

# The Reading of Ludwik Fleck: Questions of Sources and Impetus

Eva Hedfors

*The rediscovery in the mid-1970s of Ludwik Fleck's initially neglected monograph, Entstehung und Entwicklung einer Wissenschaftlichen Tatsache, published in 1935 and translated in 1979 as Genesis and Development of a Scientific Fact, has resulted in extensive, still ongoing, secondary writings, mainly within the humanities. Fleck has been interpreted as furthering a relativistic conception of science. Nowadays, he is often viewed as an important contributor to contemporary sociology of science and a forerunner to Thomas Kuhn. Fleck's account of the Wassermann reaction, which forms the basis of his epistemology, has been praised as developed by a scientist well acquainted with the field in question. Because of the scarcity of available material on Fleck, however, the question of his sources has remained an unsolved issue. In the present article, an alternative reading is suggested. By focusing on the scientific content of the monograph, mainly neglected in the modern interpretations of Fleck, and on the so far overlooked sources of his writings traced back to their German origin, a better understanding of Fleck's account of the Wassermann reaction can be given. The consequences of this alternative reading for the conception of Fleck's monograph and for the impetus of his mission are discussed.*

**Keywords:** Ludwik Fleck; Wassermann Reaction

## Introduction

Although he was almost ignored by his contemporaries, Ludwik Fleck is now identified as an important founder of or contributor to contemporary sociology of science. His at the time neglected monograph, *Entstehung und Entwicklung einer Wissenschaftlichen Tatsache* (EET), first published in German in 1935, was translated into English in 1979,

---

Eva Hedfors is an MD experienced in both clinical medicine and medical research with a PhD in immunology. With an additional background in philosophy and in the history of ideas, she is at present working in the field of history and theory of science. Her main focus is on the differing conceptions of science within the humanities and the sciences in historical perspective. This paper is part of her PhD thesis in Philosophy at the Department of Philosophy and the History of Technology, Royal Institute of Technology, Sweden. Correspondence to: Eva Hedfors, Division of Philosophy, Royal Institute of Technology, Teknikringen 78 B, S-100 44 Stockholm. E-mail: ehedfors@infra.kth.se

as *Genesis and Development of a Scientific Fact* (GDSF). It is now not only included in the curriculum in history or theory of science studies at several universities but also the object of erudite discussions and philosophical dissertations in various countries (Simmons 1991, Belt 1997).

There is an agreement bordering on consensus concerning the merits of Fleck's monograph, not least regarding its anticipation of the theses displayed in *The Structure of Scientific Revolutions* published by Kuhn in 1962, which was immediately acknowledged and has been immensely influential ever since. This anticipation is exemplified in the vocabulary deployed by Kuhn. His concepts, such as *paradigm* and *scientific community*, have been seen as equivalents to Fleck's much earlier formulated notions of *thought style* and *thought collective*. The link between the two writers is to be found in the by now well-known reference to Fleck, made in passing by Kuhn, in the preface to *The Structure of Scientific Revolutions*, which later turned out to be decisive for the almost serendipitous rediscovery of Fleck's monograph. Kuhn's own reluctance to acknowledge a more substantial influence from Fleck, as clarified in his preface to the English translation of Fleck's monograph, has, if anything had the opposite effect. A deep influence is nowadays often depicted as a fact (Goldman 1983, Simmons 1991). Reservations remain the exception (Harwood 1986, Wettersten 1991, Lindenmann 2001).

In the monograph, Fleck uses historical examples from his discipline, medicine, notably "How the modern concept of syphilis originated" and "The Wassermann reaction and its discovery", which are also the titles of two of the chapters. The approach has been viewed as both novel and refreshingly productive. It also favourably distances Fleck from the almost exclusive use of historic examples drawn from physics in the philosophy of science at that time (Trenn 1981).

Fleck is also claimed to have been an active and successful scientist within his field, bacteriology. It has further been asserted that he influenced the formation and development of a new discipline – serology. The basis of this claim is to be found in numerous publications in prestigious scientific journals. Besides his lifelong interest in leucergi, (Leukergie) his involvements in typhus research and, not least, his production of a vaccine against the disease during his imprisonment in Buchenwald concentration camp during the Second World War have been emphasized.

When this prolific activity is taken into account, the lack of recognition of his philosophical work and the seemingly complete neglect by his contemporaries has been difficult to comprehend (Schnelle 1986b). The favoured explanation has been that Fleck was far ahead of his time in anticipating future developments in epistemology and the theory of science. That being the case, his contemporaries, especially the logical positivists, members of the Vienna Circle who have been deemed the target of his criticism, did not have the acumen to disentangle his work, whether concepts, statements, or ideas, or to launch an appropriate criticism.

My first reaction to Fleck's monograph, when I encountered it a few years ago during my studies in the history of science, was puzzlement. At the time, I barely knew that it had the status of an icon in certain circles. It was, however, soon apparent that, having a background in medical science, notably immunology, I read and interpreted it differently from my colleagues within the humanities. The focus of my immediate attention

was on his extreme interpretations of various questions within the natural sciences and his contradictory use of concepts. For example, Fleck opposed the postulate of Koch and, in consequence, he was questioning that of specificity. Further, his choices of historic examples must in many ways be considered unfortunate. His highly acclaimed description of the Wassermann reaction was equivocal, as was his description of the development of the concept of syphilis. His references in support of his statements were not only insufficient but also haphazard and often outdated. My conclusion was inevitable. According to my reading, Fleck emerged as a person out of touch with his time and its science. It was also evident that the complexity of the concepts drawn from natural science, used by Fleck to support his historic examples, must make substantial parts of the book unintelligible to a reader lacking experience of the subjects that were the basis for his doctrines and his epistemology. The inevitable question arose – if the basis does not hold, what about the superstructure? Or, to put it differently, how could a book written by a natural scientist, to a great extent inaccessible to an audience not trained in natural science, develop into an icon for that same audience?

The discrepancies between the possible interpretations caught my interest and very soon I became, as so many before me seem to have been, deeply involved in the world of Fleck, though, still, my questions and concerns continued to be atypical. Among the immediate simple ones was, of course, why he wrote his monograph. The question has been conspicuously absent from contemporary inquiry, even though it was written by a Jewish writer in Poland, and by all accounts completed extremely rapidly during the critical pre-war period from 1933 to 1934. Further, to whom did he address his monograph? Where did he find his inspiration? What were his sources? The queries seemed relevant as the book, according to my reading, despite its medical content, seemed to draw on broad areas of science, as evidenced by examples, quotations, and metaphors. This further relates to the important question of his concepts. Borrowing a recent notion from his somewhat younger co-prisoner in Buchenwald, the Nobel laureate Imre Kertes, Fleck has in contemporary debate in a strange way consistently been depicted as “fateless” in the sense that everything seems to depart from Fleck. Inevitably, he must have had some relation to his past and to his contemporaries. Was he appreciated, neglected, or perhaps justifiably ignored, and if so on what grounds?

It is evident that no solution to the above questions can be claimed with certainty. However, their examination might help to broaden the view of Fleck and his epistemology. In a more philosophical setting, they can be related to the two concepts usually conferred to Fleck, notably *esoteric* and *exoteric* knowledge, and the way we view these as well as their relation to the incommensurability claims in science.

I do not aspire to an interpretation of Fleck’s epistemology. Rather, one of my tenets is that, when one considers the text as a whole, this is an impossible mission. Thus, by slightly changing one of Fleck’s many metaphors, inadvertently translated in this way in one of the issues of the monograph, the comprehensive question can be formulated as “Prometheus or Chameleon” rather than the formulation of Fleck himself, “Proteus or Chameleon” (GDSF, 12), though Fleck somewhat inappropriately writes *Chameleonta* in his German text. This brings me to the need for a final caution. Fleck’s original text is written in German, which was not his native tongue. Although he mastered the

language well, in certain ways his German can be described as idiosyncratic. Any translation of it poses great difficulties. Despite the great merits of the English translation, a comparative reading of the German text has often been necessary.

### The Rediscovery of Fleck

The rediscovery of Fleck's monograph in the mid-1970s is credited to W. Baldamus, a former German refugee, at the time retired from a professorship in the sociology of science at the University of Birmingham (Baldamus 1976, 1997). Using the scanty information that Baldamus managed to obtain from Fleck's publisher in Basel, he concluded that the fate of Fleck was interesting and anticipated him as a possible forerunner to Kuhn. However, he cautiously pointed out that there was not yet enough information to render any conclusions possible. Baldamus also advised his German student, Thomas Schnelle, to work on Fleck. The latter, mastering Polish, complied with the advice and became involved in a three-year project supported by the Volkswagen Stiftung. It was initiated in 1979 and headed by Lothar Schäfer, professor in the theory of science. The project aimed to investigate the biography of Fleck, elucidate the philosophical and cultural background of his thinking, and publish his writings (Cohen and Schnelle 1986a).

Thaddeus Trenn, professor of history and theory of science, also in Germany, became attracted by Fleck's monograph in the mid-1970s when lecturing on Kuhn, and included it in the curriculum for further study. Trenn was also, in collaboration with Fred Bradley, responsible for the English translation of the monograph.

Schnelle's dissertation *Ludwik Fleck. Leben und Denken. Zur Entstehung und Entwicklung des soziologischen Denkstils in der Wissenschaftsphilosophie* was published in 1982. Following the reissues of the monograph, the English GDSF in 1979 edited by Trenn and Merton, and the German EET in 1980 edited by Schäfer and Schnelle, two meetings were arranged. The first took place in Hamburg in 1981 and was headed by Schäfer and Schnelle, and the second took place in Berlin in 1984 and was headed by Trenn and Cohen. Both meetings aimed to inform an English-speaking audience of the monograph and launch Fleck as an important contributor to contemporary science and as a forerunner to Kuhn.

The material presented at the two meetings was published in an extensive volume titled *Cognition and Fact* (CaF), edited by Cohen and Schnelle, in 1986. The major part of the book consisted of deliberations on Fleck's epistemology by international scholars in the fields of sociology of science and philosophy. In addition, three articles were included that commented upon the medical and immunological content of Fleck's monograph, putting it into the context of the science of the period in question. These articles were unavoidably more technical in character and they have frequently been neglected in the subsequent literature.

Fleck's work, notably the monograph, has been subject to an interpretative tradition in which the scientific notions used in the case histories have been excluded. A cautious estimate would be that almost half, probably more, of the monograph is directly devoted to the above-mentioned issues – bacteriology and immunology. In other words, at least half of the book, or the basis of his epistemology, has been considered

dispensable. The neglect of these parts has necessarily led to a failing understanding of their relation to the rest of the text.

Critical voices have been raised against this interpretative tradition but, considering the esoteric nature of the basis of Fleck's assumptions, they have easily been put aside. It is, however, of interest to note that Baldamus and Kuhn seem to be the only writers concluding that the complexity of Fleck's conceptual and theoretical premises make great parts of the monograph unintelligible for a non-specialist reader. Baldamus, in his initial writings on Fleck, also observes that the monograph comprises two parts: the first two of the four chapters deal with syphilis and the last two with the Wassermann reaction. The two parts differ not only in content but also in form (Baldamus 1977, 143). Baldamus also observes that, despite Fleck's references to 19th-century sociologists such as Jerusalem, LeBon, Durkheim, and Lévy-Bruhl, contemporary ones such as Weber, Lukács, Mannheim and the Americans seem unknown to him (idem, 136). Baldamus's impression is, from his viewpoint as a sociologist of science, that Fleck's style is dated and strongly old-fashioned. His conclusion is that Fleck is evidently an amateur within sociology and philosophy. He further concludes that the monograph was written intermittently over quite some years and towards the end evidently under great pressure. The German issue is poorly edited and repetitive, for which Fleck apologizes (idem, 140).

In his brief preface to Fleck's monograph, Kuhn recalls not only how he found Fleck's German extraordinarily difficult to decipher but also how he had great difficulties because he "possessed neither the background nor vocabulary to assimilate discussions of medicine and biochemistry especially when viewed from the to me then unknown and yet vaguely repulsive perspective of a sociology of the collective mind" (1979, ix–x). Further, he questioned the notion of a "thought collective," which he equated with a hypostatized fiction that he found "intrinsically misleading and a source of recurrent tension in Fleck's text." In broad agreement with Baldamus, Kuhn concluded that Fleck's theoretical outlook and technical vocabulary inescapably rendered the monograph inaccessible. If more readers than Baldamus and Kuhn had reached the same conclusion, the interpretation of Fleck's monograph would have been different. According to Harwood in his review of *CaF*, most readers, neglecting this theoretical context, seem to have used the monograph in one of two ways: either as a starting point for relating Fleck's epistemological or scientific views to various intellectual and social contexts or as an ally in advancing their own epistemological positions (Harwood 1986, 185). The resurrection of Fleck's monograph has, however, like *CaF*, created extensive secondary writings. The scientists participating in the discussion broadly belong to one of three different fields of scholarship: philosophy, sociology of science, and natural science. Scholars belonging to the first two fields have written the vast majority of the articles.

### **Biographic Data**

An extensive biography of Fleck can be found in Schnelle's dissertation, published in 1982. The greater part of the biography is included in *CaF*. The main features, repeated in numerous articles, include a brief outline of Fleck's medical studies and his professional career as a microbiologist in the Polish city of Lwów, including the period during

which he served as an assistant to the well-known typhus specialist Rudolf Weigl. He was dismissed from the position after about one year. After that he worked at various state-owned laboratories in parallel with work in his private laboratory set up in 1923. After the Nazi capture of Lwów in 1941, Fleck was evicted with his wife and his son to the Jewish ghetto of the city. Under primitive conditions, he worked in the ghetto hospital on the production of a vaccine against typhus using urine from patients who had already contracted the disease. After being deported in 1943, first to Auschwitz and then to Buchenwald, he continued his work. After the war, Fleck returned to Poland and worked as a microbiologist in Lublin. After his habilitation with Professor Hirszfeld, he became a full professor in 1950. In 1952 he moved to Warsaw, which gave him time to focus on research, mainly studying the phenomenon of leucergi. In 1957 he and his wife immigrated to Israel, where he continued his research. Fleck died in 1961 at the age of 64.

Besides the monograph, Fleck's philosophical work includes six articles published between 1927 and 1947. These philosophical articles, and an additional unpublished one from 1961, rejected by four journals, have been translated and are all included in CaF. A complete bibliography including his works in natural science, listing more than 150 papers, is added. The English edition of the monograph includes a brief account of Fleck's biography, in line with the above-mentioned sources. It is written by Trenn, as is a descriptive analysis of the monograph.

### Conflicting Views

According to the predominant interpretations, Fleck was a prominent and highly regarded representative of his professional fields – bacteriology and serology – indeed, influencing the development of the latter discipline. In the introduction to the German issue of his monograph, EET (Schäfer and Schnelle 1983, vii), which closely parallels the introductory chapter in CaF, Fleck's writings in philosophy and sociology are depicted as comparable to Popper's *Logik der Forschung*, published in 1934. In the English issue, GDSF (xvii), as in CaF (ix), this claim is also made in relation to Robert Merton's pioneering study of economic relations. Fleck's familiarity with the two fields of scholarship is praised, and the opinion is based on his references to writers such as Jerusalem, Mach, Durkheim, and Lévy-Bruhl. An opposition to members of the Vienna Circle has been maintained, supported by a reference made in passing to "Schlick, Carnap and others" (GDSF, 50). Bohr and Heisenberg, mentioned in his 1929 article "Krise der Wirklichkeit" in *Die Naturwissenschaften*, but not in the monograph, have also been recognized as influential in his epistemological deliberations (Trenn 1981; Schnelle 1986b; Löwy 1990). Despite these references, the difficulty of tracing and evaluating Fleck's sources has frequently been articulated. It has also been stressed that he does not always mention his sources. Thus the linking of a particular statement to its postulated origin becomes problematic. A further claim is that he added references that had not served as material for his writing after the completion of his work (Schnelle 1986b, 12).

The situation easily invites the interpreter to more or less far-reaching speculations. Thus, according to Harwood, Schnelle devotes a great part of his thesis to trying to link



Fleck to the logic-centred contemporary Polish philosophers at the University of Lwów, Twardowski, Ajdukiewicz, and Chwistek. However, as Schnelle rightly notes, these were never mentioned by Fleck (Harwood 1986, 175). A more recent attempt to trace contemporary influences on Fleck's writings is found in the prize-winning article by Bonah "'Experimental Rage': The Development of Medical Ethics and the Genesis of Scientific Facts" (Bonah 2002). The hypothesis communicated by Bonah is that the main motivator for Fleck's writings, including what Bonah characterizes as his awareness of the crisis of medicine and medical science, was the impulse to react to what Bonah calls the "The Lübeck Totentanz." In early spring 1930, the adverse effects of mass vaccination of newborn children against tuberculosis resulted in an unprecedented number of deaths in Lübeck, and an ensuing court procedure. Although Fleck never refers to the catastrophe or deals with tuberculosis in his writings, Bonah makes his claim on the assumption that Fleck must have been well aware of it.

In contrast, Löwy, mentioning that Fleck never viewed himself as a philosopher, gives a detailed and thorough account of the rich medico-philosophical tradition in Poland including the Polish School of Philosophy and Medicine (PSPM; Löwy 1990). There is ample evidence that Fleck visited the PSPM meetings in Lwów. He also presented a paper that was published in its journal, *Archiwum Historji i Filozofji Medycyny oraz Historji Nauk Przyrodniczych* (The Archives of History and Philosophy of Medicine). In her thesis Löwy further includes a translation of correspondence between Fleck and one of the more prominent members of the PSPM, Bilikiewicz, initiated by a comment made by Fleck on a book written by the latter (idem, 249). Löwy does not comment upon the correspondence, but mentions that Fleck did not seem to have been accepted by the PSPM. Despite his apparent familiarity with it, he never refers to the PSPM or its members in his writings. That Bilikiewicz's lack of acceptance affected Fleck's philosophical writings is, however, substantiated by the correspondence. After sharp criticism of Fleck's epistemic views, Bilikiewicz in his final rejoinder frankly states that further discussion would be pointless because of their contrary positions on scientific questions (idem, 274). Löwy's well-supported conclusion is that the PSPM has to be viewed as part of the formative background of Fleck's writings.

In addition to these speculations, the alleged lack of apparent sources in Fleck's epistemic writings has also lent itself to a conception of Fleck as having prescient views of science in general and the sociology of science in particular. This conception of Fleck is in agreement with the purported indebtedness of Kuhn towards Fleck.

My own concern was not surprisingly focused on Fleck's representations of syphilis and the Wassermann reaction. Both seemed remarkable and somewhat at odds not only with current thought in natural science but also with the history of science. Numerous interpreters have praised Fleck's account of the Wassermann reaction. References to it include comments such as "masterpiece" (Elkana 1986, 310), "first hand experience" (Bloor 1986, 387), and "beautifully analyzed historical case study" (Trenn 1979, xiv). The awareness expressed by Kuhn concerning his inability to comprehend the underlying parts of the monograph due to lack of medical and biochemical background and vocabulary stands out as an exception (Kuhn 1979, ix).

However, in one of the three articles dealing with natural science included in CaF, Zalc gives a thorough account of the Wassermann reaction as viewed today and clarified why it was an unfortunate choice as a basis of an epistemology (399). Löwy (421) and Moulin (407) added to the early criticism of Fleck's unfortunate choice of the Wassermann reaction as a basis for an epistemology.

Fleck's representation of syphilis as a four-stage transformation of the underlying concept, presented in the first chapter, has been unquestioningly perpetuated (Schäfer 1993, 24). The four stages conveyed by Fleck include, from the 15th century onwards, carnal scourge (*Lustseuche*) implying sin, befouled blood bearing mystical-ethical overtones, a concept of cure, and identification of a causative agent. In this context, the ancient concept of befouled blood has been portrayed as the actual impetus for the search for a blood test – that is, the Wassermann reaction, which, when successful, finally transformed into a scientific fact (Trenn 1979; Schäfer 1993). Tsouyopoulos's rejection of the above account seems to have attracted little attention. The same applies to her remark that Fleck substantiates this first part of his story with but one quote from 1484 taken from a poem. Her main criticism is, however, not directed against Fleck but rather against the uninformed comprehension of medical science displayed by the humanist interpretation (Tsouyopoulos 1993).

The easiest explanation of my own conception of Fleck as a professional out of touch with his time and his science was, after having excluded high age, defective scientific background, marginal position, and want of literature – in other words, frequently associated phenomena. This seemingly harsh conclusion was based on my reading of the text, relating the scientific statements made by Fleck, and the beliefs and opinions expressed by Fleck in his scientific contemplations, to the relevant time periods (cf. Löwy 1986). Fleck's selective references in support of his statements added to the impression. An additional clue was his bibliography, put together in CaF, where many of the titles suggested speculation rather than substantial data (445).

Even stronger evidence was found in the introductory or explanatory sections of CaF and the two issues of the monograph. Behind the bright and cheerful narrative of Fleck's glorious scientific accomplishments and progress was another all too easily perceived, though so far unarticulated, story, austere and dismal, inescapably linked to the history of Poland and Germany, including the two World Wars, which both deeply affected the life of Fleck. When one takes that story into account, Fleck can be depicted as a man whose medical studies were interrupted and substantially delayed by his participation in the First World War. His early focus on bacteriology could be linked to the importance of the discipline in wartime conditions and to his teacher, Rudolf Weigl, who was at the time already involved in typhus research. The scientific accomplishments within bacteriology during the preceding decade were not only amazing but of utmost practical importance. Since ancient times, typhus, one of Fleck's scientific concerns, had been an inevitable companion of war.

Fleck's brief research experience includes about a year as an assistant to Rudolf Weigl. During that stay, he also completed his medical studies. This took place in a country deeply affected by the devastating consequences of the war and the ensuing complicated peace process. Apart from an early six-week study visit to Vienna, he was



for almost the next 20 years confined to the city of Lwów, precariously located, torn between German and Russian interests and warfare.

Fleck's dismissal as a research assistant has been ascribed to personal conflicts with his colleagues rather than anti-Jewish tendencies. After this brief experience of research he was referred to non-prestigious (*Krankenkasse*) routine laboratory work in non-academic settings and finally, without any employment to support his family, he became dependent on his private laboratory. By that time he had already published his monograph. What followed could be viewed as a brief recapitulation of wartime and post-wartime European history epitomizing its long-lasting deleterious influence on scientific pursuit in east and central Europe.

The above reading, taking the subject matter as well as history into account, has served as a complementary heuristic device in my efforts to comprehend how the genesis of the Wassermann reaction was understood by Fleck and his modern interpreters. As the latter closely parallel the work of Schnelle and Trenn, summarized in CaF or in the prefaces and the descriptive analyses of the various editions of Fleck's monograph, my comparisons will, besides the monograph, mainly refer to these sources.

### Fleck's Account of the Wassermann Reaction

In the first three sentences of the third chapter of the monograph, which deals with the Wassermann reaction, Fleck expounds his longstanding contemplation of how to make it comprehensible to a layman. He realizes the impossibility of mediating the deep insight acquired by years of practical experience and wonders how to explain this "complex, extremely rich field related to many branches of chemistry, physical chemistry, pathology and physiology" (GDSF, 52). At the time of his writing, the reaction had been extensively used for nearly three decades. What follows in this chapter is an outline, almost mystical in character, of the reaction based on "five little-known factors" performed in "a little world of its own," which cannot be described in words because of their lack of fixed meaning. The introduction to the field Fleck is to describe is thus, according to his view, neither purely rational nor intellectual *per se*. Its history cannot be logically reconstructed due to the by now detailed and differentiated description of the field of thought and the complex, interrelated, and mutually dependent definitions of its concepts. Fleck's conclusion is that the current description of the Wassermann reaction cannot be anything but a construction based on hindsight and retrospective knowledge of how things turned out to be. This account implicitly justifies his own mission.

After this introduction Fleck turns to what he portrays as the "rite introduction into the field according to the German ritual." For this purpose he chooses a somewhat unusual rhetorical strategy by inserting into his text a four-and-a-half page excerpt from the first edition of a 1910 textbook of immunology written by Citron, a student of Wassermann. The introductory statements conveyed by Citron relate to the by then formulated concepts of specificity, immunity, and antibody, the law of specificity, and the importance of control experiments (GDSF, 55–59).

The impressively clear outline by Citron, in essence still valid, takes as its departure the diagnosis of infectious diseases, the main concern of immunology at the time, and

also the legitimacy of serology as a field of its own. Citron correctly underlines that specificity is the precondition of sero-diagnosis. This means that the logic of sero-diagnosis demands elaborate controls to guarantee the specificity of the reaction, though the non-specialist may easily perceive these as excessive and confusing. Citron further stresses the additional possibilities for arriving at a diagnosis, particularly elaborate clinical observation and the identification of the etiological agents (that is, different strains of bacteria). He does not mention the Wassermann reaction.

Fleck devotes the next six pages to a rejection of Citron's text divided into five sections. He disputes the concept of infectious diseases, which, in his view, is based on the erroneous notion of the human organism as a closed unit. He further claims that the classical concept of immunity (that is, acquired) has to be abandoned. Moreover, the conception of a division between cellular and humoral immunity cannot be confirmed any more than specificity in the "distinctly mystical sense," as, according to Fleck, is exemplified by Citron's text. In the two remaining paragraphs, Fleck repudiates the strict methodology stressed by Citron, thereby claiming an absence of universally accepted systems of measurement in biology and, especially, in serology.

Fleck's final rebuttal concerns the general precepts in immunology and serology viewed as a coherent system and its repudiation of "intuition and empathy with the phenomenon" (*Sichempfinden*; GDSF, 63). The basis of Fleck's relativistic views, which contrast with what he depicts as "the thought style of the serologic collective" exemplified by Citron's text, is that no opinion will last forever and that "even specialist knowledge does not simply increase but basically changes" – facts which according to Fleck call for a sociological method in epistemology.

The rest of the chapter is devoted to his discussion of the Wassermann reaction or, more explicitly, the interaction between the individual, the collective, and the fact. According to Fleck this is amply evidenced by his own conception of the genesis and development of the Wassermann reaction. He also includes in his text a full-page scheme of the reaction after Bordet and Gengou 1901, illustrating the relation between the three concepts complement, amboceptor, and antigen (GDSF, 67).

Fleck briefly mentions Ehrlich and his pluralistic views on complement, the haemolytic system, and the complement fixation method devised by Bordet and Gengou in 1901, attempting to disprove Ehrlich. The method was rapidly spread and successfully used by Widal and Le Sourd as well as Wassermann and Bruck. In accordance with his views of immunology, Fleck's use of the concept of antibody is slightly confusing. Sometimes bacteriolysin or amboceptor are substituted.

Fleck's idiosyncratic use of current concepts makes his description of the test in the German edition somewhat obscure. Quite understandably, it has been improved in the English edition (GDSF, 66). In brief, the complement binding reaction, which, according to Fleck, is the most important instrument within serology, can be used on the one hand to demonstrate either the presence of antigen in blood and organ extracts or bacteria or, on the other, to detect the presence of its corresponding, specific antibody. However, according to his preconceived views, Fleck excludes the concept of antibody, idiosyncratically employing the notions bacteriolysin, bacteriolytic amboceptor, and haemolytic amboceptor.

According to Fleck's account, which quotes Wassermann, the stimulus to the syphilis research was said to have emanated from "head of the Ministry, Friedrich Althoff" (GDSF, 68). Troubled by the French lead in experimental biology, Althoff urged Wassermann to work on the disease. Fleck's conclusion was that the Wassermann reaction, from the very beginning, was linked to social motives such as rivalry between nations in an important field and the *vox populi*, personified by the ministry official, assenting to the voice articulating the importance of the question at issue (idem). In support of his claim that no-one but the collective can claim the discovery of the Wassermann reaction, Fleck refers to a polemic in a German medical journal, *Berliner Klinische Wochenschrift* (Bruck 1921). He repeatedly mentions Ehrlich, Bordet and Gengou, Widal, Wassermann, Bruck, Siebert, and Schucht, the last three at the time among Wassermann's research assistants, but also the names of Citron, Althoff, Landsteiner, Levaditi, and Marie (GDSF, 69). Fleck's long list of names is impressive. He concludes, however, that many more must have been involved in the detection and the consequent erection of the edifice of knowledge that, in reality, nobody had foreseen or even intended. Only one thing seems clear to Fleck. The fate of Wassermann and his closest co-workers was the same as that of Columbus. In searching for India the latter discovered America, whereas Wassermann, who, according to Fleck, wanted evidence for an antigen or an amboceptor, inadvertently fulfilled the ancient wish of the collective – the demonstration of syphilitic blood (GDSF, 69; see also Belt 1997).

The above narrative, which according to Fleck's references spans about 20 years, constitutes the basis of his epistemology, including his conception of the importance of old pre-ideas (*Uridee*) and mistakes (*Irrtum*) that, in addition to the collective, are the factors at work in the genesis and development of a scientific fact.

The last third of the chapter, containing an abundance of technical details, gives an apparently detailed account of the arduous work on the development of the Wassermann reaction. It also includes an abundance of names referring to the actors in the extended collective that, according to Fleck, was the real originator rather than Wassermann, who, simply through the retrospective construction of history, came to be depicted as its creator. From the references Fleck gives in support of his narrative one can, however, infer that actors of special importance, besides Wassermann, included Bruck, Neisser, Weil, and Citron.

### The Origin of the Wassermann Reaction

The first report on what was later to be denoted the Wassermann reaction was a paper published in 1906 in *Deutsche Klinische Wochenschrift* by Wassermann, Neisser, and Bruck titled "*Eine serodiagnostische Reaktion bei Syphilis.*" According to the outline in GDSF, the purpose of the investigation was "to demonstrate, by means of the complement fixing method *primarily* antigen in syphilitic organs and in syphilitic blood and *secondarily* antibodies (cum amboceptors) in the syphilitic blood of patients" (GDSF, p. 70).

According to a scientific reading this looks like a somewhat problematic mission. Luckily the German text is there, providing necessary clarification. No italics are to be

seen, no “cum amboceptors” are mentioned and the purpose is described as two-sided (“sowohl Antigen ... als auch – und zwar an zweiter Stelle – Antikörper ...”; EET, 92). The “improvement” in the English translation is most easily attributed to a conflation of the methodological basis of the Wassermann reaction and the relativistic interpretation of its genesis and development.

Another query arises, in the same context, from the frequent use of the notion “discovery” (*Entdeckung*). The rapid detection of different bacterial strains that could be linked to various infectious diseases that followed the pioneering work of Koch, including his 1882 discovery of the tubercle bacillus, was soon followed by other important discoveries. Schaudinn made the detection of the *Spirochaeta pallida*, which could be linked to syphilis, in 1905. The latter discovery, amazing at its time, was, however, disallowed by Fleck. Instead of Schaudinn, he viewed the collective as its true originator. In the same vein was his claim that syphilis *per se* did not exist (GDSF, 39). However, in the above contexts, the use of the notion of discovery makes sensible reading whether the discoveries were approved of by Fleck or not.

In contrast, depicting the Wassermann reaction as a discovery creates confusion when read scientifically. Given it was a test based on the application of the complement binding reaction, the notion of development (*Entwicklung*), as used in the title of the monograph, would have been more appropriate. The choice of the enigmatic notion of discovery will, however, soon get its explanation, though, admittedly, a somewhat unexpected one. The discovery of *Spirochaeta pallida* was the necessary precondition for the Wassermann reaction. The inability to culture it on the (at the time) available media added to the difficulties when designing the reaction. However, these essential preconditions are hardly deducible from Fleck’s narrative. This also holds for the fact that the complement binding reaction had already been successfully applied in diagnosing various infectious diseases, e.g. as in tests designed to diagnose abdominal typhus, by Weil, or epidemic meningitis, by Wassermann and Bruck (GDSF, 68).

It is much easier to apprehend Fleck’s persuasive concluding statements. Some of these are among the most quoted in the secondary writings. A few will perhaps suffice to exemplify. Some are according to Fleck true, others false, all in italics in the original. To the allegedly true ones belongs the most frequently quoted of them all: “From false assumptions and irreproducible initial experiments an important discovery has resulted after many errors and detours” (GDSF, 76). Another is: “[T]he true authorship is due to the collective, the practice of cooperation and teamwork” (GDSF, 78; Fleck uses *Gemeinschaft*, which is a concept in line with his epistemology – therefore “community” would have made more sense than “teamwork”). Another of Fleck’s true statements reads as follows: “[T]his compulsion becomes resolved only by comparative epistemological considerations and is explained as an intrinsic constraint imposed by the thought collective” (GDSF, p. 80) (cf. “[D]ass dieser Zwang sich erst durch vergleichende erkenntnistheoretische Betrachtung auflöst und als innerer Denkstilzwang erklärt”; EET, 106).

In contrast, among statements by Fleck deemed as false, one is quoting a reference from 1924: “... the original method which remains unchanged even today” (GDSF, p. 76). According to Fleck, a workable Wassermann reaction was the result of

innumerable smaller and larger changes undertaken by the collective, whereby nothing of the original remained. Though most of these countless contributors were unknown, the end of the chapter includes an undeniably impressive number of purported ones properly mentioned by name. Wassermann is far away. It is the discovery made by the collective that initiated the avalanche, including its innumerable publications (GDSF, 80).

### Epistemic Deliberations on the Wassermann Reaction

The fourth chapter, by far the longest of the monograph, is an epistemological deliberation on the history of the Wassermann reaction as outlined in the preceding chapter. Interpreters of Fleck have frequently concluded that the chapter must have been written in great haste as evidenced by its at times contradictory character (Baldamus 1977). The claim is supported by a remark made by Fleck, excusing himself for the fragmentary nature of his presentation concerning the communication of ideas within the thought collective (GDSF, 111). A detailed account of this extensive chapter would divert from the main topic. What follows is a brief summary of some of its main points disclosing the link to the Wassermann reaction.

Fleck starts by concluding that the history of the Wassermann reaction, when compared with that of syphilis, requires a much greater number of technical expressions and greater reliance on expert opinion and, also, that many more names have to be mentioned (GDSF, 83). His inference that the penetration of a scientific field discloses its dependence on the collective, thereby demonstrating a general phenomenon, is, however, easily invalidated by the fact that his two accounts span two wholly different time periods. In the first of these neither syphilis nor the Wassermann reaction was defined. Moreover, his choice of, or access to, sources makes any comparison incoherent.

A further claim is that the above characteristics are paralleled by an increased number of passive connections as an inevitable consequence of an increased number of active elements, thereby exemplifying two concepts central to his epistemology. He supports his claim by the use of an example – the employment of alcohol in the preparation of suitable tissue extracts applied as an antigen in the Wassermann reaction (GDSF, 83). The statement sounds undoubtedly all too reminiscent of the conception of the cumulative increase of knowledge Fleck is said to oppose. In the same vein is his second example, which refers to the relation between the atomic weights of oxygen and hydrogen (GDSF, 83), which could be read as an allusion to the periodic table, at the time in fashion related to modern physics (cf. Infeld 1934 and Hedfors 2006b).

In summary, according to Fleck's exposition of the Wassermann reaction, Wassermann's basic assumptions were untenable, the initial experiments he and his co-workers performed were impossible to reproduce, and the first period of the history of the Wassermann reaction was characterized more by hope than by results. Still, according to Fleck's narrative, these experiments had a tremendous heuristic value. In the text Fleck identifies himself with Wassermann and thereby realizes that the latter "had a clear plan and felt certain of the results" (GDSF, 85; "*Er hatte einen fertigen Plan und war vom*

*Ergebnis überzeugt*"; EET, 113). The method was however not yet ripe. It had to be worked out and so it was. Through arduous work, Wassermann discerned a melody in the muddled scatter of tunes that, although inaudible to others, corresponded to his inner melody and, thus, the method was changed accordingly (GDSF, 86). Fleck's main point, that the exact date when this occurred cannot be stated, is hardly surprising. Of greater interest, however, is that his review of the event implies a perfectly clear account of the hypothetic-deductive method and its successful use in science, including the strong predictive power of a well-founded hypothesis. Fleck also touches upon the ambiguous results of the Wassermann reaction, stressing that it often had to be decided whether the results should be considered positive or negative (GDSF, 86). The latter statement has been viewed as supporting his purported relativism (Schäfer 1993).

After a detour dealing with his own experimental experiences in bacteriology, Fleck returns to his subject, enumerating factors of importance for the proper employment of the Wassermann reaction. The individual and his experience are emphasized, as is the necessity of an assumption (*Voraussetzung*) and a whole system of controls. Comprehensive experience in serology including its practicalities is thus a prerequisite for reproducible results and might involve the irrational "serological touch." According to Fleck, however, "Wassermann's reports about his reaction contain only the description of the relation between syphilis and a property of the blood" (GDSF, 96). This made Fleck conclude that the practical experience Wassermann and his students gradually acquired was the most important factor for the reproducibility and practical use of the Wassermann reaction. Thus Fleck relates many of the problems to the fact that the early opponents of the Wassermann reaction, in his view, were lacking that experience. Writing almost 30 years later, Fleck concludes that experience still has the same importance in the whole of serology.

When summing up comprehensive experience, including technical skill and careful experimental design, and stressing the importance of adaptations and mistakes, Fleck can finally conclude: "The factuality (*Tatsächlichkeit*) of the relation between syphilis and the Wassermann reaction consists in just this kind of solution of the problem of minimizing thought caprice, under given conditions, while maximizing thought constraint" (GDSF, 98; "*den stärksten Denkwang bietet*"). Thus, *the fact* represents a *stylized signal of resistance in thinking* and, as already mentioned, the collective is the carrier of the thought-style as well as the stylized signal of resistance, the latter notions being central to his epistemology.

In the next paragraph, Fleck deliberates on thought styles and thought collectives and their relations, the nature of facts, and the social forces at work in the formation and preservation of various thought collectives. This section is followed by one that deals with Fleck's conception of some characteristics of the thought style of modern science. The latter few pages are among the most quoted (Bazerman 1988; Schäfer 1993; Andersen 2002). They include Fleck's division of the modern Western scientific enterprise into four parts: journal science, handbook science, textbook science, and popular science (GDSF, 111). According to Fleck the first two, journal and handbook science, constitute expert knowledge confined to the esoteric circle, thereby exemplifying another of his central epistemic notions.



He starts with a discussion of popular science, which is said to furnish the major portion of every person's knowledge, thus creating an epistemological problem so far not investigated. In a strict sense, according to his views, it is science for non-experts – for generally educated amateurs (*gebildete Dilettanten*) – and as such it cannot be classified as an introductory science. Characteristic features include omission of detail and, in particular, controversial opinions and, as a result, an artificial simplification. Popular science aims at plain, vivid, and attractive reading of an incontestable science easy to either accept or reject depending on personal belief or ideology epitomized as *Weltanschauung*. Thus popular science is a part of the exoteric field whose members easily conceive of scientists as belonging to one of two categories – the “good guys” who find the undisputable objective truth or the “bad guys” who miss it (GDSF, 116).

It should be remembered that in his humanist pursuits – philosophy, history of science, and history of medicine – Fleck was confined to the exoteric circle of amateurs. He was a member of several such associations in Lwów. Fleck also uses the slightly derogative notion *Dilettanten* when referring to members of the exoteric circle rather than the German equivalent *Amateur*, the expression applied in connection with the above-mentioned associations.

### Harmony of Deceptions

The account by Schäfer and Schnelle, in the preface (*Einleitung*) of EET, of the Wassermann reaction presented by Fleck is extensive and elaborate, but also unambiguous and treacherously convincing. For those familiar with the subsequent interpretations of Fleck it is, however, evident that this first outline constitutes the fundamental unquestioned basis of ensuing scholarship. Trenn accords with the above in his descriptive analysis in GDSF (1979, 154). Although he does not expand on the Wassermann reaction, he mentions in passing that the Wassermann reaction is the basis of Fleck's case history, concluding: “Specifically he has drawn attention to the discovery of the Wassermann reaction as the fulfillment of the ancient belief that a connection exists between syphilis and the blood” (165).

As the German text might not be as easily accessible as its somewhat briefer English counterpart written by Cohen and Schnelle (1986a), the following brief summary of the main points is extracted from the latter.

Thus we read that Fleck in his epistemology sociologizes and historicizes the theory of knowledge. By reformulating the concept of a scientific fact, he also relativizes the understanding of science and fact as exemplified by his case history, the discovery of the Wassermann reaction. (idem, xi, xxiii)

Accordingly, “[I]t is by no means to Wassermann this achievement [i.e. the discovery of the Wassermann reaction] can be attributed” (xiv). He was just the head of a research group dealing with syphilis where its members built on each other. Under the heading “What is Scientific Progress?” we can read that Wassermann and his co-workers at the beginning of their research “were above all looking for a diagnostic demonstration procedure for syphilis with the help of which antigen could be

demonstrated in luetic organs and blood" (xxii). We also learn that despite "initial successes it turned out to be impossible to accomplish the program" but remain ignorant as to why. The failure of the mission, however, turned the group to concentrate on the demonstration of antibodies. Initial attempts were not very successful. Rates of success ranged from 15% to 20% but, finally, success was achieved in at least 70% to 90% of the cases. It is claimed that this accomplishment cannot be reconstructed but only understood by referring to the large numbers of unnamed co-workers trying out technical tricks and making minor changes until success was achieved. The goal was, however, also reached "by changing the interpretation of the data" (xxii). As changes evolve slowly, the collective knows nothing of them in retrospect. Therefore the following inference is self-evident and further supported by a quote from the monograph:

The following state of affairs is therefore firmly established and can be regarded as a paradigm of many discoveries. *From false assumptions and irreproducible initial experiments an important discovery has resulted after many errors and detours.* The principal actors in the drama cannot tell how it happened, for they rationalize and idealize the development. Some among the eyewitnesses talk about lucky accident, and the well-disposed about intuition of a genius. It is quite clear that the claims of both parties are of no scientific value. (xxii; see also GDSF, 76, italics in original)

In the section devoted to the interpretation of Fleck's outline of "the historically embedded character of knowledge" (xxiii), Fleck's reference to his work in bacteriology is briefly alluded to. Emphasis is given to Fleck's introduction of the concept of "proto-idea" or "pre-idea" (*Uridee*), which he is said to use as an instrument to grasp notions arising in the distant past but still heuristically influential in regulating science or, as quoted from the monograph: "[P]roto-ideas must be regarded as developmental rudiments of modern theories and as originating from a socio-cognitive foundation" (xxiv; see also GDSF, 25). The quoted translation differs, however, somewhat from Fleck's original: "[D]ie Urideen sind... zu betrachten und ihre Entstehen ist denksozial zu begründen" (EET, 37). Of special importance "was the notion of the corruption of blood of syphilitic patients taken from the doctrine of the mixture of humors" (xxiv), but despite the fact that Wassermann's primary goal was different, the latter notion is said to have prevailed and, finally, "The ancient social wish was fulfilled: the proof of the corrupted blood of the victims" (xxiv).

In the section devoted to knowledge and social context Cohen and Schnelle exemplify Fleck's demonstration of "the horizontal embeddedness of knowledge" including the presence of appropriate "social moods." Only the latter could be at all sufficient to motivate research of such intensity as that exemplified by the Wassermann group (xxv). The phenomenon was to be explained by the following fact: "The syphilis disease still represented at that time a problem that due to its characterization as the pleasure plague [*Lustseuche*], received great social attention. It was for that reason that Wassermann's researches were initiated on the part of the state" (xxv). What was at stake was national pride and prestige. On the contrary, tuberculosis, which at the time claimed many more victims, was not made the subject of elaborate research as it was considered a romantic but not dishonoring disease (xxv; see also GDSF, 77). This part of the

narrative, illusory as it regrettably is, shows a particular perseverance in the interpretations of Fleck (Schäfer 1993).

### The Question of Sources

By now it seems reasonable to conclude that the third and the fourth chapters of the monograph, at least when referring to the Wassermann reaction, contain contradictory statements. The account of Fleck's epistemic views, as presented in the introduction to CaF (ix–xxxi), resulting from a reading of the same two chapters, forms, however, a unified, non-contradictory epistemology. This paradox quite naturally gives rise to the query: Is the seemingly unified epistemology as a whole to be viewed as a result of selective reading or incomplete knowledge of the sources? If that could be clarified, the harmony of deception, by his commentators perceived as one of the main themes of Fleck's monograph (xv, xxviii; see also GDSF, 27, 89, 93), could possibly be reinterpreted.

The question of Fleck's sources has been of concern ever since the reappearance of the monograph, and great scholarly pains have been taken in trying to solve the problem. Because of the perceived difficulties, the view of their purported inaccessibility has been more or less transformed into a fact. Fleck's description of the Wassermann reaction does, however, include some sources, referred to in the third chapter, although conspicuously spaced in time. Two were from 1906 (cf. Wassermann et al. 1906 and GDSF, 175), the purported year of the discovery of the Wassermann reaction, one from 1914, the following four from 1921, and the one most frequently referred to from 1924. A handbook chapter on the serological diagnosis of syphilis from 1930 and an article commemorating the 25th anniversary of Schaudinn's discovery of *Spirochaeta pallida* were also noted. Two final sources were from 1931, one on immunology and the other on the theoretical exposition of the Wassermann reaction. All these references, except the one from 1924 and the handbook chapter, were from German medical journals. The relative scarcity of references was thus in apparent contrast to the undeniably crushing number of names referred to by Fleck when describing the Wassermann reaction as discovered by the collective.

### An Academic Debate

A search of the medical journals of 1921 amply disclosed an academic quarrel of an intensity that makes the fiercest of our own seem but pale reflections. Further study revealed its intricacies. Moreover, it shed some light on Fleck as well as his sources and his monograph.

It all seems to have started with an article by Wassermann (1912a), referred to by Fleck, titled "*Neue Experimentelle Forschungen über Syphilis*" and published in February 1921 in the widely read weekly medical journal *Berliner Klinische Wochenschrift*. The article was a reprint of a speech delivered two months earlier at a meeting of the Berlin Medical Association. True to his assignment Wassermann gave a vivid, clear, and fascinating overview of research spanning the previous 15 years, intended for a

broad medical audience. Whether this audience, according to Fleck's epistemology, was to be viewed as an esoteric or an exoteric circle remains an open question. Wassermann was, however, invited to give the speech, which implied that the organizers as well as the audience expected the references to his research. At the time he was a leading figure in experimental research with an impressive list of publications of great ingenuity.

The article provoked the fury of Bruck, a former assistant of Wassermann at the time when it all started. Wassermann, Neisser, the well-known dermatologist deeply involved in experimental research on syphilis, and Bruck were, in order of priority, the authors of the first seminal paper on the Wassermann reaction published 1906. Bruck's somewhat late claim to priority, however, was not quoted by Fleck, who only quotes Wassermann's first rejoinder, titled "*Zur Geschichte der Serodiagnose der Syphilis*" ("On the History of the Serological Diagnosis of Syphilis") (1921b). Wassermann's rejoinder was followed by a piece from Bruck, referred to by Fleck. The final reply by Wassermann, however, is also unquoted by Fleck (Weil 1921, Lange 1921). Joining in the battle were some additional writers such as Weil and Lange, the latter also a former research assistant of Wassermann, though not quoted by Fleck. Both focused on the complex theoretical questions of the Wassermann reaction and the methodological changes undertaken. These articles were also followed by replies by Wassermann. The fierce discussion between Bruck and Wassermann was, however, after the final rejoinder by Wassermann (1921c), closed by the editor, who described it as "hardly refreshing" (*unerquicklich*), sharply reprimanding Bruck for his late and stubbornly maintained claim to priority after 15 years of tacit assent. In a final caustic sentence, the editor articulated his hope that this type of debate, from now on, would be eliminated from the journal.

After this final editorial rebuke, one would perhaps imagine a somewhat gloomy future for Bruck. He did, however, surprisingly soon reappear as the author of a book published in 1924 on the serological diagnosis of syphilis – a book that was to become the most frequently referred to source in Fleck's narrative when he was deliberating on the Wassermann reaction (Bruck 1924; GDSF, 186). In the preface Bruck, a professor and head of the department of dermatology at the city hospital of Altona, gives an account of the book: it should be viewed as the second edition of his earlier monograph published in 1909; because of tremendous development in the field, a more comprehensive account should be of interest. According to Bruck's retelling of the story, he was approached by the publishing company, Springer, at the end of 1921, and asked to renew and expand on what he depicts as the first edition. Due to the by now extensive field in question he secured the assistance of three of his colleagues, all heads of serology departments at different city hospitals. The four authors were to be responsible for separate sections of a handbook titled *Handbuch der Serodiagnose der Syphilis* published by (*herausgegeben von*) Bruck and thus, according to Fleck's definition, meant for the esoteric circle.

The ensuing more than 500 pages include all conceivable applications of the Wassermann reaction and, as a corollary, abundant information on syphilis. Bruck contributed a detailed history of its development and a thorough account of the complement binding reaction with all the complexities and perplexities that arise when one attempts

to apply it to syphilis. Bruck is also the author of the section dealing with the clinical value of the reaction, which is closely linked to the question of its specificity.

Throughout his writings Bruck refers to the Wassermann reaction as either "*die Reaktion*" or the somewhat awkward "*die Serodiagnose der Syphilis*." The other three writers are all mainly dealing with different aspects of syphilis and its diagnosis by the use of other methods including flocculation tests. Somewhat surprising, however, in the latter context, is one of the headings, "*Brucksche Reaktion*," or "The Bruck Reaction," not preserved by history (idem, 298).

In the introductory part of Bruck's outline he returns, however, to the question of priority, though with becoming modesty, in a footnote in small print. He concludes that the final verdict on the issue is now left to the history of medicine, alluding to one of Wassermann's rejoinders in the 1921 debate revealing Wassermann's perspicuous awareness of its scientific importance at the time in question. All of Wassermann's correspondence from the early years had already been deposited among the documents (*die Akten*) of the Kaiser Wilhelm Institut für experimentelle Therapie, serving as sources for the account of the history of syphilis eventually to be written. Bruck cannot, however, resist the temptation to, once more, repeat his story of who said what, where, and when in 1906, including the late Althoff, head of the ministry – all too reminiscent of his position in the fierce debate of 1921.

Against this background, what Bruck in 1924 depicted as the first edition of the book – the one from 1909 – would be of interest, although not quoted by Fleck. The title of the thin volume written by Bruck, at the time *Privatdozent und Oberarzt* in dermatology at the University Clinic of Breslau, was unchanged except that only the second edition was classified as a "handbook." The 1909 volume was an expansion of a previously published report of an expedition to Java undertaken by Neisser as an important part of his research involving the experimental transfer of syphilis to monkeys. The first page of Bruck's 1909 publication constitutes a solemn dedication to his two highly regarded teachers, Neisser and Wassermann. The former was Bruck's superior at the time.

The greater parts of the volume reappear unchanged in the second, 1924, edition of the book. A conspicuous difference is, however, the credit given to Wassermann in the first version in contrast to his almost complete exclusion from the second. Although Bruck's style is somewhat egocentric already in his 1909 publication with rather frequent use of "I," "me," and "myself," Bruck always mentions himself after the name of Wassermann. Papers by the latter are also amply referred to whether published alone or with his at the time many assistants including Citron and Plaut, both in various roles reappearing in Fleck's monograph.

Part of the 1921 debate could easily be viewed as reflecting an opposition towards the former relationship between a teacher and his assistants. But more importantly, the debate could also be viewed as reflecting the escalating prestige assigned to scientific institutions and scientific publications, as represented by Wassermann, and the not-so-glamorous practical clinical work, or the lesser regarded studies performed at home in private laboratories. Bruck as well as Lange had long been confined to the last two types of activities, and so was Fleck.

## Selectivity of Reading

Though Fleck's references to the 1924 edition are confined to the first pages of the book, it is obvious that he leans heavily on these few pages and, further, that many later misunderstandings have their origin in these pages as well. The remaining misconceptions can for the most part be traced back to Fleck's highly selective reading of the 1921 debate between Bruck and Wassermann as well as his three sources from 1930 and 1931, one commemorating the discovery of *Spirochaeta pallida*, another by Plaut, a former assistant of Wassermann, and a third by Hirszfeld, professor of microbiology in Warsaw.

An understanding of the 1921 debate necessitates the reading of not only all the articles by Bruck, Wassermann, and the others involved, but also the founding articles from 1906 that the debaters were referring to. Although Fleck in his monograph refers to but two of these, both written by Wassermann, the additional reading makes it highly questionable whether he had read any of them. Fleck's use of "pseudo references" has been observed by Sadegh-zadeh, though in another context (1981), and the above articles were all amply referred to in the 1921 debate and in the sections written by Bruck in the 1924 handbook. The articles from the early years around 1906, including Bruck's 1909 monograph, form, however, a badly needed corrective of a narrative that in the hands of Fleck easily comes out of order.

The problem was not only Fleck's selection of sources but also his very selective reading of the ones selected. It could, however, be concluded that the sources of his monograph so far discussed must be referred to as popular science, at least from the scientific point of view. Besides containing a fierce debate involving personal prestige and the question of future scientific remembrance, his sources were either celebrating memorials or broad review articles based on previous speeches aimed at a mixed audience presented by well-known professors at solemn meetings (*Hauptversammlungen*) of the Kaiser Wilhelm Society in Berlin. Considering the prestigious mission and the professional competence of those involved, the speeches, and the ensuing articles, are of the highest quality and exemplary in outline. Fleck seems often, however, to have restricted his reading to the introductory or concluding parts, shunning the more complex and often difficult problems posed. Furthermore, when relying on these parts his statements frequently emerge as easily identified reprints.

The latter is particularly applicable to the somewhat different overview article in *Klinische Wochenschrift* in 1931 by Hirszfeld, at the time professor at the Serum Institute of Hygiene in Warsaw. Besides having a medical education and a broad training in science including microbiology in Germany, he was also trained in German philosophy. Bruck in his 1924 handbook, though with a German spelling of the name, amply refers to Hirszfeld. His work, in collaboration with Klinger, on a suitable serological test for syphilis (*Gerinnungsreaktion*) presented in a paper in 1914 formed a suitable basis for the above-mentioned "Bruck reaction" (Bruck 1924, 242).

The article by Hirszfeld entitled "*Prolegomena zur Immunitätslehre*" ("Introduction to Immunology") is a speculative, philosophically inclined, state-of-the-art outline of immunology including its past and possible future, the latter depending on what



testable hypotheses would be posed by the scientists involved. Somewhat surprised, I could conclude that the very first section of Hirschfeld's article could be read as a key to many of Fleck's views on bacteriology and his conceptions of infectious diseases as well as many of his metaphors. Without that key, some of the more contradictory parts of Fleck's third chapter would have remained unintelligible (GDSF, 61).

So far, Fleck's somewhat outdated scientific views have been ascribed to the influence of his former teacher, Rudolf Weigl, who held purportedly old-fashioned views in bacteriology (Freudenthal and Löwy 1988). After reading the wartime writings of the latter, the openly quoted paper by Hirschfeld forms a more reasonable basis of Fleck's expounded views on immunology and bacteriology (cf. Hedfors 2006a). Fleck, however, avoids the more basic aspects of Hirschfeld's article. Examples of the latter are alterations in morphology and physiology within the species of bacteria including virulence and, further, the discussion referring to the postulate of Koch, at the time still disallowed by Fleck. Fleck's deliberation on rough and smooth forms of bacteria (GDSF, 90) could, on the other hand, be traced to the article by Hirschfeld, as could many of his references or metaphors, such as, to mention just a few, implicit reference to the periodical table (GDSF, 111), economy of thought, and the primitive view of disease as an ongoing battle between the individual and invading bacteria (*Kampfmetaphore*; GDSF, 59).

Plaut's deliberation on the theoretical exposition of the Wassermann reaction, published in 1931, also serves as a basis of Fleck's selective reading. The article is a reprint of a speech delivered at the Kaiser Wilhelm Association. Plaut, at the time professor at the Kaiser Wilhelm Research Institute of Psychiatry in Munich, refers in his introduction to his experience as a former research assistant of Wassermann at the time when it all started. Plaut reminds the audience of the publication on the serological diagnosis of syphilis 25 years ago by Wassermann, Neisser, and Bruck – the genesis of the diagnostic test that since then has borne the name of Wassermann. He further states that the discovery of Salvarsan by Ehrlich (in 1909) and the discovery of *Spirocheta pallida* by Schaudinn (in 1905) just a few years earlier, in conjunction with the Wassermann reaction, constitute the three most important accomplishments of German science on syphilis, enabling the definition of cause, the confirmation of diagnosis, and the possibility of cure. Furthermore, the application of the Wassermann reaction enabled the delineation of a highly enigmatic disorder. The protean manifestations, thus far only suspected as belonging to the same disorder – if such a suspicion arose at all – could now be united by either retrieving the causative organism from different organs or by the use of the Wassermann reaction. The latter could be applied to both blood and cerebrospinal fluid. Once diagnosed, the disease could in many cases be alleviated or cured. The *Volksseuche*, far from Fleck's *Lustseuche*, could for the first time be counteracted, which explains why the Wassermann reaction almost immediately became the serological test most frequently applied.

The protean manifestations of syphilis include several variants and stages such as the congenital variant with its disfiguring features of the face, the neurological variants with characteristic paralysis or dementia, the cardiovascular and the dermatological and, not least, the latent stages. All these variants and stages had been the focus of

intense research activities in the early years and were, in consequence, recalled in the celebrating articles of the 1930s including the one by Plaut. Thus the old notion of *Morbus proteiformis* (GDSF, 12) could eventually be explained, and syphilis viewed as a disease of great clinical variety unified by a common cause, *Spirochaeta pallida*. Further, the old idea of befouled blood, or rather of syphilis as a disease exclusively of the blood, was disproved by the retrieval of *Spirochaeta pallida* from different organs. Bruck pointed this out in 1909 and also on the first page of the 1924 handbook. It should, however, be stressed that any contagious systemic disease, including syphilis, implies contagious blood. With respect to syphilis this risk is, however, low.

An inevitable consequence of the possibility of diagnosing syphilis with the Wassermann reaction was the emergence of innumerable attempts to design similar tests or tests based on different methodology, including the one by Bruck, all described in the 1924 handbook. Although amply discussed in the medical press, few were to remain besides the Wassermann reaction. This important part of the history is but alluded to by Fleck (GDSF, 53). While mentioning that the Wassermann reaction was a clinically workable test (GDSF, 72), as amply deliberated on in the article by Plaut, Fleck mainly concentrates on the fact that its theoretical basis remained unclear. In so doing he selects his easily identified quotes from the concluding part of Plaut's article with its allusion to the 1921 debate. Without taking part, Plaut, with the words "*die Akten sind noch nicht geschlossen*," referring to the documents of the Kaiser Wilhelm Institute mentioned above, solemnly concludes his article by stating that his former teacher, the late professor August Wassermann, will emerge in minds as as great a scientist as a benefactor of an enduring humanity ("*vor unserem geistigen Auge gleich groß als Naturforscher wie als Wohltäter der leidenden Menschheit*").

The latter statement can be interpreted as referring to the broader context necessary to make the Wassermann reaction fully comprehensible. Within this context, the history of the Wassermann reaction could be depicted as one rapidly splitting into two partly overlapping discourses. One of these consisted of theoretical and methodological expositions centred on questions of specificity and antigenicity of proteins versus lipids. These discussions had for their solution to rely on the progress of basic science, part of which, in a rather distant future. This further underscores the unfortunate choice by Fleck of using the Wassermann reaction as the basis of an epistemology.

The other discourse was mainly related to all the medical, social, legal, and ethical questions that emerged secondarily to the application of the Wassermann reaction. Both discourses were initially confined to the esoteric circle but only the former remained so. The second was rapidly transformed into a highly diverse discourse affecting wide areas of society, parts of which were esoteric, others exoteric (cf. Löwy 2004). The development was inevitable as the Wassermann reaction was, besides its clinical application, inescapably linked to the cause, transmission, and possible cure of a justifiably highly feared, deeply socially stigmatizing contagious disease affecting sexuality, reproduction, and mental capacity, causing disability and a painful death. In consequence, large parts of the 1924 handbook relate to these questions (cf. Löwy 2004).

The claim of national rivalry also belongs to the misconceptions that could be explained by the 1921 debate. Thus, Fleck never mentions that the Wassermann

reaction, in being an application of the complement binding method based on French science, speaks against that claim. The question of national rivalry and its adverse effect on science could rather be applied to the period following World War I, when the Wassermann reaction was already in use. Wassermann refers to the issue in the 1921 debate. He reminds Bruck of the now regrettably changed regulations for German research as a result of the post-war conditions. These were accompanied by a substantially impaired economy and restricted scientific openness.

Further, Fleck's impressive though sometimes somewhat tediously detailed enumeration of names in his two case histories can, besides the 1924 handbook, easily be traced to the two articles by Schuberg and Schlossberger (1930) and Plaut (1931), in which the authors give their account of memorable events in the history of German science worthy of celebration (cf. GDSF, 189). Conforming to the inherent rhetoric, the introductory or concluding parts of these articles contain, as an important part, the meticulous recollection of names. The impressive number of names given by Fleck on the Wassermann reaction (GDSF, 80), and partly on syphilis, is easily traced to these sources. It has, however, been interpreted as supporting his great familiarity with his subject, or alternatively as part of esoteric factuality (GDSF, 16). Moreover, the concluding part of Plaut's article seems to be the source of the most quoted statement of the monograph – about false assumptions leading to scientific facts, alluded to above, though taken out of context by Fleck. The statement refers to one of Bruck's earlier accusations regarding Wassermann to be found in the 1921 debate, and the rejoinder by Wassermann.

Finally, the perseverance in assigning the Wassermann name to the Wassermann reaction that so deeply troubled Bruck was, as was also explained by Wassermann in one of his many rejoinders, a mere result of history. He himself did nothing to bring it about.

### **Science of the Past**

A full account of all the articles from 1906, and those from the 1921 battle not referred to by Fleck, would lead too far. Suffice it to say that the problems of the theoretical justification of the Wassermann reaction were immediately perceived, addressed, and formulated with impressive clarity by those involved, including Wassermann, thereby forming the basis of great parts of the research into syphilis in ensuing years. Further, the prospect of designing a test for the diagnosis of syphilis rapidly attracted researchers including clinicians of varying ability working outside the great institutions. The inevitable result of all these smaller changes or idiosyncratic interpretations of the initial outline of the Wassermann reaction was to make it not only useless but, more importantly, also dangerous in its application, whether the changes undertaken implied too high or too low sensitivity or specificity of the reaction. The risk of an indiscriminate application of the Wassermann reaction was, however, recognized early. This called for regulations and its performance was restricted to acknowledged centres of expertise, all related by Bruck in his 1909 and also his 1924 writings. Although this is alluded to by Fleck (GDSF, 53), his later reference to the issue becomes misleading in seemingly

asserting that it could not always be concluded whether the tests were positive or negative (GDSF, 86). That statement has, however, been perpetuated by interpreters furthering a relativistic conception of science (Schäfer 1993). If the demand for authorization in any way affected Fleck, whether in his private laboratory or elsewhere during these years, is, however, unknown. Whatever the case may be, designing a test diagnosing syphilis, whether based on the complement binding reaction or not, was in fashion. The overwhelming number of references given by Bruck in his 1909 writings amply evidences that fact and also that the Wassermann reaction, despite its well-known shortcomings including its deficient specificity, remained unchallenged in clinical applicability for many years to come.

An impressive message conveyed by the early articles is, however, the clear exposition of the scientific method at work, exemplifying the intimate reciprocity between theory, method, and experimental data and the importance of the scientific vocabulary. Whether it is possible to communicate that vocabulary is a seminal issue both addressed and illustrated by Fleck's monograph. Of great interest is also the exposition of the difference between basic and applied science, whether relating to simplicity or change of theory, fact, or method. By not separating or apprehending often opposing issues relating to these two fields, Fleck's monograph remains contradictory. Further, the various statements easily invite relativistic interpretations.

### **Experimental Rage or the Question of Impetus**

A frequent misapprehension based on Fleck's narrative is the view that tuberculosis, although at that time claiming many more victims than syphilis, was never made the object of elaborate research, as it was a "romantic" but not "dishonouring" disease (Cohen and Schnelle, xxv; see also GDSF, 77). It is true that tuberculosis when affecting main characters in opera librettos or in novels in some sense has been portrayed as "romantic." In the real world it has, however, always been viewed as highly dishonouring, treacherously contagious, running in families, and inescapably linked to poverty, bad housing, poor nutrition, and premature death. That being the case it has, since the discovery of the tuberculosis bacillus by Koch in 1882, been the object of the most extensive research efforts imaginable and, not surprisingly, in his 1921 paper on syphilis, Wassermann characterized tuberculosis as the most important epidemic in the world followed by syphilis as the second.

It is true that the number of papers on syphilis during the decade following 1906 surpassed the number on tuberculosis. The reverse was, however, as obviously true when Fleck was writing his monograph, amply evidenced by the medical journals of the time. His apparent neglect could be explained by the fact that a workable serologic test for tuberculosis was never accomplished. Attempts were indeed made, in parallel with the work on the Wassermann reaction and often by the same researchers including Wassermann, but were soon abandoned due to lack of success. Other approaches were more successful, including microscopy of the sputum, culture of the causative organism, chest x-rays, and the tuberculin skin test – all outside the concern of Fleck.

The above relates not only to the question of sources but also to the question of Fleck's possible impetus in writing his monograph. The latter question has, as already alluded to, so far attracted little interest except for the writings of Schnelle, the article by Bonah (2002), and the one by Freudenthal and Löwy (1988).

In his 2002 article Bonah attempts to link Fleck's monograph to the tuberculosis vaccination scandal in Lübeck that was extensively discussed in the German press in 1930, as was its ensuing court procedure – the two physicians in charge were convicted. Bonah postulates Fleck's awareness of and deep commitment to the debate, although he correctly admits that Fleck's writings never refer to the event and but briefly touch on tuberculosis. According to Bonah the debate epitomized a perceived crisis in German society and the scientific community with manifold ethical, political, epistemological, and metaphysical implications. He names a few such crises, such as that of the economy, of science in general, and medicine in particular as well as bacteriology (cf. Borck 2004, 449–450). By extrapolating from the title of Fleck's 1929 article "On the Crisis of Reality" reprinted in CaF (pp 47–58), Bonah further maintains that Fleck was influenced by the moral and scientific implications of the events in Lübeck. Without any support for his claim, Bonah asserts, however, that the latter events constituted the impetus of Fleck's writing of the monograph at that same time.

According to Bonah's extensive arguments, the scientific debate envisioned three scenarios in the attempt to explain the tragic death of 77 infants following vaccination. In the first, the outcome was to be explained by medical experimentation with an implication of immoral conduct referred to as "experimental rage." The second scenario, favoured by the experts in the field, explained the outcome as an accident "with loose scientific practice responsible for exchange or contamination of vaccination cultures in the Lübeck laboratory." The third scenario also viewed the outcome as an accident, but saw it as the consequence of "accidental and unpredictable change occurring spontaneously in the bacteria itself: bacterial variability." Although at the time, according to Bonah, not compatible with the prevailing thought style, "this assumption contradicted a key-scientific dogma of bacteriology, the Koch-Pasteur concept of 'virus fixe', which asserted that an attenuated strain could not return to its initial pathogenicity" (2002, 200). As Bonah rightly stresses, bacterial variability was one of Fleck's interests in the 1930s. Fleck worked with the proteus bacillus, *Proteus mirabilis*, which is of importance for the serological diagnosis of the rickettsia strain that causes typhus, also referred to in the monograph. What Bonah does not mention however, is that although in both cases related to bacteria, the variability in these two cases was not the same. Genetic variability between the species was cherished by Fleck but had long since been denied by the esoteric circle. Functional variability within the species, at stake in the case of the vaccination, was at the time a well-known phenomenon and, as already mentioned, also referred to by Hirschfeld in his 1931 article.

Assuming that the court procedure in 1930 was the impetus for Fleck's writings, Bonah extrapolates the views expressed by the experts on whom the court had to rely to Fleck's notions of "thought collective" and "harmony of illusions." According to Bonah, what was exposed in the court procedure exemplified "the crisis in bacteriology" allegedly dealt with in Fleck's monograph (GDSF, 29). Through parallel reading

of selected sources on the Lübeck disaster and Fleck's monograph, Bonah finds implicit support underpinning his tenet that the catastrophe not only was the impetus for Fleck's monograph but also deeply influenced its content. The latter claim is based on innumerable purported disguises in the text, which, according to Bonah, are easily deciphered by any reader acquainted with the foregoing events in Lübeck.

Without denying the reasonableness of Fleck's acquaintance with the catastrophe in Lübeck, another scenario could be advanced, implying a different impetus for Fleck's writings based on and linked to his proper sources. According to his references it seems reasonable to assume that he became attracted to his subject around 1930. At that time, the period of ground-breaking discoveries within bacteriology was already over, as was the glorious harvest time, all now transformed into history. In other words, the eminent period of the past had become one of memorial celebration. The icons of the early days were numerous, including its Nobel laureates such as Koch, Ehrlich, and Bordet. The German press, not least the medical press, observed the anniversary of the discovery of *Spirochaeta pallida* by Schaudinn more than 25 years previously. The same applied to the 25th anniversary of the seminal paper by Wassermann on the Wassermann reaction. The Nobel Prize awarded to Landsteiner in 1930 reinforced the concept of specificity and Plaut, one of Wassermann's many assistants in the early days, participated by deliberating on the theoretical exposition of the Wassermann reaction (cf. GDSF 66, 73, 75). In contrast, scientific work on syphilis and the Wassermann reaction was, as already mentioned, off the front, as was serology, the latter being Fleck's main concern and the basis of his living.

Based on the footnotes in Fleck's monograph, it seems reasonable to assume that he had at least some access to German medical journals from the years in question (1930 and 1931). Browsing through the issues which include the papers referred to by Fleck, others emerge that could, or should, have caught his interest. Whether the article by Lange, published in 1930 in *Klinische Wochenschrift* on the vaccination catastrophe in Lübeck, belongs to these remains an open question. It could, however, be assumed that Fleck should have been aware of the event as it centred on questions in bacteriology.

Bonah mentions Lange's name but in passing, not quoting any of his writings. Lange's mission is, however, explained in the article in *Klinische Wochenschrift* that appeared three months after the vaccination was initiated in March 1930. Lange, a well-known professor of bacteriology in Berlin, was one of the state-ordered investigators sent to Lübeck to scrutinize all the steps of the vaccination procedure employed, to notify existing, as well as non-existing, protocols, and to interview all the personnel involved. The article is a review of the results obtained, published as an urgent attempt to keep the medical community as well as lay people informed. Lange clearly states that the final answer to the tragedy was still awaited, but also that it was highly questionable whether it would ever be reached.

Lange's paper is a meticulous, highly informative outline of the events. The strength of his paper is increased by his early and open disclosure of some undocumented changes made to the strict protocol that should have been adhered to, including failure to perform some of the controls. It was also revealed that the cultures provided by Calmette at the Pasteur Institute in Paris and used as a basis of the vaccine had, at the



time of the inspection, been destroyed by those involved. This was probably an act of desperation, unfortunate and inexcusable, but in a sense understandable considering the severity and the extent of the tragedy. The punishment of those responsible was, according to Lange, already imposed and could never be surpassed by any court procedure to come. It was, however, verified that in Lübeck the cultures had not been kept according to the protocol.

The lesson that could be learned from the tragic event sounds like an echo of Citron's sincere precepts to his students in 1910, inserted in the third chapter of Fleck's monograph (55–59). The strict adherence to established procedures and the elaborate controls have been scornfully depicted by Fleck scholars as “all too elaborate” (Schäfer 1993, 170). Fleck's interpretation of Citron's precepts in the third and fourth chapters is, as has already been pointed out, contradictory, articulating relativistic as well as non-relativistic views. What can also be concluded, in retrospect, is, however, that the catastrophe in Lübeck stands out as an isolated event. The vaccination procedure had already been successfully applied in several countries and continued to be applied even more extensively. Further, the culture of attenuated tuberculosis bacilli provided by Calmette is still in use as, in some countries, is the oral route of vaccination administration employed in Lübeck in 1930.

### **A Reasonable Scenario**

The above reinforces the impression of Fleck as a marginalized professional out of touch with his time and his science. Interpreters mainly focusing on the scientific content of his writings have reached the same conclusion, although this has been essentially overlooked (see Löwy 1986; Moulin 1986; Sadegh-zadeh 1981; Zalc 1986; Lie 1993; Tsouyopoulos 1993). The paradox of the predominant interpretations of Fleck is evidenced when his incontestable marginality is viewed as not implying any conclusions concerning his abilities to pursue research. It has rather transformed into a deep-rooted myth of Fleck as an admittedly somewhat peripheral but still important figure in academic life and science, deplorably and inexplicably neglected by his contemporaries.

Returning to the years of 1930 and 1931: celebrating as they did past achievements in bacteriology, Fleck might have been reminded of the fierce debate in 1921 between the former research assistant Bruck and the long established, now deceased, but still celebrated Professor Wassermann. This assumption is by no means far-fetched as it coincides with his own, abruptly terminated, research experience when he, himself, was working as a research assistant. The assumption receives further support from the fact that the above-mentioned articles from 1930 and 1931 and the ones from 1921 are among the very few he not only quotes, but also uses as the basis of his narrative.

Without consulting, or rather without having access to, the sources of 1906, and reading the few sources he used in a highly selective way, Fleck has his story. Considering the selectivity of Fleck's reading, the result of his pains in the strikingly sublime narrative of the third chapter of his monograph is impressive.

This is not to be viewed as petty morality or as a late-coming revenge. Rather, it could have served as an adjunct to another more urgently needed aim – being recognized as

a person well acquainted with serology, depicted as a field of sophisticated science of high clinical importance. There is ample evidence throughout the monograph of Fleck's presenting serology as a glorious pursuit, thereby emphasizing his own intimate knowledge of a clinically highly important field when it, at the time of his writing, paraphrasing Kuhn, was already transformed into normal science (cf. Peterson 1936). Long since had other, more rewarding subjects surpassed it as objects of intense research activity.

In the very first paragraph of the third chapter, Fleck depicts the Wassermann reaction as a complex, extremely rich field but also as one related to "many branches of chemistry, physical chemistry, pathology and physiology" (GDSF, 82). These were the fields of current research interest in the early 1930s that, although closed to Fleck, were fully exposed in the various medical journals. Fleck's frequent references to his own research experiences have, without any other support, been viewed as evidencing his scientific prominence (cf. Hacking 1999, 60). According to the above scenario, the neglect of his contemporaries becomes explicable. Within the esoteric circles of the time, whether in science, philosophy, or history of medicine, he could but be viewed as an amateur (cf. Giedymin 1986, 186). Moreover, Fleck's idiosyncratic reading of his medical sources should have been noticed and easily repudiated by the same esoteric circles. Their silence could thus be viewed as an adequate reaction and their neglect legitimate. Wettersten has already suggested the latter though from a different perspective (1991, 480).

Lastly, the form Fleck chose for his writing could be viewed as based on exclusion. His wanting scientific background and peripheral position far outside the academic world did not permit an open plea for serology. His delivery, however, accords with the medical journals of the years in question; they contained numerous philosophically-oriented review articles on the footing of medicine as a science. Stated differently, the philosophy of medicine, being a perennial subject, was never out of fashion. The article by Hirszfeld is but one exemplifying this type of writing. Fleck's interest in philosophy was clearly genuine, although he was an amateur. Hence, the monograph can be interpreted as the parallel pursuit of two mutually disguising agendas. One was the need for professional recognition, quite naturally including recognition by Professor Hirszfeld, later to be remembered for his work organizing anti-epidemic measures and vaccination campaigns against typhus in the Warsaw ghetto, and also, after the war, reappearing in the life of Fleck (cf. above; Schnelle 1986b, 29; see also Hedfors submitted for publication). The other was the concealment of wanting knowledge of philosophy, history of medicine, and sociology of science. This combination creates an unavoidable tension in the monograph, where purported relativistic and clearly non-relativistic statements coexist. Once acknowledged, many of the conspicuous contradictions can be reread with the two opposing projects in mind. However, the ensuing interpretation differs markedly from the received interpretation of Fleck (cf. Sismondo 2004, 53; Hedfors 2006b). Further, the history of the Wassermann reaction, and syphilis, could still be told, including that of its founding fathers of serology, Bordet, Gengou, Widal, and Wassermann. More importantly, Fleck's monograph raises intricate epistemological questions. Although the modern construction of Fleck as a prescient authoritative

ally of philosophers, historians, or sociologists of science could be challenged, the possible construction of Fleck as an early artful adversary of the adherents of those same disciplines deserves further study.

## References

- Andersen, J. 2002. The role of subject literature in scholarly communication. *Journal of Documentation* 58: 463–81.
- Baldamus, W. 1976. *The structure of sociological inference*. London: Martin Robinson.
- Baldamus, W. 1977. Ludwik and the development of the sociology of science. In *Human figurations: Essays for Norbert Elias*, publ by P. R. Gleichmann, J. Goudsblum, and H. Korte. Amsterdam.
- Bazerman, C. 1988. *Shaping written knowledge: The genre and activity of the experimental article in science*. Madison: University of Wisconsin Press.
- Belt, H. van den. 1997. Spirochaetes, serology, and Salvarsan: Ludwik Fleck and the construction of medical knowledge about syphilis. Ph.D. Thesis, Landbouwniversiteit Wageningen, The Netherlands.
- Bloor, D. 1986. Some determinants of cognitive style in science. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Bonah, C. 2002. Experimental rage: The development of medical ethics and the genesis of scientific facts. Ludwik Fleck: An answer to the crisis of modern medicine in interwar Germany? Society for the Social History of Medicine Millennium Prize Essay 2000. *Social History of Medicine* 15: 187–207.
- Bruck, C. 1921. Zur Geschichte der Serodiagnostik der Syphilis. *Berliner Klinische Wochenschrift* 58: 580–81.
- Bruck, C. 1924. *Handbuch der Serodiagnose der Syphilis*. 2nd ed. Berlin: Springer.
- Citron, J. 1910. *Die Methoden der Immundiagnostik und Immunotherapie und ihre praktische Verwertung*. Leipzig: Thieme Verlag.
- Cohen, R. S., and T. Schnelle. 1986a. Introduction. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Cohen, R. S., and T. Schnelle, eds. 1986b. *Cognition and fact: Material on Ludwik Fleck*. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Elkana, Y. (1986. Is there a distinction between external and internal sociology of science? (Commentary on a paper by John Ziman). In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Fleck, L. 1929. Zur Krise der Wirklichkeit. *Naturwissenschaften* 18: 425–30.
- Fleck, L. 1979. *Genesis and development of a scientific fact*. Translated from the German by F. Bradley and T. Trenn and edited by T. Trenn and R. Merton. Chicago: University of Chicago Press.
- Fleck, L. 1980. *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, edited by L. Schäfer and T. Schnelle. Suhrkamp.
- Freudenthal, G. and I. Löwy, I. 1988. Ludwik Fleck's roles in society: A case study using Joseph Ben-David's paradigm for a sociology of knowledge. *Social Studies of Science* 18: 625–51.
- Giedymin, J. 1986. Polish philosophy in the inter-war period and Ludwik Fleck's theory of thought-styles and thought-collectives. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Goldman Cedarbaum, D. 1983. Paradigms. *Studies In History and Philosophy of Science Part A* 14: 173–213.
- Hacking, I. 1999. *The social construction of what*. Cambridge, Mass.: Harvard University Press.

- Harwood, J. 1986. Ludwik Fleck and the sociology of knowledge. *Social Studies of Science* 16: 173–87.
- Hedfors, E. 2006a. The reading of scientific texts: Questions on interpretation and evaluation, with special reference to the scientific writings of Ludwik Fleck. Forthcoming in *Stud Hist Phil Sci*.
- Hedfors, E. 2006b. Fleck in context. Forthcoming in *Perspectives on Science*.
- Hedfors, E. 2006c. *Medical science in the light of the Holocaust: departing from a postwar article by Ludwik Fleck*. Submitted for publication.
- Hirszfeld, L. 1931. Prolegomena zur Immunitätslehre. *Klinische Wochenschrift* 10: 2153–59.
- Infeld, L. 1934. *The world in science: Matter and quanta*. London: Viktor Gollancz Ltd.
- Kuhn, T. S. 1962. *The structure of scientific revolutions*. Chicago: University of Chicago Press.
- Kuhn, T. S. 1979. Foreword. In Ludwik Fleck, *Genesis and development of a scientific fact*. Translated from the German by F. Bradley and T. Trenn and edited by T. Trenn and R. Merton. Chicago: University of Chicago Press.
- Lange, C. 1921. Entgegnung auf A.v.Wassermanns modifizierte Lipoidhypothese. *Berliner Klinische Wochenschrift* 14: 330–31.
- Lange, L. 1930. Die Tuberkuloseschutzimpfungen in Lübeck. *Klinische Wochenschrift* 24: 1105–108.
- Lie, K. R. 1993. Ludwik Fleck and the philosophy of medicine: A commentary on Schäfer and Thouyopoulos. In *Science, technology and the art of medicine*, edited by C. Delkeskamp-Hayes, and G. Cutler. Vol. 44 of *Philosophy of medicine*. The Netherlands: Kluwer Academic Publisher.
- Lindenmann, J. 2001. Siegel, Schaudinn, Fleck and the etiology of syphilis: A response to Henk van den Belt. Discussion. *Studies in History and Philosophy of Biological and Biomedical Sciences* 32: 435–455.
- Löwy, I. 1986. The epistemology of the science of an epistemologist of the sciences: Ludwik Fleck's professional outlook and its relationship to his philosophical works. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Löwy, I. 1990. Ludwik Fleck: From philosophy of medicine to constructivist and relativist epistemology. In *The Polish school of philosophy and medicine from Tytus Chalubinski (1820–1889) to Ludwik Fleck (1896–1961)*. Vol. 37 of *Philosophy and medicine*. Dordrecht: Kluwer Academic Publishers.
- Löwy, I. 2004. 'A river that is cutting its own bed': The serology of syphilis between laboratory, society and the law. *Studies in History and Philosophy of Biological and Biomedical Sciences* 35: 509–24.
- Moulin, A.-M. 1986. Fleck's style. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Peterson, H. 1936. Referat: Ludwik Flecks Lehre vom Denkstil und dem Denkkollektiv. *Klinische Wochenschrift* 7: 239–42.
- Plaut, F. 1931. Die theoretische Begründung der Wassermannschen Reaktion. *Münchener Medizinische Wochenschrift* 78: 1461–63.
- Sadegh-zadeh, K. 1981. World 5 and medical knowledge. *Journal of Medicine and Philosophy* 6: 163–270.
- Schäfer, L. 1993. On the scientific status of medical research: Case study and interpretation according to Ludwik Fleck. In *Science, technology and the art of medicine*, edited by C. Delkeskamp-Hayes and G. Cutler. Vol. 44 of *Philosophy of medicine*. The Netherlands: Kluwer Academic Publisher.
- Schäfer, L., and T. Schnelle, eds. 1980. Einleitung. Ludwik Flecks Begründung der soziologischen Betrachtungsweise in der Wissenschaftstheorie, VII–XLIX. *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*. Frankfurt am Main: Suhrkamp Verlag.
- Schäfer, L., and T. Schnelle, eds. 1983. *Ludwik Fleck. Erfahrung und Tatsache. Gesammelte Aufsätze. Mit einer Einleitung herausgegeben von Lothar Schäfer und Thomas Schnelle*. Frankfurt am Main: Suhrkamp Verlag. Germany.

- Schnelle, T. 1982. Ludwik Fleck-Leben und Denken. Zur Entstehung und Entwicklung des soziologischen Denkstils in der Wissenschaftsphilosophie. Thesis, Hochschule Verlag, Freiburg.
- Schnelle, T. 1986a. Ludwik Fleck and the influence of the philosophy of Lwów. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Schnelle, T. 1986b. Microbiology and philosophy of science. Lwów and the German Holocaust: Stations of a life – Ludwik Fleck 1896–1961. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.
- Schuberg, A., and Schlossberger, H. 1930. Zum 25. Jahrestag der Entdeckung der *Spirochaeta pallida*. *Klinische Wochenschrift* 9: 582–86.
- Simmons, L. 1991. What incommensurability claims mean: A study of Ludwik Fleck's contribution to the incommensurability debate. Dissertation, Notre Dame University, Indiana.
- Sismondo, S. 2004. *An introduction to science and technology studies*. USA: Blackwell Publishing.
- Trenn, T. 1979. Preface; also Biographical sketch and annotation. In L. Fleck, *Genesis and development of a scientific fact*. Translated from the German by F. Bradley and T. Trenn and edited by T. Trenn and R. Merton. Chicago: University of Chicago Press.
- Trenn, T. 1981. Ludwik Fleck 'On the question of the foundation of medical knowledge'. *Journal of Medicine and Philosophy* 6: 237–56.
- Tsouyopoulos, N. 1993. The scientific status of medical research. A reply to Schäfer. In *Science, technology and the art of medicine*, edited by Delkeskamp-Hayes and G. Cutler. Vol. 44 of *Philosophy of medicine*. Kluwer Academic Publisher.
- Wassermann, A. von. 1921a. Neue experimentelle Forschungen über Syphilis. *Berliner Klinische Wochenschrift* 58: 193–97.
- Wassermann, A. von. 1921b. Zur Geschichte der Serodiagnostik der Syphilis. *Berliner Klinische Wochenschrift* 58: 1194–95.
- Wassermann, A. von. 1921c. Schlusswort auf die Ausführungen in Nr. 22 dieser Wochenschrift von Carl Bruck von, August von Wassermann. *Berliner Klinische Wochenschrift* 58: 888–90.
- Wassermann, A. von, A. Neisser, and C. Bruck. 1906. Eine serodiagnostische Reaktion bei Syphilis. *Deutsche Medizinische Wochenschrift* 32: 745–46.
- Wassermann, A. von, A. Neisser, C. Bruck, and A. Schucht. 1906. Weitere Mitteilungen über den Nachweis spezifisch luetischer Substanzen durch Komplementverankerung. *Zeitschrift für Hygiene und Infektionskrankheiten* 55: 451–77.
- Weil, E. 1921. Das Problem der Serologie der Lues in der Darstellung Wassermanns. *Berliner Klinische Wochenschrift* 58: 966–70.
- Wettersten, J. 1991. The Fleck affair: Fashions v. heritage. *Inquiry* 34: 475–98.
- Zalc, B. 1986. Some comments on Fleck's interpretation of the Bordet-Wassermann reaction in view of present biochemical knowledge. In *Cognition and fact: Material on Ludwik Fleck*, edited by R. S. Cohen and T. Schnelle. Vol. 87 of *Boston studies in the philosophy of science*. Holland: D. Reidel Publishing.