
Review: Ludwik Fleck and the Sociology of Knowledge

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REVIEW

Ludwik Fleck and the Sociology of Knowledge

Jonathan Harwood

Ludwik Fleck, *Genesis and Development of a Scientific Fact*, translated by F. Bradley and Thaddeus J. Trenn, edited by Thaddeus J. Trenn and R.K. Merton, foreword by T.S. Kuhn (Chicago and London: The University of Chicago Press, 1979), xxviii + 203pp., £14.00/\$17.50, £7.25/\$6.95 pbk. ISBN 0-226-25324-4 (-25325-2 pbk).

Ludwik Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, introduced and edited by L. Schäfer and Th. Schnelle (Frankfurt: Suhrkamp, 1980), xlix + 190pp., DM 12. ISBN 3-518-07912-3.

Ludwik Fleck, *Erfahrung und Tatsache*, collected essays edited and introduced by L. Schäfer and Th. Schnelle (Frankfurt: Suhrkamp, 1983), 195pp., DM 16. ISBN 3-518-28004-X.

Thomas Schnelle, *Ludwik Fleck: Leben und Denken* (Freiburg: Hochschul-Verlag, 1982), 376pp., DM 70. ISBN 3-8107-2165-4.

Robert S. Cohen and Thomas Schnelle (eds), *Cognition and Fact: Materials on Ludwik Fleck*, Boston Studies in the Philosophy of Science, Volume 87, series editors R.S. Cohen and M.W. Wartofsky (Dordrecht, Lancaster and Boston, Mass.: D. Reidel, 1985), x + 468pp., Dfl. 180/£49.95/\$59.50. ISBN 90-277-1902-0.

In 1935 a Polish physician named Ludwik Fleck published a monograph in German entitled *Genesis and Development of a Scientific Fact*. Although it addressed central issues in the philosophy of science, the book made virtually no impact. Most of the reviews it received appeared in medical journals or popular magazines. After the war it languished in obscurity, despite

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Kuhn's passing reference to it in *The Structure of Scientific Revolutions*, until a German scholar rediscovered it in the early 1970s.¹

Recently, however, Fleck's work has been granted much more attention. In 1979 an English translation of his book was published,² quickly followed by a reissue of the German edition,³ a dissertation on his life and work,⁴ a collection of his essays on sociology of knowledge,⁵ and a conference devoted to him.⁶ The explanation for this sudden interest is clear: Fleck anticipated fifty years ago many of the current arguments for a sociology of scientific knowledge, arguments which in Anglo-Saxon (and quite possibly German) scholarship have been derived largely from Kuhn.

In an essay of this scope it is impossible to do expository justice to the works under review. I will, therefore, draw upon these works in order to venture a judgement of Fleck's significance. My question is: how are we to welcome this prescient latecomer? Is he largely of historical interest? Or is his writing still of heuristic value for the sociology of knowledge?

A Singular Career

Fleck was born in 1896 in Lwów ('Lemberg' until 1918), a city whose close cultural ties to Vienna meant that he grew up speaking both Polish and German fluently. He trained in medicine after World War I and developed a particular interest in medical bacteriology, working during the 1920s and 1930s in various hospital laboratories while conducting research in his free time. Never the narrow scientist, he devoted his evenings to philosophy, sociology and history of science and medicine. Although his reading in these areas was evidently rather unsystematic, according to Schnelle, Fleck's interest was sustained by his membership of various local scientific societies which fostered scholarly breadth and inter-disciplinarity. It was in the Lwów Society for the Friends of History of Medicine that he presented his first sociological paper (ET: 1927).

Attempting to identify the intellectual context in which Fleck's sociological thought developed between the mid-1920s and mid-1930s, Schnelle concludes that Fleck was almost totally unaware of the contemporary German literature in sociology of knowledge.

That he could fashion a concept of 'thought-style' in ignorance of Mannheim or Lukacs is remarkable testimony to the fact that sociological thinking about knowledge was very much 'in the air' in German cultural circles after the first world war. Furthermore, what little Fleck had read of the sociology of knowledge failed to satisfy him; Durkheim and Lévy-Bruhl were scolded for exempting science from sociological analysis. Even if one compares him with those of his contemporaries who did venture a sociological analysis of science, Fleck was far more radical. While Max Scheler, for example, preferred to treat science as a body of thought observed in abstraction from afar, Fleck went straight to the heart of the cognitive process, analysing perception and classification in empirical detail. Schäfer and Schnelle (EEWT) thus justifiably regard Fleck as the first writer to make a sustained case for a sociology of scientific knowledge.

Most of Schnelle's monograph is devoted to establishing that the intellectual milieu which shaped Fleck's epistemological writing was dominated by three philosophers at the University of Lwów: K. Twardowski, K. Ajdukiewicz and L. Chwistek. Much of Fleck's writing addresses problems with which these three had wrestled: is reality given or constructed? Is cognition built upon formal deductive structures? Can a multiplicity of knowledge systems (thus 'realities') be rationally grounded? In attempting to solve these problems, Fleck sometimes adopted the philosophers' assumptions on particular matters. From Twardowski and Chwistek, for example, he appropriated the idea that perception as well as concept-formation were active processes in which the knowing subject, in accord with his/her practical needs, abstracts particular features from the objects observed. From all three, he took over the viewpoint that, while the assumption of 'things in themselves' was plausible, it was of little use to any theory of knowledge because humans had only mediated access to such things.

Despite this common ground, however, Fleck's solution to the problems posed by the Lwów philosophers was distinctive. While Twardowski regarded the *individual* as perceiving cognitive objects, Fleck wrote: 'We look with our own eyes, we see with the eyes of the collective' (ET 154). Faced with a range of formally equivalent knowledge-systems, Chwistek attempted to defend their rationality by attributing their underlying assumptions to 'healthy human reason' but without elaborating on the nature of human reason. Fleck objected that human reason was not static

but historically and socially variable. Having conceded a multitude of incommensurable axiomatic structures, each consonant in some way with reality, Ajdukiewicz could not explain why people in fact chose one rather than another. Fleck argued that such choices were dictated by the circumstances in which collectives found themselves. Thus Fleck certainly gained much from his philosophical contemporaries, but his genius lay in the ability to use what little sociology he knew in order to open up for empirical analysis those epistemological issues to which the philosophers had no answer.

By the mid-1930s the most productive period for Fleck's sociological work was over. With the German occupation of Poland in 1941, he was confined to Lwów's Jewish ghetto, where appalling sanitary conditions meant that 70 percent of the inhabitants suffered from typhus. In the ghetto hospital he attempted to develop a vaccine against typhus until he was deported in 1943 to Auschwitz and subsequently to Buchenwald. There, in the Waffen-SS's Institute for Hygiene he directed a laboratory whose task was to develop a typhus vaccine which would protect SS guards from infection. Even during this period Fleck remained interested in the nature of scientific inquiry. In an essay published after the war he described the social process whereby one of the teams in his laboratory (composed exclusively of bacteriologically illiterate members) managed to convince themselves initially of the validity of their findings, as well as the subsequent process whereby that certainty was gradually undermined by particular events.

From the end of the war until the late 1950s, with professional recognition, a large research school and much improved facilities in Lublin and Warsaw, Fleck's bacteriological work flourished while his sociological interests receded. In 1957 he emigrated to Israel, where he died in 1961.

By any reckoning, Fleck was a remarkable scholar. That he managed not only to stay abreast of history and philosophy of science while pursuing a medical career but also to publish contributions to that literature is impressive. That this published work should have proved highly original, even twenty years later, is phenomenal. There can be little doubt, therefore, that Ludwik Fleck is a figure of considerable historical interest, even though his sociological work made little impact in its own time. But are his ideas still novel? What might the sociology of knowledge be able to learn from Fleck today?

Fleck versus Kuhn

When reading Fleck, the modern writer who most often comes to mind is T.S. Kuhn. Both of them developed generalizable propositions about scientific change in the course of detailed historical analysis rather than in the more abstract manner common among philosophers. Various writers have noted the striking similarities between Fleck's conception of science and Kuhn's 'normal science', the most important of which are these:

1. According to Fleck, scientists' work is characterized by a tradition of shared assumptions ('thought-style') which are largely invisible to members and thus rarely questioned (ET: 1929).

2. These assumptions, he argues, define which questions are significant and prefigure the appropriate answers (GDSF 40, 83–84, 104).

3. Using several vivid examples, Fleck shows that perception is an active and selective Gestalt process, conditioned by these assumptions. When one initially looks at objects in a visual field, the impressions are unclear and chaotic. With experience they acquire shape and identity through categories provided by the prevailing thought-style (ET: 1929, 1935, 1947; GDSF 28–30, 90–92).

4. Challenges to the thought-style as commonly rejected or assimilated (GDSF sec. 2.3; ET: 1935).

5. Members of different research communities ('thought-collectives') adhere to different thought-styles and tend to talk past one another (ET: 1936; GDSF 109).

6. Admission to the research community proceeds via a dogmatic form of education. The prevailing thought-style is transmitted to the pupil, not through the mastery of formal principles, but through a process of 'experience' that cannot be rationally reconstructed but which results in the acquisition of craft knowledge (ET: 1927, 1935, 1946; GDSF 52–54, 87ff., 95–97).

7. As scientific knowledge develops, its scope widens, we acquire more knowledge overall, and some older problems are solved, but the process is patchy. Science cannot be said to approach the truth because successive thought-styles raise new problems while discarding older areas of understanding (ET 55, 125, 132; GDSF 19, 51, 137–39).

Inevitably, there are also differences between Fleck and Kuhn, both rhetorical and substantive. Among the former, as Baldamus

has emphasized, it is evident how unreservedly Fleck embraced the sociological implications of his work. Repeatedly he attacked positivist philosophers of science for idealizing the process of knowledge-growth. An empirically adequate theory of knowledge, he insisted, must necessarily be sociological (ET: 1929, 1936). Kuhn, on the other hand, was slower to encourage the sociological extension of his work. Ambivalent about his enthusiastic reception by sociologists during the 1970s, he confesses in his foreword to GDSF to having found Fleck's sociology of the collective mind 'vaguely repulsive' (ix).

Among substantive differences, the most significant concerns meaning-change. There is, to be sure, common ground here. Both authors agree that concepts change their meaning as they are incorporated into newer thought-styles and that knowledge grows, not by accretion but by qualitative shifts as findings are reinterpreted. Moreover, both authors treat meaning in a 'Wittgensteinian' manner (that is, terms' meaning is context-dependent), despite the fact that Fleck was writing two decades before *Philosophical Investigations* was published. Nevertheless, while Kuhn and Fleck agree that meaning-change is fundamental, they differ over the way in which it occurs. When Kuhn discusses meaning-shift, it is in connection with scientific revolutions; normal science appears as a period of relative consensus on the meaning of concepts central to a paradigm until the onset of extraordinary science. Whether such a view was plausible or not became the subject of debate about ten years ago between sociologists from an older 'normative' and a newer 'interpretivist' (that is, Wittgensteinian) tradition.⁸ Whatever Kuhn had originally intended in *The Structure of Scientific Revolutions*,⁹ it has since become widely accepted among sociologists that even in normal science meanings are constantly subject to negotiation. Accordingly, the extent of meaning-stability and consensus in all phases of science must for the moment be regarded as an empirical matter.

Fleck's conception of meaning-change is more consistently Wittgensteinian. As Schäfer and Schnelle have rightly noted (EEWT xxviii–xxix, xxxix), one of Fleck's most important insights is that meaning-change is a continuous feature of inquiry *within* any given thought-style. Knowledge, he writes, consists of a network of concepts and facts in dynamic equilibrium with each other (GDSF 79). Each new fact shifts the meaning of all terms throughout the network (GDSF 102–03). Even the background

assumptions of a thought-style are constantly in flux (GDSF 64). It is precisely this flexibility of the network which allows a thought-style to resist *major* challenge while conveying the illusion of invariance. The social basis of this perpetual meaning-shift, he says, lies in the diversity of interpretations within the thought-collective at any one time:

One can assume that each observer carries out observations according to his thought-style. These individual styles differ to varying degrees; the more distinctive the styles are, the more discrepant the corresponding observations will be ... If these thought-styles were everywhere identical and invariant, each discovery (that is, the perception of something new) would be impossible. (ET 68, cf. Schnelle 30–31)

Whence this individual diversity within a thought-collective? It derives, according to Fleck, from the fact that each member of a given scientific thought-collective is simultaneously a member of many other collectives both in and outside science (GDSF 105). Since no two individuals are identical in their memberships, each scientist conducts a unique blend of meanings from the outside world and imparts, in consequence, a subtly distinctive meaning to the concepts shared by members of the research community (GDSF 110). At the research front, therefore, diversity of interpretation — even misunderstanding (GDSF 119–20) — is characteristic, as is evident from the tentative and exploratory tone of knowledge-claims in specialist journals. In the systematic accounts of the field published in *handbooks* and review papers, by contrast, the tone is confident and consensual. On the way from the journal to the handbook, knowledge-claims have circulated among specialists and been rejected or undergone shifts of meaning (sometimes so considerable as to be unrecognizable to their originators [GDSF 118–24, ET: 1935]) before becoming part of a new consensus.¹⁰ This consensus is only temporary, however; although handbook science provides members of the thought-collective with a heuristic map, the precise meanings of its terms are soon subject to negotiation by workers at the research front, struggling to reconcile the map with what they observe in nature. Thus in Fleck's account, meaning-stability is a brief and precarious phase in the genesis and development of a scientific fact.

Kuhn's concept of 'scientific revolution' as a recurring feature of scientific change has drawn criticism from various historians who have complained that such discontinuities have been non-existent

in some disciplines and very rare even in physics and chemistry. Interestingly, Fleck's model of science lacks any concept analogous to 'revolution'. The fact that there had been no such conceptual upheavals in bacteriology cannot suffice to explain this; he was clearly aware of recent upheavals in the physics of his day. Is it that by building continuous meaning-change into science, Fleck was able to dispense with the need for radical shifts in conceptual development? Perhaps Kuhn found the Einsteinean revolution in physics so striking that he concentrated on meaning-variance *between* paradigms to the exclusion of that *within* them. In any event, if Fleck has more adequately captured the latter phenomenon, the fact of rare revolutions remains. Indeed, the theoretical task of explaining them becomes more difficult. Whereas Kuhnian paradigms are unable to accommodate anomaly indefinitely, conceptual networks in Fleck's scheme possess infinite resilience, bouncing back vigorously, though altered, after each challenge. Were Fleck to have developed a model of revolution, it would have to have been a radically relativist one: revolutionary research programmes would supersede even the healthiest of *ancien régimes*.

Thought-Style and Thought-Collective

In keeping with his explicitly sociological analysis of science, Fleck has rather more to say about the structure of the scientific community than does Kuhn. Each thought-style is borne by a thought-collective. Within a given thought-style there exist many particular concepts/theories/methods which are developed by particular sectors of the thought-collective, each sector consisting of a small 'esoteric circle' of specialists and a larger 'exoteric circle' of non-experts. The thought-collective thus consists of many esoteric and exoteric circles which overlap since an individual scientist will belong simultaneously to one esoteric but many exoteric circles. Within *science*, these circles are mutually dependent: members of the exoteric circle must accept on trust the knowledge-claims generated by the esoteric circle, while the ultimate validity of such claims (hence the possibility of progress) rests on the exoteric circle's assent. In this way, Fleck emphasizes, the scientific community is fundamentally 'democratic': the esoteric élite proposes, and the exoteric mass disposes. The structure of a

religious or metaphysical thought-collective, in contrast, is undemocratic because its exoteric circles lack the power to challenge its esoteric circle's claims (ET: 1929).

Unfortunately, Fleck's discussion of social structure is very abstract. He makes little attempt, for example, to relate his richly detailed account of observation, classification and the emergence of a fact in bacteriology to an equally detailed analysis of the organization of the respective scientific communities. Furthermore, when he applies the terms 'thought-collective' and 'thought-style' to concrete science, his analysis is inconsistent. 'Modern science' is said to constitute a single thought-collective (GDSF 103, 105). Similarly, he refers frequently to *the* thought-style of modern science, as opposed to those of religion, art, fashion, sport, or politics. On the other hand, he also refers in passing to the distinctive thought-styles of different scientific disciplines (GDSF 108) and even, as in the quotation above, of different scientists, without reconciling these divergent usages. Towards the end of his monograph (section 4.5), he begins to apply the term thought-style in a more promising way, discussing different styles within a given discipline but only in widely separated historical periods. Without conceptual elaboration, therefore, his analysis is not as useful as it might be to contemporary sociologists and historians interested in contrasting styles within a given discipline in a particular period.

Quite apart from these ambiguities concerning the scope of the term 'thought-collective', the meaning of 'thought-style' is also disturbingly broad. It is said to:

direct perception (ET: 1935)

specify questions to ask and solutions to be sought (ET 48), as well as methods to use (GDSF 99)

confer intellectual predispositions and habits (ET 68)

establish 'a certain mood' (GDSF 99).

When the concept is stretched to cover so much ground, it ends up meaning little more than 'presupposition'. This minimal meaning may, of course, have been very useful for Fleck's attacks upon logical positivism during the 1930s, but it is hardly novel today when the presuppositional nature of science has been argued so often.

Much more interesting is Fleck's suggestion that presuppositions of a thought-style may arise from non-rational commitment to primitive images or metaphors (*Urdeem*) — for example, the idea of fouled blood in syphilis research (GDSF), the analogy between fire and life in medicine and physiology (ET: 1936), or the model of 'attack and defence' in the conceptualization of infectious disease (GDSF 59ff). Necessarily derived from inherited popular notions, such images are reshaped by the scientific community in order to yield concepts whose more specific meaning better serves the collective's particular purposes. Like a letter, therefore, every scientific concept bears signs of both its origin and its destination (ET 92), its past as well as its future.

To conceptualize the structure and emergence of thought-styles in this way — that is, to propose that they are based upon a small number of ontological and epistemological assumptions endorsed by the members of a thought-collective — is certainly useful. But since Fleck failed to develop these insights, he has little to offer those already familiar with more recent concepts such as root metaphor, themata (G. Holton), the hard-core of a research programme (Lakatos), or the metaphysics embodied in the models and values of a disciplinary matrix (Kuhn).

Finally, there is the question of the relation between thought-style and the process of thinking. Unfortunately, Fleck approached this relation in two contradictory ways which were never reconciled. In a materialist vein he argued that human beings control their ideas rather than vice versa, and that our knowledge is shaped, at least partly, by constraints imposed by the real world (ET 70, 75, 126, 168; GDSF 38, 46, 51). On the other hand, however, as Stephen Toulmin and David Bloor noted at the Hamburg symposium, he repeatedly insisted that thought-style 'dictates' and 'coerces' how and what the scientist sees and thinks (ET 75; GDSF 41, 122). Clearly, this latter idealist interpretation of style fits very poorly with Fleck's Wittgensteinian emphasis upon the continual renegotiation of meanings within a collective, including changes in meaning of the style itself. For example Fleck wrote:

... while it lasted, only one solution to any given problem conformed to that style ... Such a stylized solution, and there is always only one, ... is always, or almost always, completely determined within a thought style. (GDSF 100)

But how can a style 'coerce' if its meaning is not intrinsic but negotiable? It is surely the collective which coerces; style is merely the medium through which coercion is exerted. If Fleck were simply using the phrase 'style coerces' as shorthand for the latter more sociologically consistent but unwieldy claim, he would surely have pointed this out at least once somewhere in his work. I suspect that, like some others, he simply did not notice this explanatory tension in his work. Despite the best of intentions, it is quite easy to slip into the habit of regarding thought-style as a kind of intervening variable, *emerging* through the actions of the research community and, in turn, *feeding back* upon its inventors to channel their perception. But to portray cognition in this way is inconsistent: the former process is sociological but the latter remains intellectualist.

Herewith a reminder of the dangers of reifying thought-style or paradigm, of imputing to cognitive structures the power to constrain thought and action. Unlike, for example, theories, concepts or techniques which scientists consciously recognize as the objects upon which they work, style is an analyst's construct of which scientists are quite unaware. As historians and sociologists we infer stylistic differences from the fact that different cultural products from the same community (for example, its art and its science) are similar while comparable products (for example, theories of light) developed by different communities are distinctive. Alerted by the existence of such patterns, we try to tease out the ontological or epistemological assumptions which are common to several sectors of a community's culture. The concept of style thus enables us to reduce the complexity of the bodies of thought which we analyse, arriving at a handful of lowest-common-denominators. Once these have been inferred, sociological explanation of scientists' commitment is greatly simplified: it can concentrate upon these deeply embedded assumptions. At no point in the analysis need style acquire an independent status as a cognitive constraint. It is thus the *genesis* of such cognitive pattern, not its function, which requires explanation.

Fleck's Theoretical Significance

It is hardly surprising that the editors of the German and English

editions of Fleck's work are inclined to assign more than simply historical importance to his writing. In my view, however, such claims have not been adequately justified. In a brief conference paper, for example, Trenn has suggested that Fleck's writing has an important contribution to make to our understanding of both discipline-formation and science policy issues.¹¹ Since Fleck did not address the former problem, and since almost all writing in the sociology of scientific knowledge is relevant to one or other aspect of science policy, the grounds for this optimism are unclear.

Similarly, Schnelle argues that Fleck's most important and novel contribution is to conceptualize the genesis of fact in terms of 'active' and 'passive' connections (33–34). Active connections are properties of the system under study which are assumed within a thought-style. On the basis of these assumptions, other properties of the system appear obvious or inescapable, 'imposing' themselves upon the observer; these are what Fleck terms passive connections. Active connections have an arbitrary character; passive ones seem necessary. Accordingly, the goal of each research community is to maximize the passive connections in its knowledge-claims while minimizing the active ones. This distinction certainly impinges upon an important problem — namely, the rhetorical tactics which are most effective in establishing knowledge-claims. But can these concepts take us any further with the problem? Possibly novel in their day, active and passive are little more than a sociological reformulation of the concepts 'subjective' and 'objective'. That is, active or subjective connections are characteristic of narrowly institutionalized (thus contentious) knowledge-claims. Broadly institutionalized (thus unexceptionable) knowledge-claims embody passive or objective connections.

The most recent contribution to Fleck scholarship, *Cognition and Fact*, is useful in several respects. It makes available to an English-speaking audience a summary of Schnelle's work, Fleck's essays on epistemology and diverse commentaries on Fleck by historians, philosophers and sociologists. Although none of Fleck's essays represents a substantial theoretical advance over his monograph, one or two of them (especially 'Scientific Observation and Perception in General', 1935) contain vivid illustrations of the constructed and context-dependent nature of perception and should prove useful for teaching. The commentaries in *Cognition and Fact* fall into two categories. The first category is, broadly

speaking, biographical in approach. These essays (by N. Rotenstreich, Jerzy Giedymin, B. Wolniewicz, W. Markiewicz, T. Schnelle, B. Zalc, A. Moulin and I. Löwy) relate Fleck's epistemological and scientific views to a variety of intellectual and social contexts: developments in twentieth-century philosophy, interwar Polish philosophical traditions, the culture of Lwów, and schools of thought within interwar bacteriology and immunology. The second category addresses general problems of epistemology. These authors (S. Toulmin, P.A. Heelan, Y. Elkana, D. Wittich, S. Shapin and D. Bloor) use Fleck's work in various ways. Some are critical while others appropriate Fleck as an ally in advancing a favoured epistemological position. Significantly, however, none of these commentators (with the possible exception of Dieter Wittich) makes a serious claim for the heuristic value of Fleck's concepts in contemporary sociology of knowledge. Wittich argues that Fleck's work offers far more theoretical possibilities than Kuhn's, but his argument (confined to 318–19) is too condensed to be convincing.

It is thus difficult not to agree with Baldamus' judgement (see note 7) that, while enormously insightful and pioneering, Fleck's work is not systematic enough to have had the impact, even in the favourable climate of the 1960s and 1970s, enjoyed by *The Structure of Scientific Revolutions*. Various observers have noted conceptual inconsistencies within Fleck's monograph, and in several places towards the end of the book (section 4.3) he apologized for truncated discussion of various points, all of which suggests that he wrote in haste.

The comparison with Kuhn is again instructive. Like 'thought-style', the concept of 'paradigm' was originally used by Kuhn, to its detriment, in a great variety of ways. But unlike Fleck, Kuhn was a member of a professional community of historians and philosophers of science whose critical responses led him to refine the concept successfully. Paradigm as 'exemplar' characterizes the process of scientific discovery much more specifically than does 'thought-style', and even paradigm as 'disciplinary matrix' — very close in meaning to that of thought-style — is more precise in that it identifies particular kinds of cognitive elements: symbolic generalizations, exemplary problem-solutions, models and values. It is hardly surprising, of course, that Fleck's insights so often remain underdeveloped. Not only was he of necessity a 'spare-time philosopher', but the only academic community whose

response might have carried his work further was disintegrating under Nazi persecution after 1933. Fleck's work undoubtedly suffered through intellectual isolation, but this very fact exemplifies his central thesis: that discovery is a collective process. If Fleck's work is to make an impact upon sociology of knowledge in future, his admirers will have to use their ingenuity to refashion the fragmentary concepts in his work into full-blown theory. If they are successful, the outcome will be a collective achievement whose features — as Fleck himself recognized — will bear only a tenuous connection to his original intentions.

• NOTES

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1. W. Baldamus, 'The Role of Discoveries in Social Science', in T. Shanin (ed.), *The Rules of the Game* (London: Tavistock, 1972), 276–302.

2. (Under review), henceforth GDSF. The editors of this edition are to be thanked for providing an index as well as translating various passages which Fleck had left in Latin.

3. *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (under review), henceforth EEW. T.

4. Schnelle, *Ludwik Fleck: Leben und Denken* (under review).

5. *Erfahrung und Tatsache* (under review). Reference to specific passages in this book will be given in the text as ET, followed by the relevant page numbers. General references to entire essays will be indicated by 'ET:', followed by the year of publication.

6. 'Kolloquium Ludwik Fleck', Hamburg, 13–16 September 1981. The proceedings of this conference have been recently published as Cohen and Schnelle (eds), *Cognition and Fact* (under review). The volume includes English translations of Fleck's essays on sociology of knowledge, originally published as *Erfahrung und Tatsache*:

'Some Specific Features of the Medical Way of Thinking' (1927)

'On the Crisis of Reality' (1929)

'Scientific Observation and Perception in General' (1935)

'The Problem of Epistemology' (1936)

'Problems of the Science of Science' (1946)

'To Look, to See, to Know' (1947)

'Crisis in Science' (1960)

7. Brief introductions in English to Fleck's ideas may be found in GDSF 154–65, and in W. Baldamus, 'Ludwik Fleck and the Development of the Sociology of Science', in P.R. Gleichmann, J. Goudsblom and H. Korte (eds), *Human Figurations: Essays for Norbert Elias* (Amsterdam: Amsterdams Sociologisch Tijdschrift, 1977), 135–56. A bibliography of the small secondary literature on Fleck up to 1982 is to be found in Schnelle, *op. cit.* note 4, 70–71.

8. Cf. J. Law and D. French, 'Normative and Interpretive Sociologies of Science', *Sociological Review*, Vol. 22 (1974), 581–95, as well as the exchange between M. Mulkay and B. Barnes/J. Law in *Sociological Review*, Vol. 23 (1975), 509–26; Vol. 24 (1976), 115–33.

9. That Kuhn may not originally have been aware of meaning-change within normal science is perhaps reflected in the facets of Fleck's work which he singles out for praise (GDSF ix): (a) the distinction between journal and handbook science (discussed below) and (b) the transfer of ideas between thought-collectives. Central to both is the phenomenon of meaning-change.

10. Unlike those modern sociologists of science influenced by ethnomethodology, Fleck seems to have held that the process whereby consensus emerges cannot in principle be explained (GDSF 72).

11. Thaddeus J. Trenn, 'Some Reflections on the Chicago Edition of Fleck's Monograph', mimeo distributed at the Fleck symposium (*cf.* note 6).

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