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Message in a bottle from ‘the crisis of reality’: on Ludwik Fleck’s interventions for an open epistemology

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Abstract

The continuing relevance of Ludwik Fleck’s work, above its status as a classic in science studies, lies in his reflexive conceptualisation of an open epistemology. As early as 1929, in the midst of a broad debate about a ‘crisis’ of science, Fleck offered a socio-historical analysis of the widely assumed crisis in form of an epistemology. He presented his argument, which was in itself an interrogation of the metaphysical foundations of science, as a reflexive and political intervention to this debate. His book of 1935, *Genesis and development of a scientific fact*, in which he developed the notion of the thought collective, can be read in a similar vein as a functional analysis of science under National Socialism. His traumatic experiences during this period fostered his drive for science, democracy and epistemology. When he returned to the epistemological debate in 1946, he sharpened the presentation of his argument, presenting it rhetorically as an ironic political dialogue. This surprising text unfolds as an epistemological intervention by a participant observer. In this respect, Fleck’s epistemology, combining the historical analysis with reflexive intervention, is still a message in a bottle.

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1. Introduction

In one of the Polish epistemological papers following his German monograph, Ludwik Fleck in 1936 postulated the double historical situatedness of any reflec-

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tion, with regard to its context of production and its intended audience: ‘Each formulated thought, which is destined for real use, bears a sign of the producing unit and an address of destination’ (Fleck, 1986c, p. 85). The first half of this postulate was in close agreement with the contemporary sociology of science as, for example, developed by Karl Mannheim (in particular his ‘*Seinsverbundenheit*’) and has since become one of the cornerstones of social constructivism. Fleck’s work and especially his 1935 book *Genesis and development of a scientific fact* have rightly become classic in science studies. The historical problem remains, however, that the book acquired this status retrospectively, after social studies of science were established as scientific discipline quite independently of Fleck’s work. It is by and large a neglected classic, rediscovered and celebrated belatedly as a forerunner to contemporary preoccupations in sociology of science. In this reading, Fleck has turned into ‘a Gregor Mendel of history and philosophy of science’ (Bonah, 2002) envisioning a constructivist epistemology ahead of its time and doomed to failure for various political and historical reasons. John Wettersten once argued with regard to what he called ‘the Fleck affair’ that fashion is as unfair as ignorance and that we had better ‘abandon efforts to use the making of amends the occasion for making intellectual society seem fairer than it is’ (Wettersten, 1991, p. 475). Is there an alternative to simply stating that Fleck was, due to the political circumstances, ignored throughout his lifetime and became fashionable too late? Such an account may well be a fair description of Fleck’s reception thus far, but it falls short as a critical reflection on Fleck’s epistemological writing. As I want to argue in this paper, his epistemological writing addresses us even today; it has an ‘address of destination’ in a political sense which it has not yet found.

Fleck’s postulate of the dual implication of any thought sounds like a distant echo from the social constructivists’ corner—the logical positivists would call it a distorted echo—of Reichenbach’s famous distinction between the contexts of discovery and justification. This distinction was made famous in Reichenbach’s *Experience and prediction* (1938), but it may well have been that Fleck was aware of the development of this argument in the debates among the Vienna circle (Howard, 2002) while writing his own work. In his monograph of 1935, Fleck had addressed the positivism of the Vienna circle and he had also written to Moritz Schlick asking for the opportunity of publication.¹ As is well known, Fleck’s book did not find its intended audience and ‘made virtually no impact’ (Harwood, 1986, p. 173). There is hardly any traceable resonance of the book among the Vienna circle; even after the war, when Fleck sent his book to prominent philosophers of science, his work was still ignored, until Thomas Kuhn (1962, p. viii) mentioned it in the introduction to his *Structure of scientific revolutions* (Graf & Mutter, 2000).

It would be an interesting thought experiment to speculate about the effects of Fleck’s epistemology upon the philosophy and sociology of science had it been recognized earlier (Merton, 1985). However, it is not my aim to identify the inten-

¹ Dated September 1, 1933, Fleck’s letter is reprinted in the exhibition catalogue by Arens et al. (2002).

ded audience of Fleck's writing. The now flourishing scholarship on Fleck has done much to clarify the socio-epistemic context of his work. Thomas Schnelle (1982; 1986) positioned him in the Lwów philosophical school debating perspectivist and conventionalist stances in the philosophy of science. Ilana Löwy (1986, 1990) complemented his analysis with a detailed reconstruction of the rich contemporary philosophy of medicine in Poland, thereby identifying a context with particularly close ties to Fleck's professional background in medicine. In a joint paper with Gad Freudenthal (1988), she argued furthermore that the gradual radicalization of Fleck's anti-reductionist philosophy of medicine into a social constructivist epistemology went hand in hand with his meandering aspirations for a scientific career despite his rather marginal academic position as a practising serologist. We now have a detailed picture of the context in which Fleck's writing originated which does not really demand further evaluation. By alluding to Reichenbach's famous distinction I want to suggest a reflexive reading of a selection of Fleck's papers in order to demonstrate how his writings engaged with the philosophical concept of an ahistorical context of justification by suggesting an alternative open epistemology which recognized the historical situatedness of scientific concepts. In his first epistemological paper in German, in 1929, Fleck made clear the observer-dependence of scientific objects: 'To observe, to cognize is always to test and thus literally to change the object of investigation' (1986a, p. 53). Fleck took this statement seriously, I will argue, and applied it equally to his sociology of science. He thereby conceptualized an open epistemology which changed his socio-historical analyses into epistemological interventions. As we shall see, the trajectory of Fleck's life reinforced this idea as a continuing effort towards a democratization of science (Fleck, 1986d). I will develop my argument in three steps. First, I want to contextualize Fleck's early paper from 1929 (Fleck, 1986a) as a specifically positioned reaction to a rather broad intellectual debate of the late 1920s. I will then go on to his monograph of 1935 (Fleck, 1979) and very briefly discuss the interrelations of its framework with its contemporary reception. Finally, I will discuss Fleck's paper from 1946, his first epistemological writing after the end of the war (Fleck, 1986c), as a performative exemplification of his open epistemology.

2. The 'crisis of reality'

Fleck's paper of 1929 (Fleck, 1986a) appeared in the leading German science magazine *Naturwissenschaften*, the journal of the Kaiser Wilhelm Society, and was the response by an almost unknown Polish microbiologist to an earlier article in that journal with the catchy title 'The crisis of reality'. With such a heading, the modern reader would probably first associate the lingering political crisis of the Weimar Republic and its incipient dissolution, or perhaps the worldwide economic depression. The more philosophically minded might think of the shock waves caused by the conceptual revolutions in physics as they reached the general public during these years in a stream of articles and lectures questioning realism. In 1928 and 1929, for example, *Naturwissenschaften* published Niels Bohr on the con-

sequences of quantum theory, Erwin Schrödinger on the statistical concept of causality, and Philipp Frank on the epistemological implications of modern physics.

These articles are indicative of a much broader intellectual debate in the late 1920s concerning fundamental epistemological questions, of which the theoretical developments in physics were just one aspect. The psychologist Wolfgang Köhler contributed to the debate about scientific realism in *Naturwissenschaften* a condensation of his *Die physischen Gestalten*. From diametrically opposed points of view, the psychologists Karl Bühler and the philosopher biologist Hans Driesch tried to diagnose a crisis of psychology (Bühler, 1927; Driesch, 1926). In biology, Ludwig von Bertalanffy opened his *Kritische Theorie der Formbildung* with a lengthy discussion of the ‘crisis in biology’ (Bertalanffy, 1928, pp. 1–57). Not long after, Pascal Jordan’s contribution to *Naturwissenschaften* bridged these debates by discussing the consequences of quantum mechanics for biology and psychology (Jordan, 1932). When the psychiatrist Oswald Bumke was elected rector of the University of Munich, he delivered his inaugural address on November 24, 1928, under the title ‘A crisis of medicine’ (Bumke, 1929). Crises everywhere, but this was a broad intellectual upheaval rather than an economic depression or political dissolution.

The abounding crisis rhetoric must not be mistaken for a single and coherent discourse; it was a concert of discordant and overlapping voices.² Whereas the physicists debated theoretical developments within physics and their implications for epistemology, the psychologists discussed the dissipation of their discipline into ever more specializations, and the biologists argued over the lack of a coherent framework for their science. The crisis talk in medicine had another twist again (Harrington, 1996; Timmermann, 2001; Bonah, 2002). It was a concoction of a holistic critique of an allegedly inhumane techno-medicine with a lament about the socioeconomic crisis of the insurance system and the resulting fear of an imminent impoverishment of physicians. Looked at from the more general perspective of its implications for a scientific *Weltanschauung*, these debates amounted for some writers to a ‘crisis of mankind’ (Reibnitz, 1929), a ‘crisis of present times’ (Dennert, 1928), or *Die Krisis des abendländischen Menschentums* (Wust, 1927), causing philosophers like Max Scheler to rethink ‘Man’s place in nature’ (Scheler, 1928). These revolving debates prepared the background for Edmund Husserl’s unfinished *Crisis of European sciences* and furnished it with its first title, given that Husserl started this project with a lecture ‘Die Krisis the europäischen Menschentums und die Philosophie’ delivered to the Kulturbund in Vienna in 1935 (Husserl, 1970).

Such a brief sketch, insufficient as it is for a full analysis of the outlined phenomena, may suffice to indicate two points. First, the article by Kurt Riezler (1927) to which Fleck responded was deeply embedded in a rich debate on a crisis in the sciences with widely acknowledged consequences for the traditional concept of reality. Second—the differences in the various disciplines notwithstanding—it was, in gen-

² Fritz Ringer (2000) has argued that this intellectual crisis was decisive for the development of Mannheim’s sociology of science. The activities of the Vienna circle were no doubt another reaction to this crisis in form of a search for a new unity of science (Stadler, 1991).

eral, not a crisis *of* scientific productivity but a crisis *resulting from* scientific productivity. Apart from the somewhat differently framed crisis debate in medicine, it was a crisis due to the success of the sciences which had led to their departure from a coherent and plausible world view, their split into conflicting branches, the development of counterintuitive concepts, and the construction of theories undermining common sense assumptions.

Kurt Riezler's contribution (1927) to this disparate literature on the 'crisis of reality' started with the traditional distinction between a 'primary' reality of sensorial impressions, a 'second' reality of scientific representations and a 'third' objective or absolute reality, towards which science should gradually evolve. For the German neo-Kantian philosopher Riezler, the contemporary theoretical developments in the sciences, especially in physics, provoked a 'crisis of reality', because of the introduction of statistical explanations as a replacement for classical causality, the diversification of the sciences into incommensurable branches instead of their gradual unification, and the tendency towards abstract mathematical formalizations instead of concrete concepts. All these factors led to a gradual undermining of the idea of an absolute reality. The very progress of the sciences threatened the concept of a guided scientific progress in a metaphysical sense. On the one hand, Riezler took seriously the progress of the sciences along divergent trajectories, concluding that this meant that there was no easy solution to the epistemological problem: 'The situation is quite bizarre and of an indescribable confusion. Our knowledge is sufficient to challenge every belief up to now, but does not suffice to create a new one' (Riezler, 1929, p. 708). On the other hand, he did not want to abandon the idea of an absolute reality, which would ultimately unite the heterogeneous theories in the various branches of scientific activity. To him, the unification of the sciences could not come from the emergence of a scientific super-theory, not even from the most advanced branches of theoretical physics. Quite the contrary, theoretical physics with its strange opposition of chaos at the micro-level and order at the macro had brought the limits of a physicalistic reductionism to the fore, thereby opening the way for other forms of explanation such as historical processes or biological functions. The search for an absolute reality in terms of static and eternal laws, he argued, would never overcome the partiality of scientific theories and would never be able to account for the dynamic nature of scientific progress. Instead, Riezler offered a solution to the crisis he had diagnosed in the form of a parable: the search for an absolute reality was itself part of a game which made sense only by active participation, which might even include an alteration of the game's rules. Sense had to be created: 'The true reality you are looking for is the sense of the game and the engagement of the participants [...] The world is not finished, but still in the making' (ibid., p. 712). '*Menschengeschichte*', human history, was Riezler's response to the crisis of reality:

Anybody declaring in earnest the metaphysics of the newest sciences will be called a barefaced joker [*schamloser Witzbold*]. Until recently, he would have earned scorn and derision, but nowadays, the crisis of the traditional concept of reality will provide him with a sceptical, yet patient audience. (Riezler, 1929, p. 712)

Fleck's response to Riezler was worse than that of the imagined *schamlose Witzbold*—and he earned neither scorn nor a patient audience but simple ignorance. Fleck replaced the nicety of the game parable of competing scientific endeavours with a scientific critique of Riezler's presupposed realistic ontology. He undercut the whole discussion with an irritating combination of sociologically grounded knowledge and epistemological critique. He argued against the diagnosis of a 'crisis' and called for what he labelled a 'physiology of cognition' (*Erkenntnisphysiologie*). In a slightly strange use of this notion, Fleck did not refer to Helmholtz's project of a physiologically grounded epistemology, but instead offered a social explication of the intellectual functioning of a primarily collective cognizing body. To him, the social predated any act of knowing in every instance, and there was no point zero for a theory of knowledge to start with, neither in the senses nor in the mind. Since every act of knowing relates to previous knowledge, tradition and education, epistemology for Fleck had to start with 'the social and cultural–historical context' (Fleck, 1986a, p. 48). In his view, knowledge did relate to a realm of nature, but in all its concrete materializations, knowledge was moulded and shaped by its own history and by its socio-cultural context. The recent developments in physics, demonstrating the experimenter-dependence of observation, as well as the 'mangle of practice' (Pickering, 1995) in medicine, of which he had first-hand experience from his work in the field of serological testing, served Fleck as welcome examples for his idea of the primacy of the social in epistemology. He replaced Riezler's triple-layer model of reality with an epistemology which drew its consequences from the very fact that 'wherever and whenever we touch on something, we are always in the very midst and never at the beginning of cognition' (*ibid.*, p. 48). Fleck removed the ground for any possible distinction between Riezler's first and second realities. And with that, the assumption of a third reality, of which the concepts of the second reality should become ever more truthful representations, no longer made sense. The postulate of an absolute reality was meaningless, since every possible representation of this reality was just another way of constructing reality, moulded upon pre-existing concepts and experiences. Fleck replaced the triple-layer model with a manifold of partly overlapping and partly conflicting realities:

Every knowledge has its own style of thought with its specific tradition and education. Out of the almost infinite multitude of possibilities, every way of knowing selects different questions, connects them according to different rules and to different purposes. (Fleck, 1986a, p. 49, translation adapted)

The contemporary conceptual developments in the sciences questioned the traditional theory of knowledge and thus prepared the ground for a coherent social epistemology. Instead of diagnosing a crisis, Fleck offered a socio-epistemic look *through* the crisis, taking the nervous and often loose talk about crises as a window upon new epistemological developments. The 'crisis of reality' was nothing but the sign of a scientific overcoming of both naive realism and the logical positivism of the Vienna circle. Although Fleck did not draw this conclusion explicitly in this short early essay, he presented the rupture in epistemology which his ideas

represented as a test case of itself. The critical study of the history of science would reveal a continually changing and shifting enterprise, the permanent and infinite reconstruction of bits and piece of knowledge, concepts and theories. All these changes were open to a reflexive socio-historical analysis which offered a new epistemological foundation, when they were set free from the teleology of an approximation to the absolute.

Fleck's response should not be mistaken for relativism, since even in this paper of 1929 he had already anchored his open epistemology in a historical process guiding the identification of scientific objects. Moreover, he had embedded this historical process in a political setting. The democratic competition between different styles of thought, the different forms of knowledge, set the sciences free from 'emotions' and 'mysticism' to the 'never-ending labour' of shaping a 'democratic reality' (ibid, p. 54).

Fleck mentioned Riezler's paper on only two instances, and on both occasions in passing. Fleck probably would have agreed with Riezler's concluding call for the history of human thought to be considered the single possible horizon for meaningful interpretations of the world, although certainly without the grounding in a metaphysics of absolute reality.³ Their critiques of contemporary scientific epistemology clash in a strange way. Where Riezler was radical in his description of the limits of science for reasons of his philosophical presuppositions, Fleck offered a scientific critique of every theoretical concept including philosophical ones. But his socio-cultural analysis of the sciences was itself grounded in a firm belief in the scientific method. He aimed at fostering a belief in science as the way towards democracy.

3. The genesis and reception of a scientific book

Fleck's paper of 1929 (1986a) is—if not the symptom of a productive crisis—the document of his transition from a holistic critique of medicine to a general epistemology. Tentative notions such as 'physiology of cognition', which later disappeared from his writing, testify to the epistemological openness of this transition and to his background in medicine.⁴ The open-minded discussion of a 'crisis' crystallized over the following years into the social epistemology of *Genesis and development of a scientific fact* of 1935 (1979), Fleck's masterpiece of epistemological writing.

³ No response to Fleck's criticisms by Riezler could be traced.

⁴ Beginning his scientific career with work on the so-called Wasserman reaction, a particularly controversial diagnostic procedure for syphilis, his experiences in this field of research may well have influenced him when he developed his epistemological framework with its focus on social factors in scientific processes. His continuing close ties with microbiology explain why Fleck, throughout his philosophical papers, almost exclusively referred to examples from the field of medicine for illuminating his theory. However, his epistemology as presented in *Genesis and development* (Fleck, 1979) transcended the limits of medicine or the life sciences for a general description of the dynamics within scientific developments.

Against the standard account, Fleck's book did find an audience in Germany and was reviewed in several German journals.⁵ I want to draw the attention to one of these reviews, which appeared in a leading clinical journal, the *Klinische Wochenschrift*, since it started with an irritating observation. The reviewer celebrated, at least initially, Fleck as the theoretician of the new world view of National Socialism. In the notions of the *Denkstil* and the *Denkkollektiv* the reviewer felt reminded of the new spirit as established in the German intellectual life together with the nationalist movement and the political takeover by National Socialism:

In a slightly disturbing move, not expected from this quarter, Fleck joins our new German 'style of thought' which denies the existence of an 'absolute' science without premises and which views it instead as always being part of a cultural totality. Science shares its presuppositions and commitments with the living conditions of its cultural context. In science, the style of thought is, according to Fleck, the 'guided observation and the guided processing of the observed', which finds its direction in the cultural context as is historically determinable for every period. [...] His second concept, the 'thought collective', sides also with our 'style of thought' assuming that science is a *social* phenomenon produced in a circle of intellectually co-operating people. A '*community*' (even this word is once used) and not the detached individual is the starting point of scientific life. (Petersen, 1936)

In 'a slightly disturbing move, unexpected from this side' did a Polish Jew, of all people, formulate the theory of knowledge for National Socialism. Hans Petersen opened his discussion of Fleck's book with this definitely disturbing observation. He did so for the sole purpose of accusing Fleck in the rest of his review of mistaking a proper German *Denkgemeinschaft* (thought community) with a dysfunctional communist collective, and for confusing brilliant discoveries by German scientists with mere technological constructions. The new German spirit, well grounded in the superiority of the German race, would transcend the narrow limits of the tinkering described by Fleck and liberate the true ingenuity of the German scientific personality. The argument of this review can be left to history, but its opening observation retains a disturbing peculiarity.

Did Fleck write the epistemology of science under National Socialism? Did he address the new Germany when writing and publishing his monograph in German in 1935? Certainly not: Fleck conceptualized *Genesis and development of a scientific fact* as a fundamental theory of knowledge and not as a description of any particular developments. The case studies in the book serve as instructive examples for his general outlook on the development of science. Furthermore, the bulk of the book was already written by 1933. Therefore, the book was not a response to National Socialism (Pois, 1986, pp. 72ff) nor to the atrocities of the new regime

⁵ Schnelle (1982, p. 341) lists twenty-one references to Ludwik Fleck's work from the 1930s, among them nine German reviews of Fleck's book.

(Martin, 1999, pp. 367ff). Although it was not Fleck's intention to address National Socialism with regard to its scientific world view, let alone to supply the National Socialists with an appropriate epistemology for their scientific ideology, I want to argue that the book does address National Socialism in two ways. Firstly, the book's argument was a general theory of knowledge, claiming applicability to all forms of science, including obviously ideological ones. Scientific ideologies served Fleck as welcome examples for illustrating the importance of the social factor in the production of knowledge. Furthermore, in the concept of the thought collective the book offered a methodological framework for a structural analysis of science which went beyond an ideological critique of inferior forms of knowledge. For that reason, Fleck's social constructivism is—beyond its general applicability—particularly well-suited to a historical analysis of science under National Socialism since it helps to explain the emergence and fostering of scientific assumptions during this period. Fleck's book offers a critique of science under National Socialism which goes beyond an evaluation by moral standards into a functional analysis.

A brief look at Fleck's depiction of the coercion within a thought collective, forging 'esoteric' scientists and, with respect to them, an 'exoteric' circle within the same collective into a rigid and authoritarian system may illustrate this point. To explain the mediation within a thought collective, Fleck chose the example of fashion, but this passage in particular provokes a second reading, when viewed against the historical circumstances of the publication of the book. In half a page, Fleck described how media technology and 'communication through official channels' (*im Dienstwege des intrakollektiven Verkehrs*) result in a 'coercion of the strongest form', i.e. 'in the guise of [...] necessity'. Intended or not, this passage was a lucid picture of a totalitarian regime:

The most dedicated followers of fashion are found far out in the exoteric circle. They have no immediate contact with the powerful dictators forming the esoteric circle. Specialized 'creations' reach them only through what might be called the official channels of intracollective communication, depersonalized and thus all the more compulsive. Nothing is motivated in petty style; they are simply told 'ce qu'il vous faut pour cet hiver' [what you need for this winter] [...] It is coercion of the strongest kind, because it appears in the guise of a self-evident necessity and is thus not even recognized as a coercive force. (Fleck, 1979, p. 108)

The steep rise of eugenics as the predominant theme of the life sciences, for example, would make a particularly telling case study for illustrating such synergy effects of science and propaganda by communication via the intracollective *Dienstweg*. The persuasive power of the new world view was mediated, as described by Fleck, by the anonymity of its omnipresence and by its disguise of self-evident necessity. He did not locate all the controlling power in the centre but conceptualized stabilizing loops connecting centre and periphery. The scientists, for example, did not simply obey the commands coming from the centre of political power in National Socialism, but had their share in strengthening the system by

adapting their research to its perspectives and needs. I see a specific strength of Fleck's concept of the thought collective in the inclusion of such positive feedback mechanisms, turning the thought collective without external disturbances into an ultra-stable system. Thereby, Fleck's radically social epistemology offers a framework for a detailed analysis of the specific and peculiar productivity of science under National Socialism.

Fleck's notion of the thought collective should not be mistaken for being part of an ideological critique. It was not the existence of thought collectives (which are part and parcel of any established scientific activity), but the lack of outside disturbance and negative feedback which allowed science under National Socialism to take its murderous turn. Fleck identified the main driving force of science as the 'differential reproduction' (Rheinberger, 1997, p. 75) of scientific objects, concepts and ideas between and within various thought collectives. Difference is the motor of science, but, according to Fleck, it is not an active achievement. Rather, difference is always at work, since every act of transmission among scientists, between the cutting edge science of the journals and the established science of textbooks, or the transmission of scientific 'facts' to other circles of society, is always a transformation (Fleck, 1979, pp. 123ff).⁶ Indeed, this very process of transformation turns scientific observations into facts: 'This tendency to reify and objectivize the conceptual creations of scientific thought arises [...] during the migration of idea throughout the collective and is inseparably bound up with it' (Fleck, 1979, p. 144). Difference operates also at the very heart of every epistemic act, as already noted above: 'To observe, to cognize is always to test and thus literally to change the object of investigation' (Fleck, 1986a, p. 53).

At this point, Fleck's epistemology merges with his political views, since only democracy allows for difference, which is crucial if the sciences are to realise their full creative potential and avoid stagnation into ultra-stable mythologies. However, Fleck's plea for democracy is not an argument for the appropriate political system for the development of science. In his 'crisis' paper of 1929, Fleck had already grounded democracy itself in science: 'Natural science is the art of shaping a democratic reality and being directed by it—and thus being reshaped by it' (p. 54). Fleck viewed his concept of scientific development as being in almost perfect agreement with modern research in the natural sciences, and thus he praised their democratic productivity. Fleck's analysis of 1935 combined both a philosophical analysis and a critical diagnosis of ongoing developments, which arose from the more or less hidden normativity of his social epistemology. This provided the book with its critical underpinning.

The emphasis on the thought collective, which in my view is the particular strength of Fleck's theory especially with respect to the analysis of science in totalitarian regimes, is at the same time its shortcoming, because it risks overlooking the

⁶ Shortly after the publication of his book, in the Polish paper quoted above, Fleck states apodictically (1986b, p. 85): 'Another fundamental phenomenon of epistemology is the fact that the circulation of thought is always related, in principle, to its transformation'.

importance of the material culture of research practices.⁷ Any critique of Fleck should be cautious at this point, since Fleck certainly did not argue for social relativism. Fleck located the origin of a scientific fact in the mute but tangible resistance of the material world for which he coined the insightful notion of the *Widerstandsaviso*. Although he sketched the genesis of an observation out of ‘the complex confusion and the chaos’ as a process in which the scientist ‘must distinguish that which obeys his will from that which arises spontaneously and opposes’ (Fleck, 1979, p. 95), Fleck did not provide any further conceptual elaboration of the *Widerstandsaviso*. His analysis of the effects of scientific instruments, where he embedded and dissolved the material culture in social practices, reveals the ambiguity in his work about the epistemological role of this resistance. Scientific instruments and their experimental use, for example, are rather broadly conceived of as mere products of preceding thought styles, guiding new research ‘automatically’ along the trajectory of a particular thought style:

It can be mentioned briefly that a scientific appliance, which is a realization of some result of a definite thought-style, directs our thinking automatically on to the tracks of that style. Measuring instruments force one to apply the notion of unit for which they were constructed; even more so, they force one to apply the notions from which they originated. [...] A telescope makes it impossible to see ‘fantastic’ forms in clouds, i.e. forms foreign to the scientific style, which means that the telescope directs us toward the scientific style, just as molten wax, a pack of cards or other similar devices direct the fortune-tellers towards their thought-style. (Fleck, 1986b, p. 109)

It would be unfair to accuse Fleck having failed to anticipate the insights of recent research in the history of science on the complexity of instruments and experiments. But I add the criticism here, because even for the analysis of science under National Socialism, Fleck’s notion of the thought collective needs to be augmented by a richer understanding of the articulation of instruments, technology, theory and styles of thought. In high-altitude physiology for example, a small team of scientists specialized in the construction of machines for driving the brains of pilots beyond their natural limits. Besides the well-known lethal consequences of this research project in the Dachau concentration camp (Roth, 2000), the personnel supervising that research arrived at the conception of a cyborg physiology; constructing man–machine hybrids in the form of automatic detector devices, sensory replacements and brain wave controllers.⁸ This research convinced at least the American authorities to staff their space flight program with German physiologists. The technological power of research under National Socialism was not solely the product of Nazi ideology, but the result of a complex interaction of scientific management, scarcity of materials, technological enthusiasm, engineering skills and so on.

⁷ As was observed, notably, by a materialist historian of science (Wittich, 1983).

⁸ This is a tentative and preliminary argument. For a fuller analysis see Borck (in press).

4. How to forge intelligent interventions for scientific democracy in a complex world

In the years following the completion of his book, Fleck published several more epistemological papers, including one in German which appeared in the *Klinische Wochenschrift*, the selfsame clinical journal which would soon afterwards publish the book review by Petersen.⁹ Due to the tragic and dramatic circumstances of his life, Fleck evidently could not pursue this path any further in the years following the outbreak of World War II. After the end of the war, he devoted his studies to microbiological and serological questions which he had never given up and made a career in the Polish medical system. Although he published at least three further epistemological papers (which deserve attention for his reception of gestalt psychology, for example), Fleck's socio-historical epistemology was completely ignored during his life, and even now, the historical and philosophical discussion of his work concentrates almost entirely on his monograph.¹⁰ The book certainly gives the fullest description of the methodological framework that informed his epistemology. Because of Fleck's amazing anticipation of central tenets in modern history of science, it seems as if his text should have reached its 'address of destination' long ago. I want to argue in the rest of this paper that the abstraction of *Genesis and development of a scientific fact* from Fleck's further work and its historical context, however, fails to notice a quite remarkable turn in the way how Fleck pursued his critical struggle for an open epistemology. This turn leaves Fleck's writing a message in a bottle still awaiting its recovery.

In a few sentences from *Genesis and development*, Fleck vividly anticipated his personal future:

Whole eras will then be ruled by this thought constraint. Heretics who do not share this collective mood and are rated as criminals by the collective will be burned at the stake until a different mood creates a different thought style and different valuation. (Fleck, 1979, p. 99)

Fleck survived the holocaust only because he was a typhus specialist, an expert in 'Fleck-Fieber' (the German for typhus)—one of the most imminent biological threats during the war. Arrested in June 1941, immediately after the German conquest of his home town, the then Soviet city of Lwów, Fleck was forced to spend the following years in the German camp system at the permanent disposal of the German authorities. In December 1943, Fleck was brought from Auschwitz to the concentration camp Buchenwald to work as a typhus specialist in an in-camp

⁹ Fleck (1935). This paper summarised the argument of his book on basis of the case study on the history of anatomical illustrations from the final chapter of his book. This paper has not been included in the English or the German edition of his collected essays, although it takes up many of the arguments from his earlier 'crisis' paper.

¹⁰ Cackowski (1982) and Biagioli (1992) are among the few discussing Fleck's 1946 paper.

laboratory for the production of an anti-typhus vaccine.¹¹ The SS physicians complemented the production of vaccines with a series of murderous ‘clinical’ tests for evaluating their efficacy. As one consequence of these experiences, after his liberation at the end of the war Fleck (1948) called for informed consent as the strict requirement of all human experiments and acted as expert witness in the IG-Farben trial at Nuremberg.¹² Fleck’s moral commitment to these political activities is certainly noteworthy, but his intellectual position was much more refined (Bonah, 2002) and transcended the dual logic of scientific freedom and code of conduct that resulted from the Nuremberg trial against physicians in form of the Nuremberg Code of medical ethics (Weindling, 2001).

Fleck used his experiences in Buchenwald in one of his early post-war epistemological papers in quite another way: he employed this episode to illustrate his general theory of knowledge or social epistemology (Fleck, 1986c). The Buchenwald lab served Fleck as an extreme example of his notion of a thought collective. For obvious reasons, the group of prisoner scientists had to work without proper training and in the absence of appropriate cooperative critique. Nevertheless, these people struggled for sound work; believing their results were accurate (even in the absence of clear evidence), driven as they were by a desperate need for success:

The development of this ‘science’ did not proceed at all rapidly—on the contrary, one indulged long discussions and repetitions of tests. Sometimes even some findings were cancelled; in other words, admission of errors was in order [...] This collective illusion functioned for one-and-a-half years; it was formulated in a system which did not have more logical lacunae than an average scientific output. The epoch of ‘discoveries’ was followed by the epoch of ‘routine’, with established methods, with a specific acquired fund of experience and skill. And everything was in agreement among the members of the collective, no less and no more than in true science. (Fleck, 1986c, p. 120)

How science proceeds was more easily grasped, according to Fleck, by a closer look at the extreme case of the Buchenwald lab than by an analysis of one of the centres of excellence, because it allowed for distance from the results of the

¹¹ Eugen Kogon reported that the prisoners in the Buchenwald lab sabotaged the vaccine production (Schäfer & Schnelle, 1983, pp. 28 ff.): whereas the German Wehrmacht received large quantities of an ineffective product, the inhabitants of the lab and a few more prisoners were supplied with the true vaccine, of which Fleck supervised the production. There were several competing methods for producing this vaccine at that time, but no established scientific standard. Most of the workers in the lab had no efficient training in any of these methods and had to rely on their knowledge from books. According to Kogon, the group only realized that it had concocted an entirely useless product after Fleck’s arrival, and the sabotage was at least partly a strategy to minimize the potential danger of this predicament by continuing along well established lines. Fleck came as scientific authority to the lab, but precisely this role brought him into serious conflicts which, after the war, resulted in both a glorification of his role and in accusations of involvement in Erwin Ding-Schuler’s murderous medical experiments. For a careful analysis of the historical evidence, see Weindling (2000), pp. 352–370. Nothing about this sabotage is mentioned by Fleck in his 1946 paper (Fleck, 1986c).

¹² Fleck (1948). I thank Martina Schlünder for drawing my attention to this paper.

research process, necessary for critical reflection. The holocaust survivor sharpened his epistemological gaze on the extreme case of science under the conditions of a concentration camp. Fleck placed his ‘physiology of knowledge’ in the hopelessness of the concentration camp where human life was reduced to mere physiology. Here on the dark side of science and politics, research exhibited its dynamics more clearly than in the brightness of scientific progress. He undercut the whole debate over whether there was ‘proper’ science under National Socialism, which hampered the historical and political debates of the 1970s and 1980s, by presenting this extreme case as exemplar (Biagioli, 1992).

But does the camp really hold a lesson for science studies? Are the structural parallels close enough to draw such a conclusion? Is the epistemological situation of modern science in any way similar to the desperate research at Buchenwald, as postulated by Fleck?

Simplicius. You do not describe anything out of the ordinary. We all know that it is always possible to err, to lose one’s way, and to stray Lord knows how far. We know quite many such examples.

Sympathius. This is not a simple error, but a complex system of errors. What was at stake was not to establish one isolated fact (if such facts do exist at all) but the relations of numerous facts, i.e. that which we call the structure of a certain field, and which some people consider to be something that is doubtless ascertainable intersubjectively, i.e. something with respect to which one can always find a complete mutual understanding. [...] No automatic process whatever would be able to give rise to corrections, to an increased experience, to reflection. (Fleck, 1986c, pp. 121–123)

How could a physician and philosopher of science who had barely escaped the murderous Nazi regime resist the temptation to forge iron limits for the sciences in order to prevent a repetition of the atrocities he had just endured, and instead continue to believe sternly in the creative and liberating potential of the sciences themselves? A first hint is given in the text, by the way in which Fleck presents his argument. Fleck adhered to his social epistemology even more radically than before, but he argued for it by different means. Compared to his other epistemological writings, the most outstanding characteristic of this paper from 1946 is the rhetoric. Over and above the apparently straight-forward nature of his argument, this text is Fleck’s most rhetorical piece of writing. The paper’s argument unfolds as a fictitious dialogue between one ‘Simplicius’ and one ‘Sympathius’—significant nominations, since Simplicius represented the standpoint of scientific positivism and Sympathius that of a radical socio-historical constructivism out of a sincere ‘sympathy’ for science.

Furthermore, the dialogue was interlaced with clearly ironic passages, in which the ideal of absolute knowledge, the ‘Codex Pansophiae’, was taken to the absurd by explaining its content as ‘*panta rei* . . . or, if you prefer it; $A = A$ ’ (Fleck, 1986c, p. 116), and in which even the necessity of a totalitarian regime for protecting and policing the consistency and constancy of such a codex was invoked. In a

psychological interpretation of this rhetoric, one could conclude that Fleck was writing for his life and against the horror he had borne. Or one could identify in Fleck's double role as physician and as sociologist of science, a biographical hint to this rhetorical striving for liberal sciences out of a desperate situation and against his own rather bleak experiences.¹³ But the specific impact of this paper transcends such clues to a better understanding of its author's psychology. The paper of 1946 argues for a new way of reflecting and writing about science—an engaging, subversive way, and the paper exemplifies it at the performative level. Fleck did not argue that the scientific practices themselves should be changed, since they were pretty much determined by the thought collective anyway, but instead that writing and thinking about science should adapt to the insights from social constructivism in order to gain its full creative potential and hence a more humane future. This is Fleck's lesson for science studies. Their business is the scientific analysis of science by critical and reflexive scholarship (Jasanoff, 1996).

The search for the absolute picture would require our own annihilation—that is what Fleck's rhetoric called for by alluding to absolute science vis-à-vis the *endlösung*:

Since every major discovery has its repercussions upon the whole of science, such a final state, were it only for one only major problem, would be attainable only after all of the problems had been solved. But what does 'all of the problems' mean, when new problems can arise again and again? One would have to stop the movement of the planets, the scintillate of dust in the air, the evolution of living beings and—what is most important—the movement of human thought; otherwise there will arise new, unexpected problems whose solution will force us to revise the entire system. (Ibid., p. 114, translation adapted)

Several members of the Fleck family were murdered by the Nazis, reduced to a 'scintillate of dust in the air' as Fleck once remarked while still imprisoned in Buchenwald.¹⁴ His 1946 paper sketched out a picture of science derived from this inferno, and yet, he did not want to give up the sciences, or his sociology of science in particular—it remained his pledge for enlightenment. Fleck was the Sympathius of his paper, who described science poetically as 'a round fruit, with a juicy pulp, and a thick, indigestible skin'. Sympathius continued:

In order for this miraculous fruit to grow, it must be taken between two fires: the hot, though dark, fire of romanticism and the cold, but bright fire of scepticism. For, the romantic daydreaming of the creator is equally as necessary as

¹³ The question remains whether Fleck would have developed his social constructivism had he not had training in medicine. Even though there are clear hints that Fleck's epistemology was rooted in his scientific research in the contested field of microbiology, his innovative reflections about 'crises' in the sciences demonstrate his intellectual independence. The life sciences and their specific context during the 1920s certainly mediated Fleck's transition from philosophy of medicine to sociology of science, and thus enabled him to formulate a general epistemology.

¹⁴ Quoted by Adelaide Hautval, in Arens et al. (2002).

the envious scepticism of competitors. I would even say that it is precisely this envy that creates the social value of cognition, by deprivatizing the results. The aim of my inferences is not to belittle the value of science, but, on the contrary, to raise it. (Ibid., p. 117)

With all its rhetorical methods, Fleck's paper presents itself as a mimetic appropriation of this double fire of scepticism and daydreaming.

Fleck's strategy of rhetorical interventions into scientific debates remained a difficult one; its problematic applicability was demonstrated by Fleck himself, albeit inadvertently. Late in his life, after his emigration to Israel, Fleck made a final attempt to find an audience for his epistemology. He wrote a paper entitled 'Crisis in science'—which bends the bow back to the beginning with Fleck's 1929 paper in *Naturwissenschaften*. Indeed, the new piece was also kindled by a discussion on 'science and human welfare' in a scientific journal, this time the American periodical *Science* (Fleck, 1960). He sent his contribution to the ongoing discussion in the journal, but although he was now an internationally recognized scientist, his contribution was left unpublished. Riezler was proved right: the arguments of the *schamlose Witzbold* were ridiculed and turned down without discussion. It is time to recover Fleck's message in a bottle.

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