Available online at www.sciencedirect.com
ScienceDirect
Stud. Hist. Phil. Biol. & Biomed. Sci. 38 (2007) 136–158
Studies in History and Philosophy of Biological and Biomedical Sciences
www.elsevier.com/locate/shpsc

# The reading of scientific texts: questions on interpretation and evaluation, with special reference to the scientific writings of Ludwik Fleck

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Received 13 October 2005; received in revised form 12 May 2006

**Abstract**

Ludwik Fleck is remembered for his monograph published in German in 1935. Reissued in 1979 as *Genesis and development of a scientific fact* Fleck's monograph has been claimed to expound relativistic views of science. Fleck has also been portrayed as a prominent scientist. The description of his production of a vaccine against typhus during World War II, when imprisoned in Buchenwald, is legendary in the scholarly literature. The claims about Fleck's scientific achievements have been justified by referring to his numerous publications in international scientific journals. Though frequently mentioned, these publications have scarcely been studied. The present article discusses differences in interpretation and evaluation of science in relation to the background of the interpreters. For this purpose Fleck's scientific publications have been scrutinized. In conjunction with further sources reflecting the desperate situation at the time in question, the results of the study account for a more restrained picture of Fleck's scientific accomplishments. Furthermore, based on the review of the latter, certain demands characterizing good science could be articulated. The restricted possibilities of those not trained in science or not possessing field specific knowledge, evaluating science are discussed, as are also formal aspects of scientific papers and questions related to research ethics.


*Keywords:* Ludwik Fleck; Scientific writings; Typhus vaccine; Research ethics

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doi:10.1016/j.shpsc.2006.12.008

## 1. Introduction

In recent years, the study of science has been the focus of increasing interest among scholars within philosophy and sociology of science. Studies have to a great extent centred upon the pursuit of institutionalised science, its knowledge production and its knowledge claims. Though non-consenting voices have been articulated, the image of a unified natural science still informs philosophy and social sciences, including a theoretical and methodological outlook (cf. Knorr Cetina, 1999).

Many of the studies focus on theoretical or ontological questions related to theories, hypotheses or explanations in science, not infrequently undertaken by philosophers or sociologists of science not trained in science, or by those unacquainted with the scientific field in question (cf. Koertge, 2000). This fact is not changed by numerous notable exceptions, Kuhn being one of them. Rather, when undertaken without training in science, or without knowledge of the specific field in question, the task has often been accomplished by a tacit disregard of the scientific concepts constituting a necessary prerequisite for the proper understanding of the scientific enterprise, or the content of science, as it appears in the scientific texts. Whether or not this type of philosophising results in a distorted or biased interpretation of science—thereby possibly deepening the divide between the humanities and the natural sciences—is an urgent question.

Despite the early path-breaking discoveries within biology such as those by Darwin and Mendel, soon followed by fundamental discoveries within bacteriology in the latter half of the nineteenth century and, as a result, rapid developments in microbiology, genetics and cell biology, until recently physics has been viewed as epitomizing science, whether the studies were to be performed within the frame of philosophy of science, epistemology, sociology of science or applied linguistics (Hyland, 2002).

In this context, the monograph *Entstehung und Entwicklung einer Wissenschaftlichen Tatsache* (Fleck, 1980) published in 1935 by the Polish–Jewish bacteriologist Ludwik Fleck, is of interest. Though neglected at the time, the monograph has been met with an almost unanimous but ever-increasing acclaim. Since its almost serendipitous rediscovery in the late seventies (cf. Schnelle, 1986), it has not only been cherished by philosophers and sociologists of science as deeply influencing Kuhn (1962), but is also considered an important contribution to the sociology of science expressing prescient views in epistemology. It was soon translated into English as *Genesis and development of a scientific fact* (Fleck, 1979), and reissued in German (Fleck, 1980). Extensive material on Fleck is found in *Cognition and fact* (Cohen & Schnelle, 1986).

The apparent neglect by Fleck's contemporaries has been difficult to account for. The judgement of his modern humanist interpreters regarding this neglect has, if not been harsh, rather been viewed with confusion and surprise. Whatever the reasons might be, Fleck's choice of examples taken from his own discipline has been cherished as creating a refreshingly productive alternative to the so far almost exclusive use of physics in exemplifying science.

In addition to his contribution to epistemology and sociology of science, Fleck has been viewed as a prominent scientist at the time who has influenced his own professional field, serology. He is claimed to have manufactured an anti-typhus vaccine during the Second World War. He is also asserted to have developed a test for the early diagnosis of typhus, known as the Exanthin test, and to have pursued important research in a field denoted leukergy—all supported by his prolific writings as evidenced by the numerous publications in international scientific journals (Cohen & Schnelle, 1986, pp. 445–456).

Fleck's two lines of inquiry—one in philosophy, theory of science or epistemology, and another in natural science or bacteriology and microbiology—have, quite naturally, been interpreted as interrelated and mutually informing. A comparative study of his two fields of inquiry would thus be of interest. This is further substantiated by the fact that in the studies of science mentioned above, Fleck's epistemology—including his epistemic concepts (such as thought style, thought collective, signal of resistance, *Widerstandsaviso*, active and passive associations or linkages, *Kopplungen*)—have been applied in the analysis of scientific writings (Bazerman, 1988).

## 2. The reading of Fleck

The monograph by Fleck can be read in different ways. Firstly, it can be read as a text; secondly, contextualised with regard to the time of its origin, the mid-thirties; or, thirdly, as reflecting recent thoughts within epistemology or philosophy of science, corresponding to the immediate interests of the readers in question (cf. Harwood, 1986; Wettersten, 1991; Borck, 2004; Hedfors, 2006a). As the three readings could be further separated according to whether the scientific parts of the monograph have been included or excluded, at least six different readings emerge—all highly conceivable and also retrieved in the various interpretations. The present approach attempts a reading that is contextualized historically, and in which the scientific parts of his writings are integrated. It is founded on the assumption that the epistemic deliberations of Fleck are based on his field-specific knowledge, bacteriology. Accordingly, a study of his scientific writings could aid in the interpretation of the former.

In a recent study, Fleck's description in his monograph of the Wassermann reaction, including his often conflicting views and his scanty and sometimes surprising sources forming the basis of his account of the reaction, have been scrutinized. In contrast to the current received view, an alternative reading, integrating the scientific assumptions, has been proposed (Hedfors, 2006a). The present article focuses on the scientific writings of Fleck. Several reasons can be adduced. Firstly, Fleck's scientific writings, though frequently referred to and commented upon by his humanist interpreters, have scarcely been studied. Secondly, Fleck's scientific writings, cherished by his current interpreters, might reflect issues related to the evaluation of science. Thirdly, by its sobering distance, the study of past accomplishments might help to illuminate recent questions on different interpretations of science and, also, different interpretations of a common past.

## 3. Materials and methods

Twenty-two articles by Fleck have been selected from the bibliography compiled in *Cognition and fact*, constituting the ones published in international scientific journals between 1927 and 1962. A manual search of the relevant journals of the period in question has added no further articles. On the other hand, some articles listed as appearing in prestigious journals seem to refer to submissions rather than publications (cf. Cohen & Schnelle, 1986, p. 448).

The selection has been made on the assumption that the internationally published papers are not only the best, but also representative of Fleck's scientific writings. This is supported by the findings of double publications, with the same articles first appearing in local journals followed by publications in international journals. Not infrequently the

latter are presented as summaries of work previously published in Poland. Although the bulk of the articles are published in Polish, one can, however, infer that Fleck's scientific publications can be assigned to four different themes, three of which also separated in time. The themes are well covered by his twenty-two internationally published papers.

The first theme, of the Exanthin reaction, spanning 1923–1934, is manifested by a local publication in 1923 followed by five international publications between 1929 and 1934. The second theme is devoted to antigenic substances in the urine of patients with typhus. It appears in a post-War article of 1947, preceded by one in Polish in 1946 with the same title. The third theme is that of leukergy, spanning from 1946 to the end of his life with a last posthumous publication in 1962. The fourth theme could be depicted as one of general bacteriology, including various theoretical or applied questions sometimes exemplified by case reports or experimental studies attempting to elucidate the aetiology of certain diseases. This fourth theme runs parallel to the previous ones, though leukergy remains the focus of greatest concern immediately after the War and onwards. As has already been noted by his current interpreters, Fleck's philosophical writings are also restricted in time, spanning the pre-War and immediate post-War periods (see Cohen & Schnelle, 1986).

In the following, the various scientific themes are discussed in chronological order as manifested by Fleck's publications, related both to his professional life and to the current interpretation of his writings. The focus of the inquiry is whether notions such as good or bad science could be justifiably applied to his scientific writings. Given an affirmative or non-affirmative answer, the study will further elucidate whether a reader not informed by field-specific knowledge could deduce qualitative statements about his writings, independent of that knowledge. Finally, the question of ethics, inherent to any research procedure, will be briefly touched upon.

As a tentative hypothesis the evaluation of the merits of Fleck's papers will be performed according to the general review criteria as applied to papers published within the biomedical sciences. They basically centre upon the scrutiny of the questions at issue, the selection of material and controls, the applicability of methods used, the relevance and the robustness of data and the adequacy of the references chosen. Furthermore, ingenuity, feasibility and reproducibility as well as general research ethics are evaluated and all related to the standing of the scientific field at the time in question.

## 4. On Exanthin

By reviewing the scientific publications devoted to Exanthin, it seems reasonable to infer that, while working as a research assistant to Weigl in the early 1920s, Fleck, in collaboration with Krukowski, was assigned a project implying the design of a skin test related to typhus complementing the Weil–Felix reaction that was in current use. With World War I in recent memory, the question was both relevant and in line with current work of the period. The drawbacks of the Weil–Felix reaction were easily perceived, such as its dependence on a laboratory, and the fact that the reaction was not positive until the end of the second week of the infection. Moreover, several tests could serve as models, all attempting the rapid diagnosis of different bacteriological disorders: for instance, the Schick test of diphtheria, and the Dick test of haemolytic streptococci. Exemplifying early barren endeavours could be the Luetin test of syphilis, or lues, aimed at supplanting the Wasserman reaction.

Fleck's early studies resulted in a paper published locally in 1923, in translation titled 'The reaction of the skin in cases of typhus fever by ProteusX$_{19}$ and related bacilli' (Fleck & Krukowski, 1923). Though Fleck at that time was dismissed from his position (cf. Schnelle, 1986, p. 37), he seems to have pursued the question on his own, at the laboratory of the health insurance fund, *Krankenkasse*, as evidenced by four papers published in international journals 1930–1933. Two of the latter were written in collaboration with a paediatrician named Hescheles. The final paper by Fleck on the topic is a rejoinder to a paper published in 1934. During that period the topic had, however, lost its immediate attractiveness, as typhus was no longer seen as a disease of great concern to the German society—a view that was to change drastically during the war to come.

The first of the four articles is a brief recount of animal experiments appearing in 1930 in *Krankheitsforschung* titled 'Über die Exanthin Reaktion bei Meerschweinchen und Kaninchen' (Fleck, 1930b). At the end of the paper, Fleck expresses his thanks to professor Weigl for his provision of necessary material needed for the immunization of guinea pigs, *Ricksettsia prowazekii*, the micro-organism causing typhus. Though aspiring at a scientific presentation, as shown by an abundance of technical concepts, there is no question of issue or any theoretical frame anchoring the reported experiments. The paper is purely descriptive and devoid of primary data. The results are presented in the current text by the use of notions such as *more or less*, *at least*, *often*, *more than*, *not always*, *normal* or *exceedingly strong reaction*, *not always clear*, *first after some weeks*, *not wholly parallel* and *typical*. No references to the work of others are given—only one, to Fleck's own article from 1923. The two-and-a-half pages long paper ends abruptly with the assertion that more experiments and more elaborate reports are in progress. The reader is also informed that reports on studies already undertaken on humans, performed in collaboration with Hescheles, will follow.

The next publication is a paper, eighteen pages long, published 1931 in *Zeitschrift für Immunitätsforschung und Experimentelle Therapie*, entitled 'Versuche über eine lokale Hautreaktion mit ProteusX$_{19}$-Extrakten (Die Exanthinreaktion)' (Fleck, 1931). As in the previous paper, professor Weigl is acknowledged for his provision of needed material, *Ricksettsia prowazekii*, as well as for having performed agglutination tests, that is, the Weil Felix reactions, in his Institute. The paper is formally opened with a 'historical introduction', and the ensuing description of the experimental procedure and the characterization of the test substance, Exanthin, are followed by sections on experiments on guinea pigs and rabbits. Three tables and four figures are included and the paper ends with a summary and a list of references. In a final addition Fleck informs the reader of his figures, which stand out as remarkably high. Seventy-seven guinea pigs and thirty-two rabbits have been used, occasional controls uncounted.

The third publication is a brief report, undertaken in collaboration with Hescheles, appearing in *Klinische Wochenschrift* (1931), 'Über eine Fleckfieber-Hautreaktion (die Exanthinreaktion) und ihre Ähnlichkeit mit dem Dicktest' (Fleck & Hescheles, 1931). The Dick test, mentioned above, is based on the injection of toxin into the skin attempted at the diagnosis of haemolytic streptococci infection. Is reaches its peak within twenty-four hours in susceptible individuals and reverts to a negative reaction following infection.

In Fleck's study, the test substance, or Exanthin, was applied to newborn children, attempting to elucidate differences in skin reactivity between newborns and adults where the data on the latter are summarily retold. Professor Weigl is not mentioned and no references are given, except to articles by Fleck. By an added footnote one is, however,

informed that Exanthin is a concept coined by Fleck and introduced by him in his first publication in Polish in 1923.

The fourth paper, 'Über die Eigentümlichkeiten der Exanthin-(Fleckfieberhaut)-Reaktion beim Menschen', also in collaboration with Hescheles, and published in *Zeitschrift für Immunitätsforschung und Experimentelle Therapie*, deals with the peculiarities of the Exanthin or typhus skin reaction in humans, that is, differences in skin reactivity to the test substance (Exanthin), related to age and to the influence of experimental serum injections or the active immunisation of newborns (Fleck & Hescheles, 1933).

The rejoinder by Fleck in *Klinische Wochenschrift* marks the end of his studies on the Exanthin reaction (Fleck, 1934). If we start with a summary of the article to which Fleck is responding, the issue is more easily comprehended. A young Russian microbiologist, Nemschilov, wrote a succinct article, one and a half pages long and titled 'Über Hautreaktion mit Exanthin' (Nemschilov, 1934). In the introduction the author gives a brief account of the question at issue, including its historical background that dates back to 1916, the World War I period. The primary intent at the time was to work out a skin test disclosing acquired immunity to typhus that could be used primarily for epidemiological purposes, but also on isolated cases when the question of past rickettsial infection was in doubt or the Weil–Felix reaction was equivocal. The crucial question was of finding a suitable test antigen, and the attempts had so far been unsuccessful.

The aim of Nemschilov's paper is thus to ascertain whether the results presented in 1931 by Fleck could be successfully reproduced, thereby serving their stated purpose in separating individuals with ongoing or past typhus from the ones not exposed to *Ricksettsia prowazekii*. It is difficult to miss the caustic irony in the reserved remark by Nemschilov concerning the technical procedure, in reality simple, though elaborately outlined by Fleck, a crude suspension of *Proteus*$X_{19}$ bacilli cultured on agar, giving the remarkably clear-cut results asserted by Fleck (Fleck & Hescheles, 1931).

Nemschilov continues by giving a concise description of the production of Exanthin, extracted from Fleck's paper. The experimental procedure, now repeated, is fully exposed in Nemschilov's report, including definitions and numbers of test subjects and controls—all those details either missing or disguised in the papers by Fleck. Based on the results, Nemschilov's conclusion is that Fleck's results are not reproducible, thereby inferring, in contrast to Fleck's assertion, that Exanthin is useless for its intended purpose.

In the second part of his short paper, Nemschilov gives his account of further experiments using the 'O' form of the *Proteus* $X_{19}$ as starting material for the production of a suitable test antigen, rather than the unseparated suspension used by Fleck. The substantially improved, though not fully conclusive results are discussed against the theoretical frame of the, then existing, conception of cellular receptors that still reads as highly relevant. It should be added that the Weil–Felix reaction, which is used as both point of departure and reference, is based on the agglutination of Proteus $OX_{19}$.

In his rejoinder published six weeks later, Fleck shuns the repudiation of his data clearly exposed by Nemschilov's pedagogically outlined experiments (Fleck, 1934). He starts by concentrating on intricate technical details of no importance for the procedures or relation to the results. He continues by pointing out that animal studies, though hardly at issue, are important. He further expresses the conviction that the application of statistics would solve many current obscurities, implicitly resorting to future formalised procedures in securing faulty data. Fleck finally claims, though without any support of data or any references to previous publications, that he has, himself, already performed systematic

experiments on the 'O' forms of *Proteus*$X_{19}$, though with results contradicting those of Nemschilov.

After the strong disproval of his research appearing in the widely read *Klinische Wochenschrift*, Fleck's experimental work seemed to slow down. He was, however, as prolific as before, but now with a clearly philosophical orientation. His monograph was issued and some of his philosophical papers were published (see Cohen & Schnelle, 1986). His few scientific papers during this period address philosophically minded questions (for example, two on reactions versus pseudo reactions, published in 1938: Fleck, 1938a,b). Other Polish papers address questions on the organisation of medical services and bacteriology.

Though the refutation by Nemschilov marks the end of Fleck's work on Exanthin, one can infer that the Exanthin reaction, albeit without the appearance of any further publications, was the subject of Fleck's Polish habilitation thesis. This was published immediately following the War and, according to Schnelle, approved by or 'with professor Hirszfeld' (see Schnelle, 1986, p. 29; cf. Hedfors, 2006c).

## 5. General characteristics of Fleck's scientific writings

The first scientific theme might seem esoteric and of little general relevance. It reveals, however, the main features that are to characterize Fleck's future pursuits in science. Furthermore, the comparative reading of contemporaneous texts exposes the (by then) current criteria of the scientific method aimed at by Fleck. Moreover, in uncovering the links to the monograph, Fleck's first scientific theme also exposes some of the contradictory parts of his epistemology (cf. Hedfors, 2006a).

In all his writings Fleck portrays himself as an able scientist claiming international recognition. This image conforms to the early-established interpretation of Fleck, ever since propagated in a vast, mainly unquestioning scholarship (cf. Trenn, 1979a,b, 1981; Schäfer, 1993). In relation to the scientific writings these easily revealed means do not suffice. Working outside the academy, Fleck's main drawbacks seem to be defective training, as well as wanting contact with science (cf. Löwy, 1986). When in practice bypassing, or neglecting, the scientific method, the results rather conform to attempted or mimicked science. In recalling Fleck's strong critique of the scientific method, scorning Citron's precepts in immunology (Fleck, 1979, pp. 54–57), the contradictions of his pursuits become manifest.

Fleck's critique of the scientific method has usually been cherished as exemplifying a prescient relativistic view of science, anticipating Kuhn. Nemschilov's critique of Fleck—by and large implicitly adhering to Citron's 1910 precepts, which still read as highly relevant—is explicitly launched at Fleck's lack of scientific rigour. In his verbose rejoinder Fleck claims, however, that strict adherence to the scientific method has not only been attempted but also accomplished.

Additional examples illustrating Fleck's attempts at science are his almost obsessive efforts to introduce new, often priggishly explained concepts, derived from Greek or Latin, according to easily identified patterns, for instance, tuberculin. Exanthin is but one example; vulgarin, cytordine and leukergy are others equally ephemeral. An unfulfilled dream seems to be the design of a 'Fleck test' corresponding to the Wassermann or the Weil–Felix reactions thereby conforming to the strivings of the period. The frequent assignation of eponyms to biological tests, for example, that of the Wasserman reaction, was, how-

ever, unpredictable and, in the latter case, an historical accident (Hedfors, 2006a). (The German notion *Fleckfieber*, that is, exanthematous typhus is coincidental.)

Additional characteristic features include an abundance of technical notions not serving any purpose. The same applies to an almost limitless application of technical procedures, here exemplified by the characterisation of his test substance, Exanthin. Most of the procedures are beside the point and the results are either redundant or equivocal, for example both enumeration and application of the many reagents available at the time. Still another characteristic is the use of vague concepts when describing experimental data, thereby making them highly equivocal, as exemplified above. The ambiguity of the data is further strengthened by an absence of defining criteria on what is to be considered a positive or negative result.

Fleck's idiosyncratic pursuit of science is further illustrated by his use of graphs (see Fleck, 1931). One case is frequently used to illustrate many (which are never displayed) and the graphs are not used as intended as handy complementary summaries of data. The insertion of figures or photographs seems reassuring, but when they do not illustrate the issues the merit is lost. The remarkably high numbers of animals used in his aforementioned experiments are retold as if the figures as such would confer reliability upon his data (ibid., p. 298).

Though starting with a historical introduction in his paper in 1931 (ibid., p. 1075), credit is not given to those deserving it. Rather, one easily gets the impression that everything starts with observations made by Fleck. (A parallel reading of Nemschilov's article is instructive in this regard.) Fleck's self-centred way of writing is also found in his later papers. The verbose disguise of the in reality very simple experimental procedures, including the use of esoteric concepts, is another characteristic.

Three additional points deserve mention. One is the absence of any theoretical frame forming a background to the experiments to be undertaken, another is the absence of clearly stated hypotheses suggesting why the experiments are to be undertaken, and the third is the conception of statistics as an end rather than as a means when interpreting the results of the experiments.

Though the formal outlines of scientific papers have become much more elaborate (Gross, Harmon, & Reidy, 2002) with clearly marked sections of introduction, material, methods, results and discussion, a theoretical frame and questions at issues are mostly present. In contrast to the papers by Fleck, the two latter constituents are also easily retrieved irrespectively of the less formal outline that was a frequent way of writing at the time in question.

The above is further illustrated by the summary (Fleck, 1931, p. 299) in which Fleck indirectly states his aims and what he thinks he has accomplished: the finding of data supporting his theory concerning the principal (*grundsätschlichen*) identity of all forms of life (*Lebenswesen*), genetically related by mutation or adaptation (*im Sinne von Mutanten oder Anpassungsformen*). The basis of his conclusions is the already known cross-reactivity between *Proteus*$X_{19}$ or, though unmentioned by Fleck, its 'O' antigen and *Rickettsia prowazekii*. He seems unaware of both his inductive reasoning and the fact that no hypothesis had been formulated—or, if formulated, that it would be unrelated to the outline of his experiments. His final contemplations disclose, however, a much grander agenda, far from the initial aim of the study, namely, the design of a simple skin test aimed at differentiating individuals with acquired immunity from those without that immunity. Disregarding airy speculations, the most obvious weakness of Fleck's first scientific theme becomes manifest by its lack of reproducible data.

## 6. A Symposium of Polish Medical Contribution in World War II

During the German occupation, the eviction of Fleck and his family to the Jewish ghetto in Lwów marks a turning point. Around 1942, he resumes his experimental studies while working in the ghetto hospital. This is witnessed by his publications immediately after the War. His second theme is antigenic substances in the urine of patients with typhus. Though this line of inquiry results in but two papers bearing the same title—one in Polish (Fleck, 1946) and one in English (Fleck & Murczynska, 1947)—its contribution to the current interpretation of Fleck has been critical (cf. Trenn, 1979a,b). Though references to the latter article, published in *Texas Reports on Biology and Medicine*, have been frequent, the reason why it was published has escaped notice (cf. Weindling, 2000).

Fleck's article turns out to be part of a section in the journal titled *A Symposium of Polish Medical Contribution in World War II*, comprising six articles all edited by a Polish-born American professor in public health, Ludwik Anigstein. In his introduction Anigstein explains the symposium as the result of a medical teaching mission to Poland, in the summer of 1946, for the United Nations Relief and Rehabilitation Administration, in which he was privileged to participate together with a group of distinguished colleagues. For Anigstein, it was particularly moving to revisit medical schools, institutions, libraries and hospitals, that after the World War I had been so well developing, but now were all destroyed or in ruins. Medical periodicals were unobtainable, and equipment and facilities were primitive. Nevertheless, Anigstein continues, research was continued underground during the War years by surviving scientists, many of them labouring under constant threat of penalty of death for their efforts (Anigstein, 1947b, p. 155). During the visit, the American delegation were approached by their Polish colleagues with requests to help them in finding an opportunity for the publication of some of the observations made during these years, 'which they thought might be of interest also to the scientists abroad'. Quite naturally, Anigstein was deeply affected by his visit. A clear witness is his arduous and careful editing of the six texts. He also states that 'The authors are endeavouring to continue the studies in the limited facilities available to them' (ibid.).

Two of the four short texts not written by Fleck are from the Weigl Institute for Typhus Research. Weigl, the undisputed specialist in typhus research as well as a former teacher of Fleck, gives a brief account of the production and use of the different anti-typhus vaccines during the German occupation. Thus, German, French and American vaccines were used within the eastern zone of War operations (Weigl, 1947, p. 177). The second article from the Weigl Institute, by Mosing, recounts the difficulties in evaluating an effect of the vaccines during war conditions. Though the vaccine produced by Weigl was still considered the most effective, insufficient supply and inappropriate dilutions of the scanty preparations made any vaccination hazardous (Mosing, 1947, p. 173).

The third of the four texts, written from the 62nd evacuation hospital of the Polish Army, comprises two case reports on malaria—a rare disease in Poland before the War, now increasingly observed as a result of the migration of troops and repatriation of civilians during 1944–1946. The last of the four is a case report illustrating how to treat leukaemia when no treatment is available.

According to a scientific reading, it is easily realized that the above four articles fulfil high standards of accuracy and rigour, not the least the two papers from the Weigl Institute. Weigl's succinct account—based on his own studies from 1939, and now expanded by

observations during the War—is impressive. Taking the precarious conditions of origin of the four articles into account, one easily shares the awe expressed by Anigstein. Though restrained, the four writers nevertheless disclose in various ways the hardships imposed on scientific pursuits by the War.

The two contributions by Fleck, occupying far more space than the other four taken together, open the symposium. This might indicate that Fleck was the one approaching Anigstein attempting to get his observations spread abroad, and also the one who was endeavouring to continue his studies. One of his papers is on leukergy, and the other on the detection of specific antigenic substances in the urine of patients with typhus. Scientifically read, one immediately realizes that the two texts by Fleck differ from the four discussed above. Both texts have, however, been critical in the conception of Fleck.

The article on antigenic substances in the urine of patients with typhus, his second scientific theme, is a recount of his studies in the Lwów ghetto in 1942 during the German occupation, in collaboration with three colleagues. Concerning the latter one reads, however, that 'several months later, the author [Fleck] was deprived of his collaborators who were destroyed by the Germans'. According to Fleck, 'the original plan was to elaborate a test giving earlier diagnosis than the Weil–Felix reaction', that is, though not mentioned, an elaboration of the Exanthin test. Further down Fleck declares 'it was also thought to utilize the urine as a source of specific antigens for the preparation of a preventive vaccine, very urgently needed at this time' (Fleck, 1947, pp. 168 ff).

## 7. The fear of typhus

Typhus, transmitted by body lice and thriving in overcrowded surroundings characterized by bad hygiene, is a problem of any war. Great efforts were devoted to issues concerning its prevention and cure during the two World Wars, as amply evidenced by innumerable publications in the scientific journals (cf. Lindenmann, 2002). According to well framed theory, the point of departure for the preparation of any vaccine is the causative agent in question, in this case *Rickettsia prowazekii*. A crucial problem was that *Rickettsia prowazekii* could be propagated only in living tissues or cell cultures. In this competitive research, Weigl was particularly successful in his use of lice inoculated with *Rickettsia prowazekii* and, also, human volunteers whom under strictly controlled and safe conditions functioned as lice 'feeders' (Szybalski, 1999; see also Weindling, 2000). The feeder procedure was restricted to one hour a day, when the crated lice were nourished by sucking blood, usually from the lower leg of the men and the thigh of the women. During these difficult times in Polish history the 'feeder situation', sanctioned by the German officials in desperate need of vaccine, could also be used by the Poles for other purposes, including the planning of covert resistance actions as well as protection of exposed individuals. Thus, large parts of the vaccine, produced by Weigl during German occupation, were secretly transported to the Warsaw ghetto. Once there, professor Hirszfeld, who was later to escape from the Warsaw ghetto, distributed it to its inhabitants (cf. Roland, 1992). Further, according to the same source, Fleck was, at least for some time, one of the many 'feeders' at the Weigl Institute (Szybalski, 1999).

In this atmosphere of suspicion and mutual distrust, the German officials took for granted that German doctors were using Jews as guinea pigs (cf. Szende, 1945, p. 151). That could explain Fleck's participation based on temporary supervised permissions to

leave the ghetto for the Weigl Institute. One could thus infer that Fleck must have been familiar with the prerequisites of the production of the Weigl vaccine. Survival and mild disease could, however, occur independently of any vaccination, as the morbidity and the mortality of typhus vary. The latter facts were well known to Fleck, and they added to the difficulties in evaluating the effect of the vaccines.

## 8. Fleck's vaccine

A recount of the experimental procedure extracted from Fleck's paper in *Texas Report* on antigenic substances in the urine of patient with typhus, discloses its division into three parts. Firstly, a rabbit was injected with '10 ml sterile urine from a case with typhus'. According to Fleck, this resulted in a positive—albeit highly irregularly appearing—Weil–Felix reaction in the rabbit. This was taken to prove the presence of a specific antigen in the urine of the patient. Secondly, the urine was layered on serum from the patient in narrow test tubes. Somewhat irregularly, a precipitate formed. The precipitate was taken as visual evidence of the presence of the antigen in question. Thirdly, urine from typhus patients was concentrated and dialysed against tap water. In various steps the liquid was admixed with sodium hydroxide, centrifuged, re-suspended, pH adjusted, re-concentrated and phenol added. The residue was claimed to constitute purified specific antigen.

Readers not informed by field-specific knowledge might overlook that the three parts of Fleck's experimental procedure described above are logically unconnected, and that any inferences are invalid. Without that knowledge, they might also accept that the procedure gives rise to an antigen that could be used as a vaccine against typhus—something that, according to Fleck, was to be injected subcutaneously three times. A less disguised description would, however, read that Fleck's procedure, if repeated, would result in an 'antigen' or a 'vaccine' consisting of urinary sediment sampled from patients with typhus, admixed with tap water, some table salt, and phenol usually added as a preservative.

Fleck's highly variable results necessitated, however, an endless number of experiments, including countless changes and adjustments of the procedure recounted in his paper. Though the basic outline of the experimental procedure remained unchanged, its single steps were not necessarily performed in the same sequence. In the last section of the paper, Fleck concluded that he had found an antigenic substance, of unclear origin and nature, in the urine of patients with typhus, though more frequently in severe cases. The day of its appearance did vary. Despite this, he found urine suitable for early diagnosis of typhus. He concluded by stating that the 'antigenic' substance 'may be concentrated and purified'. Attempts were made to use this antigen as a preventive vaccine. Three litres of urine would provide about forty doses of vaccine (Fleck, 1947, p. 172).

From the paper it can be inferred that what Fleck denotes as preparation K20 was also attempted as a preventive vaccine for humans. The paper states 'The author, his family and 32 volunteers were vaccinated. Later 500 people in a concentration camp at Lwów were vaccinated. With a few exceptions statistics of the vaccinated were unfortunately lost. Records are available only with regard to the author, his family and two other persons, all of whom contracted typhus and recovered after a mild and abortive course of the disease' (ibid.).

That Fleck 'vaccinated' people in the ghetto is supported by the recount of Adolf Folkman living in the ghetto until its liquidation in 1943. A few weeks after his miraculous

escape to Sweden in October 1943, Folkman gave his account to Stefan Szende, the Hungarian Jewish publicist who had lived in Sweden since 1937. After some editorial work Szende issued Folkman's account in Swedish in 1944. In 1945 it was issued in German, titled *Der letzte Jude aus Polen* (Szende, 1945).

Schnelle mentions the latter issue in *Cognition and fact* (Schnelle, 1986, p. 22), although he mistakenly ascribes the quote to Szende instead of Folkman. The quote is, however, correct in affirming Fleck's activities in the ghetto, injecting people with what was called his 'serum'. Without any reference to Folkman, Trenn asserts 'Fleck used the urine of typhus patients as a source of rickettsial antigen that proved to be a very effective vaccine. It was not until 1947, however, that he was able to publish the results of his new method of typhus "vaccination" '. Trenn also affirms that 'the specific antigen excreted in the urine was used for diagnostic purposes which permitted early detection and isolation' (Trenn, 1979a, p. 151).

In summary, the 1947 paper in *Texas Report* on specific antigenic substances in the urine of typhus patients, as discussed above, and the remark made in passing by Folkman and reprinted from the book by Szende, constitute the sole evidence on which the narrative has been based concerning Fleck's production of a vaccine against typhus in Buchenwald—where the better part, in secrecy, was said to be kept for his fellow inmates (cf. Schnelle, 1986, pp. 24–25; Kogon, 1946, pp. 214–215; Hedfors, 2006c). Perhaps even more astonishing is, however, the mistaken conviction, perpetuated by current interpretation, that the production of a vaccine against typhus from the urine of patients, who had contracted the disease, would be feasible. Regardless of Fleck's own account, the claim should have been questioned on purely theoretical grounds.

It should also be noted that Fleck, in his paradoxical post-War paper, never claims having produced an anti-typhus vaccine. Tests on guinea pigs subsequently inoculated with typhus blood did not show any immunity to typhus (Fleck, 1947, p. 171). Fleck only reports that attempts were made to use what he described as the antigen, though never defined, as a preventive vaccine that he tested on himself, his family, collaborators and thirty-two volunteers. That '500 people in a concentration camp in Lwów were later injected' is another story (ibid., p. 172).

In retrospect, the second scientific theme of Fleck could be paraphrased as unrestrained speculations staged by a magician. Fleck's airy estimate that three litres of urine would provide about forty doses of vaccine echoes like a hard-struck miracle. If so, the use of one single twenty-four hour urinary output of a single typhus patient would suffice to vaccinate six to seven individuals. However, at the time typhus was as feared by the inhabitants of the ghetto as by its SS guards. By mimicking science in an ignorant surrounding tormented by fear and unfathomable terror, Fleck's activities induced desperate hopes in unbearable situations. Enticing rumours ascribed him some local fame, which no doubt proved detrimental to his later fate. This part of the story is elucidated in a forthcoming paper (Hedfors, 2006c). Suffice to say that during those apocalyptic days, many homemade vaccines seem to have been in use, based on Fleck's recipe with ingredients bought at local pharmacies—achievements not infrequently accomplished by way of unholy coalitions between Germans and Jews (cf. Szende, 1945, pp. 214–215; Kogon, 1946, pp. 146–147).

A final conspicuous feature of this second scientific theme, and in striking contrast to the other writers of the symposium, is that Fleck on his part never articulates any restrictions or hardships imposed by the War conditions.

## 9. On leukergy

The third and main scientific theme of Fleck is that of leukergy. It could be described as an *in vitro* disclosure of agglutinated leucocytes. Seven articles have been scrutinized: three papers from 1939–1941 listed in the bibliography, though never published, indicate an interest dating back to the pre-War period that was resumed following the War. Partly due to a frequent referral to his publication on the issue in *Texas Report* in 1947, it has become a central theme in the interpretation of Fleck. The latter paper has the same background as the one on antigenic substances in the urine of typhus patients discussed above, in being edited by Anigstein. The biographical sketch by Trenn added to Fleck's monograph, has, however been even more influential for the interpretation when stating that 'In 1947 Fleck discovered a new phenomenon occurring during inflammation: a clumping of white blood cells, which he designated "leukergy" ' (Trenn, 1979a, p. 151). According to Trenn, 'the discovery had an immediate impact within the scientific community' (ibid.). Trenn supports his assertions by two references from 1947: an American one in the *Journal of the American Medical Association* (*JAMA*), and a British one in the *Lancet* (Anon, 1947a,b). Rather than supporting the claims made by Trenn, the two references reveal the reverse. They consist of two short, unsigned notices occasioned by Anigstein's report on the post-War UN medical support mission to a devastated Poland. Fleck's articles are briefly mentioned. False positive results, the possibility of considerable experimental errors, wanting description of technique, lack of controls and proper records are mentioned, thereby exposing the shared scientific criteria. Taking the precarious conditions of pre-War and Wartime Poland into account, the conclusions of the two writers do, however, concur. The flaws and deficiencies of Fleck's papers are all seen as inevitable, unfortunate consequences of the hardships of the time in question.

Besides the paper edited by Anigstein, two additional papers on leukergy appear immediately after the War. Both are published in *Schweizerische Medizinische Wochenschrift*, which, in contrast to the German scientific journals, was uninterruptedly issued during and after the War. The first article is in German (Fleck, 1946) and the second, in collaboration with two colleagues, in French (Fleck, Kowarzylc, & Steinhaus, 1947). It is obvious that Fleck, by the addition of '*vormals Lemberg*' after his name in 1946, is attempting to draw on his previous role as a scientist, and both papers refer to observations on the distribution of leucocytes in *in vitro* preparations, dating back to the thirties. The first paper in 1946 is highly speculative, resorting to statistics provided by Steinhaus, the mathematician, but devoid of primary data. The distinction between *in vivo* and *in vitro* conditions is disregarded. The assumption of an *ordnende Kraft (Zytordine) serologischer Natur* is expressed, that is, a force, or rather, serologic factor, arranging or agglutinating the cells (Fleck, 1946, p. 175). In the second, French paper, 'Zytordine' becomes 'cyto-ordines', which transforms into 'leukergines'; the phenomenon finally becomes denoted as 'leukergy'.[1] The now three authors end their paper, though without any supporting data, by concluding that the former hypothesis, though never explicated, has become superfluous. Thus, it has been replaced by the new phenomenon of 'specific auto agglutination' (that is, 'leukergy'). They finally state that their common conclusion has not been shaken *n'ebrante*

[1] Fleck derives his notion *leukergie* from Greek, *levkos*, white, and *ergon*, work, that is, the work of white [blood cells].

*en rien* by any experimental data on leukergy. All references are, however, to the work of Fleck, except for an unpublished Polish paper whose author was said to have studied Fleck's data and confirmed his *phénomène statistique* (Fleck et al., 1947, p. 1283). By now, Fleck seems to have learned his lesson from Nemschilov that data has to be confirmed by others. His two papers on the issue remain, however, far from confirming.

The third paper on leukergy is a letter to the Editor in *JAMA* (Fleck, 1949), in which by way of adhering to a quite different discussion on hemorrhagic diathesis, bleeding tendency, in Hiroshima, Fleck offers his 'leukergy test' to the readers. He further claims, without the support of any references, to have performed the experiment discussed in the referred paper. The somewhat desperate tactic used by Fleck seems, by now, familiar: through ongoing discussions, often rather remote, claiming the priority of observations and results though never supported by reliable, if ever existing, data. In this context, he refers to the two critical comments on his work made in passing in 1947 in *JAMA* and the *Lancet* occasioned by his contributions to the symposium (as edited by Anigstein and described above), and also to his article edited by the latter.

The remaining four papers on leukergy, including the one edited by Anigstein, conform to Fleck's common pattern and his abundant use of technical notions, which makes a reading not informed by field-specific knowledge difficult. Obvious flaws could easily pass unnoticed. What is, in reality, the embarrassingly simple procedure on which Fleck bases his construction of his self-invented concept of leukergy would also be hard to disentangle for the uninformed. Fleck's whole set-up, usually accessible even in less affluent environments, consists of counting the cells of a blood-smear mounted on a glass slide and viewed through a microscope—an undertaking well known to any laboratory technician.

More astonishing is, however, Fleck's somewhat uncommon sampling procedures, as well as his manipulations of test objects in order to induce leukergy. Whether these objects are humans or animals is not always possible to disclose. Intra-pleural, intra-arthricular or intra-peritoneal injections of turpentine are usually mentioned in connection with animal studies, whereas intravenous injections of living or heat-killed bacteria are frequently mentioned without stating the relation to the objects in question (Fleck & Murczynska, 1947). Sampling via heart puncture refers to rabbits, whereas spinal taps seem to refer to humans. The repeated sampling of blood from pregnant women, newborn children or patients severely affected by different diseases, be it typhus or malaria, should, however, alert any attentive reader (cf. Fleck & Murczynska, 1947, 1949; Fleck, 1952).

When briefly summarising Fleck's work on leukergy, it could be captured metaphorically as a *Mädchen für alles*, functioning as a panacea that could be fitted into any context, inserted into any circulating discussion on the most sophisticated science, no matter how far-fetched. The absence of methodological demarcation criteria, controls, display of experimental procedures, test objects or data as well as constant conceptual vagueness, or outright contradictions when evaluating results lends itself to a situation where any interpretation could be possible.

After having carefully read the papers on leukergy, it is not possible to infer whether Fleck viewed leukergy as a humoral or cellular phenomenon, inherent or induced. Theory and hypotheses are never spelled out, if ever conceived; *in vivo* and *in vitro* are conflated or used interchangeably. Leukergy manifests itself as an empty concept in being thermo-stable as well as thermo-labile; comprising few, or three, as well as numerous cells; appearing close and not so close; early as well as late; in health though mostly in disease; or after spectacular diagnostic or therapeutic procedures such as encephalography or

irradiation—at the time, exotic enough. As the whole procedure is based on inductive reasoning and circularity, any attempt to refute the data is prone to fail.

Throughout the years, Fleck continues to offer his different tests for use, though nobody seems to care. However, after the War and when finally in an academic setting surrounded by colleagues all connected by mutual interdependence, Fleck seems to have continued his endless experiments on leukergy in line with his early thoughts. As the pursuit of science in Eastern Europe during this period of Cold War was restricted (cf. Anigstein, 1947a, pp. 173–184), his unyielding perseverance during the imposed seclusion from the West and its scientific progress might have offered a manageable strategy of survival—mental, as well as physical. When Fleck finally managed to leave Poland in 1957, his life was already restricted due to his failing health.

According to Schnelle, 'Leukergy is a phenomenon recognized by the medical world, even though it has only met with lesser recognition in the West due to the somewhat isolated consideration paid to Polish medicine in these countries' (Schnelle, 1986, p. 30). The reason why leukergy, not found in medical dictionaries, has not been recognized is rather that it is a theoretical construction without empirical support.

## 10. The question of research ethics

As already mentioned, a fourth theme reflecting bacteriological questions of more general concern runs parallel to the three themes discussed above. Nine papers have been scrutinized, eight of which were published before the War (1927–1939). The clinically oriented papers are in collaboration with different colleagues, except for one case report solely by Fleck. The medical questions and the medical practices disclosed in the papers have to be related to the time in question, including not only its knowledge claims but also generally accepted procedures and often unquestioned paternalistic medical practices. A comprehensive review of this theme would lead too far (cf. Hedfors, 2006d). The papers do, however, conform to the ones already discussed without the addition of any new dimensions, except for the accentuation of one already alluded to (that is, the one of research ethics).

At the time, the question of research ethics, though formally regulated, was to a great extent left to the discretion of the researcher.[2] However, it is well documented that Weigl, who basically trained as a zoologist and introduced Fleck to research, strongly opposed experiments on humans (Szybalski et al., 2003). That he finally came to accept the implementation of the 'feeder system' involving human volunteers in his typhus research was due to a coup undertaken by his assistants, thereby convincing Weigl of its safety not only in theory but also in practice (ibid.).

Fleck does not seem to have ever shared the strict views of Weigl. In a 1927 paper titled 'Experimentelle Beiträge zur Ätiologie des Lupus erythematodes' in *Dermatologische Wochenschrift*, in collaboration with a dermatologist in charge of the patients under study, the paper ends by indicating that Fleck contemplates experimental induction in humans of a disease of an as-yet-unknown aetiology, in line with his own hypothesis on bacteriological aetiology—'Als weitere Aufgabe schwebt uns unter anderen das künstliche Hervorrufen eines Lupus-Erythematodes-herdes durch die gemeinsame Wirkung von bakteriellen Antigenen und der aktinischen Provokation bei Disponirten evtl bereits an anderen Körp-

[2] For an extensive exploration of the topic, see Roelcke and Maio (2004).

erstellen Erythematodeskranken vor' (Fleck & Füllenbaum, 1927, p. 487). More explicitly stated, the procedure would involve the testing of a hypothesis by the inoculation, injection or transfer of different pathogenic microorganisms to healthy humans, or those already affected with known light sensitivity, thereafter exposing them to 'actinic provocation', which could mean any type of artificial light.

In a second paper by Fleck in 1930 in the same journal, titled 'Ein fall von Pseudosyphiloma anorectale mykotischen Ätiologie (Kladiose)', the experimental transfer is attempted. It is described as an 'auto-inoculation', undertaken as an experimental attempt to provoke the disease in another location. Thus, by re-injecting pus sampled from one lesion into unaffected skin, Fleck induces purulent ulcers—in this case, in the patient's thigh (Fleck, 1930a). In modern parlance this equates to the use of human subjects for solely experimental purposes.

Questionable research ethics has already been alluded to in connection with the articles discussed above. The repeated injections of foreign antigens into the skin of newborn children—sometimes on an hourly basis, as described in the paper on the Exanthin reaction—exemplify the issue (Fleck & Hescheles, 1931, p. 1075). The questionable ethics is further accentuated by the absence of any theory or hypothesis guiding the experiments. Moreover, the fact that the newborn had never been exposed to typhus makes the whole project even more obscure. In retrospect, the main reason seems to have been that newborns were 'available'. The same query applies to the repeated tests on pregnant women: they too might also have been 'available', and the studies were designed accordingly.

It should, however, be remembered that Fleck's highly ethically questionable studies, as discussed above, were undertaken in a society where professional transgressions could pass. When writing in collaboration with other colleagues, the head or professor of the unit was acknowledged and the papers were in due course accepted for publication in Polish as well as German journals. However, when studied as a whole, Fleck's publications emerge as being not only ethically objectionable, but also strangely unrestrained. This includes both the number and procedures of his experiments, as well as his use of test objects, animals and humans alike.

For a recent reader acquainted with the fate of Fleck, as well as European history of the time in question, the latter part of the study might be painful reading. As the question of research ethics has broader implications it will, however, be further elaborated upon in a forthcoming paper (Hedfors, 2006d).

## 11. Good and bad science

Fleck's four scientific themes could be used to illustrate the essence of the scientific pursuit. Though overlapping, his first theme on Exanthin would thus illustrate that the results of an experiment must stand the test of refutation. His second, on antigenic substances in the urine, could illustrate the demand of well framed theory and an explicitly formulated hypothesis in conferring sense to experiments, thus making refutation possible. In the same vein, the third theme, on leukergy, could illustrate the necessity of well defined, unambiguous, and mutually shared concepts. The fourth theme on general bacteriology, covering human diseases with purported bacteriological aetiology, could illustrate the ethical aspects that include the justification of experiments, which is congenial to any research project.

Closely related to the above demands is the logic in science. This is not to be mistaken for formal logic. Rather, the logic in science refers to the intra-disciplinary logic dependent

on relations between theories, hypotheses, concepts and facts restraining valid inferences. The latter point could be exemplified by the kinetics of an immune response. In being dependent on the specific knowledge of the different scientific disciplines, it is highly informative for specialists but would, by necessity, elude non-specialists or those, according to Fleck, outside the esoteric circle, devoid of *esoterisches Wissen*.

Wanting knowledge of this type of logic easily lends itself to unrestrained speculations, which are such a prominent feature of Fleck's scientific writings, though he deals with the issue (Fleck, 1979). His own notions, *aktive und passive Kopplungen*, as well as *Denkzwang* and *Widerstandsaviso*, as introduced in his monograph, could be subsumed under the notion of logic in science (cf. Löwy, 2004). Not on any point, however, do the scientific writings of Fleck meet the above demands, and his many transgressions are painfully obvious.

As to the second concern, whether a reader not informed by field-specific knowledge could deduce qualitative statements on Fleck's writings depends on what evaluative statements in science could be based upon. A simple rule of thumb, though frequently violated—as already alluded to—could be that normative statements based on scientific content should be restricted to the specialists or, according to Fleck, members of the esoteric circle.

It is, however, of interest to note that Baldamus—who is credited with the rediscovery of Fleck's monograph in the mid-seventies—in his early perspicuous writings on Fleck, finally managed to obtain two of Fleck's scientific papers. With amazement, Baldamus recognized that, almost certainly from at least 1942 and onwards, Fleck had ceased to apply his theory, as expressed in the monograph, to his own research. Baldamus concluded that 'Both papers are concerned with specific discoveries in bacteriology, and there is not the slightest trace of any doubt about the reality of scientific advances'. His inference was that the positivist attitude now expounded by Fleck was a probable effect of discoveries in his own field of science that made him abandon his relativistic epistemology. The two papers that finally reached Baldamus were the ones in *Texas Report* from 1947, which are discussed above (Baldamus, 1976, pp. 50–51, ix–x).

Baldamus took for granted that the numerous papers by Fleck in as diverse fields as bacteriology, immunology, microbiology and philosophy of science, now scattered in German, Polish, Russian and English journals, would have to be utilized to answer the question of whether—and if so, how—Fleck changed his 'theory of error' in the course of his extensive later work. Baldamus added: 'Perhaps he changed it altogether? In that case, it would surely be ironical that it was a falsified theory of knowledge that came to dominate the central controversy of the present philosophy of science' (ibid.). The answer is that Fleck did not change it; his scientific writings conformed to the same 'positivist attitude' from beginning to end.

Baldamus bases his conclusions on the formal aspects of Fleck's papers. He is, however, cautious in inferring any conclusions based on their content. The reason is his explicitly stated lack of field-specific knowledge, including his ignorance of the scientific concepts used by Fleck. According to Baldamus this precaution, by necessity, also applied to Fleck's monograph (Baldamus, 1977, p. 143). Kuhn was later to articulate the same concern (Kuhn, 1979, p. ix).

Whether Fleck's current interpreters univocally praising his science and scientific accomplishments share the concern expounded by Baldamus and Kuhn is unknown. Rather than reading his articles, the basis of their claim seems to be that his articles, or at least some of them, were published in international journals. As the above discussion

has disclosed, there are many ways in which to get one's name inserted into scientific journals. Moreover, the publishing system was different in the time in question. Some of Fleck's post-War articles may have been published due to courtesy—for example the ones in *Texas Report*. His precarious situation had become known through Anigstein's presentation (Anigstein, 1947a; see the bibliography in Cohen & Schnelle, 1986, pp. 445–456).

If one agrees with Baldamus and Kuhn, as well as Fleck, that scientific content including scientific concepts and descriptions of technical procedures are out of reach for the uninformed, formal aspects remain. Though for the uninformed never conclusive, they could contribute in evoking further inquiry.

Part of the formal aspects are also questions referring to time and place as well as education, training, competence, position and possibilities of the scientist in question. A remarkable disregard of such simple facts characterises the interpretation of Fleck. His fragmented medical education (cf. Schnelle, 1986), restricted training in science, lack of affiliation with academic institutions, and research done mostly on his own in scanty spare time—all undertaken in a devastated country between, during and after two World Wars—should at least give rise to some suspicion as to whether the major scientific deeds ascribed to Fleck could all be true (cf. Trenn, 1979).

Other formal aspects referring to the text, as disclosed by a common-sense reading, are obviously open to non-specialists, or the exoteric circle. The above review of Fleck's scientific writings—which display his use of vague evaluative concepts, contradictions, inappropriate figures and graphs, and questionable application of procedures and selection of materials—would, however, provide material enough for some, if not conclusions, but rather suspicious thoughts. Admittedly, in the case of Fleck, even this could be difficult, due to his highly tortuous way of writing. Whether the above alternatives suffice for the non-specialist who contributes evaluative statements to science is, however, left to the discretion of the reader in question.

Löwy's careful interpretation in *Cognition and fact*, twenty years ago, is mainly based on the monograph but also on a few of Fleck's scientific articles that were available to her at the time; the interpretation itself is highly considerate. Though clearly disclosing Fleck's obsolete views in bacteriology and immunology, Löwy might easily be interpreted as being appreciative of his scientific work. Her extensive and highly informative footnotes added afterwards, comprising seven pages in small print, end by referring his anachronistic views to solitary elaboration and reflections, based on ideas about fundamental problems in bacteriology and immunology of the early years (1910–1920). These ideas had long since been abandoned when Fleck issued his monograph and wrote his scientific papers, showing altogether little awareness of the basic conceptual or theoretical innovations in his discipline (Löwy, 1986, p. 442). Albeit thorough, her early deliberations do not seem to have informed Fleck's interpreters within the humanities (cf. Sadegh-Zadeh, 1981; Zalc, 1986).

The conception of Fleck as a prominent scientist of his time is an early, successively cementing construction. Additional work has mainly centred on his monograph, attempting to disclose prescient views in epistemology and sociology of science anticipating Kuhn (see Cohen & Schnelle, 1986; Trenn, 1979a, pp. 154–157; Trenn, 1981; Schäfer & Schnelle, 1980, 1983; Schäfer, 1993; Belt, 1997). The curious disregard of Fleck's obvious marginality (cf. Löwy, 2004) and his precarious situation at the time in question, reminiscent of wishful thinking, is difficult to understand. The account of his scientific accomplishments

and his production of highly innovative and purportedly effective vaccines in the ghetto of Lwów and in Buchenwald implies, however, a repression of reality—and, in the scholarly context of philosophy and theory of science, an unfortunate revision of history.

## 12. Remaining issues

Though many questions remain two additional ones will be addressed. The first is whether the tentative application of modern criteria, in the evaluation of Fleck's international scientific publications, is justified or might stand out as anachronistic. Though mostly overlooked in the current interpretation, Fleck aims at applied science, that is, the design of diagnostic tests or the attempted refinement of well proven ones, already in current use. In being based on prior scientific accomplishments of basic science, the attempted tests have to conform to theories or hypotheses, admittedly not always explicitly articulated or properly understood. Ignorance, or neglect of the basic theoretical assumptions implies, however, unguided studies resulting in endless trials and the accumulation of an abundance of non-intelligible data, as exemplified by Fleck's studies on Exanthin and leukergy.

Though constantly violating the well known precepts of the scientific method, Fleck's outline on the matter in the monograph is perfectly clear. When recalling the work of Wasserman, Fleck identifies himself with the latter, concluding 'Er hatte einen fertigen Plan und war von Ergebnis überzeugt' (Fleck, 1980, p. 113; Fleck, 1979, pp. 85–86). The method was, however, not yet ready. It had to be worked out and so it was through arduous work. Out of the muddled scatter of tunes, Wassermann discerned a melody. Though still inaudible to others, it corresponded to his inner melody, and the method was changed accordingly (cf. Hedfors, 2006a).

The difference between Wassermann and Fleck is that the unarticulated hypotheses of the latter remain unarticulated. Furthermore, Fleck's lacking familiarity with the restraining *Widerstandsavisos* of nature, or of immunology, results in unrestrained speculations concerning nature as it could be, or rather, as Fleck would like it to be. His grand theory concerning the principal identity of all forms of life, genetically related by mutation or adaptation, belongs, however, to an already distant past (cf. above). Fleck's questioning of both the concept of antibody and the validity of the postulate of Koch are additional examples dating his implicit premises (Fleck, 1979, pp. 26, 58).

Numerous contemporaneous studies indicate that the highly sophisticated, still valid outline of scientific studies in applied biomedical research, was already in current use at the time in question. This is amply exemplified by the German publications during the Nazi period indulging in attempted scientific rigour (cf. Bormann, 1944; Hedfors, 2006c). Thus, the application of the modern criteria in the evaluation of Fleck's scientific papers seems appropriate. The almost over-explicitly pedagogic paper by Nemschilov, in directly commenting on the deficiencies of Fleck, should be read as a convenient paradigm. However, at the time, highly divergent papers were displayed in the different journals. Though mainly ignored by the scientific community, the intricacies of bacteriology or immunology were often explained by resorting to airy speculations. The exceptionality of Fleck was, rather than his pursuit, his unyielding perseverance.

The question on research ethics has partly been introduced to illustrate some of the difficulties involved in the evaluating past science. For obvious reasons, the criteria used to

evaluate the scientific merits of papers within the biomedical sciences are unanimously shared and basically uncontested. Omissions of the methodological criteria result in untrustworthy data, immediately recognized, if not by the scientist, by other scientists within the field in question.

In contrast, research ethics, which forms such an important part of modern biomedical science, has successively grown stronger in response to the ever increasing, elaborate investigations attempted in the evaluation of human pathology. In contrast to the naturally emerging evaluative scientific criteria, aimed at securing uncontested data, ethical regulations, aimed at restricting unrestrained experimentation, have to be repeatedly negotiated. This often implies heated debates involving contesting parties, all defending different interests and values, dependent on social setting, time and culture, including the standing of science (cf. Warlow, 2005). In consequence, extreme care has to be applied in passing judgement on the ethics of past science.

For a modern reader, Fleck's transgressions are obvious and difficult to comprehend. This opinion might not have been articulated at the time in question. What is most disturbing for a modern reader is, however, the almost endless repetition of experimental tests on humans, including newborn infants and pregnant women. In the early regulations of the time, children were to be excluded form medical experiments (cf. Roelcke & Maio, 2004). The definition of what was to count as an experiment was, however, often left to the discretion of the scientist. The abhorrence felt by a modern reader when confronted with the experimental transfer of infectious diseases of unknown aetiology (see above), could also be handled by the changing of definitions. As already described, the head in charge of the patients approved of Fleck's studies, and they were accepted for publication in the widely read scientific journals of Germany. However, in exemplifying interwar publications they call for further study.

The second issue, whether revealing that Fleck was a bad scientist has to have any bearing on the monograph or his epistemology, might already have been answered. The contention is that it has, not the least as Fleck's familiarity with the two fields of scholarship has been praised as being of particular value for his epistemology. Fleck's work, notably the monograph, has preferably been subjected to an interpretative tradition in which the scientific notions used by Fleck in his case histories have been excluded. A cautious estimate would be that almost half, probably more, of the monograph is directly devoted to the scientific issues, mostly bacteriology and immunology. In other words, at least half of the book, or the basis of Fleck's epistemology, has been either neglected or unquestioningly accepted. That the neglect of these parts could obscure their relation to the rest of the text seems obvious though it has, so far, not been contemplated (cf. Hedfors, 2006a).

The present study aims at illustrating the necessity of taking both Fleck's lines of scholarship, historically contextualized, into account (cf. Hedfors, 2006b). Recalling the political situation of the time, which also deeply affected Fleck, the emerging picture is, not surprisingly, complex and contradictory. Though the monograph was highly questioned by Fleck's contemporaries in the natural sciences and his philosophy viewed as both untenable and obsolete (Hedfors, 2006b), he is nowadays routinely invoked in highly diverse, seemingly incompatible contexts, as a key forerunner of science studies (cf. Biagioli, 1992, p. 199; Bloor, 1998; Sismondo, 2004, p. 53). Thus, the final hypothesis might be that the received view of Fleck has solely been made possible by neglecting great parts of the monograph. The corollary question concerning the meaning of the latter is left to further contemplation.

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