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The ‘Aristotle Experience’ Revisited: Thomas Kuhn Meets Ludwik Fleck on the Road to *Structure*

<https://doi.org/10.1515/agph-2020-0160>

Abstract: This article takes issue with Kuhn’s description of the ‘Aristotle experience,’ an event that took place in 1947 and that he retrospectively characterized as a revelation that instantly delivered to him the key concepts of *The Structure of Scientific Revolutions* (1962). We trace a certain transformation of this narrative over time: whereas it commenced from a description of his impression of disparity between the textbook image of science and the study of historical sources, Kuhn started to characterize it as a revelation after learning of the English translation of Fleck’s 1935 *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*. This book anticipates many central Kuhnian claims. Kuhn read it as early as 1949, but never fully acknowledged it as a source of inspiration. We discuss four hypotheses concerning the possible influence of Fleck’s theory on Kuhn’s in light of the available evidence. We conclude that the degree of similarity between them is too great to be coincidental.

1 Introduction

Usually, entries in popular encyclopedias and books situate a philosopher within some movement and identify his teachers, predecessors or at least sources of inspiration. We all know that Roman Ingarden was a pupil of Edmund Husserl; we know which thinkers inspired Michel Foucault; anyone can learn from Wikipedia and philosophy textbooks that even the great Immanuel Kant needed David Hume to be awakened from his dogmatic slumber. In contrast, when reading popular and even some specialist publications about Thomas Kuhn’s philosophy of science, one might get the impression that Kuhn was a figure like the Coperni-

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cus we know from popular legend: a solitary genius who “stopped the Sun, and moved the Earth,” as he is described in a Polish poem from around 1822 by Jan Niepomucen Kamiński. Before Kuhn, people thought about science in one way, then Kuhn revolutionized philosophy of science, gave rise to meta-sciences such as the social studies of science, and succeeded in changing our understanding of science in general. The public image of Kuhn is imbued with an element of legend, which he nourished himself despite an array of circumstantial evidence that suggests a notable influence on his thought by other thinkers, especially Ludwik Fleck.

The aim of the article is to provoke a discussion on the genealogical relationship between Fleck’s and Kuhn’s works. Although there is no incontrovertible proof that Kuhn directly appropriated ideas from Fleck in the writing of his *Structure of Scientific Revolutions* (henceforth *Structure*), an analysis of the development of Kuhn’s ‘Aristotle experience’ will provide part of the evidence of such an influence in a more indirect way. Perhaps the time has come to more fully acknowledge the impression that Fleck’s work left on Kuhn, making the latter appear less like a legendary figure and more like a scholar indebted to the work of his contemporaries.

We will begin by briefly characterizing the prevailing image of the revolution allegedly achieved by Kuhn. The second section outlines the core similarities and differences between Kuhn’s and Fleck’s ideas. After highlighting the tendency of both Kuhn himself and commentators to overlook Fleck’s original contributions, we examine the development of Kuhn’s account of his ‘Aristotle experience’. This term refers to a moment of revelation that purportedly delivered to him the notions of paradigms and scientific revolutions. He used this narrative to underwrite the originality of his ideas, but there is a notable transformation in that narrative which we will single out. In light of this transformation and related evidence from other studies, we consider four hypotheses to explain the similarities between Fleck’s theory of thought styles and thought collectives and Kuhn’s approach to scientific paradigms and revolutions. These hypotheses range from coincidental parallel discovery to illicit appropriation of key ideas from Fleck. While the evidence at hand is insufficient to make a definite decision on these hypotheses, our contribution will help to outline and structure further paths of inquiry.

2 The Prevailing Image of the Kuhnian Revolution

In 2012, *The Guardian* celebrated the 50th anniversary of the publication of *Structure* with an article tellingly titled “Thomas Kuhn: the man who changed the way the world looked at science.” According to the author,

[Thomas Kuhn] singlehandedly changed the way we think about mankind's most organised attempt to understand the world. Before Kuhn, our view of science was dominated by philosophical ideas about how it ought to develop ("the scientific method"), together with a heroic narrative of scientific progress as "the addition of new truths to the stock of old truths, or the increasing approximation of theories to the truth, and in the odd case, the correction of past errors." (Naughton 2012)

The article from *The Guardian* (with a quote from the Stanford Encyclopedia of Philosophy) nicely exemplifies the tendency to divide the world into the one before and the one after the publication of *Structure*. Along the same lines, the academic handbook *An Introduction to Science and Technology Studies* contains a chapter titled "The Kuhnian Revolution" (Sismondo 2010). It encapsulates the self-referentiality that has captured the imagination of *Structure*'s many readers: here is a book about scientific revolutions that itself triggered a scientific revolution.

This simplification is present in part of the specialist literature, too:

Kuhn's main contribution to the philosophy of science was [...] to provide a dramatic break with the past. His true originality was to mount a serious, sustained, and largely successful attempt to revolutionize the whole field of the philosophy of science. (Mladenović 2017, 8)
[Kuhn's] concepts of paradigm, paradigm change, and incommensurability have changed the way we think about science. (Nickles 2012, cover text)

If we follow this kind of interpretation, philosophers and the general public alike have been mistaken in their concern for how science *should* work, in thinking that it proceeds in a cumulative fashion and in believing that this process gets us closer to the truth. Kuhn "single-handedly" liberated mankind from these errors. As another article puts it:

There are grounds to say that Kuhn was a myth breaker. His aim was to break the "tourist brochure" conception of science and it is fair to say that this is what he managed to do at least in the HPS [History and Philosophy of Science] community. Very few philosophers of science, and even fewer historians of science, advocate the naïve cumulative model of science that is based on the retainment of the theoretical elements of the older science. (Kuukkanen 2013, 91)¹

¹ In another text the same author writes about Kuhn: "the philosopher who became famous by destroying the myth of cumulative progress of science (Kuukkanen 2007, 558). Another author contrasts Kuhn's theory with positivism: "In *The Structure of Scientific Revolutions* Kuhn developed a novel and interesting account of the dynamics of scientific change, one that was deeply at odds with the assumptions that had previously informed the outlook of philosophers of science. [...] Kuhn was giving us an account of science very different from the positivists' account.

Kuhn was successful in that most educated people today have come to accept a historically and sociologically more enlightened view of science.

Opposing positivism, Kuhn presented a model according to which science develops discontinuously and involves “revolutions” in which one paradigm is replaced by another one that is incommensurable in terms of “Gestalt” seeing: you can see either one or another Gestalt, but it is not possible to see them at the same time. Since real scientific progress is made through revolutions, many people have come to the conclusion that it is revolution, not normal science, that really matters in science. On many accounts, the Kuhnian revolution consisted precisely in exemplifying the revolutionary nature of science.

3 Parallels Between Fleck’s and Kuhn’s Theories

Ludwik Fleck (1896–1961), a Polish-Jewish physician, formulated his theory of thought styles and thought collectives in the mid-1930s (Fleck 1979/[1935], 1981/[1935], 1986b/[1935], 1986c/1936/[1934]). Writing in Polish and German, his research culminated in his 1935 monograph *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (henceforth *Entstehung*), i.e., 27 years before the publication of Kuhn’s *Structure* (1962). In 1938 and 1939, Fleck published short polemical articles with Izydora Dąmbska, a representative of the Lwów–Warsaw school (Fleck 1938) and Tadeusz Bilikiewicz, a psychiatrist, historian and philosopher of medicine (Fleck 1990a/1939, Fleck 1990b/1939). Following the outbreak of the Second World War, he survived the Soviet occupation (September 1939 through June 1941) of his hometown Lwów (now Lviv in Ukraine, also known in German as Lemberg). During the German occupation (beginning in June 1941), he was first moved to a ghetto and then to concentration camps (Auschwitz and Buchenwald). He survived. After the war, he lived in the People’s Republic of Poland, subordinate to the Soviet Union, and got a job at a university as a microbiologist. In November 1949, Fleck and his wife for the first time applied for a permit to emigrate to Israel. However, they had to stay behind the Iron Curtain until 1957, at which point the communist authorities finally issued them with one-way passports (probably because Fleck had already become seriously ill). They had to move out within a month. Fleck spent the last few years of his life in Israel and died one year before the publication of Kuhn’s *Structure*.

It seemed that he was denying every assumption that the positivists made about science.” (Wray 2011, 1).

Despite the widespread image of Kuhn and his work as solitary, the remarkable similarities between his theory and Fleck's earlier work have not gone unnoticed.² Abstracting for the moment from a genealogical relationship between Fleck's and Kuhn's theories, the main parallels can be summarized as follows:

No normative philosophy of science: Philosophy should abstain from stipulations concerning how science should work. Instead, it is important to observe how it actually does work.³

No universal criterion of truth: Scientific practice is not committed to a definition of truth as an absolute, context-independent ideal. Where Fleck mounted a critique of the traditional philosophical concept of truth, Kuhn even explicitly avoided using the term "truth" in *Structure*.⁴

No pure observation: Unbiased observation and an observation language that could be separated from theoretical presuppositions are unattainable.⁵

² See especially Cedarbaum 1983, Rossi 1983, Jacobs 1987, Brorson and Andersen 2001, Jacobs 2002, Babich 2003a/b, Braunstein 2003, Condé 2005, Jacobs 2006, Marín 2010, Mößner 2011, Peine 2011, Collins 2012, Sadegh-Zadeh 2015, Dahms 2016, and Condé 2018.

³ In Chapter V of *Structure*, Kuhn argues that scientific practices are hardly ever based on learning and following explicit rules but mostly rest on recognizing Wittgensteinian "family resemblances". For this reason, he claims that we should not presuppose "an underlying body of rules and assumptions that additional historical or philosophical investigation might uncover" (Kuhn 1970, 46). Fleck in turn wrote: "One speaks too much about what cognitive thinking *ought* to be like, and too little about what it really does look like. Yet do we really know that much about what it ought to be? Do we know at least one example of perfect thinking, a thinking that would deserve fixing once for all, so as to prevent any further change?" (Fleck 1986c/[1936], 80).

⁴ In Fleck's own words: "Thus classical theories of cognition ought to distinguish between: (1) the ideal, unattainable truth, (2) the official 'truths' which 'should' somehow approach it, (3) illusions and mistakes. At the same time, they have to admit that there is no general criterion of truth" (Fleck 1986c/[1936], 111). See also Fleck (1979/[1935], 100). Kuhn observes: "We are all deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance." (1962, 171). That goal is typically conceived of as truth, but since Kuhn considered this is a mistaken conception, he largely abstained from using the concept in *Structure*.

⁵ Fleck rhetorically asks: "Now where is this pure observation without bias? [...] a 'good' observation, valid once for all, independent of the surroundings, their traditions and epoch? One does not find it anywhere in history or today" (Fleck 1986b/[1935], 77; see also Fleck (1979/[1935], 90). Kuhn is equally explicit here: "What a man sees depends both upon what he looks at and also upon what his previous visual-conceptual experience has taught him to see. In the absence of such training there can only be, in William James's phrase, 'a bloomin' buzzin' confusion'." (Kuhn 1970, 113). "But is sensory experience fixed and neutral? Are theories simply man-made interpretations of given data? [...] The operations and measurements that a scientist undertakes in the laboratory are not 'the given' of experience but rather 'the collected with difficulty'." (Kuhn 1970, 126).

Science as a social endeavor: The production and possession of scientific knowledge is only partly dependent on the cognitive accomplishments of individuals. Instead, it is realized by what Fleck called “thought collectives” and what Kuhn called “scientific communities”.⁶

The importance of history of science for philosophy of science: If the traditional epistemological criteria of scientific knowledge are unsuitable to scientific practice, and if science is a social endeavor, then comparative studies of scientific practices and their transformations over time become central to philosophy of science.⁷

Use of case studies: Because the concrete conditions of a thought collective or scientific community have a bearing on the content of the practices and knowledge involved, case studies are required for a proper understanding of science. Fleck used case studies from the medical sciences, most notably the Wassermann reaction and the development of the medical concept of syphilis (1979/[1935]), while Kuhn 1970 chose examples from physics and chemistry.

Non-cumulativeness and holistic change: Given the aforementioned conditions, scientific knowledge cannot be overall cumulative in nature. It only accumulates *within* a given thought style/paradigm. *Between* thought styles/paradigms, established concepts and frameworks change in holistic fashion instead.

⁶ Fleck claims: “Cognition is therefore not an individual process of any theoretical ‘particular consciousness.’ Rather it is the result of a social activity, since the existing stock of knowledge exceeds the range available to any one individual” (1979/[1935], 38). He introduces the notion of a thought collective as follows: “If we define ‘thought collective’ as a community of persons mutually exchanging ideas or maintaining intellectual interaction, we will find by implication that it also provides the special ‘carrier’ for the historical development of any field of thought, as well as for the given stock of knowledge and level of culture. This we have designated thought style. The thought collective thus supplies the missing component” (Fleck 1979/[1935], 38). Kuhn outlines the characteristics of scientific communities in Kuhn 1970, 37, 43, 167, but especially in the postscript he prepared for the second edition where he claimed that “a scientific community consists of men who share a paradigm” (Kuhn 1970, 176) and then more precisely: “a scientific community consists [...] of the practitioners of a scientific specialty [that] have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it.” (Kuhn 1970, 177).

⁷ “Epistemology without historical and comparative investigations is no more than an empty play on words or an epistemology of the imagination” (Fleck 1979/[1935], 21). Kuhn likewise embedded his specific historiographical approach to scientific change in a larger framework that addresses normative epistemological questions, something that neither a purely descriptive history of science nor an axiomatic philosophy of science are able to do (Kuhn 1970, 8 f.).

Where Kuhn used the concept of paradigm shifts to characterize this peculiarity of scientific knowledge, Fleck used the analogy of a force field.⁸

Thought styles and paradigms: Fleck argued that thought collectives are constituted by specific “thought styles” – which include not only sets of shared theoretical presuppositions but also non-verbalizable elements.⁹ Likewise, Kuhn considers scientific communities to be unified by shared “paradigms”, which he simultaneously understands as guiding examples and as conceptual frameworks of a specific way of doing science.¹⁰

Cycles of development of science: If paradigms and thought styles succeed each other and change in holistic fashion, the development of science can be modelled as cyclical, from the establishment of one thought style or paradigm to its succession by another. Whereas Kuhn has become most famous for proposing

8 Fleck describes holistic change as follows: “This network in continuous fluctuation is called reality or truth” (Fleck 1979/[1935], 79). “Every fact reacts upon many others. Every change and every discovery has an effect on a terrain that is virtually limitless. It is characteristic of advanced knowledge, matured into a coherent system, that each new fact harmoniously – though ever so slightly – changes all earlier facts” (Fleck 1979/[1935], 102). Kuhn was particularly insistent upon the non-cumulative elements of science: “The same historical research that displays the difficulties in isolating individual inventions and discoveries gives ground for profound doubts about the cumulative process through which these individual contributions to science were thought to have been compounded.” (Kuhn 1970, 2f.) Later on, he notes that “scientific revolutions are here taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one.” (Kuhn 1970, 92).

9 “Neither the particular coloration of concepts nor this or that way of relating them constitutes a thought style. It is a definite constraint on thought, and even more; it is the entirety of intellectual preparedness or readiness for one particular way of seeing and acting and no other. The dependence of any scientific fact upon thought style is therefore evident” (Fleck 1979/[1935], 64). “Like any style, the thought style also consists of a certain mood and of the performance by which it is realized” (Fleck 1979/[1935], 99). “One cannot look upon the sciences as being only a set of sentences or a system of thoughts. They are complex cultural phenomena, at one time perhaps individual, at present collective ones, made up of separate institutions, separate actions, separate events. Written sentences, unwritten customs, one's own aims, methods, traditions, development. Preparation of the mind, cleverness of hands” (Fleck 1986a/[1946], 118).

10 In Kuhn's words: “I take [paradigms] to be universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners.” (Kuhn 1970, viii). Kuhn notes that “some accepted examples of actual scientific practice – examples which include law, theory, application, and instrumentation together – provide models from which spring particular coherent traditions of scientific research” (Kuhn 1970, 10). However, even in these quasi-definitions, Kuhn oscillates between a narrow meaning of “paradigm” as “an exemplar to be followed” and its broader meaning of “conceptual framework”. In the *Postscript* of the second edition of *Structure*, Kuhn writes that “[paradigm] stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community.” (Kuhn 1970, 175).

a cycle defined by the succession of normal science, crisis and revolution, Fleck distinguished between a classical stage in the development of a thought style and a late stage during which exceptions (called “anomalies” by Kuhn) can be perceived for the first time.¹¹ In moments of “intellectual unrest”, scientists are said to alternately see the old *Gestalt* and the new one.¹² After the breakthrough, the world in which scientists live has changed.¹³

Incommensurability and untranslatability: The meaning of scientific concepts and conceptual change depends on thought styles or paradigms to such an extent that they can acquire an opposite meaning in a succeeding thought style or paradigm and cannot be translated without loss.¹⁴

11 “Every comprehensive theory passes first through a classical stage, when only those facts are recognized which conform to it exactly, and then through a stage with complications, when the exceptions begin to come forward. [...] In the end there are often more exceptions than normal instances” (Fleck 1979/[1935], 28 f., *passim*). “For instance, many theories pass through two periods: a classical one during which everything is in striking agreement, followed by a second period during which the exceptions begin to come to the fore” (Fleck 1979/[1935], 9).

12 “This case could be considered the very paradigm of many discoveries. The mood-conforming gestalt-seeing and its sudden reversal: the different gestalt-seeing” (Fleck 1979/[1935], 179). “When reading papers from that period we begin to think that the respective authors suffered from a specific dizziness, that their eyes were jumping, that they saw alternately the medieval world and the path to the new world” (Fleck 1986b/[1935], 75). “Hence the new observation, i. e., the discovery, is carried out in such a way that, during the epoch of equilibrium, there arises a certain intellectual unrest and a tendency towards changes: a chaos of contradictory, alternate pictures. The picture, fixed up to now, disintegrates into blots which arrange themselves into different, contradictory shapes” (Fleck 1986b/[1935], 76). Kuhn: “Therefore, at times of revolution, when the normal-scientific tradition changes, the scientist’s perception of his environment must be re-educated – in some familiar situations he must learn to see a new gestalt. After he has done so the world of his research will seem, here and there, incommensurable with the one he had inhabited before” (Kuhn 1970, 112); “Nevertheless, the switch of gestalt, particularly because it is today so familiar, is a useful elementary prototype for what occurs in full-scale paradigm shift.” (Kuhn 1970, 85).

13 “Here every discovery is actually a recreation of the whole world as construed by a thought collective” (Fleck 1979/[1935], 102). Kuhn writes: “That is why the unexpected discovery is not simply factual in its import and why the scientist’s world is qualitatively transformed as well as quantitatively enriched by fundamental novelties of either fact or theory.” (Kuhn 1970, 7); “Examining the record of past research from the vantage of contemporary historiography, the historian of science may be tempted to exclaim that when paradigms change, the world itself changes with them.” (Kuhn 1970, 111).

14 Fleck refers to incommensurability as follows: “The old concept of disease thus becomes quite incommensurable with the new concepts and is not replaced by a completely adequate substitute” (Fleck 1979/[1935], 62). “Here is again a different theory from ours, not contradictory, but incommensurable with it. These are not only linguistic differences, because the words ‘plenty’, ‘hand’, ‘everything’ used to describe the number five have a completely different scope

Apart from these similarities, a number of differences between Fleck and Kuhn have been documented in the literature as well. Bird 2000/2012 highlights that, despite some nods towards sociology, Kuhn was more inclined towards psychological explanations, while Fleck's approach was sociology of science *avant la lettre*. Kuhn's and Fleck's notions of incommensurability do not seem to be fully co-extensive either, as only Kuhn clearly distinguishes it from untranslatability. Moreover, Kuhn's and Fleck's uses of the term "paradigm" are at variance, because only Kuhn coined it as a quasi-technical term – in the "narrow sense" – as an exemplar to be followed.¹⁵ Fleck's and Kuhn's theories were also formulated on the basis of analyses of different empirical material, i. e., the history of medicine and the history of physics and chemistry respectively, which might affect the analysis. However, the most pertinent distinction between the two authors is that Fleck viewed science as developing in an evolutionary way, while Kuhn considered scientific change to be revolutionary.¹⁶

Even if from today's perspective Fleck's and Kuhn's theories appear different in many respects, one has to bear in mind that such differences were probably not nearly as apparent at the time of their writing as they are today. In the wake of the publication of the *Structure*, a wide variety of approaches to the social and cultural studies of science developed, and today we are aware of nuances that probably were imperceptible to people who read the *Structure* right after its publication.

In order to adequately discuss the differences between Kuhn's and Fleck's theories, we must first carefully study and explicate the similarities between

than the word five. This is a completely different thought style" (Fleck 1939a, for an English translation see Fleck 1990a/[1939], 270). "It is not possible to express with today's words the content of the view of a distant epoch, because the particular concepts of that epoch are incomensurable with those of today" (Fleck 1939b, for an English translation see Fleck 1990b/[1939], 253). Untranslatability is a more fundamental problem: "If a thought style is so far removed from ours as this, no common understanding is any longer possible. Words cannot then be translated and concepts have nothing in common with ours" (Fleck 1979/[1935], 139). "It is impossible to translate exactly the utterances of one of them into the language of the other one. Bergson's motion, the motion in itself, absolute motion, does not exist at all for Maxwell" (Fleck 1986c/[1936], 83). See also Fleck (1979/[1935], 36). Kuhn discusses incommensurability in terms very similar to Fleck's in (1970, 103, 111f., 149f.) and takes great care to distinguish it from untranslatability (again defined in very similar terms to Fleck's) in his 1969 postscript (1970, 198–205).

¹⁵ Fleck used the term 'paradigm' only three times in his *Entstehung* (1935, 82f., 117, 128f.; 1979, 76, 111, 120f.). For Fleck, 'paradigm' is not so much a normative exemplar as a descriptive example.

¹⁶ An interesting suggestion is made by Oliveira and Condé 2002, namely, that Kuhn was not able to finish his final book on the evolutionary development of science because he would have had to reiterate the accounts of Fleck and Wittgenstein.

them and try to identify, to the greatest extent possible, which of these similarities rest on an exchange of ideas.

Similarities and differences of the aforementioned kind and degree typically exist between the founder of a certain school and his student, who later introduces some modifications to his predecessor's theory. However, they are rarely found to exist between two philosophers who developed their concepts independently of each other. This would be the case if the 'legendary' image of Kuhn as the solitary inventor of the historical and social turn in the philosophy of science were accurate. However, this image is unlikely to be accurate, if only because Kuhn was familiar with Fleck's work to some degree at some stage before publication of *Structure*, which he partly acknowledged himself.¹⁷ In light of these considerations, the degree of similarity between the theories and concepts brought forward by Fleck and Kuhn calls for a comparison in more genealogical terms.

4 Glossing Over Fleck in the Reception of Kuhn's Theory

One significant 'external' distinction between Kuhn and Fleck is their respective degree of scholarly success. Kuhn has become one of the most famous philosophers of science of all times. By 2012, 1.4 million copies of the *Structure* were sold in at least a dozen languages (Naughton 2012). The book initiated the entire discipline of history and philosophy of science as we now know it. In contrast, Fleck failed to reach a large audience during his lifetime and never gained the degree of intellectual influence that Kuhn enjoyed. To paraphrase the title of one memoir about Kuhn (Geertz 1997), Fleck's book might have been either right or wrong, but it was certainly published at the wrong time and in the wrong place. Accordingly, it was easily overlooked – even by some scholars who might have known better.

The overwhelming majority of the more popular publications about Kuhn's philosophy of science do not mention Fleck at all, whereas they take a keen interest in Kuhn's 'Aristotle experience'. Under the entry "Thomas Kuhn," there is no mention of Fleck in the *Encyclopaedia Britannica*, nor in the *New World Encyclopedia*, although the latter speaks of the 'Aristotle experience'. Nor is there any mention of Fleck in the *Guardian* article that celebrated the 50th anniversary of *Structure*, while the 'Aristotle experience' is referred to also here (Naughton

17 We deal with this issue in Section 7 and 9 below.

2012).¹⁸ Even a chapter about the “prehistory” of STS in *An Introduction to Social Studies of Science* (Sismondo 2010) that precedes the chapter on the “Kuhnian Revolution” has no single word about Fleck; before Kuhn, there was only the Vienna Circle and Karl Popper.

Although leading Kuhn scholars obviously do not present such a simplistic picture of the “Kuhnian revolution”, their accounts of the relationship between Fleck and Kuhn are limited in scope and depth. A book about Kuhn edited by Thomas Nickles mentions Fleck only once in noting that Kuhn “claimed that neither Ludwik Fleck (1935) nor Stephen Toulmin (1961; 1972) had influenced him much” (Nickles 2002, 171). Bird 2000 discusses Fleck in some detail but merely as one of many constituents of “Kuhn’s context” (chapter title), which jointly “encouraged the rejection of the picture of science as the accumulation of knowledge driven by a rational scientific method” (Bird 2000, 20). There is no attempt to uncover how any of those elements may actually have shaped Kuhn’s views. James Marcum’s book about Kuhn contains only one paragraph devoted to Fleck, which repeats with full confidence what Kuhn had said, namely, that reading the *Entstehung* confirmed the validity of his own ideas (Marcum 2015, 11; see Section 7 below). Fleck is mentioned only as one intellectual influence among others, including Wittgenstein. In his otherwise extremely rich and detailed monograph on Kuhn, Hoyningen-Huene 1989, 10, mentions Fleck only once, referring to “the sociology of science of Fleck [*die Wissenschaftssoziologie Flecks*]” as one out of eight traditions referred to by Kuhn himself as being connected to his theory.

Given these observations alone, it is evident that philosophers of science, and especially historians of the philosophy of science, have not entirely ignored Fleck. In fact, there is a substantial body of Fleck scholarship, but neither does this knowledge penetrate into accounts of Kuhn’s achievements, especially in the English-speaking world, nor is there a significant amount of research into the Fleck-Kuhn relationship, especially as regards the genealogical question. In fact, to the extent that scholars take notice of the similarities between Kuhn and Fleck, they tend to be reluctant to consider the possibility of a genealogical relationship between their works. A widespread conviction among Kuhn scholars is articulated by K. Brad Wray:

¹⁸ The same observation applies to the *Scientific American* articles by Horgan 1991/2012. Nor will we come across any mention of Fleck under the “Thomas Kuhn” entries in the *Internet Encyclopedia of Philosophy* (Marcum n.d.) and the *Stanford Encyclopedia of Philosophy* (Bird 2018), nor in “A Biographical Memoir” of Kuhn on the website of the National Academy of Sciences (Swerdlow 2013) and the Kuhn entry in the *Routledge Handbook of Social Epistemology* (Wray 2019), whereas the ‘Aristotle experience’ is mentioned in all but the first of these sources.

Fleck (1979/1935), for example, is often cited as having anticipated Kuhn, although more careful studies suggest that the similarities between their views are superficial. (Wray 2016, 4)

Commentators generally seem to take for granted Kuhn's claim that he came up with his ideas before he read Fleck:

Jacobs' account of Polanyi as the source of Kuhn's notion of incommensurability is incomplete, as it does not temper Kuhn's debt to Polanyi in light of Kuhn's debt to Ludwik Fleck. [...] Fleck's concept of a 'thought-style' clearly anticipates several senses of Kuhn's central notion of a 'paradigm', and Fleck had presented a phase-model of scientific development covering pre-normal and normal science. He even illustrated the idea with some of the same examples Kuhn later adopted. [...] Kuhn, who was instrumental in having Fleck's (1935) translated and then published in English, emphasized that his "revelation" (his discovery of incommensurability) had occurred several years before he had read Fleck (in 1949 or 1950). (Oberheim 2012, 128f.)

Even authors who are aware of and sympathetic to Fleck, such as Peine 2011 or Collins 2012, usually do not recognize the Fleck-Kuhn relationship as a genealogical one. Peine's intention, for instance, is to show that some of Fleck's concepts are complementary to Kuhn's theory, as if their works were synchronous. Because Peine *set out from a discussion of the differences* between two 'ready-made' theories, he readily convinces himself that Kuhn rejected more from Fleck than he took from him: "while Kuhn has indeed resembled [sic!] some of Fleck's central ideas, he was reluctant to accept Fleck's more radical claim that science is an *essentially* social process" (Peine 2011, 491, original emphasis).

In a nutshell, scholars who undertake comparative analyses of Fleck and Kuhn typically (1) focus on one or a few details and lose sight of the overall similarities, especially as they would have appeared to a contemporaneous reader; (2) compare the text of Fleck's book with the text of Kuhn's book without exploring the diachronic development of Kuhn's ideas; and (3) presume that Kuhn had made his discovery before he read Fleck.

These points demarcate the research gap that we are trying to narrow on these pages. Let us begin by exploring what the genealogical relationship between Fleck and Kuhn might have been, and what role the 'Aristotle experience' narrative promoted by both Kuhnians and Kuhn himself might have played in obscuring this relationship.

5 Kuhn's 'Aristotle Experience'

Kuhn dated the 'Aristotle experience' to 1947, i. e., fifteen years before the publication of *Structure*.¹⁹ It was even mentioned as the "single 'Eureka!' moment in 1947" in Kuhn's 1996 obituary in *The Tech*. Many authors repeat this story using different words. Three years after Kuhn's death, for example, this experience was called a "dramatic awakening" – much in the same way as Kant's proverbial awakening from his dogmatic slumber:

Kuhn describes a dramatic awakening in 1947 while reading Aristotle on physics. Kuhn had been struggling to understand differences in Aristotelian and Newtonian views about fundamental concepts such as motion and position. [...] This was Kuhn's "Eureka" experience, which, using the words "my own enlightenment," he describes as a kind of epiphany. (Andresen 1999, 55)

The standard version of the legend about Kuhn has it that it was this experience (and not the reading of other authors) that became the main source of his subsequent philosophy of science:

This shocking experience of 1947 rather quickly led Kuhn to formulate the essential core of the philosophy of science he would debut in *Structure* fifteen years later. Before the Aristotle experience, his conception of science was fairly conventional and, indeed, conventionalist with regard to theories. (Reisch 2016, 16)

The authors of the entry "incommensurability" in the *Stanford Encyclopedia of Philosophy* also refer to the 'Aristotle experience' when describing Kuhn's discovery of incommensurability:

According to Kuhn, he discovered incommensurability as a graduate student in the mid to late 1940s while struggling with what appeared to be nonsensical passages in Aristotelian physics. (Oberheim and Hoyningen-Huene 2018)

The author of one article, which seeks to prove the influence of Kuhn's psychoanalytical therapy on his theory, goes so far as to compare Kuhn to St. Paul:

This experience took place in 1947 and thus right in the middle of his two years of psychoanalysis. (Forrester 2007, 790).

Kuhn entered analysis as a budding theoretical physicist, gauche with women and discontented with his work in a diffuse and unfocussed manner; he left analysis two years later

¹⁹ See Kuhn *et al.* 2000, 292f., and other texts in Table 1 below. So far, the greatest attention has been paid to this experience by Wayland 2003 and Reisch 2014/2016.

with a doctorate in physics, married, and training to be a historian of science, having undergone a conversion on the road to Damascus which would be one of the seeds of his future method as a historian and his great contribution to the history and philosophy of science. (Forrester 2007, 788)

The last quote is, of course, a rare exaggeration, but it shows well how willing people are to pick up the story of the ‘Aristotle experience.’

The story of the ‘Aristotle experience’ was also repeated at family meetings, as can be seen from the following remark by Kuhn’s daughter Sarah:

The importance of the Aristotle story to my dad’s views can’t be overstated, since we heard it many times in numerous iterations. [...] [My father] had started to read Aristotle while preparing a teaching case on the development of mechanics. Staring out the window and holding a four-colored mechanical pencil of the sort I remember from my childhood, he said that he suddenly understood that Aristotle was not a very bad Newtonian physicist, but instead a “very good physicist indeed, but of a sort I’d never dreamed possible.” His vigorous pursuit of the implications of this insight cost him tremendous and painful effort, but I think, was also the source of his greatest rewards. (Andresen 1999, 56)

This passage confirms the image of Kuhn as a solitary genius who experienced a revelation – an image that is also found in *Structure* itself:

Instead, the new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis. [...] Almost always the men who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigm they change. (Kuhn 1970, 89 f.)

The prominence of the ‘Aristotle experience’ motif in the literature notwithstanding, we would like to point out a certain inconsistency in the narrative. Let us use a metaphor in an attempt to illustrate this point. Kuhn published his first major monograph, *The Copernican Revolution*, in 1957, i. e., five years before *Structure*, and this work does not yet contain the key ideas of *Structure*. If Kuhn “originally conceived” of these ideas already in 1947, how is it possible that he got pregnant with his ‘second child’ before giving birth to his ‘first child’ in 1957? This would seem like an anomaly that requires explanation. In fact, some scholars are willing to go to great lengths when proposing that Kuhn, while writing *The Copernican Revolution*, already knew he was writing untruths:

Though *The Copernican Revolution* supported the image of scientists as curious, open-minded explorers of “the unknown,” Kuhn had long before come to reject this image. What Kuhn called his ‘Aristotle experience’ had been pivotal in this. It had occurred in the summer of 1947, while he was studying the history of physics at Conant’s direction. (Reisch 2014, 373)

Other authors who try to make the itinerary of Kuhn's thought consistent provide the following explanation: the ideas were born in 1947, but Kuhn "wrestled with the ideas awakened in him by Aristotle for 15 years" (Horgan 1991, 40) before putting them into words and writing a book.²⁰

"I sweated blood and blood and blood," he says, "and finally I had a breakthrough." The breakthrough was the concept of paradigm. "Paradigm," pre-Kuhn, referred simply to an example (often, one used to teach a language, such as *amo, amas, amat* in Latin). In *Structure*, Kuhn defines the word most narrowly as an archetypal experiment or "problem solution" [...] that implicitly tells scientists how to look at the world. (Horgan 1991, 40)

A year before his death, in one of the discussions that came out in print, Kuhn himself gave another explanation. After he left physics, he had to do more history of science and above all, having not yet had a permanent position, he could not reveal his controversial philosophical ideas for the sake of his further scientific career:

My notion was, and my application indicated, that there was important philosophy to come out of it; but I needed first to learn more history, to do more history, and to establish myself professionally as a historian before I let the cat out of the bag. (Kuhn *et al.* 2000, 281)

So, we see that in 1995 Kuhn believed that the cat was in the bag as early as 1947, i. e., that important philosophy would come out of this experience once his career was secured. Let us now investigate how tenable this narrative is in light of Kuhn's own statements of the 'Aristotle experience' over the years.

6 Genesis and Development of Kuhn's 'Aristotle Experience' Narrative

While trying to locate the source of the 'Aristotle experience' motif, we found that it permeates Kuhn's own autobiographic reflections from 1953 onward. In Table 1, we list in chronological order all found descriptions of Kuhn's 1947 experience. The years in the left-hand column indicate the time of writing of the respective remarks, not their publication. Kuhn's own and reported words are in italics.

20 The first attempt to write *Structure* were the Lowell Lectures that Kuhn prepared in 1951, i. e. after reading Fleck's book. "The essential step after which the remainder of the work came rapidly" (Swerdlow 2013, 8) – a breakthrough – came around 1958/1959, when Kuhn became able to formulate the concept of "paradigm".

Table 1: Kuhn's references to his 1947 experience

1953 Application for Guggenheim Foundation Fellowship	<i>"Upon detailed historical study of the same theories I was profoundly impressed by the disparities in the conceptions of research which emerge from historical and from philosophical analyses. Since that time I have been primarily concerned with the sorts of historical problems which, by displaying research method in action, can succeed in effecting a rapprochement. Because it discovers the roots of scientific theories in the aspirations, needs, and capabilities of human beings, the historical study of scientific creativity seems best suited to dispel the mystical contemporary image of the scientist as an ineffable discovery-machine."</i> (Hufbauer 2012, 45)
1957 Talk: <i>A Historian Views the Philosophy of Science</i> , which Kuhn gave at Berkeley	<i>"This, Kuhn exclaimed, was a 'shocking experience'. His notes read, 'Nothing in my physics education or my philosophy reading had prepared me for the way science looks when viewed through writings of dead scientists.'</i> The word 'shock' appears twice again as he elaborated on the differences between science described in textbooks and science revealed in ' <i>letters, diaries, laboratory notebooks and, above all, in the articles in scientific periodicals published ten, twenty, thirty years before theory was ready to be embodied in a text.</i> '" (Reisch 2016, 14)
1961 A letter to J. Conant	<i>"The shocking experience appears again in a letter Kuhn reportedly wrote to Conant in June 1961. Months before, he had sent Conant a draft of <i>Structure</i>."</i> (Reisch 2016, 15)
1962 <i>The Structure of Scientific Revolutions</i>	<i>"The essay that follows is the first full published report on a project originally conceived almost fifteen years ago. At that time I was a graduate student in theoretical physics already within sight of the end of my dissertation. A fortunate involvement with an experimental college course treating physical science for the non-scientist provided my first exposure to the history of science. To my complete surprise, that exposure to out-of-date scientific theory and practice radically undermined some of my basic conceptions about the nature of science and the reasons for its special success."</i> (Kuhn 1970, v)
1968 Lecture at Michigan State	<i>"In a lecture at Michigan State in 1968, 'shock' and 'surprise' were joined by Kuhn's 'astonishment' to discover 'that science, when encountered in historical source materials, seemed a very different enterprise from the one implicit in science pedagogy and explicit in standard philosophical accounts of scientific method.'</i> (Reisch 2016, 16)
1976 Foreword to L. Fleck's <i>Genesis and Development ...</i>	<i>"That revelation was the role played in scientific development by the occasional noncumulative episodes that I have since labeled scientific revolutions."</i> (Kuhn 1979, vii)

1977
The Essential Tension
"My own enlightenment began in 1947, when I was asked to interrupt my current physics project for a time in order to prepare a set of lectures on the origins of seventeenth-century mechanics. [...] One memorable (and very hot) summer day those perplexities [concerning his understanding of Aristotle] suddenly vanished. I all at once perceived the connected rudiments of an alternate way of reading the texts with which I had been struggling. [...] Since that decisive episode in the summer of 1947, the search for best, or best-accessible, readings has been central to my historical research (and has also been systematically eliminated from the narratives that report its results)." (Kuhn 1977, xi–xii)

1981/1987
What Are Scientific Revolutions?
*"I first read some of Aristotle's physical writings in the summer of 1947, at which time I was a graduate student of physics trying to prepare a case study on the development of mechanics for a course in science for nonscientists. [...] [A]s I was reading him, Aristotle appeared not only ignorant of mechanics, but a dreadfully bad physical scientist as well. About motion, in particular, his writings seemed to me full of egregious errors, both of logic and of observation. [...] I was sitting at my desk with the text of Aristotle's *Physics* open in front of me and with a four-colored pencil in my hand. Looking up, I gazed abstractedly out [of] the window of my room – the visual image is one I still retain. Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. Now I could understand why he had said what he'd said, and what his authority had been. [...] That sort of experience – the pieces suddenly sorting themselves out and coming together in a new way – is the first general characteristic of revolutionary change that I shall be singling out after further consideration of examples." (Kuhn 2000, 15–17)*

1990
Interview with Skuli Sigurdsson
*"And the experience was enlightening. What Aristotle could be saying baffled me at first, until – and I remember the point vividly – I suddenly broke in and found a way to understand it, a way which made Aristotle's philosophy make sense. It was that case history, and others, that in some sense first got me onto the idea of gestalt switches and changes in conceptual frameworks, which was to show up in *The Structure of Scientific Revolutions* in 1962." (Sigurdsson 2016, 21)*

1991
"Scientific American"
*"Kuhn nonetheless traces his view of science to a single 'Eureka!' moment in 1947. He was working toward his doctorate in physics at Harvard University when he was asked to teach some science to undergraduate humanities majors. Searching for a simple case history that could illuminate the roots of Newtonian mechanics, Kuhn opened Aristotle's *Physics* and was astonished at how 'wrong' it was. How could someone so brilliant on other topics be so misguided in physics? Kuhn was pondering this mystery,*

staring out of the window of his dormitory room ('*I can still see the vines and the shade two thirds of the way down*'), when suddenly Aristotle 'made sense.'

Kuhn realized that Aristotle's views of such basic concepts as motion and matter were totally unlike Newton's. Aristotle used the word 'motion,' for example, to refer not just to change in position but to change in general –the reddening of the sun as well as its descent toward the horizon. Understood on its own terms, Aristotle's physics '*wasn't just bad Newton*,' Kuhn says; it was just different. Although Kuhn went on to receive a doctorate in physics, he switched shortly thereafter to the history of science, intending to explore the mechanisms behind scientific change." (Horgan 1991, 40)

1995

Discussion published in
The Road since Structure

"[*This Aristotle experience was terribly important.*]" (Kuhn et al. 2000, 275)

"[*I]t was fifteen years between the time these ideas started and the time I was finally able to write Structure.*" (Kuhn et al. 2000, 292)

"*I had wanted to write The Structure of Scientific Revolutions ever since the Aristotle experience. That's why I had gotten into history of science – I didn't know quite what it was going to look like, but I knew the noncumulativeness; and I knew something about what I took revolutions to be. I mean, I think in retrospect I was wrong, in the ways I talked about the other night; but that was what I really wanted to be doing. And thank God, it took me a long time, because I managed to get myself established in other ways meanwhile, and the ideas – I didn't let go of them too prematurely. I did let go of them somewhat prematurely, but ... thank God!*" (Kuhn et al. 2000, 292 f.)

Let us first highlight the uncontroversial part of Kuhn's claims: In the summer of 1947, when he was a 25-year-old graduate student who had been working on his doctorate in physics for a year, a certain event took place. This particular event strongly motivated a change in his interests from physics to the history and philosophy of science. That event occurred while he was preparing lectures on the history of mechanics for non-scientists on James Conant's request. Kuhn had never read historical scientific texts before. While preparing his lectures, he opened Aristotle's *Physics*. Aristotle's rationality could not be denied (being a canonical ancient philosopher with lasting intellectual authority in many areas), but his claims concerning the physical universe seemed plainly ridiculous. This was a puzzle that demanded a solution. And then, on one hot summer day, Kuhn simply and suddenly began to understand Aristotle's physics. The condition for this understanding was a sympathetic approach to the historical source (based on the assumption that Aristotle knew what he was saying) and an understanding-oriented reading (forced by the need to prepare a lecture). The solution to the

puzzle turned out to be the observation that the same words might mean something different to Aristotle and the modern physicist respectively.

When considered at face value, this kind of intellectual experience seems quite universal: which philosopher never enjoyed a sudden understanding of another thinker's thoughts?²¹ Therefore, the specific meaning and significance that Kuhn ascribed to this experience cannot be derived from the content of the experience itself, i. e., the sudden understanding of the meaning of an old text as rational and consistent in its own way. There is something more specific in play.

From the quotes and observations chronologically listed in Table 1, it appears that Kuhn began to ascribe a more specific meaning to this experience gradually, but with a certain turning point at a certain time.²² In fact, it was called a "revelation" for the first time, singled out and linked to a specific date in the summer of 1947 only in the foreword to the American edition of Fleck's *Entstehung* (Kuhn 1979, vii), which he completed in June 1976.

When Kuhn learned about the planned edition of Fleck, the 1947 experience assumed a specific shape and importance in Kuhn's eyes. The first more concrete description of his "enlightenment" appeared in a collection published in 1977 (Kuhn 1977, xi).²³ A more extensive description was published in 1987 (apparently, this is the text of a lecture given in 1981 at the Center for Cognitive Science, Massachusetts Institute of Technology). After this, Kuhn spoke about his 'Aristotle experience' only when answering questions during interviews.

The relevant change in meaning of the 'Aristotle experience' in the mid-1970s is the following: Kuhn's earlier descriptions of the 1947 episode ("profoundly impressed," "shock," "shocking experience," "surprise," "astonishment") focus on the incompatibility of the cumulative picture of science presented in textbooks with a historical source. From the mid-1970s onwards, Kuhn's focus shifted towards scientific paradigms and revolutions as the key content of his 1947 experience. How did this change in the 'Aristotle experience' narrative come to pass?

From the archival records of Robert K. Merton Papers (Rare Book & Manuscript Library, Columbia University in the City of New York) and Robert Merton's

²¹ Similar observations were made by Sankey 2018, 83: "it seems to me to be a mistake to think of Kuhn's encounter with Aristotle as the discovery of the phenomenon of incommensurability rather than the experience of understanding an initially incomprehensible text."

²² Reisch also noticed this, but still took Kuhn's words at face value: "in 1977, Kuhn had begun to describe the Aristotle experience not as the result of extended historical studies in primary sources but rather as a sudden epiphany or 'revelation'— a singular moment when the 'perplexities' he had always encountered in Aristotle's Physics 'suddenly vanished'." (Reisch 2016, 16)

²³ The preface to this volume is not dated, but since Kuhn quotes a text published in 1977, this preface was probably written in 1977.

memoirs (Merton 1979, 83) we know that Robert Merton and Thaddeus Trenn wanted Kuhn to get more involved in the American edition of Fleck as early as 1975, but that Kuhn initially hesitated:

Only now, in the foreword to the English translation of Fleck's monograph which Kuhn agreed to write only after strong urging by Thaddeus J. Trenn and myself as editors of that work, does Kuhn report how he happened to come upon the monograph. (Merton 1979, 83; see also Trenn 1975a)

However, this hesitation does not sit well with the foreword to Fleck's book Kuhn wrote in 1976, according to which he had promoted a translation of the latter from the early 1950s onwards:

The appearance of an English translation of Ludwik Fleck's *Genesis and Development of a Scientific Fact* realizes a project I have urged on various friends and acquaintances (but not on the present editors) ever since I first encountered the book a quarter of a century ago. My purpose in calling for a translation was not simply to make Fleck's work accessible to an English-speaking audience but rather to provide it with an audience at all. (Kuhn 1979, vii)

The initiative for the translation of Fleck's work seems to have come from Trenn. The book proposal dates to May 10, 1975. Trenn (then working at the University of Regensburg) was to offer Merton collaboration on the project, while Merton arranged funds for translation, helped to get University of Chicago Press interested in the project and asked Kuhn if he wanted to participate (Trenn 1975b). Merton "had known of Fleck's book only through Kuhn's early allusion to it" (Trenn 1975a), that is, he learned about Fleck from the preface to *Structure* but had not read the book before Trenn contacted him in 1975. These observations do not support the assumption that Kuhn's activities were "instrumental in having Fleck's (1935) translated and then published in English" (as Oberheim 2012, 128f., claimed, see above). After all, the initiative to engage Kuhn in the edition of Fleck did not come from Kuhn himself. It seems to have been Merton's idea, who thought that:

It is symbolically appropriate that Thomas Kuhn should introduce the English-language edition of Fleck's monograph; after all, it was he who had introduced the original edition to Trenn and myself – as to countless others. (Merton 1979, 119)

The adjective "countless" indicates that Merton first learned about Fleck's book through the sentence in Kuhn's preface to *Structure*.

Thus, one might get the impression that around 1975/76 Kuhn started to create a narrative according to which the 'Aristotle experience' was the main source of inspiration for *Structure* and already provided him with its key concepts. It was during this period that Kuhn's experience during the summer of 1947 came to be

described in terms of enlightenment and revelation. It also was the time at which his 1947 experience came to be associated with the theme of progress in science. Given the chronology of events, it seems that this transformation happened *after* Kuhn found out that Fleck's translation into English was in preparation. Conveniently, Kuhn's 'Aristotle experience' narrative, in its post-1975 version, helped to preserve his claim to originality of the central claims of his *Structure*.

7 What Kuhn said About Fleck

Given that Kuhn knew about the existence and content of Fleck's work, and given the apparent similarities in content, the scarcity of references to Fleck in his published work is conspicuous. We managed to locate three places in which Kuhn explicitly refers to Fleck. The first, best-known piece is in the preface to *Structure*. As far as we know, these are the only sentences about Fleck that Kuhn has formulated without being prompted by others, and they contain an explicit but qualified acknowledgement. Kuhn recalls his "exploring fields without apparent relation to history of science", i. e., Jean Piaget, Gestalt psychologists, B. L. Whorf, W. V. O. Quine, and then adds:

That is the sort of random exploration that the Society of Fellows permits, and only through it could I have encountered Ludwik Fleck's almost unknown monograph, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basel, 1935), an essay that anticipates many of my own ideas. Together with a remark from another Junior Fellow, Francis X. Sutton, Fleck's work made me realize that those ideas might require to be set in the sociology of the scientific community. Though readers will find few references to either these works or conversations below, I am indebted to them in more ways than I can now reconstruct or evaluate. (Kuhn 1970, 6f.)

Two points in this quote require closer consideration: (1) referring to "my own" ideas means that, although similar ideas are found in Fleck, Kuhn came up with them himself; (2) contrary to the announcement in the preface (where a "few references" were hinted at), *no* further reference to Fleck follows throughout the book.

The second of Kuhn's three statements about Fleck is the foreword to the translation of Fleck's *Entstehung*, which, as was discussed above, he wrote at Robert Merton's request. The foreword has a casual tone and gives the impression of having been written without much care. It is difficult to find references to other than three paragraphs of Kuhn's foreword in the literature, in which Kuhn explains why he could not refuse the editors' request and tells us how he came aware of Fleck's book through a footnote in Reichenbach's *Experience and Pre-*

diction (1938).²⁴ In the fourth paragraph, we find a vague and qualified acknowledgement of Fleck's contribution:

I have more than once been asked what I took from Fleck and can only respond that I am almost totally uncertain. Surely I was reassured by the existence of his book, a nontrivial contribution because in 1950 and for some years thereafter I knew of no one else who saw in the history of science what I was myself finding there. Very probably also, acquaintance with Fleck's text helped me to realize that the problems which concerned me had a fundamentally sociological dimension. [...] But I am not sure that I took anything much more concrete from Fleck's work, though I obviously may and undoubtedly should have. (Kuhn 1979, viii–ix)

Kuhn proceeds by claiming that he “found Fleck's German extraordinarily difficult”, i. e., by arguing for a language barrier to a full comprehension, and reiterates that he responded primarily to what was already very much on his mind. However, the fifth paragraph begins with the words:

Rereading the book now, as I have not done in the interim, I find many insights that I might fruitfully have worked into my viewpoint. (Kuhn 1979, ix)

He claims that he is impressed by some of Fleck's ideas, such as “journal science,” “vademecum science,” the transmission of “ideas between two ‘thought collectives’,” and the “possibilities and limitations of participation in several ‘thought communities’.” (Kuhn 1979, ix). Kuhn further describes Fleck's book as “penetrating” in the sixth paragraph and calls it a “largely unexploited source,” but also points to some “fundamental problems” related to the concept of a thought collective. Confusing it with the notion of thought style, he criticizes this concept as follows:

[A] thought collective seems to function as an individual mind writ large because many people possess it (or are possessed by it). To explain its apparent legislative authority, Fleck therefore repeatedly resorts to terms borrowed from discourse about individuals. (Kuhn 1979, x)

Kuhn's further criticisms of Fleck in *Foreword* also suggest that the text was written by someone who has not read Fleck with full attention or understand-

24 The story of this footnote in Reichenbach has attracted attention from commentators: “If there had not been a marginal reference to Fleck's work in a brief footnote in a monograph on epistemology by the prominent logical empiricist Hans Reichenbach, entitled *Experience and Prediction* (Reichenbach 1938, 224 n6), it is more likely than not that his ideas would have been lost forever and the philosophy of science would not have undergone the ‘Kuhnian’ revolution that it has since 1962.” (Sadegh-Zadeh 2015, 527).

ing. At a minimum, it is difficult to meaningfully respond to the second part of the *Foreword*, because Kuhn's criticisms are very elusive. At the beginning of the eighth paragraph, Kuhn suggests some inconsistency in Fleck's book, noting that “[o]ther phrases throughout Fleck's book suggest a very different position, one far closer to my own”. He attributes to Fleck, as he puts it in the last paragraph, an “extraordinarily problematic” position, which Fleck is said to attempt to overcome by distinguishing between “passive and active elements of knowledge,” which is “unenlightening” for Kuhn. He returns with a charge of abuse of metaphors from individual psychology: “‘Passive’ and ‘active’ are again terms borrowed from individual psychology for application to a collective.”²⁵ Highlighting these problems, Kuhn declares, is not to discredit Fleck.

The impression that results from Kuhn's *Foreword* is that he considered Fleck's book an important text of historical value, a text that can serve as a source of inspiration but does not deliver a consistent theory because it remains entangled in a number of unsolvable problems. In any case, Kuhn avoids a comparison between his own ideas and Fleck's: He mentions which parts he likes, which ones he finds interesting, but he does not relate them to his own theory and, above all, he mentions twice that Fleck's book only confirmed what he had already conceived of himself.

The third occasion on which Kuhn explicitly referred to Fleck was in an interview one year before his death. Kuhn here reiterated the argument of the language barrier, the claim that Fleck confirmed his own ideas, and the claim that Fleck adopted “the mind and the individual” as a model for the thought collective:

It was I think in Reichenbach's *Experience and Prediction* that I found a reference to a book called *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*. I said, my God, if somebody wrote a book with that title – I have to read it! These are not things that are supposed to have ... they may have an *Entstehung* but they are not supposed to have an *Entwicklung*. I don't think I learned much from reading that book, I might have learned more if the Polish German hadn't been so very difficult. But I certainly got a lot of important reinforcement. There was somebody who was, in a number of respects, thinking about things the way I was, thinking about the historical material the way I was. I never felt at all comfortable and I still don't with [Fleck's] “thought collective.” It was clear it was a group, since it was collective, but [Fleck's] model [for it] was the mind and the individual. I just was bothered by it, I could not make use of it. I could not put myself into it and found it somewhat repugnant. That helped me keep it somewhat at arm's length, but it was very important that I read that book because it made me feel, all right, I'm not the only one who's seeing things this way. (Kuhn *et al.* 2000, 283)

²⁵ On this, see also Jarnicki (2021, sect. “The Ambiguous Notion of Mood”).

In this discussion, Kuhn willingly talks about his other sources of inspiration – above all Alexandre Koyré, to whom he refers in many footnotes and to whom he devoted entire texts. As we saw, Fleck was treated differently, being mentioned only and precisely in those three locations discussed above, and being the only one of Kuhn's sources to receive such sharp criticism.

As to the language barrier argument, Fleck's German (which was widely spoken in Austro-Hungarian Lwów/Lemberg, and which was obligatory at the secondary school he attended) is by no means a strange or “extraordinarily difficult” variety of the German language. It might qualify as slightly old-fashioned by the standards of 1935 and arguably bears some characteristics of the local “Lemberger Deutsch” that has a notable pedigree in German-speaking literature (Joseph Roth, Paul Celan, Manès Sperber; cf. Weinberger 2016). Either way, it poses no notable difficulty in comprehension to a competent reader of German. We also know that Kuhn himself admitted that he was able to read German and French, although with some effort (Kuhn *et al.* 2000, 59), and that in order to understand the philosophical content of Fleck's book one does not need a deep understanding of microbiological issues.

The special treatment that Kuhn gave to Fleck in several respects – rarely mentioning him while citing him as a major inspiration, highlighting putative differences against many striking similarities, insisting on independent discovery and difficulties in comprehension – is a fact that requires explanation. The development of Kuhn's ‘Aristotle experience’ narrative, in parallel with a reconstruction of the timeline of his adoption of Fleck's ideas might help to elucidate this issue.

8 Wilhelm Baldamus's Attack on Thomas Kuhn

We do not know how we would have perceived the English edition of Fleck had it not been prefaced by Kuhn's recollection of the ‘Aristotle experience’. However, there was one author, not connected to Kuhn, who published a paper in English before the ‘Aristotle experience’ was first presented in its classical form and before the English-language edition of Fleck appeared. In 1977, German émigrée Wilhelm Baldamus published an article titled “Ludwik Fleck and the Development of the Sociology of Science”. In this article, he did not directly suggest plagiarism on the part of Kuhn, but nevertheless wrote something that could not fail to bother the author of *Structure*:

The reader who is familiar with Kuhn's work will easily recognize that the majority of [Fleck's] terms is virtually identical with the basic terminology of *The Structure of Scientific Revolutions*. (Baldamus 1977, 151)

More specifically, Baldamus' charge is that Kuhn "appropriated" Fleck's ideas and, hence, made them part of contemporary sociology of science, with one crucial transformation: he omitted the genuinely sociological strand of Fleck's central theoretical concepts while implicitly relying on the "sociology of scientific communities" when developing his notion of a paradigm.

Two years later, Baldamus published another article, now in German (Baldamus 1979).²⁶ Drawing on a statistical analysis of Kuhn's vocabulary and its development over time, he again suggests that Kuhn took more from Fleck than he was prepared to admit. Baldamus notes that key concepts in Fleck's *Entstehung* can be found in Kuhn's writings with increasing frequency and in more 'exoteric', less technical and more popular meanings over the course of the years 1952–1977, with a marked shift between the pre- and post-*Structure* years (before and after 1962).

In private correspondence, Baldamus was more outspoken about his suspicion of plagiarism, especially in an exchange of letters with Robert Merton (Baldamus 1980). In response to an earlier letter from Merton, Baldamus sent him a copy of his 1979 article. In the opening sequence of the accompanying letter (dated 6 February 1980), Baldamus courteously states that for the last 30 years he has basically been developing Merton's ideas, and depreciates his own achievements to date: "I am [...] a slow, awkward and perfectionist potterer, and my English is limited". He refers to his research on Kuhn as follows:

[M]y prolonged and tortuous concern with "Kuhn and Fleck" was an attempt to prove, in the first place to myself, that plagiarism is possible in Sociology (and not only in the natural sciences). I think I have failed in this, – temporarily. [...] I have marked in red those passages which I hope may be useful even though I failed to demonstrate convincingly my (implicit) allegation of Kuhn's plagiarism. In particular, some of it may be relevant to the norms of communality (i. e. appropriate citations of sources), applied to sociology. (Baldamus 1980)

Merton does not seem to notice the irony in the understatement of Baldamus' introductory remarks: after fleeing from the Nazi regime in 1937, he lived in the United Kingdom and worked at the University of Birmingham since 1951. In his reply (25 February 1980), Merton categorically states:

²⁶ Only in 2010 was this article translated into English; it is included in Erickson and Turner 2010.

I must confess that I do not at all think of Tom Kuhn having “appropriated” some of Fleck’s fundamental ideas in some unwitting fashion, let alone having “plagiarized” him. At the most, I think, he resonated to some of Fleck’s pathbreaking notions which he did not fully grasp in what was for him Fleck’s “extraordinarily difficult” German. (Merton 1980a)

Merton, who developed the concept of “obliteration by incorporation” in relation to common knowledge (Merton 1968, 27f., 35–37), was not able to relate this concept to an individual, let alone to his friend.

To the best of our knowledge, no one except Baldamus has raised the suspicion – or even insinuated – that Kuhn plagiarized Fleck. The only *explicit* published reference to a possible charge of plagiarism against Kuhn can be found in what is actually an awkward denial by Babette Babich 2003b. She claims that Kuhn, in the context of the partly inquisitorial nature of Cold War anti-communism in the US, felt compelled to refrain from explicit references to a notion of “thought collectives” developed by an author from a communist country, choosing to resort instead to an indirect, “periphrastic” mode of reference:

[T]o say that Kuhn’s ‘paradigm’ is a periphrastic construction rendering Fleck’s *Denkstil/ Denkkollektiv* claims not that Kuhn intentionally plagiarised Fleck’s ideas but only that at the time, and on more than one level, Kuhn could make only such a periphrastic allusion to Fleck. (Babich 2003b, 104)

This argument is of limited plausibility, except from the perspective of Cold War anti-communism itself. One would expect to find some reflection on this kind of practice and its rationale in Kuhn’s private writings or later publications.

In any case, we know that Kuhn was familiar with Fleck’s work before the publication of *Structure*, and he must have had a reasonable if rough understanding of the *Entstehung* no later than 1962, when he referred to it in *Structure*’s preface. But how and when did his encounter with Fleck’s *Entstehung* first develop?

9 Kuhn’s Reading of Fleck

The most likely date of Kuhn’s first encounter with Fleck’s *Entstehung* can be deduced from the text of a speech that Trenn wrote for a colloquium on Fleck in Hamburg in 1981. In this text, Trenn, like Merton, defends Kuhn without giving any arguments of his own when categorically stating that “Fleck cannot be dismissed as a proto-Kuhn, and Kuhn did not plagiarize Fleck” (Trenn

1981).²⁷ However, in that same talk he provides information that suggests otherwise.

Trenn had seen a Harvard library copy of Fleck's *Entstehung* and knew from the control slip that Kuhn most likely read it before 7 December, 1949, which is the date the book was due to be returned. The book had been borrowed in May 1937 as well; according to Kuhn, it was probably read by Edward Shils at that time. It was borrowed again in October 1950, at which point it was probably read by James Conant.²⁸ The next loan of Harvard's copy was as late as 1974, which does not testify to any success of Kuhn's alleged attempts to make Fleck's book known to his colleagues from the 1950s onward.²⁹ It seems he only managed to convince one person at Harvard to read it – his boss.

Kuhn's stated intention would also have provided him with reasons to contact Fleck. Of course, in the first half of the 1950s, Fleck lived behind the Iron Curtain, which did not facilitate contact between scholars, but mail correspondence would certainly have been possible. While we do not know of any such attempt, we know that Fleck was inclined to seek contact with like-minded scientists – he sent a book to Polanyi, for example. We also know that Kuhn, while in France, met with Bachelard and contacted Koyré. An interview with Kuhn demonstrates that he himself initiated these meetings and felt strongly about them (Kuhn *et al.* 2000).

In continuation of his account of Kuhn's reading history of Fleck, Trenn writes:

Shortly after he returned the library copy in 1949, Kuhn purchased his own private copy from Schwabe. There is good reason to believe that Kuhn did read this monograph very carefully as the many vertical lines and occasional marginalia attest to. (Trenn 1981)

The fact that Kuhn had his own copy of the book also follows from the introduction to Fleck's book, although Kuhn insists that all he found in Fleck's book was what he had already come up with before:

The lines in the margin of my copy of the book suggest that I responded primarily to what had already been very much on my mind: changes in the gestalts in which nature presented

27 The very fact is remarkable that a need for such statements began to be perceived shortly after the publication of Fleck in English.

28 The fact that Conant is likely to have been familiar with Fleck's ideas since 1950 weakens statements in Wray 2016 about Conant's strong influence on Kuhn.

29 We know that Kuhn knew at least one person who knew Fleck personally: Mark Kac, a Polish mathematician who emigrated to the USA. We do not know when Kuhn met Kac, but they certainly interviewed Richard Courant together in 1962. (Kuhn/Kac 1962)

itself, and the resulting difficulties in rendering “fact” independent of “point of view.” (Kuhn 1979, ix)

Perhaps further archival research will confirm Kuhn’s version. However, we cannot be certain that Kuhn was already thinking about “changes in the gestalts in which nature presented itself” before 1949. In any case, since no published material confirms this version, we only have the authority of Kuhn’s own word. An interview that Daniel Goldman Cedarbaum conducted with Kuhn in 1979 indicates that the opposite may have been true:

Also, Kuhn allows that Fleck’s discussion of, and emphasis on, *Gestalt* psychology may have awakened his own interest in the field; that contribution alone would make *G.D.S.F. [Genesis and Development of a Scientific Fact]* enormously significant in the formation of *Structure*. (Cedarbaum 1983, 199 f.)

In the same interview, Kuhn also confirmed that he had read Fleck very carefully:

[T]he many marginal markings throughout his copy of the book suggest that Kuhn read *G.D.S.F.* extremely carefully, if slowly and with great difficulty, a characterization with which he does not quarrel. (Cedarbaum 1983, 199)

So, we know that Kuhn was reading in German, that Fleck’s book interested him enough to order a private copy from Europe in 1949, that he evidently worked through it, highlighting many passages and adding notes in the margins. Thus, his reading of Fleck’s *Entstehung* must have been an important experience for Kuhn.

But then, Trenn writes that Kuhn may have failed to fully understand Fleck’s writing:

But there is equally good reason to believe that his degree of comprehension was somehow limited. This would help to explain why Kuhn continued to confuse thought style and thought collective even in his *Foreword* [to Fleck 1979]. (Trenn 1981)

This assumption is both implausible and hardly supported by evidence. It is implausible because we know that Kuhn was brilliant and intellectually very efficient. We also know that, after his ‘Aristotle experience,’ “the search for best [...] readings has been central to [his] historical research” (Kuhn 1977, xii). We know that “breaking into other people’s heads” was his passion, that he considered himself exceptionally capable of it, and that he later taught his students the requisite interpretive skills. In 1995, Kuhn claimed:

[W]ith the possible exception of Koyré, and maybe not with the exception of Koyré, I could read texts, get inside the heads of the people who wrote them, better than anybody else in the world. I loved doing that. I took real pride and satisfaction in doing it. So, being a historian of that sort was something I was quite willing to be and got a lot of kicks out of being, and did my best to teach other people to do. [...] But my objectives in this, throughout, were to make philosophy out of it. (Kuhn *et al.* 2000, 276)

So why should we believe that he would find it hard to understand Fleck's writings in particular? The assumption of a language barrier is weakly supported by evidence because, if language had been the main obstacle, the presence of a highlighted and annotated copy of Fleck's German original would require specific explanation. Unless and until further evidence can be unearthed to resolve the issues that we have raised here, all possibilities, ranging from coincidental similarity between Kuhn and Fleck to some form of illicit appropriation of Fleck's ideas by Kuhn should be considered and weighted in light of the limited evidence at hand.

10 The Similarities between Kuhn's and Fleck's Theories

In our view, four different hypotheses could explain the similarity of the conceptions of Fleck and Kuhn in light of the circumstantial evidence that we have collected, with possible gradations between some of these hypotheses. A definite choice between them would depend on the collection of additional evidence.

- Hypothesis 1) Both theories were formulated independently of one another. There was no need for Kuhn to specifically acknowledge Fleck as a source.
- Hypothesis 2) Kuhn's reading of Fleck 1935 had an implicit and unconscious impact on his own theory. There was no perceived rationale for Kuhn to acknowledge Fleck to a greater extent than he already did.
- Hypothesis 3) Kuhn independently developed vaguely similar ideas to Fleck's that were elaborated and substantiated on the grounds of his reading of the *Entstehung* but remained insufficiently acknowledged by Kuhn.
- Hypothesis 4) Kuhn plagiarized Fleck, in terms of appropriating central theoretical concepts from Fleck without sufficiently acknowledging his source.

Considering the evidence at hand, hypothesis 1) is the least probable one. Even if we admit that there are some major differences in their approaches, as Kuhn

insisted, the similarities between the remaining core concepts are too many and too detailed to make hypothesis 1) very likely. It looks even less likely in light of the absence of evidence that Kuhn formulated ideas similar to Fleck's before he came across *Entstehung* in 1949. Although he took his first steps towards the writing of *Structure* in 1950, Kuhn reportedly first formulated the concept of a paradigm only in 1958. Hypothesis 1) could only be confirmed by archival documents that would show that Kuhn in fact formulated at least some of the core ideas presented in *Structure* before 1949, but for some reason kept them to himself until around 1958. More precisely, if the 'Aristotle experience' really amounted to the revelation that Kuhn later claimed it to be, these ideas should have somehow materialized between the summer of 1947 and his reading of Fleck's *Entstehung* in late 1949.

Hypothesis 2) is primarily supported by arguments that were formulated by Kuhn himself: A language barrier and, alternatively or in conjunction with the latter, the complexity of Fleck's theory strongly affected the degree to which he was able to understand Fleck's ideas. Some of those ideas would still have found their way into his thinking, but they would have done so most likely in vague and informal fashion. This hypothesis allows for a view of Kuhn being deeply, yet unconsciously, inspired by Fleck's writings. Alternatively, the similarities could be due to Kuhn's "creative misunderstanding" of Fleck – a practice of which Fleck himself discussed various examples.³⁰ Most students of Kuhn's and Fleck's writings as well as most of Kuhn's supporters seem to endorse some variety of hypothesis 2), i.e., the view that Fleck's work had some concrete but vague and partial bearing on the development of Kuhn's theory. In light of the evidence of Kuhn's detailed reading of Fleck that we discussed above, however, this hypothesis appears not very well-supported.

Hypothesis 3) attests to a deeper and more comprehensive understanding of Fleck's work by Kuhn, but retains the possibility that Kuhn developed at least some of the relevant ideas before he read Fleck. On the one hand, one should expect to find evidence in this case that Kuhn formulated such ideas at least in a

³⁰ See for instance: "Out of the understandings and misunderstandings, out of repeated transformations and recastings, a creation arises, during social circulation, for which no original, primary components can be found, just as in the legendary knife for which, in the course of centuries, once the handle was changed, and another time the blade" (Fleck 1986c/[1936], 88). "It may be demonstrated that the conscious intention of individual authors did not play a decisive role here, that it [discovery of the Wassermann reaction] was done both under the influence of and against the intention of the individual. That misunderstandings had as much creative influence as mutual understanding, that decisive changes sometimes took place in the space between authors, and that no one can actually be considered an author." (Fleck 1934, 204)

tentative form before his encounter with *Entstehung* in 1949. On the other hand, one should be able to observe how these ideas transformed over time in a fashion that was evidently informed by Fleck. While the latter part of the requisite evidence is presented in Baldamus' above-cited textual analyses in particular, these analyses are not informative with respect to the former part of the evidence. As already indicated, the formative period in question would have occurred between Kuhn's 'Aristotle experience' in the summer of 1947 and his reading of Fleck in late 1949. However, any evidence on either side of the equation would still leave open the question as to why Kuhn did not credit Fleck for his contributions in accordance with established academic standards – save his possible fears of appearing unoriginal – given that he did not publish any previous independent conception of the relevant ideas in timely fashion.

Hypothesis 4) differs from hypothesis 3) in that it does not expect evidence that Kuhn developed his key ideas before having read Fleck, because before 1949 there was only a particular 'Aristotle experience' whose philosophical and sociological implications Kuhn was not fully able to grasp in the absence of the concepts that he found in Fleck, and without which he would not have been in a position to formulate his theory of paradigms and scientific revolutions. Hypothesis 4) is the most parsimonious of the hypotheses under investigation because it does not expect to find something for whose existence there is not already some evidence. In normative terms, however, this hypothesis is the most difficult to digest: it charges a public intellectual of great standing with having purposefully appropriated another thinker's intellectual property, revealing only as much of Fleck's influence as was undeniable while constructing a narrative or 'legend' around his formative experience in order to distract from the genuine import of Fleck on his work.

In this paper, we collected the following circumstantial evidence for Hypothesis 4):

- We are not yet aware of any evidence that Kuhn developed his key ideas earlier than he found them in Fleck's work, most probably in late 1949, apart from Kuhn's own later claims that he did so.
- Kuhn bought his own copy of Fleck's book, and his highlighting and annotations testify to a careful reading (most likely in 1950 and possible in later years as well).³¹

³¹ We do not know whether Kuhn read *Entstehung* after 1950; perhaps archival research will help to clarify this. The copy borrowed by Kuhn in 1949 is most probably still in Harvard Library (Widener Library, Old Widener, Med 1759.3.85). We do not know where the private copy that Kuhn bought in 1950 currently is – it may be in the family collection, and it may be in Massachusetts

- Kuhn was able to read German, and Fleck wrote proper German, which weakens Kuhn's own argument from a language barrier.
- Kuhn prided himself on his ability to understand other texts, and there is much to indicate that these statements are not completely unfounded, which weakens an argument from limited understanding.
- Kuhn did not quote Fleck even once; of his own accord, i.e., when not prompted by others, he wrote only three sentences about Fleck (in 1962), while he devoted entire texts to other influences and predecessors.
- Baldamus reacted to the similarity of both conceptions by implicitly suggesting plagiarism and collecting some circumstantial evidence to support this claim.
- After the publication of the English-language edition of Fleck's book, the editors felt compelled to authoritatively claim that Kuhn had *not* committed plagiarism, despite no such claim having been published at the time, by Baldamus or anyone else.
- There is no evidence of Kuhn's self-ascribed attempt to make Fleck's *Entstehung* available in English, and he was reluctant to cooperate with the editors of the translation.
- A diachronic comparison of Kuhn's own descriptions of his 1947 'Aristotle experience', which he later referred to as his main source of inspiration, shows that Kuhn began to ascribe a special revelatory and inspirational meaning to this event only around the time when he learned that the translation of Fleck's book into English was being prepared. It was then that the motif of the 'Aristotle experience' was actually born.

We are currently unable to resolve which of the proposed hypotheses 2, 3 or 4 is the most probable. However, the circumstantial evidence at hand indicates that Hypothesis 4) should be seriously considered and deserves to become the focus of further research. Even if this 'iconoclastic' hypothesis has to be refuted, only research that is conducted in view of it in an open-minded fashion will make it possible to collect and acknowledge evidence to the contrary, and to settle for one of the other hypotheses instead. Research on Fleck's possible influence on Kuhn should further be complemented by research on the influence on Kuhn by other authors, especially the French epistemologists Gaston Bachelard, Alexandre Koyré, Hélène Metzger, Émile Meyerson and Georges Canguilhem, as well as James Bryant Conant, Michael Polanyi and Ludwig Wittgenstein. Moreover,

an analysis of shared influences on Kuhn and Fleck is needed – for instance of art history (Alois Riegl and Heinrich Wölfflin) and *Gestaltpsychologie* (Wolfgang Köhler, Kurt Koffka).

Concluding Remarks

We have been arguing in this essay that there has been an intellectual influence of Ludwik Fleck on Thomas Kuhn that has not been sufficiently analyzed to date, and that Kuhn's own 'Aristotle experience' narrative might have contributed to obscuring that influence. Of course, one might think that it does not matter much what Kuhn took from Fleck and whether he plagiarized him, because ultimately it is impossible to deny that it was Kuhn who had the most direct and far-reaching influence on contemporary thinking about science. Kuhn was the one who wrote the right text at the right time and was working at the right place at the right time. Fleck's influence is negligible plainly because everybody read Kuhn, not Fleck.³² Of course, one might think that the one who was successful while standing on the shoulders of other scholars is the one who carries the day. However, endorsing this notion would be cynical and intellectually defeatist. If there is any reasonable evidential support for a suspicion that Kuhn illicitly appropriated his most important philosophical ideas from Fleck, then this possibility must be considered. The case should be investigated and all pros and cons made explicit, not least in order to possibly clear Kuhn from such suspicions. This is not (only) a matter of intellectual property rights but also concerns a question with significant normative implications for the history, philosophy, and social studies of science: Does the success of scientific revolutions, and in particular the success of the scientific revolution that supposedly provided us with epistemic access to the phenomenon of scientific revolutions in general, depend on one actor being more powerful, better connected or simply more fortunate than another? Ironically perhaps, if the answer is yes, there will be no place for the notion of a solitary genius in scientific research.³³

³² Collins 2012, 420 f., writes that Kuhn was not "as original as we all once thought", because "Fleck did it first and, in a lot of ways did it best", while adding that finally Kuhn was the one "responsible for telling the history of science in a thought-collective kind of way", because he was the one who "created the social change by coming along at the right time and writing in English".

³³ The authors would like to thank both the anonymous reviewers and Artur Koterski, Zbigniew Król, Mauro Condé, Wojciech Sady, Michał Stelmach and Josef Mitterer as well as Karin de Boer, whose comments helped to improve this article.

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