, marked by a committed interest in reconsidering historicism, and debates on the clash between realism and antirealism. These ages, gathered around the pre- and the post-*Structure* personal and intellectual history of Thomas Kuhn, reveal that the success of his bestseller was not a historical accident. Unlike Fuller, who strongly believes that Kuhnian audiences misunderstood Kuhn's notoriety and underestimated the way in which the *Structure* radically modelled our perspective on science and not always in the good way, Wray remains positive, preferring to tackle the reputation of Kuhn as a product of an intellectual pedigree in which different paradigms of thought educated his mind and tailored his beliefs on the nature of progress in science.

However, there are no reasons to feel envious of Kuhn: as Wray observed, he was late in accomplishing both his career and intellectual goals, having left his work unfinished, thus causing the public to continue to wonder about his last manuscript, which remains unpublished up to this day.

Unfortunately, Kuhn remained a dramatic figure, quite obsolete in the history of science, despite his efforts invested in raising departments of history of science. Rather the philosophy of science and the sociology of science recall his name and the impact of his thinking on such domains. Brad Wray is deeply seduced by the latter: Kuhn is portrayed in the pages of this book as responsible for challenging the knowledge produced by social sciences and recalled for realizing that "what the social sciences lacked, and what characterizes the natural sciences, are paradigms" (Wray 2021, 7). This key concept is invested in explaining both the epistemic consensus and the (im)predictability in the shift of mentalities, beliefs and values that shape and determine scientific revolutions. For my taste, Kuhn's originality has at its heart an invaluable contribution to what Kubler (1962) would call "the shape of time" or the progress of mankind: Kuhn persevered his whole life to understand if the nature of progress is cumulative or dialectical,

especially in sciences; if axiological commitments might influence the change of paradigms; if the exhaustion of a paradigm is part of the success or failure of a scientific revolution. Also, it is my conviction that the attempt to replicate the structure of scientific revolutions, by its Kuhnian recipe, in other domains, such as the history of arts, for example – explicitly, but not sufficiently tackled by Kuhn – might be a matter of ingenuity. In fact, the list for Kuhn's merits in the sociology and philosophy of science is inexhaustible, and so are his contributions in changing different practices in social sciences. But in this book, we have another Kuhn at stake: at first glimpse, an Aristotelian one.

The first part of Wray's book deals with the Aristotelian influence on Kuhn's thought, from his Harvard period, when he decided to write *The Structure of Scientific Revolutions*. Wray frames "an Aristotelian epiphany" (Wray 2021, 11), that influenced Kuhn's thought more than his readings on Galileo and Newton. Next to Aristotle, they were part of a historical branch of physicists, "or so Kuhn thought" (12). Aristotle is, in Kuhn's interpretation, an author of physics without authoring "a science of mechanics" (13). Ontological differences emerging from how Aristotle and Newton understood reality and material properties of bodies convinced Kuhn that "Aristotle had not been writing bad Newtonian physics but good Greek philosophy" (Heilbron 1998, 507; Wray 2021, 14), and this contrast might have produced his interest in *incommensurability*, one of the terms that *The Structure of Scientific Revolution* has at its heart.

Nonetheless, we shall not consider Kuhn an ideal Aristotelian, but rather an unsuccessful one, for at least two reasons expressed by Wray. One is that "Kuhn was not able to appreciate the integrity of Aristotelian worldview" (Wray 2021, 14) and the other one is that understanding the Aristotelian roots of the motion and the role played by it in different contexts was not an easy task for Kuhn (see Kuhn 1977, xii). But Aristotelian readings were an enlightening experience that influenced Kuhn's perspective on the nature of scientific revolutions.

On the one hand, Aristotle proved being capable of a very integrative worldview. On the other hand, reflecting on the Aristotelian *Physics* allowed Kuhn to draw some general insights on the nature of revolutionary changes, which, when applied in science, reveal how disruptive they can be, having arisen from the experiences encountered

during the research process. Consequently, "The Road since Structure" – an interview from 1995 – portrays Kuhn as being determined to write about such a topic immediately after he got contaminated by an Aristotelian perspective on progress and change. However, since his real engagement into ancient philosophy up until the moment when the manuscript of *SSR* became the book consecrating his thought, Thomas Kuhn was convinced that there were still things to learn, crucial to explain how anomalies tend to normalize through revolutionary changes. Wray highlights that there were many steps left from this ongoing process of crystalizing the theory on *SSR*, since the knowledge growing in natural sciences was not inscribed into a cyclical pattern of change. *Normal science* and *scientific revolutions* were blended into one theory only after Kuhn successfully managed to explain the link between the concept of *paradigm* and "the notion of mopping up", which was more or less "the bulk of scientific practice" and the key to understand "theoretical breakthroughs" (Wray 2021, 19-20). From this point on, Kuhn is influenced by an interdisciplinary approach on science, reading historians and sociologists that inspired him to critically undertake the genealogy and evolution of scientific communities. Fleck's writings concerned Kuhn since the concept of collective thought proved to be problematic in terms of the predictability embraced by scientific revolutions, but also in terms of raising and securing the authority of a paradigm. "A more complicated case" was, in Wray's opinion, Alexandre Koyré, with his *Études galiléennes*, which became a mandatory reference provided to his students and offered food for thoughts to conceive an internalist approach on the history of science. Diagnosticating Kuhn's originality is not an easy endeavour and yet Wray masters such analysis accurately, offering us a rare deconstruction of *SSR*.

To date, no one was interested, nor seduced, by the impact of Conant's writings on Kuhn. Wray devotes a full chapter to bridge the gaps between Conant's perspectives on natural science and Kuhn's theory. Nonetheless, despite the sincere admiration Kuhn carried for Conant, there are at least five contributions in *SSR* which were developed independently of this biographical link, with all the correspondent affinities: "the concept of paradigm", "the concept of normal science", "the problem of scientific revolutions", "the related concept of incommensurability",

"the emphasis on the social dimension of science" (Wray 2021, 26). On this topic I consider relevant the intersecting efforts of Fuller and Wray, developed by different means, to highlight the fact that without the Kuhnian interest on the social insights of scientific communities, the public would have remained immune to the dynamics of science and insensitive to the matter of progress. But Kuhn accommodated large audiences with the idea that as conservative and rigorous science might be, at the end of the day, science is shaped by social institutions, raising a capital of innovation which catalyses welfare and interdisciplinary knowledge. In fact, by adopting this perspective, Kuhn made Fleck notorious, recognizing in the opening of the *SSR* that without a glimpse on the sociology of the scientific community, the revolutionary practices in sciences would have been very difficult to observe. Wray adds that the influences of Toulmin and Polanyi on Kuhn's writings are equally remarkable and yet, one of the major outcomes of this book is represented by the argumentation in favour of separating the genealogy of major Kuhnian concepts and themes from *SSR* from these authors, rather grounding their origin and initial meaning in Conant's writings.

What we often overlook – even the most passionate Kuhnian readers – is that Kuhn rarely used "conceptual schemes", which are more likely to be found in *The Copernican Revolution*. However, in *SSR* the term is completely avoided, which makes Wray believe that although Conant influenced Kuhn methodologically, the analysis of science is mainly based on theoretical frameworks. Interesting is the following remark belonging to Wray: "like Conant, Kuhn claims that scientists spend much of their careers making nature fit into the conceptual boxes supplied by the accepted theory" (Wray 2021, 35). In my opinion, this aspect should raise questions on the risks of falsifying research or delaying progress and on coming up with revolutionary approaches, respectively. However, I am perfectly aware that conceptual schemes should also support the dominant holistic view embraced by Conant and Kuhn, and this might be the reason for which such a matter turns out to be less invasive or problematic. Wray also reflects on how Weber tailored Kuhn's taste for a holistic methodology inspired by social sciences, which made him consider regularities not as ends, but rather as means of knowledge (see Wray 2021, 36). But as we advance

further with Kuhn's attachment to sociology, we discover a historian of science who, like Conant, considers revolutionary practices to be determined by new theories: empiricism fails, in these terms, to provide a unique and exclusive origin for such progress.

Nonetheless, one of Wray's hypothesis drew my attention in particular: "In comparison to Conant, though, Kuhn was less insistent that revolutions are the sources of the greatest progress" (Wray 2021, 37). It seems that Wray places the narratives of progress at the heart of a more deconstructivist approach borrowed by Kuhn, to drop the canonized idea that scientific progress is *cumulative*. And yet it seems to me that the distinctions between the dialectical and the cumulative nature of progress are more relevant in post-*Structure* writings. For example, in "Comment on the Relations of Science and Art" where Kuhn (1977, 340-351) considers the hypothesis of applying the *SSR* in the field of the arts. On that occasion, he reflected on "the cumulative and disruptive character of art and science; the symptomatic, character of each discipline to structure its main topics and problems in the form of a puzzle; the rivalry between the following core-concepts paradigm, style, and theory" (Șerban 2022, 90-91). It might be true that progress was never a milestone for the Kuhnian thought, or that he never wrote challenged solely by the need to unveil the nature and dynamics of progress. But whenever he compares domains, science reveals itself following the cumulative path in the shift of paradigms, while arts retake and reshape contents of style by dialectical means. This is particularly why I believe that progress suddenly becomes one of the means that Kuhn had at his disposal to secure the particularities of science and to distinguish paradigms from Foucauldian episteme or Kublerian styles because the latter were behaving rather dialectical than cumulatively. Moreover, Wray denounces other misinterpretation of Kuhn's intentions: he never followed a Cartesian path, therefore, he never aimed to support a so-called *mathesis universalis*. A unified science was never a stake, neither for Conant, nor for Kuhn, but the explanation lies on the fact that the latter believed in the incommensurability "between theories in neighbouring specialities" (Wray 2021, 38; Kuhn 1991/2000a, 98). Implicitly, I consider that this position tailored a rather new age of modernity, in which *mathesis* was a dream left behind and where methodological holistic perspectives did not necessarily impose a Cartesian project of unified science.

In spite of everything, the obsession for method remains a dominant piece of this large puzzle of scientific revolutions, revisited by denunciating the collapse of "scientific method" and raising ambitions of privileging paradigms. Epistemic achievements rather than methods will guide the appetite of reflection that scientists invest in their research. This is the main reason for which Wray finds Kuhn responsible for "shifting emphasis away from scientific method" (Wray 2021, 40).

Three arguments provided in this book by Brad Wray seem to me truly relevant to understanding why *The Structure of Scientific Revolutions* was revolutionary by itself, all of them framed by the impact of Conant's writings on Kuhn's thought. First, *The Copernican Revolution* did not anticipate *SSR*. The former barrows Conant's vocabulary (41), whereas the latter deals with *paradigm shifts*, *normal science*, and *incommensurability*. In fact, the former implements Conant's conceptual scheme, the latter totally defies it. These differences equally stand for a change mirrored by the dynamics of Kuhn's mentality on the relevancy of psychological factors impacting matters of progress and revolutionary practices. *SSR* is more committed to embedding the role of values, behaviours and beliefs in tailoring scientific education and research. Secondly, there are separations between Conant and Kuhn, as the former advised the latter to restrict paradigms to the use of examples and models, not to theories. Wray states: "Conant was initially very uncomfortable with Kuhn's use of the concept when he read the draft manuscript of *Structure*" (43). Apparently, for Conant's taste, the concept of paradigm was too general, and it was no sooner than the 1970s when Kuhn restricted the term to determine something exemplary for a unified scientific discovery. At the same time, the concept of "paradigm" became canonical for normal science, and what was still on hold was the analysis of its capacity to embed the juxtaposition between a regular and a revolutionary form of progress. This contrast is relevant because it highlights Conant's orientation to track down paradigms as effects of radical conceptual innovations, while Kuhn would plea in favour of a more particular meaning, emerging from solving a puzzling problem by means of revolutionary science. Thirdly, if paradigms are delicate, scientific revolutions are even more subtle and difficult to approach. For Kuhn, they have at their heart epistemic problems stimulating progress. Wray recalls that "Kuhn's analysis

of progress through revolutions gave birth to the notion of Kuhn-loss, a key target of criticism raised by philosophers of science" (46). In short, Kuhn was more normatively oriented, while Conant remained skeptical or disinterested regarding this aspect. On top of everything, the contrast between Kuhn and Conant has been deepen by *incommensurability*, a concept coined by Kuhn, but totally absent from Conant's works. Either conceptual, methodological, or related to communitarian consensus, *incommensurability* became a key-term in Kuhn's model of structuring the scientific revolutions. The simple fact that social factors were tailoring *incommensurability* as much as scientific ones, turned Kuhn's theory into a very fashionable construct at that time. Science was, by this perspective, explained once again as a vector to propagate social phenomena, not only knowledge and epistemic traditions. Only for this, and *SSR* was worthy to be largely and suddenly consulted by outsiders of the scientific bubble, such as sociologists or historians of different domains.

The first part of Wray's volume ends with critical remarks on the impact of the Kuhnian legacy on the history of chemistry and the logical positivists. It seems to me that beyond particular reflections that Wray competently advances in regard to both domains, what they have in common is a sacrificed, marginal disciplinary history, along which Kuhn's influence on these domains was visible, as well as the other way-round. There is a discussion on how the Cold War culture influenced Kuhn's sensitivity to chemical innovations that could affect the quality of life, or to what extent the chemical implications of pigments and techniques of painting might have raised curiosities for him to explain the possibility to commute the model of scientific revolutions in the history of art, so as scholars are still discussing the role of logical positivism to strengthen Kuhn's caprice to write *SSR* as a book capable of synthesizing the image of science. But none of them seems to me more exotic than those passionate debates on what Wray presents in the second part of his book as "the unexpected uptake", meaning the impact of Kuhn on social-sciences.

Was Kuhn a trendsetter for the sociology of science? Definitely, he takes the credit for the popularity and authoritative rise of this domain in the 20th century. Before sociologists embrace Kuhn's writings, psychologists have enthusiastically declared their support for his authentic manner of discussing the structure of scientific revolutions. Not only because Kuhn

generously delved into the psychological analysis of Piaget and Gestalt scholars, but mostly because despite the lack of formal education in psychology, arbitrarily selecting relevant references to explain the playful psychological background of paradigm shifts and revolutionary practices, he managed to convince psychologists to pursue the *SSR* as "a contribution to their field" (85). On the side of sociologists, it seems that Kuhn's legacy was made responsible to fuel their domain with arguments to track down social sciences as capable to behave as scientifically as "natural sciences" (90). Many echoes revealed the revolutionary potential of Kuhn's bestseller: Wray recalls President Truman's speech from 1965 insisting on the need to operate political paradigms with precision, and President Almond's insistence to recognize the innate capacity of political sciences to operate paradigms. Both leaders of the American Political Sciences Association, their discourses are just two pieces of examples that synthesize the Kuhn-effect on domains to which no other historian of science reached, in his century.

Nonetheless, as Kuhn's arguments were more fashionable, the public reflection on the main differences between natural and social sciences became broader and more tensioned. One of the most relevant disputes on this topic was that between Charles Taylor and Thomas Kuhn, which at first glimpse was inspired by methodological differences between the two scholars. The former insisted on the hermeneutical capacity of social sciences, which is less tackled in natural sciences, while the latter was convinced that their rivalry – if such scenario is plausible – is that social sciences operate more unstable objects than natural sciences. It is the main reason for which "the heavens remained the same" (Kuhn 1991/2000b, 223) from Greek to Copernican astronomy, which we cannot say about political and social systems (Wray 2021, 91).

However, beyond local and global disputes, the seed of the Kuhnian thought in social sciences flourished by upgrading the social scientific paradigms. Wray observes that Kuhn becomes fashionable in social sciences without taking any explicit credit for his renewed concept of paradigm: "there is no mention of the specifics of Kuhn view" (93), although Marcionis, for example, splits sociology into three main historical paradigms: the structural-functional paradigm, of Durkheimian origin; the social-conflict paradigm, which was entirely Marxist; and the symbolic-interaction paradigm, decisively Weberian (Marcionis 1997, 16-22).

What seems relevant to me is how we can link in these dishonest conditions Kuhn's name with social sciences and to what extent the natural use of paradigms in social sciences, without giving credit to their author, might affect the accurate analysis of Kuhn's successful and unfinished project, to commute the structure of scientific revolution in other domains, such as the history of art or sociology. Wray offers a precious perspective, arguing that "when sociologists discuss paradigms it is now quite common for Kuhn not to be cited at all" (94), a phenomenon targeted as "obliteration by incorporation", using Merton's formula (Merton 1988, 621). What we find out after surfing generous examples grasped from anthropology, political science, and economy, is that Kuhn's interpretation on paradigm became normalized. Or, to express it more aesthetically, he became the victim of his own way of interpreting normalizing practices of sciences. A paradigm is part of the regular discourse of a normal science. Is there any exception to this pattern? Apparently, there is one, but even more dissatisfying, and from one point onward, quite toxic, given the social impact. Wray critically undertakes Walker's argument that following paradigms, political scientist will "engage in hostile zero-sum turf war" (2010, 434) and will focus on explaining those occurrences when a revolutionary paradigm does not substitute a dominant one, but rather develops an alternative theory or a subfield of research. Exotic examples arise from Walker's analysis: "hyper-specialized tribalism within subfields and furthers the Balkanization of political science as discipline" (Walker 2010, 434). Wray explains that this standpoint is too virulent and a little bit anti-Kuhnian. In fact, Kuhn foresaw the fact that new scientific specialties will emerge not from revolutionary interpretations, but from reframing of different problems correspondent to a valid, normal and dominant paradigm. On the contrary, it was a proof of success, not a piece of tribalism and balkanization and, to be more precise, Kuhn would say it is quite desirable to assist to such forms of transition and innovation. For Wray (96-97) the eccentric point of view of Walker is worthy to be considered just to answer to how many paradigms can coexist in a social scientific field, how many specialties are valid at once and how are these new specialties or fields of expertise arbitrate the competitivity and incompatibility of paradigms.

For my taste, this problem should be also framed as a possibility to understand new fields of expertise embed the notion of predictability of changes, based on their receptivity on paradigms and puzzling problems. But Wray takes this framework as an excellent opportunity to discuss the undiscussable, "the elephant in the room": the limits and authority of the sociology of scientific knowledge. A cocktail of events and perspectives present Kuhn as deeply engaged into the sociology of science. First, his interest into Merton's theories on priorities in scientific research and discovery set up a powerful background for analysing the culture of science. The simple fact that Merton recognized Kuhn's particular reading of sociological processes as impacting historical development make us wonder not only what his influence was on reshaping the destiny of research communities, but also to reflect on how sociological the concept of structure was emphasized at that time. Kuhn has the merit of having deconstructed the multiple phases that a research community undergoes by social changes, transgressing cultural challenges, (un)popular mentalities and contractual forms of agreement, consensus, and quantified progress. Whenever we deal with a scientific crisis, there is always a social explanation as well that stands for that impasse. If scientific education stimulates progress, then socialization is part of it. Moreover, Kuhn is deeply seduced by effects of this professional cohesion: the raising and strengthening of consensus paves the way to normal science and its unproblematic uses. The rise of anomalies might be, from my standpoint, a matter of sociological deviance, but Kuhn remains loyal to matters of intellectual commitments and interests and economic negotiations that scholars tend to practice in order to predict and control a paradigm. Wray adds to these elements some other ingenious insights that justify the sociological turn of Kuhn: his criticised, and yet, intriguing *Strong Programme in the Sociology of Scientific Knowledge*; his perspectives on the moral responsibility of scientists for progress; as well as his concerns for the methods invested in measuring the level of satisfaction that scientists reach in achieving epistemic goals.

The third part of Wray's volume is consecrated to the relationship between the Kuhnian philosophy and the history of science, which is complex and inexhaustible, but brings to the spotlight the fact that surprisingly, "contemporary historians have a rather dismal assessment of *Structure* as a contribution to the history of science", an assessment

quite irrelevant, as it Kuhn's book "was not intended to be a contribution in the history of science" (Wray 2021, 119). This chapter represents a valuable contribution to the attempts of mapping what we might call *a Kuhnian ethos*. Wray is a master of linking shifts in Kuhnian scientific interests and levels of notoriety with the mental geography that emplaces his ideas. Copenhagen, Harvard, Princeton, Berkeley are tackled as places for making Kuhn's ideas either revolutionary or unpopular. Wray portraits a Kuhn who navigates through multiple academic traditions, which provided him different senses of belonging, and becomes convinced that "there was something that happened to people who'd spent too much time around Harvard" (Kuhn 1997/2000a, 28). And here is how we reach the Kuhnian paradox: being "never at home in any discipline", although he impacted all disciplines at once through his *Structure*. In short, Wray highlights the lack of narrow philosophical education that Kuhn resented from one point onward, but what strikes me is this arguable dichotomy, that I have never given attention previously: "Kuhn did not think one could work as a historian and a philosopher at the same time" (Wray 2021, 134). Should we tackle Kuhn's legacy enlightened by this exclusive disjunction that attests that one could be either a historian of philosophy or a philosopher, but never both at the same time? How many philosophers, reading this statement, nowadays, feel their careers shadowed by such discretionary approach?

Wray prefers to remain silent on this topic – there is no explicit sign that he would have been interested to dismantle different answers to this pivotal question; however, implicitly, it seems to me that the major, untouched topic here is: to what extent choosing both would alter the meaning of normal science for a historian of philosophy, respectively for a philosopher? Until we ever get a reaction on this, we find Wray's arguments that the *Structure* was not decisive for the history of science, regardless of its popularity in this field, that "insofar the book is a contribution to the philosophy of science, the sort of thing Kuhn means by structure is perfectly respectable, and is often presupposed in many philosophical studies", everything in order "to defend Kuhn against the charge of historicism" (136). By reflecting on matters of *Structure* and structures in philosophy of science, what it seems to me is that Wray succeeds in reconstructing genealogically the Kuhnian paradigm by tackling limits of its archaeological application. If we look to Kuhn's legacy

through the lenses of scientific change, then I agree, there is nothing new. I equally find more than honourable and accurate Wray's intention to save Kuhn from a Popperian form of *historicism*, that he has never authored. Nonetheless, I still question myself if this is not a very restrictive manner of devised to puzzle historicism, one which very conveniently isolate Kuhn's work from Popperian amendments. What if we leave aside Bird's deconstructivist approach on historicism, which made him consider Kuhn as a practitioner of a conservative historicism (see Bird 2015, Wray 2021, 150) and we implement a more Foucauldian approach?

To fully understand the role of Kuhn on the sociology of knowledge, that Wray seems to defend, I think we should take the risk of considering the structure of a scientific revolution influenced by the power-knowledge relationship that a discourse embeds, and through which is capable to react to standards of "normality" and "truth". It would be more useful, I guess, to arrest this Foucauldian perspective, since on the one hand is generous in setting affinities and discontinuities between *paradigms* and *epistemes*, which have been unrightfully overlapped, and on the other hand is more attentive to shed light on an anti-historicism that both Kuhn and Foucault might share, but which is tolerant with the so-called "historical *a priori*", that blends categories of time, place and culture, to alter paradigms and impose the urgency of a scientific revolution.

This link would also stimulate the reflections on Kuhn's philosophical legacy, that Wray discusses in the fourth part of his book. For a former non-philosopher, Kuhn succeeded in "setting the agenda with the problem of theory change, a consequence of reflecting on the nature of scientific revolutions" (153). In my opinion, Wray's chapter equips philosophers with all the necessary concepts and methods to answer to questions still fashionable for scholars working in this field: "do Kuhnians have to be anti-realists?" (Dimitrakos 2023) Is there any anti-realism that we might rebuild as an autonomous tradition, "from Kuhn to Foucault"? (Gordon 2012) How can we resist scientific realism (Wray 2018) and to what extent this is a worthy discussion to be carried out nowadays?

At the end of this book, we discover another Kuhn: one on which scholars remained silenced, because it is not a comfortable endeavour to bridge biographies and ideas behind well-reputed figures of the intellectual history of our last century. In the case of Kuhn, this difficulty is doubled by his own affinities and curiosities for topics that go beyond and

behind the history of science, which render this reconstruction as transgressed not only by an epistemic altruism, at different ages, but also by forms of scientific rigor, scepticism or creativity. If one is not a Kuhnian, after reading Wray's book will consider becoming one. Wray has a particular manner of overcoming a hermeneutical prudence to leave the biography of an intellectual immune or unlinked to the layer of intellectual positions; reading this book makes you wonder if this stubbornness of keeping the ideas as "clean" as possible, away from biographical occurrences, is not counterproductive; if what we miss, in our most competitive educational paradigms, regardless the academic traditions behind them, is particularly this ability to get engaged into the history of ideas and to use the intellectual history as a path to reach the heart of philosophical debates. "Charting *The Structure of Scientific Revolutions*" means to map cultural mentalities and beliefs of scientific communities that welcomed or rejected the Kuhnian inheritance with equal plausibility and rigor. I wholeheartedly recommend this book to those who would like to understand the impact of Kuhnian works in the field of the sociology of knowledge and social sciences in general, for at least two reasons. One is quite selfish: it supports my own reading on Kuhn's role in shaping the notion of progress in these domains but delivers new insights and arguments on Kuhn's preferences for certain standpoints and beliefs. The other one is quite altruistic: because it is time to raise the awareness of researchers and specialists from different fields that interdisciplinarity, which sets the trends in academic research and founding nowadays, depends on understanding paradigm shifts, the incommensurability of scientific discourses, or the relationship between scientific problems shared by different domains and the history behind them. No one can really be a master on that, lacking a particular knowledge of Kuhn's works and the intellectual paths that guided them.

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