

Thodoris DIMITRAKOS¹

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 falsificationism². 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 aspects³ and those that underline the neo-Kantian elements⁴ 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 history⁵. 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.

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 Feyerabend⁷ and of the Strong Programme in the

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 Fraassen'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 truth⁸ 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

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 theories¹⁰. 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 here¹¹ – 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 research¹³. 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.

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 rational¹⁵ 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 strand¹⁷ of [Kuhn’s] historicism’ and he defines it as follows: “In the broadest terms,

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)

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-presentist²⁰. 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

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 history²². 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 interpretations²³. 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 vocabulary²⁴. Showing how a belief modification conforms to a set of norms is not enough. Providing a thorough understanding of the

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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