

The socio-logic of knowledge-in-formation between discovery and error: some considerations from ‘normal science’ under exceptional conditions

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Abstract A sociology of knowledge of a specific kind, namely that emerging from observations on the work of scientific thought collectives, is what Thomas Kuhn acknowledges as Ludwik Fleck’s (*Denkstile und Tatsachen. Gesammelte Schriften und Zeugnisse*, Suhrkamp, Berlin, 1936) influence on his own *Structure of Scientific Revolutions* (1962). Yet the relationship between thought and collective that turns out to be one of the central problematics in Fleck’s thought, remains troubling to Kuhn. The reservations expressed by Kuhn go to the core of Fleck’s conceptualization of the structures, roles, scientific achievements, illusions, and errors of thought collectives, as well as to the sociology of knowledge with which he is credited, but which remains a theoretical blindspot. I would here like to take a closer look at this problematic, with a view to specifying the nature and the dynamic of a ‘sociology of knowledge’ that a leading thought would engender in a scientific collective, in its refractions between internal and external conditions of knowledge formation, operationalized under conditions of scientific work in Nazi concentration camps, as they were retrospectively recounted by Fleck (*Cognition and fact: material on Ludwik Fleck*, D. Reidel, Dordrecht, 1946).

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Much as the path-breaking conceptualizations of Ludwik Fleck for the philosophy of science are acknowledged to have been occluded¹ until their ‘discovery’² by Thomas Kuhn, they have been highlighted in the latter’s *Structure of Scientific Revolutions* (1962) and have attained near-canonic status ever since. They have reverberated in the definition of ‘paradigm’ and in accounts of ‘paradigm shifts’ ever since (see e.g. Agamben 2009). “Today, Fleck is usually viewed as pioneer of the sociological approach to science, constructivist epistemology, studies of laboratory practices and of investigations focused on the growth, stabilization and diffusion of scientific knowledge” (Hedfors 2007, 77).

Yet there are some rumblings in relation to central concepts of Fleck’s reflections on the history and philosophy of science, articulated by precisely those theorists who dedicated their own scholarship to Fleck’s work.

The reservations expressed by Kuhn go to the core of Fleck’s conceptualization of the structures, roles, scientific achievements and illusions of thought collectives, as well as to the sociology of knowledge with which he is credited, but which remains a theoretical blindspot. I would here like to disentangle the relationship between thought and the collective. In a further step, I will ask what kinds of ‘sociology of knowledge’ this relationship would engender in its refractions and vacillations between internalist and externalist explanations operationalized under conditions of the camps, as they were retrospectively recounted.

Let me first sketch the lines along which the role of ‘thought collectives’ under different conditions of scientific research was articulated.

The immense explanatory purchase that Thomas Kuhn attributes to Ludwik Fleck’s notion of thought collectives refers to Fleck’s 1935 monograph on the *Genesis and Development of a Scientific Fact*. While Kuhn acknowledges a great debt to Fleck’s *Genesis and Development of a Scientific Fact*, he does so in none but the vaguest of terms, simply stating “I am indebted to [this work] in more ways than I can now reconstruct or evaluate” (Kuhn [1962] 1970, vii). He follows closely in Fleck’s footsteps in attributing “a fundamentally sociological dimension” to the logic of scientific discovery, the positing of a fact, and the relation between journal science and vademecum science (Kuhn in Fleck [1935] 1979, ix), to “the difficulties of transmitting ideas between two “thought collectives”, and the possibilities and

¹ Eva Hedfors contests this view, pointing out that rather than being completely overlooked, Fleck’s views were questioned or rejected—by Gustav Mie (1932) and Hans Peterson (1936) for failing to distinguish between *Entdeckung* [discovery] and *Erfindung* [invention], *Begriffe* [concepts] and *Tatsachen* [facts], and for his exaggerated experimentalism; by Tadeusz Bilikiewicz (1939) for his bias towards a sociological criticism of knowledge (Hedfors 2007, 69–72); by Wilhelm Baldamus (1977) for anachronistic ideas on the sociology of science (see Hedfors 2006, 135); and even by Kuhn, his adept, in the preface to Fleck’s monograph (Kuhn 1979, ix–x; see also Hedfors 2006, 135). To these aspersions, Hedfors adds some emanating from her own assessment of Fleck’s serological constructions: Fleck’s confusing concept of ‘antibody’; his seemingly unified epistemology, based on selective reading of sources (Hedfors 2006, 140, 147, 150); his anachronism in insisting on the genetic (rather than functional) variability within bacterial species (Hedfors 2006, 155); his “unfortunate choice of the Wassermann reaction as the basis of an epistemology” (Hedfors 2006, 152); his imprecise formulations giving rise to the possibility of asserting a relativistic conception of science (Hedfors 2006, 153, 154); and his amateur status in esoteric circles (Hedfors 2006, 158).

² Via a footnote in Hans Reichenbach’s *Experience and Prediction* (1938) (see Kuhn in Fleck [1935] 1979, viii).

limitations of participation in several “thought communities” (Kuhn in Fleck [1935] 1979, vii, x).

It is precisely at the point of the greatest influences credited to Fleck, that Kuhn articulates a deep sense of theoretical unease. He admits to a reticence about the notion of a thought collective emanating from the “to me unknown and yet vaguely repulsive perspective of a sociology of the collective mind” (Kuhn in Fleck [1935] 1979, vii). He elaborates:

What troubles me is not that a thought collective is a hypostatized fiction, though I think it is. Fleck’s own response to *that* objection (chap. 4, sec. 3, n. 7) seems adequate. Rather I find the notion intrinsically misleading and a source of recurrent tensions in Fleck’s text. (Kuhn in Fleck 1979, x)

Kuhn’s theoretical unease is related to Fleck’s elaboration of the relationship between the individual and the collective, which, in the case of a scientific or thought collective is necessarily one characterized by tension. As if responding, Fleck preempts the concerns expressed by Kuhn: “we could agree with anybody who calls the thought collective fictitious and the personification of a common result produced by interaction” (Fleck [1935] 1979, 44). Yet Fleck castigates those in “superstitious fear” of conceding anything to the collective (Fleck [1935] 1979, 43–44) who consider social dependence a necessary evil and an unfortunate human inadequacy which ought to be overcome, for “fail[ing] to realize that without social conditioning no cognition is even possible. Indeed, the very word “cognition” acquires meaning only in connection with a thought collective” (Fleck [1935] 1979, 43). Fleck initially construes an analogy between personality and collective:

But what is any personality if not the personification of many different momentary personalities and their common psychological Gestalt? A thought collective, by analogy, is composed of different individuals and also has its special rules of behaviour and its special psychological form. As an entity it is even more stable and consistent than the so-called individual, who always consists of contradictory drives.... (Fleck [1935] 1979, 44)

The analogy posited here between individual personality and thought collective is precisely what troubles Kuhn. He infers from this analogy that

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 in Fleck 1979, x)

On closer inspection, however, the analogy that Fleck posits and Kuhn criticizes, turns out to be anything but a simple one; indeed, it exceeds the bounds of analogy. There is a realization that the collective cannot be simply modeled on the individual, and not even on a number of individuals (see Fleck [1935] 1979, 41; 99) who constitute it at any particular time.

There are obvious parallels in some of the considerations that motivate Freud to look at the integral relationship between individual and group psychology, and that

motivate Fleck to look at the integral relationship between ‘personality’ and thought collective.

For Freud, the social bond is a psychic bond – it is complete within each individual. Nevertheless, there is something that is activated in groups that members of the group do not experience individually in isolation. Both Freud and Fleck are acutely aware of this phenomenon (see Freud [1921] 1955, 71–72; Fleck [1936] 2011, 284).

For Freud, the “important emotional common quality” tying the members of the group to each other, beyond their respective individuality, and beyond the additive sum of their individualities, is generated through identification: “the mutual tie between members of a group is in the nature of an identification ...”, and that identification is sponsored, for Freud, by “the nature of the tie with the leader” (Freud [1921] 1955, 107–108).³

In exploring modulations of the social tie in different types of groups, Freud also considers the possibility that “an idea, an abstraction, may ... take the place of the leader” (as e.g. in religious groups with their invisible head ...), and [the question as to] whether a common tendency, a wish in which a number of people can have a share, may not in the same way serve as a substitute. This abstraction, again, might be more or less completely embodied in the figure of what we might call a secondary leader, and interesting varieties would arise from the relation between the idea and the leader.” (Freud [1921] 1955, 100) Freud concludes his discussion of other sources of group cohesion with the question as to “whether a leader is really indispensable to the essence of a group” (Freud [1921] 1955, 100)—a question which he leaves in abeyance, continuing with the derivations of the group tie from identifications, via the loops of the Oedipus complex, with figures modeled on the paternal prototype.

This is the boundary that Fleck attempts to push in accounting for the work within thought collectives, pursuing the possibility of group ties on the basis of common work on ideas.⁴ What remains of Freud’s group psychology in the re-direction of identification (with the leader of the group) to the leading idea in thought collectives, is the intellectual mood as expression of readiness for a particular thought style, for directed perception (Fleck [1935] 1979, 142), and an apprenticeship or didactic introduction or initiation into, and assimilation of, a thought style (Fleck [1935] 1979, 104; see also [1936] 2011, 287). In the natural sciences, “[intellectual mood] is expressed as a common *reverence* for an ideal—the ideal of objective truth, clarity, and accuracy. It consists in the *belief* that what is being revered can be achieved only in the distant, perhaps infinitely distant future; in

³ The horizontal and vertical integration of groups relies on Freud’s definition of the group as “a number of individuals who have put one and the same object in the place of their ego ideal and have consequently identified themselves with one another in their ego” (Freud [1921] *SE* XVIII, 1955, 107–108). Group members have put an object in the place of their ego ideal, the ego-ideal constituting a model to which the subject attempts to conform (Laplanche and Pontalis 1988, 144).

⁴ Fleck explicitly invokes the dynamics of group psychology in accounting for the exchanges within thought collectives: Lively exchange of ideas elicits a special collective mood which solidifies into a ‘community’ with a definite social structure. Some individuals emerge as leaders, others subordinate themselves. Rivalry, the wish to emulate, admiration, contempt, sympathy and antipathy arise in the process (Fleck [1936] 2011, 412).

the *glorification* of dedicating oneself to its service; in a definite *hero worship* and a distinct *tradition*” (Fleck [1935] 1979, 142), which simultaneously delimits itself from other groups, and which is being transmitted across generations of scholars passing through its gates, hoops, and rituals (see Fleck [1936] 2011, 285, 287). The “hero-worship” embedded in the teacher-student relation is the conduit for the transmission of certain traditional values which in turn are subjected to specific thought constraints internalized in the individual science researcher through his/her relation to a thought collective.

Fleck accords to a leading supraindividual idea within a thought collective the same binding role as Freud accords it to the group leader. Elaborating his notion of “solidarity of thought in the service of a superindividual idea” (Fleck [1935] 1979, 106), Fleck states:

Between two members of the same thought collective on the same mental level, there is always a certain solidarity of thought in the service of a superindividual idea which causes both intellectual interdependence and a shared mood between the two individuals ... This comradeship of mood can be sensed after only a few sentences have been uttered and makes true communication possible. Without it, the speakers are at cross purposes. A special feeling of dependence therefore dominates all communication of thought within a collective. The general structure of a thought collective entails that *the communication of thoughts within a collective, irrespective of content or logical justification, should lead for sociological reasons to the corroboration of the thought structure [Denkgebilde]*. Trust in the initiated, their dependence upon public opinion, intellectual solidarity between equals in the service of the same idea, are parallel social forces which create a special shared mood and, to an ever-increasing extent, impart solidity and conformity of style to these thought structures [Denkgebilde]. (Fleck [1935] 1979, 106)

In an extensive footnote, Fleck takes issue with precisely the lineage of conceptualizations of group psychology that form the background of Freud’s *Massenpsychologie und Ich-Analyse*, and with that treatise itself, as well as with Hans Kelsen’s review of the latter (Fleck [1935] 1979, 179–180, n. 7).

Fleck’s own explication of ‘Group Psychology’ poses the task for him to functionally elaborate the inter- and supra-individual source, “determinable by cognition alone” of scientific discovery, while heeding the undeniable positive values of socialization by entrusting the power of “equipping the mass with the attributes of the individual” to the “organization [of the scientific collective]” (Fleck [1935] 1979, 180, n. 7).

The work of the thought-collective is not reducible to teamwork, which implies the capacities, tasks, and results of a number of individuals added together. Collective work, on the contrary, generates new and special forms through work guided by certain rules of co-operation, which Fleck summarily terms ‘thought style’:

We can... *define thought style as [the readiness for] directed perception, with corresponding mental and objective assimilation of what has been so*

perceived. It is characterized by common features in the problems of interest to a thought collective, by the judgment which the thought collective considers evident, and by the methods which it applies as a means of cognition. (Fleck [1935] 1979, 99)

The thought style, stipulating rules of co-operation, constrains the individual by determining “what can be thought in no other way” (Fleck [1935] 1979, 99), and provides the form in which the problem is articulated, the method of solving the problem is decided upon, and the form in which the solution to the problem is presented (see Fleck [1935] 1979, 100). In the process, ‘free spaces’ are reduced: “It is as if more resistances were generated, and the free unfolding of ideas were restricted” (Fleck [1935] 1979, 83–84), thereby homogenizing the ideas and practices of the thought collective. Individuality and personal inclination are withdrawn in the service of a common ideal (Fleck [1935] 1979, 144). Closely related with this depersonalization is the inclination to objectivize the thought structures [*Denkgebilde*] created by the thought collective, and the coining of specialized *termini technici* and symbols (Fleck [1935] 1979, 144; see also Fleck [1936] 2011, 296).

This is what distinguishes scientific investigation and discovery from artistic creativity. The former is more ‘dense’ with negotiations of obstacles and restraints than the latter (see Fleck 1939, 173), owing to the fact that in artistic creativity, the position of the creative individual in relation to the artistic collective is not fixed (Fleck [1936] 2011, 291). Scientific truth, in contrast, emerges on the basis of “stylized thought constraint” restricting free-wheeling investigative pursuits (see Fleck [1935] 1979, 101).⁵ To use Fleck’s famous phrase, “[t]he general aim of intellectual work is ... maximum *thought constraint with minimum thought caprice*” (Fleck [1935] 1979, 95; see also 97).

Paradoxically, such stylized thought constraint is one of the very important mechanisms by which the group can become equipped with the attributes of the individual. The thought constraint would have to be brought home to and directly experienced by each member of the collective (Fleck [1935] 1979, 101), to make him/her ready for directed perception, for a particular thought style, which then is transmitted through theoretical and practical education, and induction of the individual scientist into the thought collective (see Fleck [1935] 1979, 96). Experience guides the building up of a new thought style from earlier knowledge

⁵ For Fleck, scientific activity demands a minimum of thought caprice, as well as its correlative, a maximum of constraint, evinced in the discovery of the Wassermann reaction: “*The factuality of the relation between syphilis and the Wassermann reaction consists in just this kind of solution to the problem of minimizing thought caprice, under given conditions, while maximizing thought constraint. The fact thus represents a stylized signal of resistance in thinking. Because the thought style is carried by the thought collective, this “fact” can be designated in brief as the signal of resistance by the thought collective [denkkollektives Widerstandsavisio]*” (Fleck [1935] 1979, 97). Michel Foucault talks of ‘knowledge’ in similar terms, as “all procedures and all effects of knowledge (*connaissance*) which are acceptable at a given point in time and in a specific domain. ... nothing can exist as an element of knowledge if, on the one hand, it does not conform to a set of rules and constraints characteristic, for example, of a given type of scientific discourse in a given period, and if, on the other hand, it does not possess the effects of coercion or simply the incentives peculiar to what is scientifically validated or simply rational or simply generally accepted (Foucault [1978] 2007, 60–61).

(see Fleck [1935] 1979, 98). It is through this process that a sociologically defined collective can attain an individual-psychological form. “The authority of a thought collective is ... more nearly logical than social, yet it exists for the individual only by virtue of his induction into a group” (Kuhn in Fleck 1979, xi).

Thus do considerations of the sociology of knowledge (of a specific kind) turn into considerations of the logic of knowledge-in-formation. Not only does the organization of exoteric conditions become internal to the pursuit of science itself, even the concepts and problematics embark on this trajectory, before they become systematized through thought constraints and included in/as vademecum science. The process of formation of vademecum science is the process

by which exoteric knowledge, knowledge originating in other collectives, and strictly specialist knowledge are all selected, blended, adapted, and then molded into a system. Concepts originating in this manner become dominant and binding on every expert. The preliminary signal of resistance has become thought constraint, which determines what cannot be thought in any other way, what is to be neglected or ignored, and where, inversely, redoubled effort of investigation is required. The readiness for directed perception becomes consolidated and assumes a definite form.⁶ (Fleck [1935] 1979, 123)

However, measured by the complexity of the accounts of ‘thought style’ and ‘thought collective’, the designation of ‘sociology of knowledge’ (see e.g. Löwy 2004; Hedfors 2007, 77) or ‘social epistemology’ is a simplistic reduction of the ‘genesis and development of scientific fact’. It is precisely the genealogical – “*ab evolutione historica*” - and comparative aspects, neither empirical nor speculative, of the formation of scientific knowledge that Fleck brings to the fore (Fleck [1936] 2011, 276, 261).

Furthermore, Fleck’s theoretical achievement cannot be characterized as a synthesis or *Aufhebung* of the distinction between internalist and externalist accounts of the history of science (as Elkana makes it out to be—see 1986, 310). Quite the contrary: the thought collective itself, for Fleck, consists of a multitude of cross-cutting esoteric and exoteric circles, with every individual member being simultaneously involved in numerous exoteric and few esoteric circles, and with scientific thought itself undergoing modification and change as it circulates between collectives (Fleck [1936] 2011, 289). In this account, the relationship between exoteric and esoteric circles of knowledge, between externalist and internalist accounts of the formation of knowledge, cannot be fixed once and for all, and would have to be contextually and conjuncturally investigated and understood.

This consideration emerges from a reading of Fleck’s account of the work of scientific collectives in the concentration camp of Buchenwald, published in 1946, which clearly indicates the changed conditions of scientific work. To understand this change, we would have to look at the historical-political context of the scientific

⁶ An example can be found in the etiological concept of disease entity: “Emerging originally from exoteric or popular ideas and from ideas formed outside the thought collective in question, it gradually acquired its present significance in the esoteric communication of thought and now forms one of the basic concepts of vademecum bacteriology. ... once part of the vademecum, it is taught and generally used. It forms the keystone of the system and thus exerts a constraint on thinking” (Fleck [1935] 1979, 121).

work in which Fleck was involved, and particularly at the experience of work in differently structured scientific collectives (of three of which we only have limited accounts, and on one of which Fleck provides more extensive reflection).

The importance of thought collectives conducive to scientific discovery would have suggested itself from Fleck's professional insertion in the geopolitical currents of his time and location in interwar Poland, with cross-cutting impulses from internationalist scientific research at the behest of the League of Nations Health Organization and the Rockefeller Foundation on the one hand, and German-expansionist claims to scientific supremacy in the aftermath of World War I, on the other. At their node, Lwów emerged as an international centre for typhus research, marked by rigorously collaborative and comparative research methods and procedures (see n. 7) which came under increasing pressure from tensions, conflict, rivalry and competition in scientific research; from militarized power blocs, and racialized geo- and bio-politics.

In 1942, Fleck worked, under the most primitive conditions, in the ghetto hospital of his home city of Lwów.⁷ Once the ghetto was moved to a small area on the

⁷ The town of Lemberg was one of the centers of typhus and other infectious diseases research and control measures since the first decade of the twentieth century at the crossroads between internationalist scientific research at the behest of the League of Nations Health Organization and the Rockefeller Foundation, and German-expansionist claims to scientific supremacy in the aftermath of World War I. In the grip of these competing influences, Polish health authorities attempted to maintain their autonomy.

Rudolf Weigl (1883–1957) instituted a typhus laboratory in Lwów, and established Lwów as international centre for typhus research during an era when the Rockefeller Foundation and League of Nations internationalized bacteriological research (Weindling 2000, 209). Prague-based bacteriologist Edmund Weil conducted research here in 1922, and Fleck assisted with laboratory work between 1920 and 1923. Comparative and collaborative methods marked this research (Weindling 2000, 214).

With endemic typhus becoming an increasing problem in this geopolitical node, Weigl intensified work on the vaccine from louse intestines from 1930 onwards, which he applied internationally in areas in which typhus had become endemic. At the beginning of the 1930s, he developed a costly, time-consuming and technically complex procedure to extract a vaccine *Rickettsia prowazeki* cultivated in lice intestines. (A complete and effective vaccination could only be achieved after three subcutaneous injection, for whose production 50 intestines were required of lice fed on persons immune to typhus). Also at the beginning of the 1930s, Herald R. Cox (1907–1986), a US-American bacteriologist, succeeded in cultivating *Rickettsia* in fertilized hens' eggs, from whose yolk an effective vaccine was produced, which was used to immunize American troops. A third major vaccine development push came from French bacteriologists Paul Giroud (1898–1989) and Paul Durand (1886–1960), who obtained a vaccine from *Rickettsia* grown in the lungs of rabbits and mice (Hinz-Wessels 2008, 102).

Polish authorities attempted to retain independence in establishing a state monopoly for serum production, centered on the Warsaw Hygiene Institute. However, this proved untenable under conditions of German expansion and occupation. Even while under Soviet occupation, German hygienists Gerhard Rose and Hellmuth Haubold (1905–1968), charged with overseeing the health of ethnic German resettlers, had visited Weigl in Lwów in winter 1939/40, to obtain 5,000 dosages of the vaccine for use on German troops in the first months of the War, when a laboratory in Frankfurt and the Behringwerke in Marburg could not produce the vaccine in sufficient quantities (Hinz-Wessels 2008, 102–103). Soon IG Farben's Behring Institute for Experimental Therapy emerged as a major player in this field, in an attempt to oust the Pasteur Institute and the Rockefeller Foundation from their leading role in the Versailles system, and to assert the primacy of German pharmacology internationally. The Behring Institute for Experimental Therapy was established, initially for typhus vaccine production, but then also as a state serum institute, with a branch in Lemberg, among 11 others by 1943 (Weindling 2000, 241).

Polish doctors resented policies of Germanization and health authorities in Poland remained staunchly independent (Weindling 2000, 55, 144). In October 1939, an Institute for Typhus and Virus Research was opened, with Hermann Eyer as director. Vaccine production at this Institute got off to a sluggish start, and

fringes of the city, under conditions of starvation, overcrowding, extremely limited sanitation facilities (see Fleck 2011, 505), Fleck estimates, 70 % of the ghetto population fell ill with typhus (Weindling 2000, 364). Together with his colleagues, Olga Elster, Bernard Umschweif, Anna Seeman, Anhalt, and Owsiej Abramowicz, Fleck “developed a diagnostic test and vaccine for typhus, based on human urine from patients who had contracted typhus as a source of rickettsial antigen. They vaccinated 500 persons in the Lemberg ghetto; survivors of the ghetto testified that the vaccine successfully reduced the severity of the disease” (Weindling 2000, 364; see also Fleck 2011, 505–506, 510–511). Medical historian Paul Weindling notes the “striking features of the Fleck-Elster urine vaccine”: namely “the adherence to normal scientific procedures in sheer defiance of the atrocious circumstances, and the success in rapidly scaling up from the laboratory to industrial manufacture” (Weindling 2000, 364; see also Fleck 2011, 507–508, 510).

Fleck’s colleagues, Drs Elster, Anhalt, Umschweif, and Kurzrok, were murdered in Bełżec and Auschwitz. “Of those who worked together”, Fleck writes, “it was my fate to be the one to now recount our common work ... on the basis of a part of our notes which I was able to hide and salvage” (1946, 663; qtd. in Schnelle 1986, 21).

Notes of his next assignment did not survive. Upon having to divulge the vaccine development project to the Gestapo, Fleck was installed in the Laokoon pharmaceutical factory until the end of January 1943, together with 9 assistants. Samples with an effective antigen were used for clinical trials, followed by large scale production, used in the main Lemberg ghetto, and then in the small Janowska camp. Here, too, Fleck adhered to protocol for medical experimentation, while offering limited protection to laboratory staff and a group of people convalescing from typhus. Adverse as these conditions were, they still allowed for ethical and scientific integrity to preside over scientific-medical experimentation. The careful observation and documentation of Fleck’s research to establish the efficacy of the vaccine was, however, foiled by the destruction of human lives in the course of the liquidation of the ghetto in August 1942 (Weindling 2000, 364; see also Fleck 2011, 507, 510).

At the end of January 1943, Fleck was taken with his family, first to the prison of Lwów, and then to Auschwitz on 7 Feb 1943. In Auschwitz, he first worked in Block

Footnote 7 continued

even with stepped-up production targets—by the end of the War, over 3 million vaccine dosages had been delivered -, it could not meet the military demand. After the Barbarossa campaign, in June 1941, Eyer opened an outpost for international (minus the Jewish vaccine researchers, who had been interned in the ghetto) vaccine research and production in Lemberg, with Weigl as co-director, supervised by the German army (Weindling 2000, 334, 335). The vaccine produced at both institutes, the main one in Krakow and its subsidiary in Lemberg, according to procedures developed by Weigl, was dedicated solely to meeting the needs of the German army, but supplies fell short (Hinz-Wessels 2008, 103–104). At the end of 1941, IG Farben promised to take over the production of vaccines in the conquered territories, and to meet the demands of the German army. In conjunction with the Behringwerke, they gained control over pharmaceutical production and supply in the Generalgouvernement, based at the Behring Institute in Lemberg (Hinz-Wessels 2008, 108–109; Weindling 2000, 242), re-inforced, from 1942 by the Sächsisches Serumwerk Dresden, the Anhaltinisches Serumwerk Dessau, the Serumwerk Hamburg, and Schering AG in Verlin (Hinz-Wessels 2008, 109).

After the discovery of DDT in 1943, the US health authorities changed their strategy of typhus control, focusing on the control of transmission (Hinz-Wessels 2008, 102).

20 (belonging to the hospital), then in the serological laboratory of the Hygiene Institute of the Waffen SS on the first floor of Block 10, heading a small international team of 4 prisoner-physicians specialised in biology, bacteriology, and pathology, tasked with the diagnosis of syphilis, typhus, and other illnesses using serological tests, which he is reported to have fudged in attempting to avert the fatal consequences for the prisoner-patients (Schnelle 1986, 23–24; Weindling 2000, 365). Not much is known about this period and these conditions of Fleck’s scientific work.

In January 1944, Fleck was transferred to Buchenwald concentration camp’s Block 50 (which housed the laboratories of the Typhus and Virus Research Division, established there in August 1943) tasked, along with other medical scientists and lay persons, with performing the Weil-Felix reaction and comparative laboratory tests with sera from people artificially infected with virulent strains of typhus (see Fleck [1948a] 2011, 498; see also [1948b] 2011; 502) to determine the efficacy of different strengths of various sera.⁸

The rather general characterizations of what is counted as ‘normal science’ belie the specific conditions of the work of scientific collectives in the camp, which were characterized by coercion, pressure to produce the desired results, pressure to produce those results in time to avert further reprisals, pressure for large-scale vaccine production, and work in non-specialist scientific collectives. Fleck spells out other external constraints:

... [the thought-style of the camp collective] was characterized, in the first place, by replacing the fundamental specialist knowledge (which as not available) and the experiments (in which one had no confidence) with speculative considerations, and by replacing the practical specialist experience (which was also not available) with so-called common sense. (Fleck [1946] in Cohen and Schnelle 1986, 124)

The absence or illegitimacy of a ‘leading idea’, the medically insufficiently trained ‘*Leiter*’ devoid of scientific authority,⁹ the coercive conditions, the absence of any reference points of traditions of, or experience in, scientific cognition, the isolation of this thought collective from other scientific collectives, the impossibility of transmitting ideas between “thought collectives” (apart from the limited correspondence between Balachowsky and Tréfouel) (see n. 10), the deductive

⁸ An additional task was to examine whether the germ of typhus (*Rickettsia prowazekii*) is found in the lungs of mice and rabbits which had been infected by a certain method developed in the Copenhagen-based State Serum Institute between March and June 1944. This and other experiments with various vaccine derivatives (those produced according to procedures developed by Weigl and by Cox (beginning of 1942), and according to a procedure involving dogs’ lungs developed in Bucharest and by Durand and Giroud (autumn 1942), were carried out under the supervision of SS Major Erwin Ding-Schuler, director of the clinical station of the Typhus and Virus Research Division of the Hygiene Institute of the Waffen-SS located in Block 46. Fleck reports that 900 persons had been infected with *Rickettsia* by SS doctors ([1945] 2011, 492). According to estimates of other inmate physicians, experiments on more than 950 prisoners were carried out in 24 series of experiments between January 1942 and April 1945 (Kogon [1946] 1974, 176–177, in Schnelle 1986, 24; Waitz and Ciepielowski 1946, 324; Weindling gives the figure of 450–600 prisoners until the end of 1944—see Weindling 2000, 355).

⁹ Fleck calls Dr Ding a “dumbhead” whose medical degree was awarded only on the basis of services rendered to the Party. He further attests to the *Leiter*’s “lack of orientation” and “illiteracy” in matters of science (Fleck [1958] 2011, 524).

reliance on an arbitrary selection of ‘textbook science’,¹⁰ and the impossibility of participating in several “thought communities”, could not facilitate connections that could become operative as internal conditions of knowledge formation and transmission.

Under these circumstances, external constraints were inversely related with internal constraints on knowledge. For the prisoner researchers, the numerous external constraints were extremely onerous, but there were few internal constraints that would be conducive to the systematic development of a thought style. Moreover, there were no systematic connections between external and internal constraints which could facilitate the formation of intellectually cohesive thought collectives on the basis of directed perception and shared discourses on ideas. The thought collective operating under extreme external and haphazard internal constraints would acquire social authority, while lacking scientific-logical authority.

...one can say that the lack of specialist education in the empirical field can best be recognized by the limitless accuracy of logical inference ... [attempts to] solve specialist problems by a speculative method, starting from a few textbook theorems which play the role of axioms, and from some data obtained from one’s own experience – which were not linked together so as to form knowledge, but which were taught and commented upon – just as one expounds dreams or comments on the declarations of a diplomat. (Fleck [1946] in Cohen and Schnelle 1986, 124)

The error, Fleck insists, grew out of the closed system of ideas transported by a collective mood in the grip of external constraints—which would have been impossible in the case of work in which the individual member, rather than being a representative of collective functions, remains a conscious source of action (see Fleck [1946] in Cohen and Schnelle 1986, 123).

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¹⁰ In the synchronic typology of the development of science at any given particular moment, ‘textbook science’ is accorded a category of its own: Expert science is made up of journal science (reports of a vanguard of the creative experts, provisional & personal, without fixed position or general recognition, but with fragmentary nature of problems, contingency of the material), vademecum science (professional science in handbook form, with a certain independence from the vanguard of journal science, impersonal, comparatively fixed, with the certainty of intracollective communication of thought exercising thought constraint: “critical synopsis in an organized system” (Fleck [1935] 1979, 118; see also Fleck [1936] 2011, 295), and textbook science (the latter educating amateurs in popular science under omission of detail and controversial opinions, given to artificial simplification and vividness) (Fleck [1935] 1979, 112, 116, 118, 119, 124; see also Fleck [1936] 2011, 295).

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