

Chapter 9

Two Encounters

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The Split of Rationality

By the middle of the twentieth century, the relation between philosophy and history of science may be characterized as amounting to a split of rationality. Whereas philosophy of science was dominated by a focus on the analysis of language and methodology, taking them as embodiments of an ahistorical scientific rationality, history of science paid attention to ideas, events and their more or less contingent circumstances without critically examining this normative rationality, let alone substituting it with its own form of historical rationality. Thomas Kuhn, with the publication of *The Structure of Scientific Revolutions* in 1962, is often seen as having closed this divide, in one sense or another, by integrating the different perspectives on science, the normative and the historical one, into one unifying framework.

By the mid-1930s, philosophers of science such as Rudolf Carnap, but also Moritz Schlick under the influence of Ludwig Wittgenstein, had retreated toward the logical analysis of language. The split of rationality represented by this retreat becomes particularly evident in the episode recounted in the following, brief written encounter between Schlick and the Vienna Circle's first important critic, the Polish bacteriologist, doctor and historian of science Ludwik Fleck.

From a modern perspective, this encounter strikingly anticipates the far-reaching conflicts between different perspectives on science, as they would come to determine the discourse throughout the following decades up until the present day. And yet, at the same time, what becomes evident in this encounter is the willingness to continue the dialogue and, in the situation of a political crisis rapidly coming to a head, the eagerness not to abandon the common battle for scientific rationality, even in the face of widely diverging criteria for science.

A Letter from Fleck

In March of 1934, shortly after the end of the winter semester at the University of Vienna, Schlick finally found the time to answer a lengthy letter from Fleck. In September 1933, Fleck had sent him an extensive manuscript under the title of “The Analysis of a Scientific Fact, Outline of a Comparative Epistemology,” asking for an evaluation and perhaps even its submission for a prize offered by the *Soziologische Gesellschaft* in Vienna. Evidently Fleck, who had no connection to the German scientific community outside of his own area of expertise, as he himself states in this letter, also had hopes that the head of the *Vienna Circle* might assist him in the publication of the manuscript. It seems that Fleck considered Schlick to be especially open-minded among the German philosophers, even though the latter must have considered Fleck’s approach to a “comparative epistemology” based on data from the histories of medicine and biology to be rather odd.

At the same time, however, Fleck sought to establish a dialogue with Schlick. He raised questions concerning the long-term processes of the transformation of knowledge, the connection between the established inventory of knowledge and the individual epistemic act, and concerning the dependence of the cultural evolution of knowledge on the social structures of thought collectives and complex systems of knowledge. Fleck was also not reticent about expressing his skepticism toward traditional epistemology, which was centered around the individual relation between subject and object, represented by the *Vienna Circle* and especially Schlick: “I could never shake the impression,” Fleck wrote to Schlick, “that epistemology examines not knowledge as it actually occurs, but its own imagined ideal of knowledge, which lacks all its real properties.”¹ And he continued his criticism:

Already the choice of data, being almost exclusively physics, astronomy or chemistry, seems to me to be mostly misleading, since the origin of elementary insights into physics dates back so far that we can only investigate it under great difficulties—and the more recent insights are to such a degree, as it were, ‘systematically biased’, so greatly suggested to all of us through our educational background and scientific tradition, that I must find them inappropriate as well as a principal target for investigation. The statement that all knowledge originates in sensations is misleading—because the plurality

¹“Ich konnte mich nie des Eindrucks erwehren, in der Erkenntnistheorie werde zumeist nicht die Erkenntnis, wie sie faktisch sich darbietet, untersucht, sondern ihr imaginiertes Idealbild, das der realen Eigenschaften entbehrt.” Ludwik Fleck to Moritz Schlick, 5 September 1933, Moritz Schlick estate, Noord-Holland Archief, inv. no. 100/Fleck-1.

of all human knowledge stems quite simply from textbooks. [...] Finally, the historical development of knowledge shows some remarkable common aspects as well, such as for instance the particular stylistic closeness of the respective systems of knowledge, which demands an epistemological investigation.

These considerations prompted me to treat a scientific fact from my area of expertise epistemologically, whereupon the aforementioned manuscript emerged.²

The Challenging Manuscript

In his manuscript, Fleck intended to give an introduction to the theory of thought styles and thought collectives. Fundamentally, he assumed that the development of knowledge was socially determined. This social determination was substantiated in the way a community processed and judged their perceptions, both conceptually and factually. By no means, however, was this meant to dismiss rationality! Rather, it was Fleck's intention to explore to what degree historically examined phenomena were amenable to a rational appraisal, despite their socio-cultural circumstances.

Against this background, Fleck had dealt with the origins and the long-term development of medical knowledge in his historically comparative study, focusing on a specific example. On the basis of the relationship between the concept of syphilis, which dates back to the end of the fifteenth century, and the so-called Wassermann reaction, which (through the cooperative work of a thought collective) for the first time in history gave an operationalizable identification of syphilitic blood, Fleck was thus able to demonstrate the dependence of one of the most well-established medical facts of his time on several social, psychological, and cultural factors. Thus it seemed that the origin and development of medical knowledge displayed a number of characteristics left unaccounted for by the epis-

²“Schon die Wahl des Materials fast ausschliesslich Physik, Astronomie oder Chemie scheint mir meist irreführend zu sein, denn das Entstehen der elementaren Erkenntnisse der Physik liegt so weit zurück, dass wir es nur schwer untersuchen können – und die neuern Erkenntnisse sind so sehr sozusagen »systembefangen«, so sehr durch die schulmässige Vorbildung und die wissenschaftliche Tradition uns allen suggeriert worden, dass ich sie als prinzipielles Untersuchungsmaterial ebenfalls für ungeeignet halten muss. Der Satz, alle Erkenntnis entspringe den Sinneseindrücken, ist irreführend, – denn die Mehrzahl der Kenntnisse aller Menschen stammt einfach aus den Lehrbüchern. [...] Endlich finden sich auch in der historischen Entwicklung des Wissens einige merkwürdige allgemeine Erscheinungen, wie z.B. die besondere stilmässige Geschlossenheit jeweiliger Wissenssysteme, die eine erkenntnistheoretische Untersuchung fordern. Diese Betrachtungen veranlassten mich, eine wissenschaftliche Tatsache aus meinem Fachgebiet erkenntnistheoretisch zu bearbeiten, worauf das erwähnte Manuskript entstand.” Ludwik Fleck to Moritz Schlick, 5 September 1933.

temology and philosophy of science of the *Vienna Circle* around Schlick, which focused on the analysis of language and methodology.

For the emergence and constitution of the fact in question, the ideas that informed the understanding of syphilis over centuries were as equally important as the socially transmitted familiarity with the material under investigation. The idea of syphilis, widespread through different social strata, as the “carnal scourge” and the “foul syphilitic blood,” was indeed deeply ingrained in the collective memory. And the familiarity with the material under investigation was only achievable for the scientific practitioner after long years in the bacteriological laboratory as a member of a community steeped in tradition. Thus the cooperative nature of human knowledge was obvious to Fleck, as it presented itself particularly in the collectively arranged dissemination of theoretical and practical resources over generations of scientists, as well as in the transformations of knowledge during the cultural evolution of structured thought communities. This point of view was, first and foremost, the result of observations and reflections Fleck was able to make in his capacity as physician and head of the laboratory in the medical business of his hometown Lwów since the 1920s. He writes:

Experience gained over several years of working in the venereal disease section of a large city hospital convinced me that it would never occur even to a modern research worker, equipped with a complete intellectual and material armory, to isolate all these multifarious aspects and sequelae of the disease from the totality of the cases he deals with or to segregate them from complications and lump them together. Only through organized cooperative research, supported by popular knowledge and continuing over several generations, might a unified picture emerge, for the development of the disease phenomena requires decades. Here, however, training, technical resources and the very nature of collaboration would repeatedly lead research workers back to the historical development of knowledge, since the bonds of history can never be cut.³

³“Infolge mehrjähriger Erfahrung in einer großstädtischen, venerischen Spitalsabteilung bin ich überzeugt, es könne auch ein mit allem Denk- und Sachrüstzeug bewaffneter, moderner Forscher nie darauf kommen, alle diese mannigfaltigen Krankheitsbilder und Krankheitsfolgen aus der Gesamtheit der vorkommenden Fälle auszuscheiden, abzusondern von Komplikationen und zu einer Einheit zu verbinden. Erst organisierte Forschungsgemeinschaft, unterstützt vom Volkswillen, und über einige Generationen dauernd, vermöchte das Ziel erreichen – schon deshalb, weil die Entwicklung der Krankheitsphänomene Jahrzehnte braucht. In diesem Falle aber würden Vorbildung, technische Mittel und die Art der Zusammenarbeit die Forscher immer wieder auf den alten Pfad der geschichtlichen Erkenntnisentwicklung leiten. Also ist Auflösung historischer Bindung keinesfalls möglich” Fleck (1979, 22).

The Missed Opportunity

Fleck's manuscript, written in a rich and elegant German, was the basis for his groundbreaking book entitled *Genesis and Development of a Scientific Fact*. Schlick had read this manuscript with great interest. In his answer to Fleck in March of 1934, he recognized it as "a first-rate scientific accomplishment."⁴ Nevertheless, he could not agree with Fleck's views on the epistemology and philosophy of science. Moreover, without professional support from a medical authority well versed in the history of medicine, he did not feel that he was in a position to recommend the book to a publisher.

It was certainly not simply the case that the realm of bacteriology and serology was unfamiliar to Schlick, but the historicization of scientific knowledge, as it was inherent in Fleck's socio-cultural perspective, must have seemed to be a threat undermining the *Vienna Circle's* view of the role of science as a model for rationality. Thus Schlick's normative rationality of methodologies and theories, based on the analysis of language, stood in opposition to Fleck's historical rationality of concrete thought collectives. And yet Schlick passed the manuscript on to *Springer-Verlag*, a publisher he had close ties to, before leaving for a longer sojourn on the Amalfi Coast at the end of March 1934.

We may assume that Schlick planned to have the manuscript printed in the series *Schriften zur wissenschaftlichen Weltauffassung*, which he published himself together with the physicist Philipp Frank, although nothing further is known about this decision. We are not in possession of the letter from Schlick to *Springer-Verlag*, which makes it impossible to ascertain conclusively if Schlick had planned to publish the book in his own series or whether he had other plans for it. Nor is it known whether the medical authority Schlick had called for had been consulted during the decision, or if indeed anyone else had been asked. In his letter to Fleck, Schlick had mentioned the sociologist and economist, Franz Oppenheimer as a possible consultant, who incidentally had practiced medicine for years in Berlin. He was also an acquaintance of Albert Einstein's, and was then working as a guest lecturer in Palestine.

In the end, *Springer-Verlag* decided not to publish the book, presumably for "external" reasons, that is, when viewed alongside the previous publications in the series, the publisher likely missed the austerity of form and the stringency of argumentation in Fleck's work. Instead, they recommended publishing the text in an abridged form in a journal, which might have been provoked by Fleck's rather literary style and his way of presentation which was, superficially speaking, almost like a collage—all of which was rather unusual for current "scientific

⁴"eine wissenschaftliche Leistung hohen Ranges." Moritz Schlick to Ludwik Fleck, 16 March 1934, Moritz Schlick estate, Noord-Holland Archief, inv. no. 100/Fleck-2.

series.” In a letter from Otto Lange, founder and director of *Springer-Verlag* (Vienna), to Schlick, he comments:

I have in the meantime had a look at the work by Dr. *Fleck*, analysis of a scientific fact, which you were kind enough to relay to me. It does not seem to me to be suitable for publication in book form. I would advise the author to perhaps publish it in a journal in abridged form.⁵

Even though Fleck’s book was not published by *Springer* – it was printed in 1935 by the *Verlag Benno Schwabe & Co* in Basel, with a famously rather ineffective reception – Schlick, too, sought dialogue with Fleck. Clearly, Schlick and Fleck admired each other despite the differences of their views. Schlick, for instance, praised “the richness of ideas, the scholarship, the sagacity” of Fleck’s arguments, “and the high intellectual standards of the whole thing.” Fleck, on the other hand, likely viewed the author of the *General Theory of Knowledge*⁶ as a partner for his historically comparative studies into scientific rationality.

However, both Schlick and Fleck also recognized the challenges that were inherent in the considerations of the other. At the same time, they must have also been painfully aware that the space available for a discussion was, due to the political circumstances, becoming smaller and smaller. In the end, due to external circumstances, Schlick and Fleck never got the chance to enter into a dialogue. In light of the existing divide between the philosophy of science and cultural studies, we may see this as a *missed opportunity*.

A Second Encounter

But from our point of view, this is not the end of the story. Is it, at least in principle, possible, from Schlick’s perspective, to take on the questions that Fleck raised in his letter, *or* was this, after all, an encounter between mutually incompatible worlds? And what is the relation between Schlick’s attempt to defend science’s claim to objective knowledge and Fleck’s emphasis on the socio-cultural and cultural-historical contexts of the long-term development of knowledge? Can both views be reconciled: one based on the reflective use of reason, intended to secure science’s claim to validity, and the realization that science is through and

⁵“Die Arbeit Herrn Dr. *Flecks*, Analyse einer wissenschaftlichen Tatsache, die Sie mir freundlichst übermittelt haben, habe ich mir inzwischen angesehen. Sie scheint mir für die Ausgabe als Buch nicht in Betracht zu kommen. Ich würde dem Autor empfehlen, sie vielleicht in gekürzter Form in einer Zeitschrift zu veröffentlichen.” Otto Lange (*Springer-Verlag*, Vienna) to Moritz Schlick, 14 April 1934.

⁶Reprinted in a second edition in 1925.

through part of our imperfect and ever-changing *Lebenswelt*? These questions are not merely relevant for a historiography of science (a counterfactual one at that!), but may even take on a certain urgency in view of the role of science for society that has grown in both importance and global extent ever since the exchange between Schlick and Fleck.

The resilience of these questions becomes evident from a second, almost symbolic encounter between an analytic and a historical perspective on science, between Rudolf Carnap and Thomas Kuhn. In contrast to Carnap's purely logico-linguistic considerations of science, Kuhn, in *Structure*, stresses its socio-cognitive and historical dimensions, and within this context specifically deals with the dynamics of theory changes. At first glance, this seems to connect him to Fleck's investigations of structured thought collectives and the transformations of knowledge systems. But, on closer inspection, Kuhn's perspective on the social dimension of science is narrower than that of Fleck. In fact, he primarily focuses on the "esoteric circles," constituted by scientific communities of highly specialized experts.

This perspective aligns him with Carnap and his view of linguistic frameworks as critical tools of science. They evidently shared an underlying conception of science as a world of its own, characterized primarily by struggles within the scientific community about the most appropriate scientific theory. This narrower focus on science and its practitioners perhaps also represents one of the reasons for Kuhn's ambivalent reaction to Fleck's book:

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 still don't with [Fleck's] »thought collective.«
(Kuhn 2000, 283)

Kuhn and Carnap's agreement on what one might call the instrumental rationality of science becomes perhaps nowhere as obvious in a letter Carnap sent to Kuhn shortly after the completion of *Structure*, which was the last book to appear in the famous Vienna Circle series *International Encyclopedia of Unified Science*, created by Otto Neurath, Charles Morris and Carnap:

Simultaneously I am returning your manuscript »The Structure of Scientific Revolution«. [...] I am convinced that your ideas will be very stimulating for all those who are interested in the nature of scientific theories and especially the causes and forms of their changes.

I found very illuminating the parallel you draw with Darwinian evolution: just as Darwin gave up the earlier idea that the evolution was directed towards a predetermined goal, men as the perfect organism, and saw it as a process of improvement by natural selection, you emphasize that the development of theories is not directed toward the perfect true theory, but is a process of improvement of an instrument.⁷

Looking back at this exchange 50 years after the publication of *Structure*, it is evident that this apparent reconciliation between historical and the philosophical points of view was premature, also in view of what Kuhn did not take over from Fleck. Moreover, Kuhn's image of science, to use Yehuda Elkana's term, does not take into account many dimensions of the scientific development that since have become central to historical and philosophical debates. Such as its embedding within a larger world of knowledge, its social construction, its material culture, its dependence on local contexts as well as on long-term processes, its implication in military and economic ventures, but also its role in generating values and its growing significance for human survival.

Even 50 years after the publication of *Structure*, the split of rationality has thus not been overcome. We are still confronted with the split between the view, if not the vision of science as the best model of rationality available to us, generalizable to other spheres of human activity as well, and the view of science as a deeply contingent, historically shaped human enterprise as any other, an enterprise that we can only practice, administer or describe.

Therefore, it is worthwhile to revisit the instances where this split became visible in the past, such as in the exchanges between Schlick and Fleck, or between Carnap and Kuhn. It then becomes evident that science's claims to rationality must remain speculative without consideration of the concrete socio-cultural and historical dimensions of this rationality, but also that any approach that reduces science to its purely instrumental character or that fails to take scientific rationality and its relevance to global human concerns seriously would ultimately become irrelevant and even cynical, since it would abandon, against better knowledge, our struggle for reason, and not only within science.

Acknowledgement

We would like to acknowledge Jendrik Stelling for contributing to the translation.

⁷Rudolf Carnap to Thomas Kuhn, 28 April 1962.

References

- Fleck, L. (1979). *Genesis and Development of a Scientific Fact*. Ed. by T. J. Trenn and R. K. Merton. Chicago: The University of Chicago Press. First published in German in 1935 as *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv*.
- Kuhn, T. S. (2000). *The Road Since Structure: Philosophical Essays, 1970–1993, with an Autobiographical Interview*. Ed. by James Conant and John Haugeland. Chicago: The University of Chicago Press.