

LUDWIK FLECK AND HIS CONCEPT OF A SCIENTIFIC FACT

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# LUDWIK FLECK AND HIS CONCEPT OF A SCIENTIFIC FACT

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In this thesis, I explore the concept of a scientific fact through the work of Polish physician Ludwik Fleck (1896-1961). Fleck has had an alleged influence upon contemporary philosophy of science, primarily through the work of Thomas Kuhn with subsequent echoes from the direction of the sociology of science. Most writers, however, have restricted their focus upon only one of Fleck's publications, *The Genesis and Development of a Scientific Fact* (1935), and have highlighted the Polish philosophical community as a primary influence upon Fleck's ideas. I argue in this thesis that since Fleck was a doctor by trade, his views must be understood in the context of the medical issues and philosophy of medicine of his time. Furthermore, in order to appreciate Fleck's concept of a scientific fact, one must turn to his other philosophical works. A more judicious picture of Fleck and his concept of a scientific fact is provided by this analysis even though, I conclude, his approach remains philosophically unsatisfactory.

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## Chapter 1

### Fleck and his Context

#### *A. Introduction*

Consider the following situation: it is 1943 in Buchenwald, Germany. Ludwik Fleck, a bacteriologist and Polish Jew, has just been deported from Auschwitz to work in the Hygiene Institute of the SS under the direction of Dr. Ding-Schuler. In Block 50, Fleck works in the company of a compendium of doctors, chemists, bacteriologists, and serologists drawn from the range of the Nazi regime. Ding-Schuler's orders are to manufacture a serum for the disease typhus, which will be shipped to immunize SS soldiers at the front. Fellow infected prisoners quarantined in Block 46 are left to die.

Isolating the germ (*Rickettsia prowazekii*) causing typhus is difficult; its defining characteristics are uncertain. Pressure is increased to produce large quantities of the serum. It is eventually found that only a small amount of an effective serum can be made, which Fleck and his co-workers use to inoculate themselves and as many prisoners as possible. Larger quantities of an innocuous vaccine are provided to placate the authorities. Upon the word of the medical experts, the veracity of the germ isolation and effectiveness of the vaccine is accepted. News of the germ's detection spreads through the camp, and belief in the discovery gains strength. With the help of Eugen Kogon, another prisoner and Ding-Schuler's secretary, effective samples of the serum and the methodology used for its isolation are sent for external verification. The director is honored for his results. The "reality" of the specialists' discovery becomes more established.

An official tissue sample with the "real" typhus germ stained creates some disturbance when received by the Buchenwald workers. The new stain indicating the presence of the typhus-causing germ is substantially different

from that which they had been using before.<sup>1</sup> However, the procedures they have been using are strongly ingrained, and with no further conflicts they smooth over the anomalous stains. In the period of one and a half years, roughly 600 liters of placebo are shipped to the troops; 6 liters of effective serum are manufactured.

This story, compiled from the work of Thomas Schnelle (1986b), Gerald Weissmann (1980), and Ludwik Fleck (1947a) not only tells a true story, but exemplifies many aspects of Fleck's conception of reality and of facts as well. A naive realist can postulate facts in a fixed world independent of human actions and intentionality. In contrast, Fleck contextualizes facts and the reality to which they contribute within the realm of human social interaction and within the historical development of ideas.

The brief narrative above illustrates how Fleck understands the concept of a fact. It also provides a general outline for the later interpretation of Fleck's writings. By my reading, Fleck takes a fact to be

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<sup>1</sup>In 1947, Fleck writes:

The [Buchenwald] collective worked on complex problems from the field of typhus; they had at their disposal fully equipped laboratories, plenty of experimental animals and an extensive specialist literature. ... [After a time, the experimenters find an unusual marking in their dyes of *Rickettsias*, but [i]n truth, they [the dyes] were the eosinophilic grains from the rabbits' leukocytes, as I found later. However, among the collective which was thirsting for a positive result, the news was spread: at long last, *Rickettsia* has been found in the preparations obtained from rabbits' lungs (Fleck 1947a, 118-120).

Schnelle quotes from Kogon (I believe; Schnelle provides no reference at the end of the quote) further on the episode:

When Dr. Ludwig Fleck came to Block 50 at Buchenwald he told us, after having seen the typhus germs which we had produced from the lungs of rabbits, that we did not have *Rickettsias* but another type of germ. We asked him not to convey this to Ding[-Schuler] but to experiment with us to help us get out of this difficulty. During the two years that he worked with us Dr. Fleck kept the secret. It was only when the Cracow Institute furnished us with mice lungs and with infected mice intestines that we were able to be sure that our animal material contained *Rickettsia Prowazecki*; after this we produced a vaccine which was without any doubt very effective, but we could only produce small quantities of it (Schnelle 1986b, 26-27).

In either event, either mistakenly (as in the first quote) or intentionally (as in the second), the workers in Block 50 were producing a serum derived from the wrong basis. Fleck, coming late onto the scene, was able to detect this, and kept the fiasco a secret from the Germans.

a relation of concepts (the typhus disease to the germ form isolated). That is, although Fleck frequently speaks of things in the world (e.g., the disease typhus), this is understood throughout his work in a qualified sense where what *is* is equivalent to *what is known*, what is cognized, understood, or conceptualized. Thus "the disease syphilis" is read "the disease syphilis as it is perceived to be/is conceptualized by the members of society S at time *t*."

Fleck captures the apparent objectivity or certainty of a fact with the use of other social mechanisms. A fact *seems* independent because the conceptualization is developed through

- (i) a process of social reinforcement (by the relevant experts, bureaucratic authority, widespread popular support),
- and
- (ii) the connection of that conceptual relation with other systems of accepted knowledge (the veracity of the Buchenwald workers' discovery facilitated by a high degree of expectancy and the relative isolation of the concentration camp from other institutions).

Fleck began publishing material of interest to philosophers of science a little over 60 years ago.<sup>2</sup> Cited as a founder of the growing field of the sociology of science,<sup>3</sup> and an acknowledged influence upon Thomas Kuhn,<sup>4</sup> his work has drawn increasing attention.<sup>5</sup> However, investigations of and expansions upon his work are frequently from sociological perspectives, and are largely

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<sup>2</sup>Ludwik Fleck (1927), "Some Specific Features of the Medical Way of Thinking."

<sup>3</sup>See Ilana Löwy (1989) esp. 39-40, (1988a) and (1988b); Gad Freudenthal and Ilana Löwy (1988), Struan Jacobs (1987), W. Baldamus (1977), John Wettersten (1991), and Jonathan Harwood (1986).

<sup>4</sup>See Thomas Kuhn's forward to *Genesis and Development of a Scientific Fact*, pp. vii-xi, and his preface to *The Structure of Scientific Revolutions*, pp. vi-vii

<sup>5</sup>This is attested to by the publications listed in fn. 6 (above), and by others concentrating upon Fleck's work. Additionally, in "History of Science and its Sociological Reconstructions," Steven Shapin notes that in the then passing decade a trend in historical and sociological investigations had emerged looking as though it were influenced by Fleck, even though his work is not mentioned (Shapin 1986, 325). Shapin's article is an analysis of trends in the history of social and historical scholarship, a version of an essay published in *History of Science* 20 (1982), hence is not an extrapolation of Fleck's writings, even though included in the Cohen and Schnelle (1986) volume on topic.



restricted to Fleck's (1935) *Genesis and Development of a Scientific Fact* (hereafter: "the *Genesis*"). Articles that do venture into the philosophical largely focus upon the Lwów-Warsaw philosophical community in Fleck's time rather than the strengths and weaknesses of Fleck's work itself. In explicating similarities and differences between Fleck's work and those of his peers, these investigations are primarily historical, with little in the way of a philosophically proposal useful for today. Perhaps Fleck's views suggest a beneficial way to evaluate scientific knowledge and activity, as well as understand that toward which these enterprises are aimed. But this would be revealed more by subjecting his approach to a rational analysis, than by treating his claims as historical events.

The goal of this thesis, then, is twofold. First, I shall restrict this investigation to Fleck's concept of a scientific fact. One point of contention among Fleck's commentators is whether his position entails a social constructivist interpretation. I believe that such an interpretation is warranted by Fleck's texts, and I will take his arguments in works other than the *Genesis* in order to demonstrate this. The second goal is to provide a philosophical examination of Fleck's arguments. My critical analysis shows that Fleck's views are not rigorously thought out. Hence, insofar as a philosophical perspective strains for clarity and consistency, Fleck has little to offer, and any useful philosophical contribution should come primarily from modifications upon what he has written.

But Fleck's lack of conceptual rigor is not surprising since Fleck was trained as a physician, not a philosopher. And in fact, to the degree historical philosophical work is confined to seeking the connections between Fleck's views and those held by his contemporary Polish logicians and philosophers of language, the investigations may be misguided. That Fleck has interesting philosophical things to say is not contested. However, searching for powerful intellectual links between Fleck and Polish philosophers overlooks the

influence that the medical profession and Polish philosophy of medicine must have had on Fleck. I will emphasize the latter connection in this thesis.

I look at the medical and philosophical context of Fleck's time in the next section of this chapter as I discuss his cultural and intellectual environment. I do this with the aim of creating a fuller picture of his views so that his place within the philosophy of science may be better understood. Since most responses to Fleck's work have taken the *Genesis* as the touchstone of his position, in the second section of the first chapter I will examine what he wrote there. In the second chapter I present other scholars' interpretations of Fleck's work, and evaluate these commentaries on the basis of the *Genesis*. His work has received conflicting interpretations, largely because those analyses proceeded without the resources provided by later translations of his other works.<sup>6</sup> The third chapter will draw upon the resources provided by Fleck's other sources.<sup>7</sup> From this final standpoint, I will point out misinterpretations and elaborate further on where interpretations are correct.

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<sup>6</sup>Not including those included in the Cohen and Schnelle (1986) Fleck volume, 14 of the main sources used in this thesis rank as follows:

Baldamus.....	1977	Löwy (a).....	1988
Trenn/Sadeh.....	1981	Löwy (b).....	1988
Wittich.....	1981	Freudenthal/Löwy.....	1988
Merton.....	1983	Löwy.....	1989
Andersson.....	1984	den Belt/Gremmen.....	1990*
Harwood.....	1986	Wettersten.....	1991
Jacobs.....	1987*	White.....	1991

Dates indicated by (\*) are articles published after 1986 with no citation to Cohen and Schnelle (1986). By this small sample, over half the work (8:6) was either published before 1986, or after with sole reference to Fleck's *Genesis*. Publications such as review articles, due to their brevity, were eliminated for the comparison.

<sup>7</sup>Fleck's articles emphasize different things. For instance, his 1936 and 1929 are more and less (respectively) developed accounts of objective reality and its relation to cognizant beings. 1947a and 1935a treat the phenomenon of perceptual experience and its interpretation. 1927, 1946, and 1960 discuss the scientific process and the analysis of the activity; 1937, which is quite brief (two pages), targets the same area, but is quite unexplored. 1935b is a considerably condensed version of his 1935c.

## *B. Fleck, his contemporaries, and his life*

### *1. Early Life and Training*

As Schnelle (1986) points out, much data on Fleck's life was lost during World War II.<sup>8</sup> Indeed, one of the first to become interested in Fleck's work, W. Baldamus (one of Schnelle's teachers (introduction to Cohen and Schnelle 1986, ix)), thought he had discovered, after an initial search in 1963, that Fleck had died in a concentration camp sometime near the end of the Second World War (Baldamus 1977, 138).<sup>9</sup> This information was, as Baldamus later learned, false, since Fleck did not die until 1961 when he suffered a second heart attack in Palestine. From the data that has been collected following the effort begun by Baldamus, however, a story of his life and his influences can be pieced together.

Fleck was born in Lwów (or, Lemberg), Poland on July 11, 1896, and grew up in an Austrian-occupied territory enjoying a degree of cultural autonomy.<sup>10</sup> He graduated from the Polish *Lyceum* in 1914, conversant in both German and Polish. That year he entered the Jan Kazimierz University to study medicine, leaving with a degree as a general practitioner. He was mainly interested in serology, or microbiological research. In 1920 he became an assistant to typhus specialist Rudolf Weigl in Przemyśl, about 50 km. from Lwów.

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<sup>8</sup>The most exhaustive work on Fleck's history (in English) is Thomas Schnelle's (1986b). A large part of the present discussion is extrapolated from that. Those well-versed in German should consult Schnelle's (1982) often-cited dissertation, *Ludwik Fleck: Leben und Denken*.

<sup>9</sup>Baldamus taught Schnelle, who continued research on Fleck's history and his writings (Cohen and Schnelle 1986b, ix). Baldamus's (1977) "Ludwig Fleck and the Development of the Sociology of Science" is a precursor to the 1979 translation of the entire *Genesis* text. It consists primarily of translated sections of Fleck's book, with a minimum of interpretative work. There is also a terminological taxonomy at the end, with significant words and phrases indexed from the German text.

<sup>10</sup>In "Lwów as a Cultural and Intellectual Background of the Genesis of Fleck's Ideas," Władysław Markiewicz explains that Lwów (then Lemberg), and Galicia, of which it was the administrative capital, were able to keep their schools and language while under Austrian rule. Other Polish provinces under Russian and German occupation didn't have that much national voice (Markiewicz 1986, 225).

Weigl, according to Fruedenthal and Löwy (1988),<sup>11</sup> was a member of a small but steadfast minority in the bacteriological community. The minority group was opposed to the dominant so-called "Koch-Cohn dogma": a method in pathology which treated bacteria and every infectious disease as entities with, so to speak, necessary and sufficient conditions for identification.<sup>12</sup> In contrast, the bacteriological community Weigl associated with held that bacteria, depending upon their environment and stage of development, could assume different morphologies and physiologies. Since the laboratory environment is a scientifically standardized and artificial environment, the characteristics of bacteria studied there were likewise artificially isolated laboratory artefacts, or constructs (Freudenthal and Löwy 1988, 637).<sup>13</sup>

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<sup>11</sup>"Ludwik Fleck's Roles in Society: A Case Study Using Joseph Ben-David's Paradigm for a Sociology of Knowledge" examines the medical profession in Fleck's time. It is also an application of a sociological method to explain Fleck's motive for branching into the history and philosophy of science: satisfying his interest of being a respectable scholar in the face of professional marginalization due to the minority medical view he held. This was accomplished by the relativism his program supports, providing a way for placing his medical views on a par with the majority position. "For Fleck, we believe, this strategy was of crucial importance, for only through it could he avoid a painful dissonance between the gloryless reality of his life and the high-flying aspirations implied by his own self-image" (Freudenthal and Löwy 1988, 641). But besides seeming psychologically suspicious (how does the historical record compel any belief about Fleck's self-image?), their conclusion is not obviously strong. For surely, had Fleck renounced his minority view to enter "respectable" research in line with the prevailing methods of his time, that would have served the goal of being an acceptable microbiological researcher. Private interest does not clearly tip the explanatory (hermeneutical or otherwise) scales one way or the other. There may have been other principles that Fleck did not want to sacrifice (e.g., he thought he was truly right, and holding fast to his beliefs was more important than being successful by going with the majority), but they were not addressed in the text.

<sup>12</sup>Henk van den Belt and Bart Gremmen (1990) present the specificity doctrine as a "lock and key" or "necessary and sufficient condition" approach to disease diagnosis (den Belt and Gremmen 1990, 469). The article also characterizes the history of the medical debate in Fleck's time. Anne-Marie Moulin's (1986) is also helpful in providing material on Fleck's position in the medical debate. According to Moulin, Fleck did not hold that diseases were reducible to the discovery of a germ, a methodology Koch's school developed especially through refinements in the use of slide-staining (Moulin 1986, 408-409). Accordingly, "Fleck gave up the idea of specific elements of diagnosis and only attributed specificity to the whole," such as a "continuum of chemical molecules, a family of bodies, and finally to a sum of elements ... none of which could be strictly said to be specific" (Moulin 1986, 415-16). For a more detailed explanation of the Wassermann (or, Bordet-Wassermann) reaction, see Bernard Zalc's (1986).

<sup>13</sup>With this in mind Freudenthal and Löwy (1988) interpret Fleck's (1929) where Fleck argues that different classifications of the "same" bacteria can be equally well maintained, depending upon the classificatory purposes at hand and the state of the community in which the classification is developed (Freudenthal and Löwy 1988, 638).

His minority views notwithstanding, Weigl was appointed professor of biology at the University of Lwów in 1921. There Fleck joined him as his assistant until 1923. Either due to professional conflicts or rising ethnic tensions among the Polish, Polish-Jewish, and Ukrainian sectors of the community, Fleck left his university position. He took a job at the General City Hospital in Lwów where, according to Freudenthal and Löwy, he worked until 1928, starting in the Internal Medicine Department, later becoming head of the bacteriological and serological laboratory of the Department of Skin and Venereal Diseases (Freudenthal and Löwy 1988, 635). Schnelle writes that Fleck took six weeks in 1927 to study in Vienna with Prof. R. Kraus at the Government Institute for Serotherapy, returning to Lwów in 1928 (Schnelle 1986b, 4). Merton describes Fleck's trip to Vienna as a year-long visit, where Fleck "encountered and rejected the positivistic ideas of the Vienna Circle which were then at their peak" and he was also introduced to the work of the sociologist Wilhelm Jerusalem (Merton 1983, 187).

Fleck had his own private bacteriological/serological laboratory. The lab was established in 1923, and uninterrupted for either no time, six weeks, or a year (depending on whom you read), he conducted routine exams and research on developing an improved procedure for diagnosing syphilis, among other things. He kept this practice while working from 1928 to 1935 as head of the bacteriological laboratory of the Lwów Sick Fund.

Also in 1923 he married Ernestina Waldman and in 1924 they had a son, Ryszard. According to Schnelle's interviews with them, Fleck preferred to discuss his ideas only with those he considered to be sufficiently experienced in the topic of discussion, which ranged from philosophy, to sociology, to the history of science. The level of expertise Fleck required of his associates is, perhaps, a main reason why there are so few people living who knew Fleck (Schnelle 1986b, 4-5). As specialists, they were targetted by Nazis

and died during the Second World War.<sup>14</sup> But before the war, Lwów-Warsaw's intellectual atmosphere was characterized by strong interdisciplinary involvement, a hospitable place for someone like Fleck whose interests spanned many subjects (Schnelle 1986b, 13, cf. Markiewicz 1986).<sup>15</sup>

Fleck's (1927) "Some Specific Features of the Medical Way of Thinking," was first presented in 1926 to the Lwów Society of Amateurs of History of Medicine, a society of which Fleck was a founding member (Löwy 1989, 42). It was published a year later in *Archiwum Historji i Filozofji Medycyny*, a journal begun by Adam Wrzosek (1875-1965), who "clearly defined as one of its principle goals the development of the tradition of Polish philosophy of medicine" (Löwy 1989, 42). During this period of intense debate in the philosophy of medicine, Tytus Chalubinski (1829-1889) was an early and a main player (Löwy 1990, 655).

## 2. Medical/Philosophy of Medicine Influences

Chalubinski held a holistic view with respect to diseases and therapy which "stressed the *unique* aspect of every illness and the *conventional* character of medical classifications" (Löwy 1990, 655; emphasis Löwy's). Diseases were understood to be essentially physico-chemical events. But trying

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<sup>14</sup>The psychiatrist Jakob Frostig was the only person whom Mrs. Fleck could remember as a regular acquaintance of Fleck's. He disappeared and presumably died during the Second World War (Schnelle 1986b, 8).

<sup>15</sup>Markiewicz indicates that booksellers were frequently their own publishers which provided plenty of opportunity for writers and scientists to get their ideas into print (Markiewicz 1986, 225). Unsurprisingly, then, Lwów also had one of the largest libraries in the Polish territories, while the "university and the technical college, professorial chairs and the students' scientific circles had their own libraries; the scientific societies had their own separate collections" (Markiewicz 1986, 226). Lwów was also unusually open to the outside world, with easy access to Vienna, Budapest, and Prague. Polish scientists and artists who could not find employment, or cultural and educational organizations "threatened with liquidation" in Russian or German occupied regions frequently moved to the region of Galicia, where Lwów was located (Markiewicz 1986, 227). Fleck's homeland was then one of great ethnic diversity, with a Polish majority but strong minorities of Ukrainians, Germans, Jews and Armenians, who "found expression in the variety of cultural and intellectual life, the architecture, the specific dialect, and even the sense of humor" (Markiewicz 1986, 227).

to capture pathological phenomena by relatively simple universal laws was quite difficult to do, due to

the intrinsic complexity of living organisms and of pathological manifestations, and by their consequences, the existence of a practically unlimited number of variations which made each living organism and each pathological situation unique (Löwy 1990, 660).

One can continue to spell out the various physical and chemical instantiations of a pathological predicate indefinitely, or close off its scope arbitrarily, either way overlooking the important thing: the sick patient (Löwy 1990, 656).

Reductionism was further seen to be misguided because diseases classifications were not natural. Instead, they were the "created" images of physicians. Disease *concepts* were useful, but strictly speaking, they referred to no object that would be the subject of a reduction. Time spent trying to carry out such a reduction missed the significance of the physician's diagnostic vocabulary.

There are no two identical epidemics, nor two identical cases of disease. . . . The disease pictures are indispensable, and it is not necessary to insist on their utility. But one should not forget that while representations in natural history -- of animals, plants, and minerals -- refer to real entities, pictures of disease are abstract representations of certain phenomena separated from their natural context of individual life. (Chalubinski, translated/quoted in Löwy 1989, 42-43)

Chalubinski's goal was to find a way of systematizing the unique practice of medicine as a process unto itself, rather than just applied physics and chemistry. This he did by showing the combination of scientific knowledge necessary for medical investigations while emphasizing the process of treating patients, each with his or her own specific pathological/biological parameters (Löwy 1990, 656).

Chalubinski taught Zygmunt Kramsztyk (1948-1920), an ophthalmologist

and another active publisher in Polish philosophy of medicine. In 1897 Kramsztyk started the journal *Medical Critique* (*Krytyka Lekarska*), which for its 11 years of existence provided a forum for the philosophy and history of science and medicine.<sup>16</sup> And he elaborated upon Chalubinski's constructivist views with respect to scientific knowledge.<sup>17</sup>

For Kramsztyk, background knowledge provided an *a priori* filter through which all subsequent observations (everyday ones, scientific ones, medical ones, etc.) must pass (Löwy 1989, 45). In 1899 he wrote that an observer sees any new fact "with the eyes of his mind, which is full of previous information, of theories and prejudices, and [the observer] sees the external world in the light of these theories and prejudices" (Kramsztyk, quoted/translated in Löwy 1989, 44). The observer is "never completely passive, but in the process of observation he unconsciously transfers his general ideas and his previous information to the observed facts" (Löwy 1989, 44). Prior ideas are the materials out of which new knowledge is built. New knowledge, formulated as it is out of cognitive materials already at hand, is second-order knowledge of a sort. New concepts, then, according to Kramsztyk, refer to the prior ideas about nature, but not to nature itself (Löwy

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<sup>16</sup>On the influence of journal literature, Barbara Rosenkrantz, in her 1981 review of Fleck's *Genesis*, points to another journal which, perhaps, Fleck should have been familiar with, but there is no direct trace. Although obviously familiar with *Die Naturwissenschaften*, he presumably "was ignorant of *Kyklos*, the journal published by Henry Sigerist and his colleagues at the Institute for the History of Medicine at Leipzig from 1929 to 1932, although he does refer to Sigerist's predecessor Karl Sudhoff" (Rosenkrantz 1981, 99). Schnelle (1986a, 1986b) discusses the difficulty of tracking intellectual influences on Fleck from his bibliographies; Fleck referenced very little, and those whom Schnelle, on independent grounds, argues were essential to the development of Fleck's views, Fleck never cites at all (Schnelle 1986a, 232).

<sup>17</sup>Kramsztyk also distinguished between the uses of scientific knowledge by, say, a physicist and a physician. He out that while both a physicist and a physician will use physical knowledge, the physicist "asks 'why', and aims at understanding the relationships between natural phenomena" (Löwy 1990, 657). The doctor's aim is different, bent on solving immediate problems. Her disposition needs to be quick, confident and firm in decisions aimed alleviating a sick patient's suffering. A scientist's doubting, critical attitude is an obstacle to that task. Thus there are at least two methods of using the "same" scientific knowledge, an investigative approach and an action-centered approach, which Kramsztyk believed were normally incompatible (Löwy 1990, 657).



1989, 45).<sup>18</sup> Thus learning the relevant background information is necessary for an investigator to make his or her observations:

[T]he physician's attention is usually directed only towards the phenomena that he has been trained to see, those which he is familiar, and those which are the most frequent. We are able to perceive only familiar phenomena, because they are the most present in our mind and because we have for them a ready-made name and a ready-made theory. (Kramsztyk, quoted/translated in Löwy 1990, 665)

Wladyslaw Bieganski (1857-1917) published in much the same spirit as Kramsztyk in *Medical Critique*. In 1907 he wrote that diseases "do not exist in nature but are *constructs* generated by physicians" (Löwy 1990, 663; emphasis mine). This, he argued, is because a disease occurs through the added effects of several individual events. Since only individual entities (and not classes) exist in nature, Bieganski concluded that class or set-like events do not, strictly speaking, exist in nature. According to Löwy, this judgment held for Bieganski throughout biology:

All biological classifications, explained Bieganski, are by definition artificial constructs, because in nature there is no such thing as a class or a genus a[s] *only individuals exist*. In the classification of diseases one does not classify concrete entities such as animals or plants, but abstract 'disease images'. (Löwy 1990, 663; emphasis Löwy's)

The influence of physicians and philosophers of medicine publishing in Fleck's time surely had an effect on his ideas. Löwy writes that

[Kramsztyk's] reflections ... led him to a dynamic perception of scientific knowledge, and to the concept that the understanding of science must be anchored in its history, ideas that could have -- and in all probability did -- influence Fleck. (Löwy 1989, 43)

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<sup>18</sup>Kramsztyk points out textbook illustrations in pathology atlases as an example. The illustrations highlight what is "typical" of a certain disease, but what counts as "typical" changes over time, depending on how the disease is understood (Löwy 1989, 45).

But it is not easy to see how the lines of influence should be drawn. There is frequently a striking similarity between what Fleck and other scholars of his time wrote.<sup>19</sup> Fleck himself does not normally either quote or cite others. Yet because Fleck was first and foremost a physician, the literature within that domain should not be underestimated and overlooked. Fleck's interests are analogous to those exhibited by writers within his own profession. Perhaps his own work resulted from merging those with ideas expressed within the philosophical community.

### 3. Philosophical Influences

Schnelle (1986a) makes the case that there were three philosophers

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<sup>19</sup>There is an interesting avenue of apparent influence that cannot be pursued here. It is a stylistic similarity, provided through Löwy's 1989 article placing Fleck within a philosophy of medicine tradition. In 1929 Fleck writes that the scientific process is "an eternal, synthetic rather than analytic, never-ending labour -- eternal because it resembles that of a river that is cutting its own bed" (Fleck 1929, 54). In the *Genesis*, he writes that

The problem of how a "true" finding can arise from false assumptions, from vague first experiments, and from many errors and detours, can be clarified by a comparison. How does it come about that all rivers finally reach the sea, in spite of perhaps initially flowing in a wrong direction, taking roundabout ways, and generally meandering? There is no such thing as the *sea as such*. The area at the lowest level, the area where the waters actually collect, is merely called the sea! *Provided enough water flows in the rivers, and a field of gravity exists, all rivers must finally end up at the sea.* The field of gravity corresponds to the dominant and directing disposition, and water to the work of the entire collective. The momentary direction of each drop is not at all decisive. The result derives from the general direction of gravity (*Genesis*, 78; emphasis Fleck's).

The point of contrast comes from Löwy's (1989) translation of Kramsztyk's work, which also incorporates a comparison of scientific activity to water: "The true nature of science -- an ever-running stream -- is revealed in the history of ever-changing scientific concepts" (Löwy 1989, 46), and

Science is like a rapidly flowing river. Its beginnings, its sources are modest. Later it is joined by other sources, it modifies its shape and its direction, it becomes wider, different, ever-changing. For a scientist science always seems immobile -- for an historian it is a rapidly flowing stream. ... It is not possible to deduce the past situation of science from its present, as it is impossible to deduce the trajectory of a river from the observation of a single site, although such a site contains all the water that flows from sources above it (Löwy 1989, 47).

Granted, a similarity of analogies is not sufficient to establish concretely any connection between Fleck and Kramsztyk. But this similarity, along with their common area of interest within the philosophy of science and medicine, the proximity of their eras (Kramsztyk died 24 years after Fleck was born), and their both being part of a relatively small, geographically limited community of intellectuals interested in these topics, does lend some weight to a connection between the two. It is a connection at least as strong (if not stronger) than those suggested by Schnelle, discussed below.

who had an important impact upon Fleck: Kazimierz Twardowski (1866-1938), Kazimierz Ajdukiewicz (1890-1963), and Leon Chwistek (1884-1944). Jerzy Giedymin (1986) reconstucts the philosophical atmosphere of Lwów-Warsaw to reflect trends echoed in Fleck's work. He identifies the influnce of psychologist Franz Brentano through his student, Kazimierz Twardowski, who taught in Lwów from 1895 to 1930. This influence continued through Twardowski's students of Fleck's age: Jan Lukasicicz (1890-1963), S. Lesniewski (1886-1939), T. Kotarbinski (b. 1886), and K. Ajdukiewicz. With help from other mathematical students (such as A. Tarski (b. 1901)), in the 1920s and 30s they established what came to be called the Warsaw school of logic and philosophy (188).<sup>20</sup> Chwistek brought Poincaré and conventionalism to Fleck's attention, as did perhaps K. Ajdukiewicz. Finally, there are Edward Poznanski and Aleksander Wundheiler. They advocated a brand of semantic holism and scientific pragmatism.

Kazimierz Twardowski left Brentano and Vienna for Lwów, where he held a chair in philosophy at Lwów University from 1895 to 1930 (Giedymin 1986, 188). He tried to clarify the ontological status of objects by ridding Brentano's method of its implicit relativity (Schnelle 1986a, 236).<sup>21</sup>

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<sup>20</sup>For another brief description of the Lwów-Warsaw group, see J. Joergensen's (1939/1970), esp. 900-901.

<sup>21</sup>In: Schnelle (1986b), the relation between Fleck and Twardowski is built upon Twardowski's popularity. Schnelle writes:

For someone living in Lwów who was even only slightly interested in philosophy it was almost impossible not to come into contact with Twardowski. ... Students from all disciplines attended Twardowski's lectures, but so did academically educated auditors working outside the university. His enrollments during all his teaching years were the highest of all of the professors of Lwów (Schnelle 1986b, 15).

All the same, Schnelle concedes that there is no biographical evidence to establish this link. Yet in spite of that, because of Twardowski's popularity, Fleck's "knowledge of Twardowski's philosophy may thus be considered certain" (Schnelle 1986b, 15). Perhaps the word "certain" carries a different weight for Schnelle than it does for me; it would seem to deserve more empirical support than a reputation to bolster Schnelle's claim.

Twardowski distinguished between psychic phenomena (e.g., judgments), objects of psychic phenomena (mental representations), and objects in the world.<sup>22</sup> But his analysis was apparently restricted to only the first two objects. "[T]o be an object', indeed 'to be' at all, is identical for Twardowski to 'being the object of representations'. Thus any being independent of representations ... is rejected as impossible" (Schnelle 1986a, 237).

So while Twardowski acknowledged the concept of "objective existence" he believed that it is a level of being that a subject cannot grasp. The closest one can get to it is in affirming a statement to that effect.

Statements on whether something 'actually exists' are thus nothing more than expressions of belief -- judgments made by the subject. It is these judgments alone which attribute or deny 'actual existence' to objects." (Schnelle 1986a 237)

Twardowski's move to avoid relativism, according to Schnelle, is to confine the concept of "objective truth" within the framework an individual uses. The external world, to which a judgment must correspond, determines the truth of a judgment (Schnelle 1986a, 238). But since there is access only to mental representations of the world, the possibility of knowing whether a judgment is true is precluded.

Leon Chwistek, on the other hand, rejected the concept of a classical objective reality from the start (Schnelle 1986b, 17-18). In order to give a rational account for conflicting reality claims, he turned to developing four axiomatic schemata which would correspond to four primary types of experiential realities (one for representations, another for things, a third for natural reality and a fourth for physical reality (Schnelle 1986a, 255). Each schemata was to be internally consistent, with its own self-contained set of meanings and its own criteria for truth. But after formal logic failed as a rationality-preserving device, Chwistek adopted the undefined concept of

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<sup>22</sup>See Twardowski (1894).

"common sense" as the ground for rationality instead (Schnelle 1986a, 256).

Both Schnelle and Giedymin cite Kazimierz Ajdukiewicz as an active and influential member of the academic environment, and a sure influence upon Fleck (Schnelle 1986b, esp. 16; Giedymin 1986). Ajdukiewicz is, and was, known for his early doctrine of radical conventionalism. Like Twardowski, he supported the idea of a subject-independent reality; unlike Twardowski he understood it to be accessible through a formal-linguistic device. And like Chwistek, Ajdukiewicz thought that the meaning of terms was a product of the linguistic structure in which they were embedded (Schnelle 1986a, 248-49). He furthermore proposed a model explaining meaning change between languages, depending upon whether the languages in question were "open" or "closed."<sup>23</sup>

Giedymin believes Ajdukiewicz borrowed from the work of Poincaré and the French conventionalists, because he sees that by Ajdukiewicz's account, the adoption of a particular conceptual system is not uniquely determined by experience. It is guided by conventions instead. Since, by Giedymin's account of Ajdukiewicz, there may exist closed languages which are not intertranslatable, and science's epistemic claims are formulated within a language (theory), then some scientific theories may not be comparable on either empirical or logical grounds. Subsequently, the transition from one such system to another would not be smooth or systematic, because there would be no linguistic or experiential bridge connecting the two. This type of transition would be indicated by a disruptive

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<sup>23</sup> Giedymin explains that Ajdukiewicz developed the following picture: consider two languages, L1 and L2. L1 is "open" if it is so constructed to include all the terms with the same meaning as L2, and other terms not included in L2, such that at least one term is meaning-related (read: "inferentially-related") to an expression in L2. An open language can, then, absorb a new term without necessarily altering the meaning of existing expressions. On the other hand, if a closed language (say, L2) absorbs a new term not synonymous with those existing, then either the meaning of some of L2's expressions changes, or L2 splits into separate, incompatible parts. What Ajdukiewicz then proved is that two closed and connected languages are either isomorphic, or share no meaning in common (Giedymin 1986, 203-204; cf. Ajdukiewicz (1949)).

change, as investigators take a cognitive jump from one closed theory to another (Giedymin 1986, 204).

Another influential conventionalist-minded scholar and mathematician cited by Giedymin is Jan Lukasiewicz. Although his connection with Fleck is not certain, Giedymin writes that Lukasiewicz's writings on mathematics and many-valued logics (3-valued in 1920;  $n$  values in 1922/29) were widely known by the Lwów-Warsaw group.<sup>24</sup> If Fleck had not known of Lukasiewicz from his own reading, Giedymin says, it is conceivable that Fleck may have been introduced through his associates, Chwistek and Hugo Steinhaus<sup>25</sup>, who were interested in math and logic (Giedymin 1986, 198).

But the aspect of Lukasiewicz's writings Giedymin elaborates upon is not mathematics. Instead, it is Lukasiewicz's emphasis upon creativity in scientific work. His (1910) "Creative Elements in Science" is summed up as a view that the successful scientist must use his *imagination* to satisfy the intellectual curiosity of fellow practitioners in the field. As such, this apparently places more emphasis on the capacity to actively create (for the patronage of cognitive appreciation) than on the capacity to routinely describe (which may or may not be satisfying, although whether it is, should be irrelevant).

Lukasiewicz distinguished four types of reasoning -- explanation, inference, proof, and testing -- which are not logically different but are identifiable by what is held constant and what is sought as a consequence. The creative element shows most clearly in explanatory reasoning, where universal laws are invented to organize and systematize the particulars of experience. Every singular expression contains creative elements as well, since they are connected with the invented universals which systematize

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<sup>24</sup>Jan Lukasiewicz (1920), "O logice trojwartosciowej" ("On Three-Valued Logic"). There is no citation in Giedymin's bibliography for a 1922 or 1929 publication by Lukasiewicz. There is one for 1928, "On Reasoning in Natural Science." Another source to turn to for Lukasiewicz's writings is his (1970) *Selected Works*.

<sup>25</sup>Fleck acknowledges Steinhaus and Biernacki for their statistical analysis used in his (1949) "The Phenomenon of Leukergy" (co-authored with Zofia Murczynska), on 264.

them. The scientific structure, or theory, contains at its base the "statements about singular observable facts; over and above them there is the superstructure of the theory which explains, orders, and predicts facts" (Giedymin 1986, 197). Creating the most intellectually satisfactory structure, according to Lukasiewicz, is the aim of scientific work.

Lukasiewicz's portrayal of scientific reasoning is different from those previously discussed, because he shifts emphasis from the rigor of formal frameworks to the use of imagination and creativity. Others interested in shifting away from axiomatic schemata and formalism were Edward Poznanski and Aleksander Wundheiler, the authors of "The Concept of Truth in Physics" (1934). They held that the truth of a statement is never at stake by itself. Instead, the sense of a statement can be understood only within the context of its web-like linguistic system. There are no privileged axiomatic statements, and verification or falsification can take many directions through a system, depending on what is held constant. An apparently falsifying instance to one statement may be redirected through an adjustment of auxiliary hypotheses; a rationale which seems to be arbitrarily terminable, and at times, cyclical. The existence of mutually inconsistent cyclical rationales, each of which may save the phenomena equally well, implies the existence of mutually inconsistent truths. Wundheiler and Poznanski hold that because two such systems (read: theory) can be both consistent and equally empirically adequate, neither coherence nor empirical adequacy are enough to determine which system (theory) is true (Giedymin 1986, 209).

The criterion Poznanski and Wundheiler highlight is the "*consensus* of competent and reliable scientists ... in general applied in order to terminate otherwise interminable testing procedures" (Giedymin 1986, 209; emphasis Giedymin's). "Consensus" is guided by individuals with normal sense-organs, who are intelligent and disinterested enough to qualify as

respondents, and agree on "elementary statements" and logic. "True," then, may be replaced with "accepted at the time," or "part of the system" (Giedymin 1986, 209).

#### *4. Synopsis*

After covering this philosophical and medical ground, it is time to highlight points of interest I will resurrect in the following discussion of Fleck. On the medical side, there is a debate over reductionism and holism with respect to illness and disease. Those, such as Chalubinski, with whom Fleck would side rejected the view that diseases are best viewed as simple chemical and physical phenomena. People with the same physiological symptoms may not have the same physico-chemical traits. Conversely, two people with similar physico-chemical traits may exhibit different physiological symptoms. While the physician uses scientific knowledge to cure the sick patient, the physician takes the patient as a whole (unit) into account as well as the physics and chemistry involved. So moving to the organizational level of the individual person as a fundamental unit of investigation is expressed in the medical community. This is a move I believe Fleck takes further, emphasizing the role of societies as fundamental in explaining the metaphysical and epistemological beliefs of individuals.

Second, there is the issue of constructing medical knowledge. Weigl makes the case for the artificiality of medical knowledge concocted in the lab, since the laboratory environment is artificial. It is not the environment a doctor confronts in practice, so the degree of the similarity between the two is questioned. Another constructive case, pointed out by Kramsztyk, highlights the historical and social contingency of the "typical" disease traits. What counts as "typical" depends upon the state of medical knowledge at a given time. Such states change, and with that there are changes in how a disease is recognized and when (or, if) the disease obtains. Thus disease entities, constructed and contingent as they are with respect to a community at a given



time, are not static phenomena, nor are they necessarily natural phenomena in the world. Instead, they are helpful, needed, but transitory conceptual classifications physicians use to carry out their work.

On the philosophical side, there are two issues that stand out. The first is the emphasis throughout on the semantic holism contained within logico-conceptual frameworks. This is done with the purpose of explaining or understanding why divergent reality claims come to be. That such conflicting claims arise says something about the role of the external world in adjudicating between them: it doesn't help out.

Metaphysically speaking, the reason the external world cannot help is because we are denied access to it. We are privy only to our personal phenomenal worlds. It is *prima facie* senseless, then, to discuss the status of the external world independent of mental states. Epistemologically speaking, talk of the external world is framed in terms of prior conceptual schemes. Due to the semantic holism that these schemes usually entail, claims about the world generated by conflicting schemes likewise may conflict. But since a conceptual scheme, or framework, is needed in order for the external world to have any sense, the external world cannot be used to adjudicate any such conflict. So, means ranging from common sense (Chwistek), to intellectual satisfaction (Lukasiewicz), to social consensus (Wundheiler and Poznanski) are proposed as signs of a system of thought's adequacy.

The second reason for exploring early twentieth century Lwów-Warsaw intellectual activity is suggested by Freudenthal and Löwy (1988). They point out that Fleck produced the majority of his philosophical work between the years of 1926 and 1936, a period in which he was not involved with mainstream institutional research. He lost, or left, his job with Weigl at the university in 1923, returning after the Soviet annexation of Poland in 1939. Freudenthal and Löwy argue that Fleck was ostracized from the research environment because of his minority views toward medical practice.

His professional status became increasingly marginalized as he relocated from the university to a hospital, then to a public lab, and eventually to his own private lab (Freudenthal and Löwy 1988, 639).

Nonetheless, Fleck desired to have his voice heard, and wanted to defend his medical views as legitimately scientific. Freudenthal and Löwy write:

had Fleck espoused the reductionist view, he would necessarily have had to admit failure: he would have had to consider his own activity -- routine analyses and goal-oriented applied research -- as having nothing to do with 'real' science, and consequently as being work of an inferior kind with low prestige. (Freudenthal and Löwy 1988, 639)<sup>26</sup>

However, since Fleck was unable to participate with prestigious medical research, he shifted his attention. He merged his ideas on diseases and their diagnoses with lines of thought in the philosophical and historical communities. The merging, or "hybridization," of medical, historical, and philosophical methods led Fleck to the view that different styles of medical research were equally legitimate, and their results equally true.

##### *5. Fleck 1935 to 1961*

After 1935, Fleck worked for four years solely in his private lab, until the Soviets annexed Lwów in 1939. At this point, Fleck was appointed lecturer of microbiology at the Ukrainian Medical Institute, formerly the Medical Faculty of Lwów University. There he remained until the Germans invaded the Soviet Union in July 1941, when he and his family were deported to Lwów's Jewish ghetto. In the face of a typhus epidemic, Fleck developed a procedure

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<sup>26</sup>According to Weissmann (1980), Fleck made a major contribution to the medical field with his research of leukery, or the function of white blood cells in inflammation. Although not called by the same name, the role of this phenomenon is still investigated with respect to diseases (see esp. 49 and 55). Two of Fleck's articles published in English on leukery appear in a special issue of *Texas Reports on Biology and Medicine* (vol. 5), a symposium on Polish medical contributions during the Second World War.

for procuring a typhus vaccine from the urine of infected patients (Schnelle 1986b, 21). The Germans caught wind of Fleck's activities, and

[i]n May, 1942, the results [of the typhus vaccine] were reported at a staff meeting of the "Ghetto" Hospital in Lwów, and several months later, the author was deprived of his collaborators who were destroyed by the Germans. (Fleck 1947b, 168)

In December 1942 Fleck and his family were arrested. In response to a German query as to whether the vaccine would work on Germans, Fleck reportedly answered "that that is doubtful, since Germans were after all of another race, and the vaccine had been made from the urine of sick Jews" (Schnelle 1986b, 22; cf. *Genesis*, 151). He and his family were relocated to the 'Laokoon' pharmaceutical factory.

In January of 1943, they were arrested again, and were sent to Auschwitz February 7, 1943. With other prisoners, Fleck worked in a serological laboratory diagnosing syphilis, typhus, and other illnesses. In August 1943, a laboratory was set up for the production and study of production methods for typhus serum at the Hygiene Institute of the SS in Buchenwald; here Fleck was moved in December 1943 at the command of the Berlin headquarters of the SS economic administration. Following the liberation of April 11, 1945, Fleck, his wife and his son were separately released and returned to Poland. Fleck's two sisters, his parents, and all his other relatives had died (Schnelle 1986b, 28; cf. *Genesis*, 151).

Back in Poland, Fleck lived in Lublin where he became a full professor, with a brief leave in 1948 to testify at the Nuremberg trials. In 1952 he moved to the Department of Microbiology and Immunology of the Mother and Child Institute in Warsaw (Schnelle 1986b, 29). In 1954 he was admitted to the Polish Academy of Sciences, and a year later was elected to the Presidium of the Academy. Although he published heavily within his medical profession (over 130 medical and scientific articles appearing in Polish, German, Hebrew,

Russian, French, and English), he published only two other philosophical works after the war. One in 1946 almost certainly reflects his experience at Buchenwald. The other, (1947a), reiterates his earlier views, and is also punctuated with wartime experience. Fleck continued in philosophical exchange, regularly attending the meetings of the Lublin Philosophical and Psychological Society (Schnelle 1986b, 31).

The remainder of his years were largely spent doing research on leukergy. Schnelle writes of a 1957 announcement by Fleck regarding the upcoming completion of a second volume to his 1935 *Genesis*: "I am working on the methodology of the natural sciences and am preparing an extensive work in this field. This will be the world's second volume: the first volume was published in Switzerland in 1935" (Fleck, quoted/translated in Schnelle 1986b, 31). However, no such manuscript has yet been found.

That same year (1957) Fleck and his wife moved to be closer to their son, who lived in Palestine. A position was created for Fleck at the Israel Institute for Biological Research in Ness-Ziona, and in 1959 he moved to the medical school of the Hebrew University in Jerusalem. However, his health was fading fast; it was found that Fleck had Hodgkin's disease after his first heart attack in 1956. Complications with his health and the language in Israel stifled his work (Schnelle 1986b, 32).

Still, in 1960 he wrote a short piece in English ("Crisis in Science") in response to a 1960 article in *Science* entitled "Science and Human Welfare." But the journals to which the essay was sent (*Science*, *American Scientist*, *New Scientist*, *British Journal for the Philosophy of Science*) turned his article down (Schnelle 1986b, 32). It made its first published appearance in the Cohen and Schnelle (1986) volume on Fleck. Shortly after writing this piece, Fleck died on July 5, 1961, of a second heart attack.<sup>27</sup>

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<sup>27</sup>Although Fleck's work attracts attention today, it was relatively overlooked in his own country and time (Markiewicz 1986, 223). In "Ludwik Fleck and Polish Philosophy," Bogusław Wolniewicz argues that the

From this point the focus of this thesis will be restricted to the medical and philosophical context discussed in sections 3 and 4 above. This should be kept in the background throughout the examination of Fleck's *Genesis* immediately following, and in the subsequent discussion of his other works.

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relativism in Fleck's work (which receives notable critique today) was not a difficulty then, so that is not why Fleck and his work were unknown (Wolniewicz 1986, 217). Giedymin seconds this, showing that relativism was not foreign or obtrusive to the Lwów-Warsaw group (Giedymin 1986, 183-185). What Wolniewicz suggests happened is that the intellectual atmosphere of Lwów-Warsaw was quite liberal. As such, the individuals stressed rationality and humans' capacity to come to agreement, and Fleck denied this: "[S]ince [the liberal conscience] could not argue well against it [Fleck's position], it *represses* it" (Wolniewicz 1986, 218; emphasis his). Fleck's views were laid to rest under tests for rigor; the logico-methodological standard they would need to pass in order to be worthy of discussion by the Lwów-Warsaw members was "uncommonly high." "Fleck's writings could not satisfy this logical standard, due to the fact that his ideas were not yet ripe enough to do so. And this is the major reason for their remaining so widely ignored in Poland" (Wolniewicz 1986, 219; emphasis deleted).

Perhaps part of the reason Fleck's *Genesis* stir more interest in his ideas at the time was because there were so few copies distributed. The first edition printed 640 copies, about 50 of which were distributed for review. Merton notes that none went to a sociological publication (Merton 1983, 187), Cohen and Schnelle write that there was only one philosophical review in *Lauriers* (1937) (but no other bibliographical data is given; Cohen and Schnelle 1986b, xvii). The rest of the reviews were in medical journals, popular journals, or newspapers (Cohen and Schnelle 1986b, xvii). About 200 copies of the book were sold (seven of which are in United States Libraries). The war destroyed another 200 in Leipzig. The publisher destroyed the remaining 200 copies in 1966 (cf. Weissmann 1980, 49).

## Chapter 2

### Fleck's *Genesis* Views on Facts; The Critics' Responses

#### A. Fleck's *Genesis*

To Fleck, facts are entwined with socially and historically conditioned, or even determined, mental events. He describes a scientific fact as

a thought-stylized conceptual relation which can be investigated from the point of view of history and from that of psychology, both individual and collective, but which cannot be substantively reconstructed *in toto* simply from these points of view (*Genesis*, 83).<sup>28</sup>

From this starting point, there are two points that need elaboration. The first is to Fleck's understanding of group, or collective, life. The second point is the thought-stylized component in the formation of a fact. The following two sections focus on the concepts of thought-collective and thought-style, in that order.

#### 1. The Thought Collective

According to Fleck, a

thought-collective exists whenever two or more people are actually exchanging thoughts. . . . A special mood arises, which would not otherwise affect either partner of the conversation but almost always returns whenever these persons meet again. (44)

This is to say, the label 'thought-collective' may be liberally applied: to two best friends, to a group of colleagues chatting over coffee, to the members of a seminar delving into a specialized topic. By 'special mood', Fleck means "a condition in which [the participant] utters thoughts he would not have been able to produce by himself or in different company" (44).

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<sup>28</sup> Unless otherwise indicated, all references to Fleck's work in this chapter are to his *Genesis*.

I take Fleck to mean that among the members of a collective there are common experiences or shared interests that provide ground for communication between them. These experiences and interests are the glue binding otherwise different people together. It creates an environment where a person, influenced by the reactions of others, is stimulated to consider ideas that may not have been conceived otherwise. And it is an environment which, if the members are interrupted, reappears when the members come together again. An example of such an environment is the dynamics among the members of a graduate seminar. Consider a collection of people coming from different backgrounds and different levels of expertise, for the aim of pursuing a special topic. Such groups typically contain few members, and if one member is absent, his or her particular manner of contribution is noticeably missed. And though the time spent in the seminar is, through the week, minimal, the regular regrouping of members through the term recalls the manner in which they interrelate and think.

Some collectives are more fragile than others. Expanding upon his two-person thought-collective above, Fleck states: "If a third person joins in, a new collective arises. The previous mood will dissolve and with it the special creative force of the former small collective" (44). With respect to the graduate seminar example I began, one may conceive that if attendance is unpredictable and unreliable, then bonds that would ordinarily identify the participants as belonging to the same group would barely unite them. A class may meet at such-and-such a time, but only nominally so.

A stable collective is one which has endured and it embodies a particular historically-developed train of thought (39). This is not because the members of that community have persisted through time. It is because the ideas, or "mood," of a group "caught hold" somehow. They gained influence, inspiring prospective members to be similarly trained. In that process, the community and its tradition continue. Fleck notes that such

collectives normally exhibit certain regular characteristics (105).

A collective consists of a small esoteric circle of experts and a larger, supporting body of exoteric laypersons (105). These groups can form "around any work of the mind, such as a dogma of faith, a scientific idea, or an artistic musing" (105). They are recognized by shared standards in communication, customs, and even rituals. Most importantly, the thought-collective is the device "carrying" a particular style of thought:

If we define 'thought-collective' as a community of persons mutually exchanging ideas or maintaining intellectual interaction, we will find by implication that it also provides a special 'carrier' for the historical development of any field of thought, as well as for the given stock of knowledge and level of culture. (39)

The collective is a repository of sorts for its members' stock of knowledge. Each member only knows "so much," while at the same time there is much more considered to be known. I interpret Fleck to mean that a collective holds, in some sense, its community's set of beliefs about the world (e.g., matters of fact). He writes that "[a] well-organized collective harbors a quantity of knowledge far exceeding the capacity of any one individual" (42; cf. 38, 64). This is akin to the experience of looking at all there is to read within a large library in consideration of what you yourself have read. The difference is that Fleck's point is a method of reconstruing the external world. "That which is known" is much more than what any individual knows. This conveys a sense of perspective of the individual with respect to everything else. And insofar as "that which is known" is about the world, it is a sense of perspective of the individual with respect to the rest of the world. With the discussion turning to talk of cognitive states (e.g., thought and knowledge), it is time to examine Fleck's conception of a thought-style. Then I will present



Fleck's association of thought-styles and thought-collectives to the formation of facts.

## *2. Thought-Style*

Fleck defines a thought-style as a readiness for

directed perception, with corresponding mental and objective assimilation of what has been so perceived. It is characterized by common features in the problems of interest . . . by the judgment which the thought collective considers evident, and by the methods which it applies as a means of cognition. The thought style may also be accompanied by a technical and literary style characteristic of the given system of thought. (99)

In other words, a thought-style is a communally-sanctioned set of cognitive and behavioral standards developing toward, or directed at, a common goal. It is manifested in the agreement of the community's members on what counts as legitimate problems to investigate, on the appropriateness of decisions in certain contexts, and on devices and methodologies employed to reach the desired ends. Among these, Fleck notes that the readiness for directed perception is the most important (92).

"Directed perception" is Fleck's phrase for capturing the goal-oriented nature of collective life. In other words, "directed" is a two-place predicate: that which is being directed is the individual's thought and behavior. This is directed toward performing in accordance with standards sanctioned by the tradition of the collective, the thought-style. Fleck writes that a thought style "is a definite constraint on thought, and even more; it is the entirety of intellectual preparedness or readiness for one particular way of seeing and acting and no other" (64). This readiness

is acquired only after much experience, perhaps with preliminary training. At the same time, of course, we lose the ability to see something that contradicts the form. But this is just the readiness

for directed perception that is the main constituent of thought style. (92)

A common style is evident among individuals on an intellectual par by "a solidarity of thought in the service of a super-individual idea which causes both intellectual interdependence and a shared mood between the . . . individuals" (106). The style, or intellectual interdependence "dominates all communication of thought within a collective," such that without it, communication is impossible, "the speakers are at cross purposes" (106). Fleck describes the scientific style of thought as

a common reverence for an ideal -- the ideal of objective truth, clarity, and accuracy. It consists in the belief that what is being revered can be achieved only in the distance, perhaps infinitely distant future; in the glorification of dedicating oneself to its service; in a definite hero worship and a distinct tradition. This would be the keynote of the common mood in which the thought collective of natural science lives its life (142).

For Fleck a thought-style's focus and limit explains the success or failure of communication, particularly with respect to epistemological or metaphysical matters:

[S]table (or comparatively stable) thought communities . . . cultivate a certain exclusiveness both formally and in content. A thought commune becomes isolated formally, but also bonded together, through statutory and customary arrangements, sometimes a separate language, or at least special terminology. . . . But even more important is the restricted content of every thought collective as a special realm of thinking (103-104).

The greater the difference between two styles of thought, the more difficult it is to think in both; a person has to operate within one or the other. And by operating within one, the prospect of taking seriously the other, different, one disappears.

To put it another way, there are several possible ways that an individual may see and react to her environment; this reflects the different social dimensions of that person's life. For example, at the scene of a traffic accident a passing physician may respond by administering aid, or she may respond as a disinterested passerby who doesn't want to get involved. If she responds the second way, one is hardly be inclined to think she behaved as a doctor. Indeed, she is even less of a doctor since she did not respond the way her method of training dictates she should. Whichever way she responds exemplifies the directed perception Fleck describes: interpreting a situation with certain cognitive and behavioral consequences.

For Fleck, the exclusiveness of a style cancels conflicting styles when it is, so to speak, up and running (92). This is why members of conflicting styles are at "cross purposes." Two people, A and B, are trained and compelled to react within their respective styles. Faced with what *prima facie* is a single situation S, they nonetheless respond quite differently, *as though* they were faced with quite different (incompatible) events T and  $\neg T$ . If A and B are brought together to consult on S, then without recourse to a common ground they talk about T and  $\neg T$ . Each believes the event S cannot be both T and  $\neg T$ ; each clings to her own perspective and "talks past," or cannot see the point of, the other.<sup>29</sup>

A cohesive and strong system of thought, once structurally complete and closed, contains many details and relations, and "offers enduring resistance to anything that contradicts it" (27). A style's organic exclusiveness" limits the domain of the community's intellectual interest

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<sup>29</sup>Considering that by Fleck's analysis even a single person internalizes several styles of thought, some of which are incompatible, then even a single person has the potential to experience an internal duel, so to speak, between methods of interpreting and reacting to a situation. A person can act but in one way. The doctor in the example above, if she were to respond as a doctor, cannot simultaneously respond as a frightened human being and run away. Tasks arise within the "doctor" mode, such as checking for vital signs, broken bones, etc., and that prohibits the possibility of other activities, such as steering safely around the scene without rear-ending a rubber-necker.

and the methodologies it employs (104). If strong or established enough, then to some extent the traditions

must be regarded as units, as independent, style-permeated structures. They are as . . . harmonious holistic units exhibit[ing] those particular stylistic properties which determine and condition every single function of cognition (38; cf. 37, 104).

This sounds as though Fleck takes a style to be a supra-individual entity. But that is questionable, given Fleck's background medical beliefs.

As indicated in Section B1 of the first chapter, Fleck was employed by, and presumably under the intellectual influence of, the bacteriologist Weigl. Weigl was a member of a medical school of thought whose tenets were mirrored in other principles of Polish philosophy of medicine (see Section B2). These views were in opposition to the dominant methodology promoting disease and germ specificity on a principle espoused by Koch, which can be summarized thus:

The principle of etiological specificity of disease implies that every disease entity is produced by a quite particular cause, that different diseases cannot arise from the same cause, nor can different causes produce the same disease (den Belt and Gremmen 1990, 469).

This was a "lock-and-key" approach: bacteriological phenomena affect one another by fitting together in a unique and singular way. After certain bacteriological combinations occur, diseases unique to that particular event follow.

Like Weigl, Fleck opposed this view; this is apparent in his *Genesis*. He writes, for instance, that "[a]n organism can no longer be construed as a self-contained, independent unit with fixed boundaries, as was still considered according to the theory of materialism" (60). If biological organisms were so segregated and independent, then they would scarcely interact with one another in order to produce a physiological reaction:

A completely foreign organism could find no receptors capable of reaction and thus could not generate a biological process. It is therefore better to speak of a complicated revolution within a complex life unit than an invasion of it (61).

But although he disagreed with specificity/reductionism, Fleck did not suggest any alternative, and he noted that there was not then any suitable replacement for the prevailing theory: "We already have the rudiments of other views, even though these have not as yet found their way into the textbooks," and even though the new views had not yet been "fully confirmed" (59).<sup>30</sup> But at least the forcefulness of the dominant style at that time had cracked enough to allow for the exploration of alternative models.

So while it is not altogether clear what Fleck conceived organisms to be, it is fairly clear that he did not believe they were closed, self-contained, independent entities. The lack of a medical alternative to reductionism may have also left Fleck at a loss for how to capture what he meant by high-order social phenomena and their dynamics. If one is inclined to believe that his medical views and philosophical views overlapped, and it does not seem absurd to think they did, then Fleck would not obviously be committed to the idea that styles of thought are closed and independent. But if styles of thought were not somehow bounded, then how could they be distinguished? The answer to this in the *Genesis* is found in Fleck's use of active and passive conceptual relations.

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<sup>30</sup>According to Löwy (1988b), Fleck lived in a period of crisis in immunology: Although Fleck was unable to indicate any solution to the problems besetting his scientific discipline, he had at least, unlike many of his colleagues, a clear insight into the existence and importance of these problems (Löwy 1988b, 350).

The doctrine of specificity, as discussed earlier, holds that there are unique reactions to certain types of foreign "invading" germs causing the particular disease. In a footnote, Löwy (1988b) explains that this conception found problems when it was learned that the body produced antibodies against a vast array of natural and artificial substances, in addition to the "invading" microorganisms. The immune system was not apparently a specifically triggered "defense mechanism," but vastly more complicated (Löwy 1988b, 350, footnote 11).

### 3. Active and Passive Associations

Active connections or linkages (*Koppelungen*) are those which, according to Fleck, are explicable in terms of history, sociology and psychology. They are comprehended through the study of thought-collectives and thought-styles, for it is here that active associations are stored.

Cognition therefore means, primarily, to ascertain those results which must follow, given certain preconditions. The preconditions correspond to active linkages and constitute that part of cognition belonging to the collective (40).

The quantity of the active associations was substantial for Fleck: "At least three-quarters if not the entire content of science is conditioned by the history of ideas, psychology, and the sociology of ideas and is thus explicable in these terms" (21). Active linkages evolve through a history of social selection in the name of the collective correspond to the active associations.

Passive associations, on the other hand, are those

that are not explicable in terms either of psychology (both individual and collective) or of history. For this very reason these seem to be 'real', 'objective' and 'true' relations. We call them the passive connections in contrast with the others we call active (10).

Passive associations are those which follow "of necessity from a given set of active assumptions [*Voraussetzungen*]" (64, emphasis mine); they are relations "which cannot exist in any other way" (101); they are results which *must* follow given certain active presuppositions (40, emphasis mine). Passive connections occur without tremendous individual or collective mental effort, as opposed to the active associations or assumptions which are the product of collective mental effort. It might at first appear that passive associations are "passive" in the sense of "passive voice," where the subject receives some action. But the passive associations are also *made* by

individuals, so that would not be the case (40).<sup>31</sup> The "passive" refers to the nature of the conceptual connection: it comes automatically, ingrained either by previous experience or by pedagogical repetition. Frequently, passive associations appear to be quasi-analytic, although Fleck does not use the term.

While passive and active associations are different, they are inextricably tied together. As a person becomes more trained within a domain, the passive associations grow in step with the active ones:

The number of passive and inevitable connections produced increases ... because for every active element of knowledge there corresponds a connection that is passive and inevitable (82).<sup>32</sup>

In a sense the relation of passive to active associations is itself a sort of passive relation: "The passive and active elements cannot be separated from each other completely either logically or historically" (95). But trying to grasp why Fleck sees these relations fitting so tightly together is not altogether easy

But as a first approximation, the message I believe Fleck intends is this:

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<sup>31</sup>What Fleck understands as being associated (either actively or passively) is not certain. Sometimes, active relations seem akin to an experimental design, where the apparatuses used and how they are put together are socially and historically developed. The outcome, constrained by experimental controls, is analogous to Fleck's passive connections, where 'passive' captures the experimenter's relation with respect to the test: waiting for the result after having initiated the event (10). The more constraints, the more stable, or obvious, or "real" the outcome. So the *relata* in this context are a community's preconceptions about the world, their tools, and an experiential outcome.

But, as I will discuss later, all phenomenal experiences for Fleck must be read through a style in order to be recognized. Furthermore, passive associations or linkages connect elements of knowledge, and by this he can be taken as talking about relations of ideas (e.g., the conceptualization of syphilis to that of the Wassermann reaction):

Versucht man in concreto das sogenannte Subjektive vom sogenannten Objektiven kritisch abzusondern, so findet man immer und immer wieder die oben erwähnten aktiven und passiven Koppelungen innerhalb des Wissens (*Entstehung*, 57).

Perhaps Fleck intended to associate "passive" links in knowledge with the constrained phenomenal results to which the concepts of knowledge refer. But this is a relation that seems undeveloped in Fleck's text. It may suggest, however, further useful explorations of and developments upon Fleck's work.

<sup>32</sup>Here we have some bad mathematics. By this rendition, the weight of passive to active is evenly distributed at 50-50. However, in an earlier quote (Fleck 1935c, 21), the weight of passive to active is 25-75. What significance Fleck wants to attribute to the non-logical (i.e., sociological, psychological, historical) is not exactly clear.

passive associations are a place holder for the inferential relations fashionable at the time with the construction of conceptual frameworks, such as those discussed in Chapter 1 (B3). However the theoretical structure is constructed, there needs to be a means for getting from one proposition or idea to another. Fleck calls his means 'passive associations', a new name for an old idea. For instance, consider Fleck's discussion of the relation of the atomic weight of oxygen to that of hydrogen:

The origin of the number 16 for the atomic weight of oxygen is almost consciously conventional and arbitrary. But if 16 is assumed as the atomic weight for O, oxygen, of *necessity* the atomic weight of H, hydrogen, will inevitably be 1.008. This means that the ratio of the two weights is a *passive element* of knowledge (83, emphasis mine; cf. 95).

I understand Fleck to be saying that, given the way certain elements are related to one another (setting the parameters for the weight of oxygen, the nature of the device doing the measuring) certain other results occur. Learning how to reach that result is part and parcel of learning a style of thought. Learning a style of thought means learning something about operating within a world that, without the style's aid, would have no direction. In this example, tradition provides the means to get from the weight of oxygen to the weight of hydrogen; the weight of tradition lends a sort of certainty to the result (e.g., because the experiment has been repeated with the same result over and over). And upon the successful completion of this exercise, a student experiences that relation has possessing greater obviousness and certainty.

But phenomenal experience is not the only component of passive knowledge. In a different example, Fleck writes

Consider a myth, such as the Greek myth of Aphrodite, Hephaistos, and Ares. Aphrodite cannot but be the wife of Hephaistos and the lover of Ares. As any poet knows, a web of



fantasy spun for long enough always produces *inevitable*, 'spontaneous' substantive and formal connections (101, emphasis mine).

By my reading, the connection between 'Aphrodite' and 'wife of Hephaistos' is more formal than the passive connection discussed before. Here, the concepts do not refer to phenomenal images; their sense comes from the way in which the terms are related together. 'Aphrodite' and 'wife of Hephaistos' mean the same thing, because given that system of thought, the way they are related makes the system that which it is. Nonetheless, understanding why the inferential relations are that way is not explained by the conceptual system containing them. The inferential relations are explicated through the historical and sociological context that created them.<sup>33</sup>

So, insofar as Fleck critiques his philosophical contemporaries, I do not believe he thinks they are completely wrong.<sup>34</sup> The point I take Fleck to make of his philosophical contemporaries is that a focus on the logical components of thought is incomplete, because that approach is mute on *why* certain relations happen to be the way they are, or were. Ignoring this renders "passive" connections (relating either ideas to experience or ideas to other ideas) as historically and socially immune, and thereby artificially true or false independent of human contingencies.

#### 4. Solidification

The number of actively-generated concepts and the way they are passively linked together forms the systematic structure of the thought-style of a thought collective. The unique manner in which a set of concepts are

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<sup>33</sup>In another example, "horse" and "steed" are, Fleck says, logically identical. But if one is writing a romantic story, he will use the latter word and not the former (101). Why, if the two words are logically equivalent, should one be chosen one over the other? It depends upon the style in which the writer writes; and why it employs some words rather than others is answered historically and socially.

<sup>34</sup>Fleck targets Carnap, Schlick and the Vienna Circle on 50, and in footnote 3 on 177. He especially rejects the notion of neutral observation, or direct, "given" experience (177).

connected illuminates the characteristic style of that mode of thought. And, the greater the number of connections between concepts, and to other styles of thought, the stronger the force of that style of thought. "[T]he more systematically developed, the richer both in detail and in its relations to other branches a given branch of knowledge is, the fewer will be the differences of opinion about it" (9). It becomes natural, or customary, to think about the world in that way, in that context. This method of thought provides the epistemological and metaphysical foundation of the world, or what can be known, according to Fleck. But because different styles of thought are in some sense closed to one another, there are different worlds closed to one another (remembering that for Fleck what counts is the world *as conceived* through the apparatus of a style).

### 5. *Relativism*

"Whatever is known has always seemed systematic, proven, applicable, and evident to the knower. Every alien system of knowledge has likewise seemed contradictory, unproven, inapplicable, fanciful, or mystical" (22). While within one style of thought, thinking and behaving in a conflicting way is virtually inconceivable, as in the doctor/frightened passerby example discussed in the section 2 of this chapter. However, Fleck acknowledges the capacity to internalize several, different styles of thought. The human versatility to respond in many ways is testimony to the capacity to internalize different styles of thought.<sup>35</sup> The logical consistency of all the styles together is relatively unimportant. What counts, by Fleck's lights, is the power of a social context to draw out a particular manner of thinking: "The stylized uniformity of his thinking as a social phenomenon is far more powerful than

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<sup>35</sup> For example, in the face of a job transfer one can, and in weighing one's options, does, evaluate the situation from the perspective of the ambitious business person, from the perspective of the wife/husband with another career and family to consider, etc.

the logical construction of his thinking" (110). Again, Fleck makes a gesture toward his contemporaries, who used axiomatic frameworks as a model for thought. Thinking is, by this alternative account, a social phenomenon, not a logical one.

Unfortunately, Fleck also removes the possibility for evaluating or rationally justifying one style of thought over another:

We must object in principle that nobody has either a feeling for, or knowledge of, what is physically possible or impossible. What we feel to be an impossibility is actually mere incongruence with our habitual style of thought (48).

That is, if one develop an authoritative account about an empirical matter -- say, on the matter of how and why we think the way we do -- and if someone else thought that the description was false, then it wouldn't *really* be false. It is only incongruent with someone else's manner of thinking. Conversely, the story couldn't really be *true*, either.

Evaluating any thought style is furthermore complicated due to the relativism Fleck builds into his theory:

One can never say that the same thought is true for A and false for B. If A and B belong to the same thought collective, the thought will be either true or false for both. But if they belong to different thought collectives, it just will *not* be the *same* thought! It must be either unclear to, or understood differently by, one of them (100, emphasis Fleck's).

A and B disagree about the truth value of a knowledge claim. If they belong to different thought-collectives, then they don't really disagree because they are not talking about the same thing. But this picture does not work well for two reasons. First, if A and B belong to the same collective, then they agree on the truth value of any particular thought within their shared style. This is because since they are members of the same collective, the thought is

presumably the same. But given the intuitive understanding discussed above of what a thought-collective is, one must acknowledge that we disagree with our associates all the time, such as in a graduate seminar. So two associates disagreeing on the truth value of a claim would not be members of the same collective, and perhaps a graduate seminar is not a viable candidate for a Fleckian collective after all. But Fleck does not help clear this up; rather, his writings further complicate how the members of a collective may understand and meaningfully communicate with one another at all.

Thoughts pass from one individual to another, each time a little transformed, for each individual can attach to them somewhat different associations. Strictly speaking, the receiver *never* understands the thought exactly in the way that the transmitter intended it to be understood (42, emphasis mine; cf. 109).

So even within a style of thought, two members A and B never talk about the same thought. Fleck leaves us with individuals, each restricted to living within his or her own cognitive world and little to explain how, or if, we communicate at all. And if it is not clear how two people can communicate, by Fleck's presentation, then it is subsequently unclear how there could be any thought-collectives (where any people are actually exchanging thoughts (44)). Unfortunately, Fleck did not appear to have noticed this difficulty. Somehow, he has it that communally-sanctioned connections are developed which enables all members to understand what the others mean. But it is not obvious how that occurs.

## 6. Conclusion

Weak or unstable thought-styles do not have the power to generate a belief in the reality that the conceptual relations construct. In the *Genesis*, Fleck takes considerable time to trace the history of the concept of syphilis. He explains that in the mid-1800s, the concept of syphilis wavered between an

ethico-mystical understanding and an empirical-therapeutic one. As such, the concept

still remained too variable, and not sufficiently entwined or interwoven within the fabric of contemporary knowledge to be considered finally established with a firmly based, objective existence, and thus to appear as an undoubtedly "real fact" (6).

In a fact's early stages, the focus on what is important is not clear. Concepts that seem applicable are borrowed from different styles, thrown together in the attempt to capture a fixed experiential image.<sup>36</sup> In this developmental state, "[t]here is neither support, nor constraint, nor resistance and there is no 'firm ground of facts'" (92). All the proposed conceptual relations have problems, and there is no persuasive force to adopt one over another. When support develops for finding one method more fruitful than others, the development of a fact begins.

At first there is a signal of resistance in the chaotic initial thinking, then a definite thought constraint, and finally a form [*gestalt*] to be directly perceived. A fact always occurs in the context of the history of thought and is always the result of a definite thought style (95).

A fact depends upon a style of thought, because as a conceptual relation it needs a conceptual system within which to fit. The more systematic and interconnected the elements of a system of thought, the firmer, or more obvious, or more "real" its world.

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<sup>36</sup>Fleck (1935a) discusses the genesis of the diphtheria bacilli system. It started by characterizing the bacilli as similar to "spaced fingers, fingers of two superimposed hands, the letter V," or other familiar figures (Fleck 1935a, 71). At one time or another, alternative characterizations seemed more suitable, until for one reason or another the comparisons were abandoned and a unique, "specific" arrangement developed that was "non-comparable with anything" (Fleck 1935a, 71). He later explains that

from the store of traditional pictures one fits on some pictures and their combinations; next some of them are rejected, others are stylized, then a battle takes place with the alternately intruding pictures -- and finally, a new readiness is formed, i.e. the readiness to see a new, specific form (Fleck 1935a, 72).

A maximum of information is demanded, the greatest possible number of mutual relations between individual elements, in the belief that the ideal of objective truth is all the more closely approached as more and more relations are found (144-45).

This also means that the more a particular relation is entwined within a style of thought, the more certain and obvious and real it is. The discovery of a new fact is the incorporation of a new relation into a pre-existing system of thought. The facts "hang together" (*Zusammenhang*) in a sort of socio-mental space:

Facts are never completely independent of each other. They occur either as more or less connected mixtures of separate signals, or as systems of knowledge obeying its own laws. As a result, every fact reacts upon many others. Every change and every discovery has an effect on a terrain that is virtually limitless. ... *Here every discovery is actually a recreation of the whole world as construed by a thought collective* (102, emphasis mine).<sup>37</sup>

The recognition of any new fact is fostered by a change in thinking: either by the expansion of a style to incorporate a new piece of data, or by a change in direction by which a style of thought proceeds. The change in thinking opens the possibility for new inferences, or the discovery of new facts. The balance between the members working together in the collective and the style of thought framing their purpose creates the world.

A universally interconnected system of facts is thus formed, maintaining its balance through continuous interaction. This interwoven texture bestows solidity and tenacity upon the "world of facts" *and creates a feeling both of fixed reality and of the independent existence of the universe* (102, emphasis mine).

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<sup>37</sup>However, Burian (private conversation) points out that in the German text there is no corresponding phrase for the 'as construed by' in the English translation: "Jede Entdeckung ist in diesem Falle eigentlich eine Neuschöpfung der ganzen Welt eines Denkkollektivs" (*Entstehung*, 109). This indicates that a closer look at the German text is in order.

After a while, a habit of thinking about the world within a particular context comes to be the only possible way for the world to be understood (107). Nonetheless, the manner of thinking does shift over time, as do the facts. Yet, in being constructed from familiar concepts at hand, new facts maintain some residue of the social and historical background from which they sprung. A fact continues to exist as long as it remains in line with a community's interests, in being an object of trained experience, and in being expressed in the community's style of thought (102). Thus, my interpretation of Fleck's concept of a fact outlined at the end of Chapter 1A is complete.

A fact evolves through the conceptual and social evolution of a tradition emphasizing certain relations of idea to ideas, or ideas to actions (Chapter 2, A1 and A2). As the relations become more enforced (or entrenched), they become more "obvious" or "certain" (A4), similar to Fleck's passive relations (A3).

But there are interpretations expressing the place of Fleck's facts to the external world that are quite different from my reading. The next section looks to those.

### *B. The Interpreters*

In this section I turn to what other scholars have had to say about Fleck's conception of facts. As we shall see, they have a variety of interpretations to offer. Since Fleck's concepts of thought-style and thought-collective are fundamental to his methodology, these are considered first in the subsection that follows. Next, I present two renditions of Fleck's concept of a fact. The first is a circulatory view, where a fact is understood to condense through the simplification of an idea suggested by the professionals of a collective. The second rendition emphasizes the importance of passive associations in the development of a fact. But since there are many different accounts of what "passive association" means, there are several castings of

what this says for Fleck's development of a fact.<sup>38</sup> After sifting those interpretations warranted by Fleck's *Genesis* from those that are not warranted, in the final chapter I elaborate upon the interpretations that remain from a position fortified by contributions from his other texts.

### 1. *Thought-style, thought-collective*

Writers with Fleck's concepts of thought-style and thought-collective in mind include Jonathan Harwood (1986), Yehuda Elkana (1986), Ilana Löwy (1988a), Straun Jacobs (1987), and Robert Cohen and Thomas Schnelle (1986b). The problem areas these authors target in Fleck's presentation fall into two categories: logical/conceptual and ontological. The category of logical/conceptual difficulties include issues such as vagueness, inconsistency, incommensurability and individuation.<sup>39</sup> Individuation is a difficulty in the

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<sup>38</sup> will not, however, cover the entire range of interpretive work. Wettersten (1991) starts as an exploration into why it has taken so long for Fleck to receive attention. It turns into a critique of paying too much attention to Fleck as a precursor to Kuhn, ignoring others:

The thesis that Kuhn is of great importance and that Fleck is the most important precursor of Kuhn can only be defended when a studious attempt is made to ignore the work of Michael Polanyi. Why should one ignore Polanyi's work precisely when one praises Kuhn's so highly? ... Why, then, prefer Kuhn to Polanyi? What was Kuhn's debt to Polanyi? And why ignore this question when discussing his debt to Fleck? (Wettersten 1991, 481).

Dieter Wittich's (1981) review of Fleck's *Genesis* is initially a synopsis of Fleck's work. But after five pages it becomes a Marxist critique:

L. Schaffer ... cites other bourgeois philosophers (besides Fleck) to show that the historical and social dimensions of scientific work had been known and discussed long before Kuhn. Unfortunately, Schaffer himself "overlooks" the authorities most important for his purpose, namely, K. Marx and F. Engels. Many of the ideas found in Fleck and Kuhn on scientific research were expounded by Marx and Engels much earlier and on a sounder philosophical basis... (Wittich 1981, 7).

And Patrick Heelan chooses to "elucidate Fleck in [the tradition] of hermeneutic-phenomenology" (Heelan 1986, 294). Although I will borrow from some of what Heelan writes, the half of his article presenting his particular interpretation I am not qualified to comment upon.

These articles are largely tangential to my own purposes, and will not be discussed here.

<sup>39</sup> Wolniewicz, after describing the lack of attention to Fleck's work by his peers, discusses the circularity of some of Fleck's concepts. For instance, Fleck defines a thought-style as a directed manner of perceiving. "Directed manner of perceiving" is perceiving in accordance with a style of thought. "Style of thought" then amounts to perceiving in accordance with a style of thought, a thoroughly empty and useless idea (Wolniewicz 1986, 219).



ontological category as well. There is not only a question of how one should conceptually distinguish one thought-style from another. Even if this were understood, then how a thought-style should be considered as an object or entity is a puzzle as well. Ontological individuation presumes an answer to whether entities such as thought-styles can or do exist, which calls up the issue of reification.

Harwood's review of Fleck's *Genesis* is not complimentary. This is largely due to the inconsistency and vagueness Harwood finds with the concepts Fleck uses. Fleck's analysis of thought-style and thought-collective, Harwood reports, is inconsistent (Harwood 1986, 181). Fleck speaks, for instance, of *the* thought-collective and *the* thought-style of modern science: "This is the situation in which contemporary science finds itself as a specific, thought-collective structure [*denkkollektives Gebilde*]" (Harwood 1986, 103). Harwood points out that this "refers to the thought-style of modern science, as opposed to those of religion, art, fashion, or politics" (Harwood 1986, 108). But there are also thought-styles of disciplines within the thought-style of science, as, for example, between style of physics and that of biology. And to make matters worse, Fleck discusses the thought styles of individual physicists and biologists (Harwood 1986, 108). The term "thought-style" applies equally well to individuals, groups, and sub-groups, or virtually anything.<sup>40</sup> The scope of the terms is not resolved throughout Fleck's text.<sup>41</sup>

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<sup>40</sup>Joseph Pitt's (1981) review of the *Genesis* strongly criticizes the lack of clarity in Fleck's text:

[T]he book is badly written. Fleck's style is almost aphoristic. There is little by way of recognizable argument for the views expressed. ... One of the major problems ... is that it is pretty well left to the reader to integrate a number of diverse items (Pitt 1981, 59).

But 14 years later, Pitt expresses an interest in going back to read it again (private conversation, 1995). Rosenkrantz (1981), in another review, dubs the 1935 work as a piece of "unsophisticated relativism" (Rosenkrantz 1981, 97).

<sup>41</sup>Harwood (1986) points out that "[q]uite apart from these ambiguities concerning the scope of the term 'thought-collective', the meaning of 'thought-style' is also disturbingly broad" (Harwood 1986, 181), a baffling claim since the prior material apparently dealt with the subject already. However, Harwood's points are still cogent, and follow from what has already been discussed. When the functions of the

Harwood is right; the concept of a thought-style can be predicated of virtually anything dealing with mentality. *Any* thought *t* presupposes a community and its cognitive tradition through which the formulation of *t* was learned. By Fleck's account, even a schizophrenic has an stylistic reality, or world, unto himself constructed from concepts recycled from other social origins (Fleck 1929, 49).

Perhaps the application of 'thought-style' could be clarified thus: the (thought-stylistic) cognitive/behavioral activity of an individual is a tokening of a certain thought-style type. The type is an abstraction from the behavior of the members as a whole, in the way that diseases were understood by some physicians in Fleck's time: as an (invented) abstraction from a variety of physical/ chemical/physiological characteristics of sick patients.<sup>42</sup>

With this in mind, Harwood's criticism of reification in Fleck's work is not assuredly on the mark. It is notoriously easy, Harwood writes, to "slip into the habit of regarding thought-style as a kind of intervening variable, *emerging* through the actions of the research community and, in turn, *feeding back* upon its inventors to channel their perception" (Harwood 1986, 183, emphasis Harwood's). But there is no need for a thought-style to attain any independent status (Harwood 1986, 183). And although appealing to Fleck's medical contemporaries offers a constructive avenue freeing Fleck of ontological commitments, Fleck's own words commit him to the opposite position.

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term are tallied, it means not much more than "presupposition," an idea that may have been novel in the 1930s but is not anything new today (Harwood 1986, 181). However, this soft constraint does not mesh well with the function Fleck frequently attributes to a thought-style: coercing or directing an individual's thoughts.

Jacobs also finds it regrettable that Fleck did not use more care in analyzing the concepts of thought collective and style, because individuating them is difficult. He notes, for instance, that in the *Genesis* the term "collective" is applied to science writ large, to particular disciplines within science, as well as to as few as two people (Jacobs 1987, 269). If one is inclined to say that the collective style imposes a constraint upon an individual's thought, how to identify the style responsible for creating a thought is difficult to understand, at best.

But Ian Hacking currently finds something to it (Hacking 1992, 3).

<sup>42</sup>An understanding of the physicians Chalubinski and Kramzstyk (see Chapter 1, B2).

Systems of opinions "must be regarded as units, as independent, style-permeated structures" (37), whose development is governed by "specific historical laws" (9). If a community is stable, its style becomes "fixed and formal in structure" (103) and exerts a force Fleck describes with the help of Ludwig Gumplowicz:

What actually thinks within a person is not the individual himself but his social community. The source of his thinking is not within himself ... His mind is structured, and necessarily so, under the influence of his ever-present social environment, and *he cannot think in any other way* (46, emphasis Fleck's).

A formal, fixed unit governed by laws that determines in the strictest sense an individual's thought certainly would be *something*. It is virtually impossible to understand Fleck as not reifying. But Elkana tries to provide an alternative interpretation.

Elkana concentrates primarily upon Fleck's (1929) "The Crisis of 'Reality'," a sociological response to Kurt Riezler's (1928) "Die Krise der 'Wirklichkeit'." He sees Fleck's project as accounting for the "*social origin of the individual thought-style*, i.e., the way the thought-collective expresses itself in the very thinking of the individual" (Elkana 1986, 310; emphasis Elkana's). The proper path, then, for understanding Fleck's work is to dismiss his distinction between individual cognition and the communal thought-style acting upon it. Thought-styles are quite dependent upon, or perhaps even reducible to, the community's individuals' thoughts:

[S]upport for this view can be seen in the fact that Fleck is aware that different thought-styles can coexist in the same person simultaneously typifying different cognitions. These thought-styles, although distinct, do influence each other by their very coexistence and frequent clashes thus showing their *dependence* on the individual (Elkana 1986, 314; emphasis Elkana's).

That is, thought-styles are peculiarities of different people's cognitive states. Individual thought and social thought do not refer to different things; the latter is equivalent to the expression(s) of the former. Ergo, Fleck does not reify.

But Elkana acknowledges that an opposite reading is easy to find:

[a] fundamental mistake has crept in, which misleads Fleck's readers. Fleck, having stated his case succinctly, actually saying that no external/internal dichotomy exists and that the thought-collective expresses itself through the thought-style of each cognition, he now says that "like all that is socially conditioned, the cognized has its own life independent of the individual" [1929, 49]. This saying, and other similar ones, have mislead also John Ziman... (Elkana 1986, 313).

Other similar statements by Fleck (1929) that Elkana does not include are "They [stylistic realities] develop, flourish, endure, waste away leading their own lives," (Fleck 1929, 50); "The relative independence of the cognized [that which is known, housed by the collective style] from the individual is well illustrated in the fact that different individuals frequently make the same discovery ... independently from one another" (Fleck 1929, 50); and "Natural science is the art of shaping a democratic reality and being directed by it -- thus being reshaped by it" (Fleck 1929, 54, emphasis deleted). In other words, for Fleck, thought-stylistic reality is not just an individual phenomenon.<sup>43</sup> It is described as something independent of the individuals, that leads its own life, and that re-acts upon the individuals creating it.

Elkana does not suggest what one should do with the quotes that contradict his claim; there is no alternative interpretation offered that would

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<sup>43</sup>This is an interpretation shared by Jacobs: "A pervasive motif of Fleck is that science is holistic in character or irreducible to individual psychology," although Fleck's arguments on the point are "poorly formulated and difficult to unravel" (Jacobs 1987, 269).

bend them into supporting Elkana's position. The thesis that Fleck does not reify is therefore weakened.<sup>44</sup>

Elkana argues that Fleck did not reify, because if he had, Fleck's thought-stylistic reality would be analogous to a Platonic/Popperian third-world reality, independent of individuals and groups alike (Elkana 1986, 314). But Fleck would have rejected an independent social reality with the same aplomb as he dismissed classical objective reality. The collective realities that groups construct are dependent upon individuals, insofar as groups are composed of individuals. But this reality is in some sense, by Fleck's lights, independent of individuals, because it is contained or supported somehow by the collective, and is therefore not independent of groups.<sup>45</sup> How to understand what that means without buying into reification and the ontological commitments this brings is difficult. More difficult still is the inconsistent triad Fleck tries to embrace that will be discussed further below:

- (1) No one can speak with complete authority about the status of the external world (i.e., all such claims are socially-relative and equally true) (48).
- (2) Beliefs about the external world are constructed (or, are "harmonies of illusion" (87)) through social dynamics.
- (3) Claim (2) is not just true relative to my own theory; it is (in fact) true (Fleck 1947a, 142).

So while it is apparent that Fleck aimed his social epistemology at describing

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<sup>44</sup>It is furthermore weakened because Elkana does not provide substantive justification from Fleck's text to support what Fleck allegedly believed. There are many citations and discussions of Kuhn (Elkana 1986, pp. 310, 311, 313-14), Francis Bacon (Elkana 1986, pp. 310, 312; fn. 11), and himself (Elkana 1986, fn. 12, 15). But the reasoning is opaque as to why Kuhn's or Bacon's view would have anything to say about Fleck's position.

<sup>45</sup> Rotenstreich makes the same point. With respect to a style of thinking, Fleck does not refer to a personal attitude of a scientist or of a thinker, an attitude which he describes as related to a moment and guided by personal bias. He wants to detach the style of thinking from the personal aspects which as a matter of fact are endowed with feelings; hence he refers to what he describes as a collective attitude (Rotenstreich 1986, 162).

human cognition, it is less likely that the method he used will let him reach that goal. Let us do what Fleck did not, and qualify his method as a useful fiction. While his story may not refer or describe anything with accuracy (since there is no way to check), it may be nonetheless sensible, and possibly useful. The next issue concerns how or if different styles of thought (either tokened by different individuals, or internalized within one individual) relate to one another. Since truth and meaning are relativized to within styles of thought for Fleck, can two people operating within different styles communicate?

Löwy believes there is no difficulty in cross-collective communication because Fleck's conceptual holism does not endanger his position with the problem of relativism.

[Fleck] was not ... a complete relativist (although his work is sometimes represented as such). Fleck did not believe that observations are radically theory-laden and he considered science as capable of cumulative improvement (Löwy 1988a, 135).<sup>46</sup>

Löwy does admit that Fleck is committed to *partial* relativism, because mutually exclusive and equally well-founded thought styles can co-exist at the same time. But how this is different from *complete* relativism is not explained. And the disease-related example she uses to explain how different (indeed, incommensurable) styles of thought work together assumes there is no difficulty, rather than explaining the difficulty away:

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<sup>46</sup>Löwy does not always argue that Fleck did not hold to complete incommensurability, and the relativism strong incommensurability would entail. Other times, Löwy discusses how "Fleck claimed that different 'thought-styles' lead not only to different classifications, but to different observations as well" (Löwy 1989, 41). This is an approach she traces through a tradition of philosophy of medicine in Poland before and during Fleck's, and would seem to call in as strong a degree of relativism as one could muster. For assistance here, as well as in her (1988a), Löwy appeals to Toulmin's (1986): "In the words of philosopher of science Stephen Toulmin: 'Fleck put all *a priori* explanations aside in favour of investigating *wirkliche Erkenntnis*'" (Löwy 1989, 39). Perhaps Löwy's reference to Toulmin supports an idea of non-relativism in an peculiar way: Fleck's view itself was/is not a relative one. But this, as I will discuss, is incorrect.

[I]ncommensurability of different thought-styles dealing with pathological phenomena can be explained at the cognitive level by the impossibility of finding a single explanatory theory able to embody the whole richness of pathological phenomena, and at the social level by the process of specialization in medicine, during which the increase in capacity to recognize some phenomena is necessarily accompanied by the loss to perceive others (Löwy 1988a, 143).

Depending upon whether a person is a bacteriologist or a general practitioner, how one diagnoses an illness is different. The bacteriologist sees microscopic rods and shapes of bacilli arranged in a certain pattern characteristic of diphtheria bacilli. The general practitioner works from the bacteriologist's authority to diagnose the illness (Löwy 1988a, 144-45, 147).

Löwy describes the bacteriologist and the physician as belonging to different thought-collectives (Löwy 1988a, 145). But if they belong to different collectives with different styles of thought, then any relativism would seemingly provide difficulties in their attempts to understand one another. This is not to reject the obvious -- that physicians and bacteriologists can and do communicate. The question is, if Fleck admits of a relativism or an incommensurability that is not complete, then where and how does he do so?<sup>47</sup> Löwy doesn't explain it; instead she assumes it because the only justification she offers for her attribution of Fleck's "partial" relativism is a vague parenthetical reference to Fleck's (1929) and (1935), and to Toulmin (1986).<sup>48</sup> But where, exactly, such support is to be found in Fleck's essays is not

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<sup>47</sup> Wittich finds incommensurability in Fleck's writings, although its occurrence is rare: "at best it refers to ideas of thought-styles which are historically very far removed from us" (Wittich 1986, 319).

<sup>48</sup> If one asks why is relativism or incommensurability a problem in Fleck's texts, Löwy's tone suggests it is because she does not prefer it. I can supply a reason why it would not be good for Fleck's view to have this critique.

One of the general goals he discusses often is that of developing a comparative epistemology.

[O]ne of the most important tasks in comparative epistemology is to find out how conceptions and hazy ideas pass from one thought style to another, and how they are preserved as enduring, rigid structures owing to a kind of harmony of illusions. It is

pointed out.<sup>49</sup> Toulmin (1986) does lend itself to Löwy's view, but since it concerns the passive associations of knowledge, it will be discussed in the next section.

## 2. *Development of a Fact I*

There are two main ways scholars have understood Fleck's presentation of the development of a fact. First, there is the idea that a fact develops from the circulation of a thought through a collective. This method is adopted primarily by Ilana Löwy (1988a) and Robert K. Merton (1983). Second, many writers find that Fleck's "passive elements" of knowledge provide a niche for establishing an objective world or an objective kind of knowledge. The passive route is taken by authors such as Robert K. Merton (1983),

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only by such a comparison and investigation of the relevant interactions that we can begin to understand our own era (*Genesis*, 28; cf. 22, 41).

But without designating from what standpoint one might legitimately evaluate or even discuss different styles of thought, the capacity to have any authoritative voice on how styles of thought *compare* is precluded. Indeed, rejecting the idea that there *is* any privileged standpoint, as Fleck himself does prohibits reaching the goal he conceives. At least, the situation obtains unless Fleck means something besides what would ordinarily be understood by "comparative epistemology." Intuitively, "comparative epistemology" would entail an external standpoint into which propositions from different conceptual frameworks could be translated without changing the meaning, so that they may be semantically compared.

Fleck does not propose an alternative, and I would suspect that he had none in mind. Instead, I believe that he simply did not think the line of reasoning through. Both ideas (no privileged standpoint, comparative epistemology) seemed good, so he asserted them. But since there is no chain of argument connecting the two together in the texts, it appears that the connection has not been thought through.

Fleck's program of developing a comparative epistemology is also discussed in David Bloor's (1986) "Some Determinants of Cognitive Style in Science." There Bloor proposes a comparative finite analysis of different styles of thinking by examining responses to a "stranger," or an anomaly by different groups.

<sup>49</sup>Löwy concedes that there are imperfections with the amorphous meaning of "thought-style" and "thought-collective." She cites external factors as "partial explanations" for the difficulties with Fleck's text: his life in a provincial town, his marginal institutional position, and his lack of contact with other philosophers and historians of science (Löwy 1988a, 150-151). This is an apologetic move made elsewhere:

Fleck's attempt was very fragmentary; his marginality, his lack of sustained contacts with the philosophical and sociological milieu in Poland and abroad, and the practical difficulties in conducting his studies did not allow him to further develop the ideas indicated in this work (Löwy 1988b, 351).

How this characterization of Fleck is supported by available biographical data is not certain.



Jonathan Harwood (1986), Gunnar Andersson (1984), Nathan Rotenstreich (1986), Stephen Toulmin (1986), and Patrick A. Heelan (1986). The interpretation given to Fleck's passive elements of knowledge does vary, however, from writer to writer. In this section I examine the circulatory theory. In Section 3, I take up the passive theory.

Robert Merton is one of the editors of the English translation of Fleck's *Genesis* and his "Sociology of Science in Poland," an exposition of Polish sociologists of science. Part of Merton's discussion of Fleck is geared toward establishing an objective component in his work:

Fleck avoids the radical relativism of much contemporary theorizing along these [social constructivist] lines in recognizing that along with the social interactions between individuals there is also, for him, "the objective reality (that which is to be known)" (Merton 1983, 189).

The sociologist's task is to explain the process by which "that which is known" occurs. Fleck accounts for this process, Merton believes, by using the concepts of thought-style and thought-collective, particularly though the interplay between the esoteric and the exoteric circles within a collective. The experts

are informed in the greatest depth along with general experts at work on related problems; in due course, their findings are transmitted to the exoteric circle, who take these as fixed and unquestionable (Merton 1983, 190).

This can be seen in the style of publications within the different circles. Specialized journal literature contains tentative suggestions ("it appears," "this seems"). It is written from a personal perspective, with reference to the particular person or group conducting the test or writing the article.

This information is translated for the layperson into the impersonal and indubitable style of a textbook or popular essay. To be cognitively

digestible the data are simplified; detail and controversy are omitted. Merton quotes Fleck on this:

If a fact is taken to mean something fixed and proven, it exists only in vademecum science. The preliminary stage of disjointed signals of resistance within journal science really constitutes only the predisposition for a fact. Later, at the stage of everyday popular knowledge, the fact becomes incarnated as an immediately perceptible object of reality (*Genesis* , 124-125, quoted in Merton 1983, 190).

This is to say that the certainty one would attribute to a fact exists at the point where the likelihood of other possible states of affairs is low: there are no probability measures, opposing viewpoints and the like. The judgments of the professionals circulate down through the masses, simplifying and becoming more "obvious" along the way.

Löwy also takes this interpretation. In the previous section, I discussed her views on relativism and incommensurability in Fleck's philosophy. A bacteriologist and a general practitioner were described as representatives of distinct collectives and styles of thought: "For Fleck, the specialists (the esoteric circle) and the general practitioner (the exoteric circle) belong to distinct thought collectives" (Löwy 1988a, 145). The fact that patient *x* has diphtheria then occurs in the simplification of the specialist's prognosis. Her idea is transmitted from one style of thinking to another,

characterized by the omission of detail and of controversial opinions which produces an artificial simplification. The 'genesis and development of a scientific fact' described by Fleck -- in this case the unambiguous affirmation that a given person is suffering from diphtheria -- is the result of the circulation of ideas through thought collectives (Löwy 1988a, 145).<sup>50</sup>

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<sup>50</sup>Andersson also contributes to a circulatory theory, but differently than Merton and Löwy. The latter construe the emergence of a fact at the end of a collective cycle, when the data have been simplified and rendered less contestable for the level of the layman. The "fact" is obvious or intuitive; it is unproblematic at the level of common sense. On Andersson's view, a fact emerges when as an effect

While Löwy and Merton agree that the simplification of an idea renders that idea as a fact-candidate, it is contestable whether that rendition of Fleck's view is either (a) warranted or (b) true to what Fleck means. Concerning (a), by Löwy's and Merton's portrayals of Fleck, it would seem that the facts exist primarily at the level of the layperson while specialists operate in a sort of metaphysical twilight zone. But the overall thesis in Fleck's *Genesis* concerns the establishment of a professional, and not a layperson's, fact: the relation (so-construed by the bacteriological community) of the Wassermann reaction to syphilis. The textual justification for Löwy's and Merton's interpretations, then, is relatively scant.

Hence I am suspicious as to (b): I do not think that Merton and Löwy accurately capture what Fleck means overall. There is a role for simplification and the eradication of opposing viewpoints in establishing a fact, but it is not singular in its establishment. This point will be pursued further in Chapter 3.

### *3. Development of a Fact II*

Most writers find the grounding for Fleck's facts in his use of active and passive linkages of knowledge. To recapitulate, active components are those concepts which are historically, socially, or psychologically explicable. Passive elements are those elements of knowledge that are not so explicable. But what *that* means is unclear, and has received many interpretations.

In "Ludwik Fleck and the Historical Interpretation of Science," Toulmin remarks that the nature of scientific facts from the *Genesis* is "the most opaque part of his position" (Toulmin 1986, 227). He uses Fleck's distinction between passive and active elements in knowledge in order to clear up Fleck's points.

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or a recognizable entity, "the end product [is in the] form of reproducible observations of fixed patterns (*gestalts*) in accordance with a certain style of thought" (Andersson 1984, 29). Given that the average layman is not likely to perform a scientific observation, but read about it instead, it seems plausible that such "reproducible observations" or facts pertain to the professional scientific community.

Toulmin explains that passive elements appear "inevitable," or demand the "passive acceptance whether one holds [a particular] theory or not" (Toulmin 1986, 279). The active elements are what is made of experience through the exercise of judgment; they involve conceptual relations actively built into the theories scientists use (Toulmin 1986, 279). A fact for Fleck, as Toulmin sees it, is strongly associated with Fleck's passive elements. It is apparently inevitable due to one of the following three reasons.

(1) A fact may be inescapable because it is conceptually certain. It "expresses a conceptual relation actively built into the theories that shape the *Denkstil* current in the professional 'thought community' of the science concerned" (Toulmin 1986, 279).

(2) A fact may be indisputable because it captures a certain, or sure, theory-neutral relation in the world. A fact expresses "an empirical relation that demands passive acceptance from all scientists working in that science regardless of their current theoretical views" (Toulmin 1986, 279).

Or, (3) a fact may be accepted as such for both reasons: an active conceptual or theoretical relation which any person (or scientist) must agree captures a relation among objects in the world (Toulmin 1986, 279).

Toulmin finds the first possibility unagreeable.

Only if we regard all scientific facts as being *facts of the first kind* -- i.e., as being radically "theory laden" -- shall we thus be justified in saying that every new theory, or *Denkstil*, brings with a *totally new set of "facts,"* which has to be defined and described in its own novel terms (Toulmin 1986, 281; emphasis Toulmin's).

He believes that Fleck "certainly did not want to cut links between successive theoretical accounts of the same phenomena; nor did he imply that the observation to which any scientific fact refers are radically theory laden" (Toulmin 1986, 281). He understands Fleck to have subscribed to the third possibility mentioned above: facts are a function of variable theoretical

constraints and stable relations obtaining in the world.

[O]n a deeper level, we can surely say that elementary chemistry, molecular physics and quantum mechanics have ... given us progressively more discriminating accounts of *one and the same* set of facts (Toulmin 1986, 281; emphasis Toulmin's).

I have to disagree. In 1947 Fleck did write that science is progressing toward some point (cf. footnote 65, below). But that idea is not in his *Genesis*: "Here every discovery is actually a *recreation of the whole world* as construed by a thought collective" (102, emphasis mine).

Clearly, on the basis of the medical constructivist/conceptual holist context in which Fleck wrote, and on the basis of Fleck's own words, it is absurd to think that he did not hold what would be called a theory-ladenness position. Recall that, for Fleck, the world as conceived through the thought-style of one's thought-collectives is as much of the world one can reach. For a new discovery, or a new fact, to emerge, the style of thought must first change. So when there is a new discovery, facilitated by a thought-style's change, there is also, in a sense, a world change.<sup>51</sup>

This is the state of the argument supporting Löwy's claim that Fleck

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<sup>51</sup> This is largely due to Fleck's equation of "that which is known" to his version of the objective world. As such, Wittich is sympathetic to my own interpretation. To Wittich, all knowledge by Fleck's lights is "necessarily socially and historically conditioned ... [and Fleck turns this insight] against the objectivity of the content of knowledge" (Wittich 1986, 320). Because of the contextualization Fleck promotes, Wittich dubs him an "epistemological relativist." And in blurring of the epistemological/metaphysical distinction, Fleck would similarly be a metaphysical relativist.

But Toulmin believes that there is some hope of evaluating or measuring different styles of thought: "a case can still be made out for saying, in a more qualitative sense, that -- despite all the differences in their 'styles of thought' - physicists today do 'know more than' those who preceded them, and similarly for the other natural sciences" (Toulmin 1986, 285). Unfortunately, Toulmin does not suggest any measure to compare them. The only suggestion is provided by Löwy (1988b), an article focussing upon Fleck's (1937) "Some Specific Features of the Serological Way of Thinking: A Methodological Study." There Fleck identifies bibliographical citations in medical articles as a measurable indication of intra-communal communication. But this is not obviously helpful for measuring the content of a group's knowledge or for ranking their world-views.

In any event, Kevin White would disagree with Löwy, and, apparently, Fleck. According to his interpretation of Fleck's work, there can be no ground for comparison or evaluation. "[T]hought-styles do not admit of some weighing from an Archimedean point, but like Kuhn's paradigms are incommensurable" (White 1991, 58). But because White's analysis of Fleck's work is not altogether systematic or thorough, for present purposes no substantive elaboration upon his view is taken. Along a similar line as White's, though, see Wolniewicz 1986, 219-220.

was not a complete relativist. But Toulmin is not the only person trying to carve some trans-theoretical niche in Fleck's views. In "Problems in Ludwik Fleck's Conception of Science," Andersson follows suit.<sup>52</sup>

Andersson sees Fleck's facts as emerging through an historico-social synthesis of active and passive conceptual connections. Under the rubric of "active associations," Andersson places hypotheses and definitions:

Fleck says that the active linkages of concepts in the form of hypotheses and definitions must be chosen by us ... the choice [determined] by the style of thought that is dominant in a certain community of thought (Andersson 1984, 28).<sup>53</sup>

By "passive associations," Fleck is taken to mean connections which are

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<sup>52</sup> Andersson identifies Fleck's main thesis as "science is influenced by social and historical factors" (Andersson 1984, 26). From the analysis Fleck develops, certain problems emerge, namely: (1) demarcating between a scientific and non-scientific thought-style, (2) ascertaining the degree to which experience, perception, and judgments are theory (or thought-style) laden, and (3) the possibility of criticism, if the adjudicative role of perception and judgment is thoroughly biased by theory (Andersson 1984, 26-27). Andersson uses Fleck's active and to define and resolve these three problem areas.

There is at least one place where Fleck identifies the characteristics of the scientific collective that Andersson does not employ:

Neither [logical structure of the system, the consensus of the collective, nor practical applicability is the touchstone of science.] The only touchstone of science is in the specific features of scientific cognition: the historic singularity of their development, the structure of the relevant thought-style. It is only by the comparative method, in the framework of general sociology of thinking that we can get acquainted with the features of scientific thinking (Fleck 1946, 126-127).

The problem with this is that even though it is ostensibly a definition of a scientific style of thought, it does little to distinguish the scientific from other styles.

In passing, Harwood notes that Schnelle finds something important in Fleck's distinction between active and passive linkages (Harwood 1986, 184). However, insofar as an active connection is that which seems arbitrary and passive connections are those which seem necessary, Harwood sees nothing extraordinary.

Possibly novel in their day, active and passive are little more than a sociological reformulation of the concepts 'subjective' and 'objective'. That is, active or subjective connections are characteristic of narrowly institutionalized (thus contentious) knowledge-claims. Broadly institutionalized (thus unexceptionable) knowledge-claims embody passive or objective connections (Harwood 1986, 184).

What Harwood does find interesting is "Fleck's suggestion that presuppositions of a thought-style may arise from non-rational commitment to primitive ideas or metaphors," such as, for instance, the idea of 'attack and defence' in the conceptualization of infectious disease (Harwood 1986, 182). Scientific concepts bear the marks of their history, in addition to containing the seeds of their future.

<sup>53</sup> Andersson does not identify just where Fleck says this.

necessary or compulsory; they "exercise a 'constraint of thought'. Thus, observation and experiments to some extent limit the choice of hypotheses" (Andersson 1984, 28).

Andersson next asks, in the face of apparent cognitive determinism, how a critique can be recognized as genuine, as opposed to incomprehensible? If thinking and the status of the world are so ingrained by one's style of thought, then something not conforming to that would seem to be unrecognizable and unnoticed. Andersson's response is that Fleck unwittingly pays too much attention to cognitive factors contributing to facts believed to obtain in the world. Thought-stylistic constraints aside,

Fleck stresses that scientific facts [depend] on objective factors. In order to keep to Fleck's analogy from gestalt psychology: the gestalt perceived when a picture is observed does not only depend on the style of thought of the observer but also on the objective properties of the picture.... [W]ith his insistence on the importance of styles of thought and stylistic conformity, Fleck completely ignores the objective factors which, according to what he says in other places, *also* determine scientific facts (Andersson 1984, 29-30; emphasis Andersson's).

By associating observation and experiment with Fleck's passive elements of knowledge, Andersson constructs a method for adjudicating scientific conflicts about what is the case. What one ought to do is derive from a contentious style of thought an unproblematic proposition to be tested.<sup>54</sup> To illustrate this, Andersson uses an example from Fleck's *Genesis*: the determination of whether different forms of streptococcus were genetically related. By Andersson's interpretation, the genetic style represents the problematic style: one could not immediately perceive by observation or

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<sup>54</sup> Andersson employs some of Karl Popper's ideas in the form of what he calls "empirical criticism" in order to resolve the three difficulties he presents. For the demarcation problem, identifying scientific styles of thought is done by seeking those communities of persons who have as one of their working hypotheses that their statements be testable. In other words, should there be a hypothesis which is

experiment whether or not there was a *genetic* relationship. Due to this difficulty, the experimenters switch to a simpler approach: testing generations of colonies for continuity in color or texture, and piggyback the difficult relation atop the simpler one. The original hypothesis, that differently colored colonies were genetically distinct, turned out to be false; the distinction came to be founded on the colonies' texture ("flocculent" versus "smooth").

The immediately tested [i.e., passive] propositions were simple propositions, like "this colony is yellow" or "this colony is flocky". These simple propositions did not depend on any problematic style of thought or any kind of indoctrination or conversion. By using such simple and unproblematic statements, the more problematic and hypothetical statements about genetic differences could be tested (Andersson 1984, 31)

Fleck's interpreted method of experimentation, discovery, and fact-finding are, by Andersson's lights, "logical and rational" processes (Andersson 1984, 31). The passive elements of observation and experiment lock the investigator and the world together with a test in a nefariously neutral ground. Andersson's interpretation parallels Toulmin's (1986) because Andersson finds Fleck's text amenable to upholding a "same set of facts," or an objective gestalt-in-itself. But neither Toulmin nor Andersson quote or cite where Fleck emphasizes such objective factors. This is unsurprising, because Fleck held no such position. If he had, that surely would have stymied alternative interpretations of what he means by "passive" associations or linkages, such as the ones below.

The passive and the active associations in knowledge and their relations to facts appear only briefly in Heelan's (1986) "Fleck's Contributions to Epistemology." Even so, it is an informative attempt to capture Fleck's

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contested or uncertain, one should be able to derive a testable version of it which is independent of the theory in question (Andersson 1984, 30).



sense of the terms, also reflected briefly in Rotenstreich's (1986) "Proto-ideas and Their Aftermath." These sources provide another way of representing what Fleck meant.

Heelan contrasts Fleck's position to that of scientific realism in order to highlight the difference between the traditional sense of a mind-independent reality and Fleck's attitude toward facts.

[S]cientific reality is not what exists independently of human systems of inquiry; scientific facts are recognized by being the passive response of the world (experienced as 'given') to the active deployment of a historically changing system of perceptual inquiry - a performance -- initiated and perfected by a collective of scientific researchers (Heelan 1986, 287).

According to Heelan (1986), Fleck's facts are a function of the perceptual powers, skills and practices of the scientific community's members. Since one's ability to perceive a fact is determined by his or her degree of training, the closer a person is to the core or esoteric circle, the more manifest a scientific fact will be. The scientist will "with objectivity [perceive] the scientific structures that the process of inquiry has made manifest" although the "objectivity" is apparent and not true to what is in actuality in the world (Heelan 1986, 290). The further one is removed from the inner (specialists') circle, the less direct contact one has with the fact. If

one is distant from the core research group, one does not enjoy direct perceptual access to scientific facts, but one may come nevertheless to accept on the authority of the culture the reports of scientific experts about such facts as objectively true (Heelan 1986, 290).

This is the flip side to the view presented in Section 3. There I discussed how Löwy and Merton found Fleck's facts in the uncomplicated region of the layperson. Here, Heelan understands a fact to exist in the specialist's proximity to the phenomena of investigation.

Another way of phrasing Heelan's reading of Fleck is that the passive response or crystallization of facts from the world emerges through the active casting of scientific conceptual and experimental nets upon the world. Thus, the scientific facts caught are a function of the scientists' cognitive nets and the skill with which these devices are wielded. Since scientific theories and scientific skills are historically and culturally shaped, so are the facts they produce. Therefore, facts change as theories and cultures do:

If scientific theories articulate physical reality by making its structures manifest to 'stylized' perception in and through appropriate observational procedures or 'readable technologies,' and if scientific accounts are historical, then physical reality is like social reality, it is roughly just that part of reality that is mediated by scientific instruments or readable technologies (Heelan 1986, 303; cf. 288).<sup>55</sup>

What I believe Heelan means is that all observational procedures are social. Physical reality is made manifest through (social) observational procedures. Therefore, physical reality is *manifested* through social procedures. I do not know if Heelan would agree with this conclusion. The "social reality" he imports has no corresponding name in my rendition, nor, I think, should it unless one bifurcates the social from the physical. In any event, it is different from the account discussed by Andersson and Toulmin above. There is no one constant set of facts; they shift and change with the

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<sup>55</sup>The function of the second premise ("scientific accounts are historical") is curious, as it doesn't seem to give Heelan the conclusion he wants. The formulation of his argument is:

1. Scientific theories make physical structures of nature manifest through observational procedures.
2. Observational procedures are socially arranged.
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3. Scientific theories make physical structures of nature manifest through social arrangements.

There is no obvious "social reality" that derives from the argument, nor an equation of physical reality to social reality, whatever "social reality" might be. The claim that physical structures are gleaned through social groups such as scientific communities is unproblematic. The difficulty comes when the physical state of the world (independent of social groups) is claimed to be one and the same as the product of social groups, something Heelan (and Fleck) tries to do.

(social) concepts and technologies used in investigating them.

Rotenstreich echoes Heelan's themes, albeit not in the same words. He acknowledges that, for Fleck, the conceptual formulation of a fact is constrained by the limits imposed by a collective's style of thought. Alternative views are discouraged by the force of the collective's thought-style. Another constraint is "a kind of primary resistance, when we first encounter something to be scientifically and thus deliberately explored" (Rotenstreich 1986, 164). Rotenstreich targets that which thought is aimed at, i.e., the phenomenal object of investigation. Although it can only be meaningfully discussed in terms of experience (as opposed to what causes the experience), such a perceptual object pushes back to the probe of a scientific investigation or experiment. The general line of scientific work for Fleck, according to Rotenstreich, is to reach "the greatest resistance of thinking versus the smallest arbitrariness of thinking" (Rotenstreich 1986, 164).

This resistance is identified with the signaling of a fact. The aim of the scientific enterprise is to hone thought into a precise direction, meeting this with the greatest amount of empirical push. Thus, "what is called a fact in the sphere of knowledge is called a 'viso' of resistance which as such is pushed against the free arbitrariness of thinking" (Rotenstreich 1986, 164). This viso, or "push," is akin to the passive response of the world discussed by Heelan. Conceptual and experimental constraints on scientists are tantamount to Fleck's active elements in knowledge.

How is Rotenstreich's "push" or "viso" to be understood, or, if possible, measured in order to see whether an investigation is progressing or not? As is, it is very unformulated, and insofar as it purports to offer a criterion for success (the greatest empirical bang for the least conceptual ambiguity), as a criterion it fails on its own accord. It is conceptually unclear ("viso" is not defined in Rotenstreich's text). In so being, understanding what should count as an empirical "push, or "viso," is uncertain.

Merton's (1983) interpretation of Fleck's passive linkages is different from all of those above. Merton claims that Fleck avoids the relativism prevalent in social constructivism because Fleck recognizes that besides social interactions among the members of a collective, there is also an objective reality: that which is to be known (Merton 1983, 189). Thought directed at this "objective" reality consists of two components: an objective part and a subjective part which, according to Merton, Fleck calls "passive" and "active" respectively.

The active associations are those which the observer, as a card-carrying member of a collective, brings to the world or what is to be known. Passive elements are the necessary consequences of certain active presuppositions, "which operate as an objective constraint" (Merton 1983, 190). Necessary consequences, as Merton sees it, are roughly translatable into logical consequences: claims or beliefs which *must* follow once certain presuppositions are assumed. To illustrate, Merton uses the example discussed in Section A above, with atomic weights.

After the atomic weight of oxygen had been "conventionally adopted" as 16, the atomic weight of hydrogen was then fixed and fully (passively) determined as 1.008 (Merton 1983, 190). The passive element, according to Merton,

introduces and maintains the objective component in what flows from the thought style. It is not the case, as a radical epistemological relativism would have it that anything and everything goes. (Merton 1983, 190)

Like Löwy (discussed in Section 2B above), Merton wants to dispel Fleck's account of theory-ladenness and relativism. He does this by limiting the passive associations to logical consequences of active assumptions. The logical consequences would presumably be trans-theoretical: no matter what the active assumptions, or the framework of the investigator, once the

assumptions are known, one could deduce what follows. If an experimenter calibrates her theoretical apparatus to register "16" in the presence of a certain sort of phenomenon, when 1/16 of that phenomenon appears it is a logical matter that it will register "1".

However, the relativism Merton portrays is wildly radical; one is inclined to characterize it as a straw man. The relativism that Fleck's view encourages is not that *anything*, with no constraints whatsoever, goes. The truth or meaning of a particular claim is *relative to* the conceptual system/framework it comes from. But it does not follow from this that everything and anything can be said meaningfully or thought to be true. If a claim cannot be contextualized to a framework, then it cannot have any meaning, nor could it be said to be true.

Merton's view also risks missing the point of Fleck's relativism. Given certain active assumptions, certain consequences passively follow. Such consequences are indeed contingent upon the assumptions made. But the apparent certainty or solidity of the passive conclusions Fleck tries to capture is also a function of the degree to which the conclusions have been circulated through and supported by the consensus of the community in question. For instance, the distance between the earth and the Sun, 1 astronomical unit (AU) is now understood to be 93 million miles. For anyone familiar with astronomical terminology, the appearance of the symbol '1 AU' automatically calls up '93 million miles'; that is what '1 AU' means. However, the distance between the Sun and the earth was not always understood to be 93 million miles; developments in astronomy after the Second World War helped to pin this relation down. In other words, given other certain active presuppositions such as what is meant by 'Sun', 'earth', what is used to measure the distance between the Sun and the earth, the answer in ancient Greece is different than in 1965.

What automatically, or quasi-necessarily, comes to mind to a person in a community given a certain set of assumptions is not simply a matter of logic-chopping. The process also depends upon the degree of empirical research and social confidence that the relation investigated is sound. The relation found may be portrayed such that it has all the appearance of necessity, but it is not simply a matter of *a priori* certainty. If it were, new discoveries or new facts would be found through of armchair derivations. But that is not the case.

The cogent point Merton makes is this: a theory imposes certain cognitive constraints, recognized in terms of conceptual habits, upon the subject. Given these active assumptions, other results have to follow; i.e., if one is speaking in terms, say, of molecules, the terminology of the consequences that follow are suitably constrained. The active assumptions set the domain of "what is to be known": the external world. Knowledge of that world comes through the effort of finding the passive results or propositions. Furthermore, for Fleck those "passive" consequences may also serve as active assumptions for a new and different problem (*Genesis*, 95).

Thus far the discussion has covered difficulties with Fleck's concepts of thought-collective and thought-style.<sup>56</sup> And any incompatibility or

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<sup>56</sup>Two final examinations should be looked at briefly. Their points are not discussed in the main text because their points do not quite cohere with what else has been covered.

The first is a proposal by Rotenstreich. He asserts not only that the facts of science and of everyday life ought to be distinguished (unproblematic), but he also claims that facts of everyday life "do not lend themselves to a transformation into objects of an epistemological investigation" (Rotenstreich 1986, 163). But this is not obviously true. Since since Fleck conceived of a thought-style of "everyday life, of which everyone is a member" (Fleck 1936, 102) it appears that everyday facts would indeed be as examinable with Fleck's method as a scientific one.

Like others before, David Stump attempts to carve a niche for a thought- and theory-independent world out of Fleck's theories. Stump, like Heelan, identifies the importance of skill in Fleck's methodology. Indeed, it is along these lines that Stump formulates one of Fleck's main theses: "The central argument for [Fleck's] provocative claim that facts have a history is that skill is needed to perform serological tests" (Stump 1988, 303). While conceding to many of Fleck's claims, such as theory-laden observation and social influences on scientific practice, Stump does not agree that

knowledge is culturally conditioned in the sense that it depends on a particular historical thought-style, where "thought-style" is Fleck's catch phrase for methods, ontologies, and

incommensurability between conflicting styles of thought carries over into the metaphysical realm, since what is *thought*, or *conceived* to be is as good as what *is* for Fleck. Some authors have tried to pry an objective trans-conceptual world into Fleck's views, but I will show that throughout Fleck's work this is a view he does not hold. This is an especially keen blow to non-relativists since the passive linkages Fleck describes in the *Genesis* do not obviously play a role that the authors subscribe to him. In order to justify my views, the next chapter looks to his other works in order to more fully show his constructivism and his relativism.

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theoretical commitments which change historically, much like Kuhn's (1974) "disciplinary matrix" and Laudan's (1978) "research traditions". (Stump 1988, 303)

This is not an internal critique of Fleck's work. Rather, it is an acknowledgement of what Fleck says, and then a rejection of it. This occurs again when Stump responds to Fleck's rejection of objective elements in knowledge, again from an alternative standpoint:

Fleck denies that there is any accessible neutral elements in knowledge. ... [I]f we deny that independent facts make our sentences true, we may seem forced to say that truth is determined within a style of reasoning and are faced with relativism and perhaps even a self-referential paradox (Stump 1988, 305).

Fleck is admittedly misleading in this respect: he speaks frequently of objective factors, but without always specifying his own sense of "objective". Stump notes that Fleck wavers back and forth between relativistic language and objective language, but it is not possible for him to define the objective elements of knowledge as a passive element because his own position is that it is impossible to separate passive and active elements of our knowledge (Stump 1988, 305).

In other words, it is impossible by Fleck's view to find a theory-independent means for justifying the truth of a knowledge claim. Even if there were objective factors, in the classical sense, in scientific knowledge, they are in principle inseparable from historical, social, and theoretical elements. Hence any objective element to knowledge would be inaccessible, but Stump invokes Hacking's experimental realism to try and navigate a way out of the quandry.

In the process, Stump's disagreement with Fleck's use of "fact" becomes clear.

The Wasserman reaction is a phenomena, not a fact. Its invention has a history and is the creation of human skill, but the creation of phenomena is shown by Hacking to be less mysterious than the creation of facts. Experimentalists do not create facts, they add phenomena to the world. We should note that while the first part of Fleck's book also deals with a creation, what is created is the modern concept of syphilis, not a fact (Stump 1988, 305).

The conceptual relation of the Wassermann reaction to the presence of syphilis is not, according to Stump, a fact. But it *is* what constitutes a fact for Fleck. With this in mind, the effectiveness of Stump's critique of Fleck's work is unclear: the lack of an obvious alternative for Fleck's proposal renders the critique as simple nay-saying. Otherwise, if Stump's presentation is intended as an examination to the inner working of Fleck's arguments, then one must conclude that Stump did not see Fleck's point.

### Chapter 3

#### Fleck's Others Works, and His Interpreters Revisited

Thus far, the work covered has centered upon Fleck's *Genesis*, and the many responses to his book that the text encourages. These interpretations have been warranted to the degree they portray Fleck's view of a fact as a fixed conceptual relation with a corresponding perceptual image. A fact becomes established due to the simplification of ideas given by the professionals within a field, ideas which are subsequently learned and accepted by laypersons. There is also a sense in which Fleck is understood to view a fact as an obvious, or quasi-logically necessary, consequence of certain assumptions characteristic of a particular historically-developed community. By either account, for Fleck facts are relative. Their existence and significance are relative to the society in which the ideas are circulated. They are relative to the conceptual or theoretical framework restricting the domain of inquiry. To show that this view is justified, in contrast with interpretations claiming Fleck was not a relativist, this chapter examines Fleck's other writings. After filling in his position, sketched briefly at the beginning of Chapter I, this chapter concludes by re-evaluating Fleck's stance.

##### *A. The Foundations: A Fortification?*

The first area of interest is Fleck's foundational concepts of thought-collective and thought-style. Harwood (1986) rightly critiques the absence of clarity in Fleck's use of "thought-collective." Its meaning is bypassed by other writers addressing different issues. But since the idea of a thought-style, which is frequently discussed, rides upon the concept of a thought-collective, difficulties with the latter transmit to the former. Part of the problem is that the terms are largely defined in terms of each other, so the two concepts are not easily distinguished. Therefore, the first task is to distinguish between them in order to clarify each. This is worthwhile since the remainder of



Fleck's program hinges upon this conceptual juncture. The next subsection takes up the idea of the thought-collective, followed by a fuller treatment of the thought-style.

### *1. The Collective*

In the first chapter I described Fleck's thought-collective as a group of people who share a bond of communication, a bond stabilized around an idea or system of ideas that the members of the group mutually support. While admirable in its brevity, this description is unsatisfactory, because *how* stable and for how long is unexplained. Should it imply that I and a total stranger asking me for directions form a collective? On a larger scale, does the discipline of astronomy as a whole form a collective? Or, since radio and optical astronomers use different devices and ask different questions within the domain of astronomy, are there only sub-collectives with no overarching means of identifying them as proper subsets of a larger group? The material below has been selected with these questions in mind.

The most helpful clue appears in Fleck's (1936) "The Problems of Epistemology":

The fundamental feature of all stable collectives is their more or less exact delimitation. The thought-collective is delimited formally by customs and statutes which subordinate the admission of members to some conditions and ceremonies, such as for the members of specific religions, special professions, etc. Also the special disciplines have conditions of admission which are by no means logically motivated, but merely traditional. Among the formal factors limiting thought-collectives, we can also number special words ... used in that group, sometimes special phrases, and even a separate language (Latin!). (Fleck 1936, 99)

Here there are two things outstanding in Fleck's description. The first is time. This underlies the historical, traditional, social, behavioral, and linguistic dimensions of group life. These are manifested in rites of passage one passes in learning to perform in accordance with group standards. A community

"raise[s] its young participants" in its discipline, fostering a sense of "mutual confidence" through their shared language, conceptual categories, view of life and how to act in it (Fleck 1947, 148). Trust and language-learning do not occur immediately. Existing through a substantial amount of time is therefore a prerequisite condition for a stable collective.<sup>57</sup>

A second point of interest is that Fleck appears to view collectives as closed, or delimited, units. However, as discussed earlier it is suspect whether a collective should be understood that way. Furthermore, Fleck points out that there are varying degrees of initiation into a collective (Fleck 1936, 101). Membership occurs by degrees, which is tantamount to denying any necessary and sufficient conditions for identifying a person as a member of a particular collective. So a boundary outlining the range of a collective would not be simple to discern.

Perhaps if we use an historical tradition of teaching and practice, and identify a domain of people thereby affected, we could label that group as a thought collective. A person may be considered affected to the degree he or she is exposed to and tested on relevant selections of literature and practice. But how will these people be recognized? Should people be included as members who, having been trained within a certain tradition, leaves that area never again to return? Should a person training herself in the texts and recreating the habits of an historical tradition (such as the alchemists) be considered a member of a collective that no longer exists? While intuitively one would want to say those people should not be counted, there is no ready justification. By Fleck's construal, any contact whatsoever with a discipline (by reading a popular book, or watching a PBS special on television) automatically enlists that person in that ranks of that community.

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<sup>57</sup>"Substantial" means it takes at least long enough to train a new generation of members in its style. Of course, this holds only for the "interesting" ones lasting long enough to be studied (given that such an investigation would be fruitful). However, Fleck attributes these concepts with an unfortunate generosity, even to a schizophrenic, living in his or her own reality because the patient employs thought-stylized concepts (Fleck 1929, 49). In aiming to account for everything, Fleck risks saying nothing substantive.

An evolved system of training may be important for identifying members who have passed through its ranks. It is also important for setting a common ground for communication among the members. But it does not provide a criterion for telling when someone is *not* a member, enabling one to tell the members of different communities apart. So, although Fleck does not say as much, I propose the following: a person is a member of a particular collective only if (a) the training of the collective is *continuous* and (b) the person exhibits *active involvement* by performing in accordance with the community's standards in the appropriate situations. Let me explicate these criteria in turn.

By 'continuous' I mean that the discipline has a lineage of training its newcomers in a way of getting around in the world. This would discourage counting people as members who are learning the language and manners of groups that no longer exist, or groups that do exist but are studied from a meta-level as the object of investigation. If the continuity of cognitive and behavioral performance is broken because of either collective dormancy or a change in the level at which collective standards are made manifest, then a person is not a member of that collective. This brings in the second ground for counting a person as a member of a collective.

In order to be counted as a member of a collective, one needs to be *actively* associated with certain recognized, taught methods of cognitive and behavioral performance. The members of a collective are trained to react in appropriate pre-defined ways in certain contexts. The person trained as a doctor who happens upon an accident would presumably respond in the way she was taught, namely: administer aid. Alternatively, someone trained as a lawyer may find she abhors the profession and switches to engineering. In the context where legal assistance is needed, if the ex-lawyer passes on giving advice even though she *could*, she neither acts as a member of that collective, nor is she. Of course she could with qualifications proffer some advice, which

reflects the graded structure of collective membership. But a person who has fleeting contact with a certain community, but does not actively perform in accordance with that community's standards is not a member of that community. And a person who does perform in accordance with certain standards, but exemplifies the standards of a historically exhausted way of life is not a member either.<sup>58</sup>

My proposal is also theoretical. It suggests a method for identifying the members of a collective: marshalling all the possible candidates into what is decided to be a relevant situation to allow a head count. But in practice such a situation is unlikely; in theory it would be impossible, if past and future members should be included. In other words, there is no such thing as The physics collective or The Republican collective. The terms are sociological shorthand for individuals who think and act alike, but the vocabulary, as I interpret it, carries no ontological weight. There is no need, given this analysis, to suppose there are any group entities in existence exhibiting a causal force upon individual people.

This proposal captures the importance Fleck places on communication as well. The continuous tradition provides a stable foundation with an historically established framework through which, for instance, precedents have been set, or goals identified. The manner in which members are trained to actively think and behave supplies the substance for common interests and stakes such that members talking to one another do more than exchange noises. They agree on what is important to talk about, and how:

[O]n an apparently identical subject, e.g., a certain disease or a celestial phenomenon, a physicist will understand a biologist, but

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<sup>58</sup>It may be tied by a sort of resemblance to an historical style of thinking, the relation identified by a "neo-" prefix. The new group may well behave in accordance with historical standards. However, because the temporal and societal context has changed, and with that the sorts of situations encountered, the neo-group is not exactly of a feather with the historical one; the style of thinking was not formulated with the new circumstances in mind. Consider, for instance, an historicist interpretation of the Bill of Rights in the context of contemporary issues (e.g., equal rights).

will be unable to come to an understanding with a theologian or a gnostic. They will talk next to one another, but not to one another: they belong to different thought-collectives... What, for one of them, is important, even essential, is for another a side issue, not worth discussing. What is obvious for one, is nonsensical for the other. What is truth ... for one of them, is a 'base invention' (or naive illusion) for another (Fleck 1936, 82).

To use an example of what I think Fleck means, consider the confrontation of a political scientist and a physicist discussing the concept of force. The former may explicate it in terms of police or political effects upon citizen liberties. The latter applies the concept to bodies or particles and their attractions or repulsions. Do they talk about the same thing? Of course not. Laws describing force in physics do not apply in the domain of political science. If these interlocutors should misguidedly set out to settle whose force is the "real" force, they would miss each other's point. For each, the concept does not entail similar consequences. How to test for the presence of "force" is markedly different for each. Without recourse to another ground for communication, these representatives of different collectives will fail to understand one another. Perhaps it tells of the cogency of Fleck's view that the situation above, absurd if actually encountered, is not prevented by his theory. While Fleck's styles and collectives may explain how differently trained individuals disagree, or fail to communicate, it falls short on explaining how differently trained people successfully communicate

Communication is at its most stable between the members of the same collective. Yet, according to Fleck, there is never perfect understanding: "[T]he circulation of a thought is always related, in principle, to its transformation" (Fleck 1936, 85). The change presumably is not significant enough within a collective to create confusion. But if collectives are far enough removed in something like practical and semantic space, the communication of a thought "can be completely impossible, and transformation of a thought consists, in such a case, in its complete destruction" (Fleck 1936, 85).

Change in a thought's meaning occurs for many reasons. It may be due to the degree of training one has in the field; the specialist has "a more direct relationship to [the] product [of collective life]," while the layman's participation is mediated by the collective's professionals (Fleck 1936, 101). The object of thought, the object of collective life, that a specialist tries to transmit to a layperson could not be what the layperson understands. The novice has not had the direct experience with the object of investigation that the specialist does; the novice pulls upon the background he has at hand. This leads to a second factor contributing to change in the communication of a thought.

A person participates in many collectives in her life, with "various degrees of initiation and numerous links between them, just as there exist paths and links which unite different circles" (Fleck 1936, 101). By the many-membershiped status, she carries a background for interpretation different from every other person. When pulling from her background to understand someone else's message, she places a slightly different slant on the idea than what the speaker intended. But there is a danger in this feature.

Each individual, for Fleck, harbors a way of thinking different from every other. And if a manner, or style, of thinking implies the presence of a collective, then each individual is his or her own collective. This is especially problematic due to the lack of any core of characteristics that all members of a collective share. Cast in his own lights, Fleck's appeal to the social is apparently ineffective. In lieu of a condition of identity or similarity, there is no tool for linking like-minded people together. There is a hint of help in Fleck's brief mention of the collective every person belongs to: "[E]veryone takes part in the collective of the practical thought of 'everyday life'" (Fleck 1936, 102). The idea of such a collective could be developed as a bridging device everyone uses as a common background for interpretation. But Fleck's

attention to the "everyday life" collective is virtually just as brief and unexplored as in the quotation above.

At this juncture, then, a collective may be best reconstructed functionally as a group of people identified by their degree of active participation within a continuous tradition of practice. The metaphysical issue of how the members may be said to communicate meaningfully, for my purposes, remains fairly well untouched. I suggest a more epistemologically-oriented view. We may find that people communicate successfully insofar as their verbal or behavioral interplay contributes to reaching their presumed aim, where a part of our aim may well be to encourage alternative views so long as the alternatives were understandably defensible (whether the understanding was real or apparent). The bacteriologist's and physician's collectives, discussed above, represent alternative views of diseases and their diagnoses. The physician can accept and support the difference, though, because the bacteriologist's alternative aids in recognizing and, hopefully, curing a patient's disease even though she may well not "understand" the bacteriologist's technical vocabulary and apparatus. But a person holding alternative views whose defense is unacceptable to another person may be said to belong to a different collective. The justification for the alternative, in being unacceptable, renders the consequences (verbal or behavioral) from that view unacceptable as well.

The people with more practice and training toward a communal goal are closer to the product of collective life; those with less live the collective life vicariously through the performance of the collective's professionals. The product of collective life is the collective's style of thought:

[Thinking's] product is a certain picture, which is visible only to anybody who takes part in this social activity, or a thought which is also clear to the members of the collective only. What we do think and how we do see depends on the thought-collective to which we belong.... In natural sciences which comprised a certain thought-

style and led it to a system, we call the style-determinism the scientific reality. It develops along with the development of the scientific thought-style (Fleck 1935a, 77).

The collective's style of thought is a method of parsing and picturing the world. The next subsection examines Fleck's style of thought a little further.

## *2. The Style*

Since people are more tangible than thought, grasping the concept of a collective is intuitively less problematic than grasping that of a style. But it is not simple. One difficulty is that what identifies a collective just *is* a shared style of thought.

For Fleck, one of the most important functions of a thought style is its role in restricting, directing, funnelling and determining an individual's thought in a particular direction. Secondly, a style of thought necessarily restricts or suppresses contradictory styles of thought. As early as 1929 Fleck stated that

every knowing has its own style of thought with its specific tradition and education. Out of the almost infinite multitude of possibilities, every way of knowing selects different questions, connects them to different rules and to different purposes. (Fleck 1929, 49)

In 1960, Fleck still maintained that "a communal mode of thinking develops which binds all participants, and certainly determines every last act of cognition" (Fleck 1960, 154). He added the hope that different styles of thinking may at least appreciate each other, even though they cannot understand one another (Fleck 1960, 157).

All the same, Fleck did sometimes seem unsure as to whether a style of thought is an individual or a social phenomenon. For instance, in his (1935a), the definition of "thought style" has a distinctly individual tint:



The thought style thus understood is the result of the theoretical and practical education of the given *individual* ; in passing from teacher to pupil, it is a certain traditional value which is subjected to a specific historical development and specific sociological laws (Fleck 1935a, 66; emphasis mine).

Other times, thought-styles are described as group phenomena:

[T]hought-style is *not* an *individual* peculiarity, but a *group* one: it is based on a certain education and training and on a certain defined historical tradition. Thus one should speak about a different philosophical, scientific and mystical thought-style. Each of these styles had passed through specific historical evolution, and each occupies a specific place in the mental life of mankind (Fleck 1936, 84; emphasis mine).

For Fleck, styles of thought are dependent upon individuals insofar as groups are composed of individuals. But earlier misgivings about coherently transcending the individual in order to construct the collective aside, Fleck wants to capture a social dynamic. There is a level of activity that occurs when like-minded people get together, fostering an environment for developing ideas, through, for instance, encouragement or disagreement. The result is not just a matter of the individuals working alone, but comes from the efforts of one in conjunction with others. This result or style of thought is, so to speak, supervenient upon its members but not strictly reducible to its component parts. Indeed, Fleck portrays the collective as such:

It is precisely this division [between esoteric and exoteric] that results in the collective not being the simple sum of individuals, in the fact that the circulation of thought in it has creative capabilities whose result is not an individual work, but precisely a collective one (Fleck 1936, 101).

However, a style of thought is *prior to* individual cognition. Indeed, without a style of thought in which to be trained, an individual would be "condemned to mental sterility" (Fleck 1960, 155; cf. Fleck 1935a, 77). A style is "a social progeny: it is fashioned within a collective as the result of social

forces" (Fleck 1935b, 254). A style represents a way of life for the sake of which people live and die (Fleck 1929, 50), and it is refined and articulated through "[i]mitation, propaganda, mutual completion in collective actions, ... veneration of common ideals -- these reinforce and specify the style" (Fleck 1947a, 137). The style or mood

becomes consolidated, composed of two closely and mutually connected sides: a readiness for directed perception and a readiness to act suitably in a definite manner. ... This style is then transmitted from one generation to another, by 'initiation', training, education or other devices whose aim is introduction into the collective (Fleck 1936, 99).

As a social product, a thought style becomes a form of life unto its own:

The relative independence of the cognized from the individual is well illustrated in the fact that different individuals frequently make the same discovery or invention simultaneously but independently from one another (Fleck 1929, 50).

Upon the completion of a community's standards of thought and practice, a style of thought becomes "a limited unit, a closed organism, and there is no access to it by way of a universal, so called 'logical' or 'rational' path" (Fleck 1936, 100).<sup>59</sup>

Nonetheless, different styles of thought may be compared, indicating that Fleck conceived that they were somehow examinable from an external point of view. One can compare different thought-styles:

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<sup>59</sup>However, consider what Fleck says in 1935:

[T]he special ability, be it called basic training, expert knowledge, gift of observation, skill, or what you will, can and must be investigated. It must not be regarded (as happens frequently) as a metaphysical agent, a sacrament which acts absolutely, through its very existence. It follows special laws, and as a specific readiness for directed perception it forms the main element of a thought style. Thought styles present closed structures and have their own historical development (Fleck 1935b, 254).

Here he appears to want to have his ideas *both* ways: that styles are *not* metaphysically independent entities, yet at the same time they are a *something* which follows certain sociological laws, with an historical development. If the style is not an entity in itself, what, then, might be said to follow a law, or have a development? Admittedly, Fleck again is unclear and contradictory.

Some thought-styles approach each other, e.g., the physical and biological styles, others are more distant from one another, e.g., physical and philological, and finally some are distant from one another as, e.g., the physical and the mystical (Fleck 1936, 84).

And one can formulate an idea to be received by another, differently-oriented person:

If I formulate a certain idea for the members of another thought-collective, I transform it so as to render it approximate to the style of that collective. Thus I try to create a common collective, somewhat intermediary, poorer in substance, but wider. I try to change the style of the given idea (Fleck 1936, 85).

Yet because Fleck writes of the impossibility of thinking in a way different from the style of thought in use, then how could a person formulate an idea geared toward a different style of thought? By what measure are they be compared? These questions did not apparently occur to Fleck; his text does not address them. Any solutions would need to be provided, or denied, by us.

### *B. Critics Revisited*

Chapter 2 (B) covered interpretations and critiques of Fleck's writings by Harwood, Elkana, and Löwy. Issues primarily associated with thought-collectives and thought-styles were (a) vagueness, (b) relativity, and (c) reification. Let us examine these areas in light of the new material that has been covered.

#### *1. Vagueness*

By and large, any critique addressing the vagueness in Fleck's work is justified. However, simply to point to the vagueness is to do only half the work, for it leaves out positive criticism. If one were sympathetic to Fleck's views, and believed that they contained something worth further investigation, then how ought one correct for Fleck's shortcomings?

The trouble with Fleck's ideas of collective and style is that they do not

have a stable sense or referent, even if his intention was to create a useful fiction. The terms apply equally well to disciplines, fields within disciplines, and individuals since the presence of *any* cognitive activity presupposes the presence of a style of thought and its collective carrier. But with such a wide application, Fleck's concepts apply to virtually any human activity. As such, his description fades into vacuity, explaining nothing. In Section A of this chapter I proposed an interpretation that puts the brakes, so to speak, on Fleck's ideas. I open the door for one to develop a method so that Fleck's suggestions may be meaningfully used. The primary purpose of Fleck's work, as I see it, is to be a tool for resolving epistemological and metaphysical disputes, whereby one may realize that such conflicts are caused more by differences in training and tradition than by the degree of accuracy of their external world claims. Nonetheless, residual problems remain.

Consider, for instance, the fact that people frequently disagree with one another. We would presumably want to distinguish between substantive and non-substantive disagreement. The first would apply to members of the same collective who recognize they genuinely disagree and can work on rectifying that. They can see what would have to be changed in order to come back to agreement, changes which, for some reason or another, are not made. In the other case, people only *appear* to disagree but fail to comprehend each other's point because they cannot come to terms on what would be fair ground for resolving their differences. But there is nothing in Fleck's work that prevents one from saying that in both cases, the people are working within different styles of thought and, by implication, are members of different collectives. Ultimately, since no two people agree perfectly on everything within a particular domain, this whittles down to the result that *each* person is his or her own collective. In the absence of an identifying core, Fleck's claims weaken due to the peculiarities of individuality.

Another problem for Fleck is that he moves too fast. He confuses and

conflates different levels of cognitive and behavioral activity, from sweeping large-scale ideals of disciplines to local concerns of a person living her life. His lack of the philosophical predilection for making distinctions renders his work sloppy and half-done. What needs to be undertaken is an examination of different sub-fields of larger disciplines, culling an identification of what collects them together under the same header (e.g., optical, gamma-ray, radio, stellar, and planetary astronomy, to name a few, within a single science), and what distinguishes them.

However, this may turn into a rational analysis of styles of thought and their interaction, something Fleck at times denies can be done (Fleck 1936, 100), although he is not consistent. In an apparent reversal of his earlier views, he writes in 1947 that

[t]he advantages of the sociology of thinking thus understood are clear: it will enable us to *rationaly* direct the intellectual life of societies. As a *comparative science* it will counteract fascism, this enemy No. 1 of mankind (Fleck 1947a, 151; emphasis mine).

So at least by the late 1940s, Fleck admits rationality back in, a good thing in light of his overall goal of turning epistemology into a comparative science of thought-styles (Fleck 1936, 98; cf. Fleck 1935c, 22-23; 1947a, 151; and 1960, 157). If science is at least in part a rational process, and styles of thought are to be the object of scientific investigation, then styles of thought cannot be completely closed to rational analysis. Fleck's arguments are significant, I believe, in taking a look at the way scientists perform, at how they *act*. Actions, are non-cognitive and thereby non-rational, represent what Fleck believes is closed to rational analysis. In turn he seeks to find an explanatory basis for the nature of scientific activity in other, social, mechanisms: institutions, training, the absorption of a discipline's standards of practice, and the responses of these institutions, etc. to other segments of society. But because actions do follow, one would think, from decisions (which *are* subject

to being rational), then styles of thought cannot be completely closed to rational analysis.

## 2. *Relativity/Incommensurability*

Löwy (1988a) argues that Fleck's work is not open to the threat of incommensurability. I submit that the text can go either way. On the one hand, truth and meaning is highly relativized to their respective styles of thought. As such, a position for comparison seems precluded. Yet Fleck also speaks of styles of thought that are "closer" than others, suggesting that there is a way to somehow compare them (Fleck 1936, 84). Is there any way to make sense of Fleck's view?

There are two steps in approaching this question. First, we need to consider the fact that Fleck was a bacteriologist by trade; hence, there are aspects of his professional life that have affected his philosophical work. Second, continuing the account of thought-collective developed earlier in this chapter, I show how some styles may be considered "closer" than others.

As discussed in Chapter 1, Fleck was enmeshed within a medical debate concerning how disease entities ought to be treated -- as (a) closed, specific entities or (b) complex systems of events. For Fleck, a disease was not accurately indicated by the presence of certain toxin and anti-toxin reactions. "These have nothing directly in common with being ill, although they are important mechanisms of the disease" (Fleck 1935c, 62). He considered a disease to be "a process which changes continually, and which has its temporal genesis, its course and decline" (Fleck 1927, 44). Like a thought-style and thought-collective, a disease is not a static phenomenon; it constantly changes.<sup>60</sup>

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<sup>60</sup>For instance, in the *Genesis* Fleck notes that at the same time work was being done in order to diagnose and treat syphilis, tuberculosis was virtually unrecognized as a disease (or, at least, a "bad" disease) and went untreated:

And as a disease, for Fleck, is not effectively captured in the reductionist's terms of physics and chemistry, neither is cognition effectively or accurately captured in a reductionist logical process. An idea producing a reaction in one setting may be ignored in a different one, so the social and historical environment is critically important for Fleck. Awareness of this context makes sense of the popularity or rejection of an idea better considering abstracted from the context that confers truth upon it.

So what constitutes an hospitable environment, one in which a person can communicate versus one in which a person cannot? Overall, the idea emerging from Fleck's texts is that styles of thought are close to the degree that a thought may be transmitted from one to the other with a constancy of content. But as I have argued (Chapter 2 (A6), Chapter 3 (A1)), following Fleck's ideas here quickly leads to solipsism. But building upon Fleck's expression that systems of thought select their own particular questions and link them together by their own rules and purposes, a path is opened for understanding stylistic similarity and difference.

Specifically, the degree to which questions or concepts have responses similar to what one would expect or could be conceived as consequences, signals a similarity of thought-style. This may occur in a formal sense, where the responses are linguistic consequences from certain suppositions. This may also occur in an active sense. To the degree that certain linguistic or behavioral responses to a situation are expected, or are conceivable under the type of training one has experienced, a similarity of thought-style is engaged.<sup>61</sup>

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For centuries tuberculosis had done far more damage [than syphilis], but it never received comparable attention because, unfortunately, it was not considered the "accursed, disgraceful disease" but often even regarded as the "romantic" one. (*Genesis*, 77)

<sup>61</sup>Further, to the degree that consequences (verbal or behavioral) are not conceivable, differences may enter to the point that one may ultimately say that the meanings of an apparently identical sentence are "incommensurable" between two different styles. Such differences, then, may only be apparent; how much we understand is a function of how much we learn.

However, this still leaves the question of where a point of view from which one could compare styles of thought could be found. For Fleck, the only region where this could be done is in what he calls the "everyday thought-collective" to which everybody belongs. It is curious because *had* Fleck developed the idea, it could have served as replacement for a neutral observation language. Though

There is the possibility that a variety of styles of thought are close to one another due to the way in which they work together toward accomplishing some goal (e.g., diagnosing/curing illnesses, learning about the nature of electromagnetic radiation when we point devices at the sky). In other words, a difference in training is not *ipso facto* grounds for supposing two people will be unable to communicate, as Fleck's writings lead one to believe. By his account, (1) it is impossible for two people to communicate with another since no two people perfectly agree, because (2) if two people do agree we are led to the conclusion that they are under the rubric of the same thought-style and should think perfectly alike. Neither of these situations is plausible. The route to finding how we understand or fail to understand each other Fleck does not provide by his conceptions of thought-styles, collectives, and their apparent incommensurability. He does suggest other helpful clues: our actions, and the way we act together toward accomplishing some aim.

### 3. Reification

The final question to consider before moving back to facts is whether Fleck reifies the concept of the thought-style. Harwood (1986) believes Fleck does; Löwy (1988a) does not. I believe the best we can conclude from Fleck's work is a draw.

On the one hand, thought-styles direct individuals' thoughts; they follow certain historical and sociological laws; they act as a storehouse of

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conceptually laden in its own right, in being common to everyone it is, for all practical purposes, neutral enough. Perhaps Fleck was not as divorced from other styles of thought as one might be led to believe. The "common sense" thought-style of everyday life would provide a medium into which conflicting styles could translate their ideas, although admittedly those ideas in the process would change and the accuracy of the translation is questionable. Nonetheless, the numbers of connections between styles of thought creates a larger and more stable, social cognitive structure, something I believe Fleck finds extremely important in fixing our notion of a stable external world.

This starts to look remarkably similar to contemporary conceptual role semantics. My intention here is not to broadcast that Fleck was a conceptual role semantacist, but to suggest that putting such an interpretation upon Fleck's writings allows his ideas to make sense. But what this does not illuminate, and what Fleck does not show us either, is the nature of the connections between what we may call overlapping styles of thought. Is it syntax, the pure form of a sentence? Or is it practice associated with a concept, gained through similar practices and traditions? Maybe it is both. For more on this, see Churchland (1988), Fodor (1988), and Shapere (1964, 1987).



information surpassing the storage capacity of any individual member. Thought-styles, according to Fleck, *do* things and are subject to certain constraints. In this sense, they are conceivably *things*. If one were to develop a theory of thought-styles, they would be quantified over. Fleck seems ontologically committed.

On the other hand, given the constructivism in the medical community of Fleck's time, and the way a social style disintegrates into individual methods of thinking and behaving under analysis, I believe that there is not enough support for reification. This is not necessarily what Fleck intended, but it is, I believe, the most fruitful way of understanding his views until or unless a method is developed for identifying a collective and its consequent style of thought (e.g., what counts as standards of practice, types of "typical" responses for a member of a collective, methods of thinking through a situation).

A thought-collective realist would also be expected to explain how the relation between the collective and individual begins. For as long as one assumes a style of thought is sociologically necessary to any individual cognition or act, then how a collective may ever form is precluded. Individuals, already thinking, are *prior* to these groups.

There is a final difficulty with Fleck's position. Presumably he would want his beliefs to be justified by more than a nonrational consensus. But given his own framework, the claim that "there is no other possibility" to his view is far too strong (Fleck 1947, 147). Fleck urges his reader to move from considering how thought ought to look like, and consider instead what it "really does look like" (Fleck 1936, 80). He means what he says literally. But at best, what we have is truth effectively relegated to a group of people who can work together toward establishing some objective. This means that any truth in what Fleck has to say is relegated to a community that happens to take interest in what he has to say. Why this is so will become clearer after a

further examination of Fleck's views on the development of facts, the topic of the next, and final, section.

### *C. Fleck's Concept of a Fact*

Contrary to Löwy's (1988a), Toulmin's (1986), and Andersson's (1984) interpretations presented in Chapter 2 (B), one of Fleck's goals is to locate the notion of "objective reality" in something other than a fixed, singular world existing independently of human action. This is not a new idea; it was suggested before Fleck by others like Chalubinski and Kramsztyk, as discussed in Chapter 1 (B2). These men conceived of diseases as medically-constructed abstractions from samples of physical data. But Fleck's approach is different; he incorporates ideas from physics, such as quantum indeterminacy, as an aid for keeping the objective world (ordinarily understood) at arm's reach. For instance, in 1929 Fleck quotes Bohr on the effects of measuring devices upon atomic phenomena, writing that "'no independent physical reality in the ordinary sense can be ascribed either to the phenomenon or to the means of observation'" (Fleck 1929, 52). From this Fleck infers that

[t]his statement applies to all observations of any phenomena whatever, but the mutual relationship with the means of observation is relatively negligible in most cases. Yet, if the 'treatment' of the phenomena, with whatever instruments, goes on over centuries, will the effect not become significant? To observe, to cognize [erkennen] is always to test, and thus literally to change the object of investigation (Fleck 1929, 53).

In 1935 Fleck again alludes to the work of Heisenberg and Bohr:

A fundamental change has recently taken place in this respect: [in Bohr's atomic theory] ... Bohr claims that 'an independent physical reality in the ordinary sense can be ascribed neither to the phenomena nor to the medium of observation' (Bohr, 1928). Jeans proved that what the physicist calls Nature turns increasingly into something he has created or at least selected or obtained through abstraction, and that the previously stark contrast between the physicist and Nature is thereby reduced (Jeans, Haas 1934).

Observing is therefore fundamentally the same thing as creating, the 'subject' and the 'object' of cognition thus enter into a mutual relation; it is altogether impossible to ascribe a meaningful independence to either of them (Fleck 1935b, 240).

Fleck's parallel of our epistemic uncertainty to quantum uncertainty dropped out of his later writing, for good reason. As Kazam Sadegh-Zadeh explains, Fleck misrepresents Bohr, Heisenberg and Jeans either by misquoting their own writings or secondary reviews thereof. Fleck's representation of their ideas is

simply an epistemological *and* ontological nonsense superimposed on [the] Heisenberg uncertainty relation. To know that an object 'changes' requires knowledge about its 'unchanged' state. This, however, is, according to the same philosophers, impossible because 'the observation changes the object'. (Sadegh-Zadeh 1981, 267)<sup>62</sup>

Even if Fleck's interpretation of quantum physics was correct, it would beg the epistemic authority of physics to ground the metaphysics of his claim.

In any event, quantum uncertainty dropped out of Fleck's attempt to keep the objective world out of humans' reach. He later concentrates almost exclusively upon a combination of phenomenological, psychological, linguistic, and social factors. These factors corral reality within a style of thought, an idea present in his earliest works, and an idea that constitutes what I believe houses his fundamental concept of facts: the solidification and depersonalization of an idea as it becomes embedded within a stable system of thought. As he wrote in 1929:

If one wanted to make [absolute reality] independent of so-called 'appearances', one ought to consider that all 'appearances' are nothing but the expression of the interrelations between a number of elements of cognition.... There exists no fundamental difference

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<sup>62</sup>In other words, the significance of accounting for the means of observation, according to Bohr, is in specifying them as their characteristics will contribute in interpreting atomic results; different apparatuses react to quantum phenomena in different ways. See E. Scheibe, *The Logical Analysis of Quantum Mechanics*, chapter 1.

between 'appearance' and 'truth', the difference is one of development (Fleck 1929, 56).

In order to elaborate upon Fleck's concept of a fact, we must revisit thought-stylistic phenomena is needed. This is needed because Fleck substitutes appearances, or objects of perception and experience, for objects in the world, giving the former the name of the latter. The route to meaningful perception is reached by a style of thought.

The theme relating experience with expressions in the *Genesis* is echoed in Fleck's later work: "[A] word is the living picture of the object; even more than that, its magical equivalent" (Fleck 1936, 94). As ideas or concepts develop and take their own "form," so do the objects of experience to which the ideas or concepts refer. Hence the usefulness of the 'directed' in "directed readiness to perceive;" it is a certain conceptual cookie-cutter putting a thought-stylized pattern onto an otherwise unformed and confused mat of perceptions. An effective style "creates a certain definite readiness, imparts it by sociological methods to the members of the collective, and dictates what and how these members do see" (Fleck 1935a, 72). One comes to recognize phenomena of experience in certain pre-defined ways: "As regards the details: every detail, ever so minute, remains connected with general views, and its discovery depends on this relation" (Fleck 1935a, 75). The connection between individual thoughts and the thought-style is so close that Fleck is "convinced that the more accurate our study of the development of ideas involved, the more complete would be the historical determination actually found" (Fleck 1936, 94).

The relation between perception and thought has foreseeable consequences, reflected from the incompatibility of conflicting thought-styles. Since objects of perception are channeled by one's style of thought, the loss of thinking in one style is matched by a lack of seeing the objects of that styles' trained perceptions. The more different or "removed" one style of thought is

from another, the more different are their participants' observations:

Stressing some elements and downgrading others likewise depends on ... style. One therefore has to say that *two observers possessing fairly different styles of thought have no common objects of observation*; each of them observes in principle another object (1935a, 66; emphasis mine; cf. footnote 66, below).

There is no conventionalism, or choice, with respect to how one will think. According to Fleck, the manner in which a person thinks is determined by the social style of thought:

[T]he research worker has no consciousness of choice [of the range of subject of investigation]; on the contrary, the choice is imposed upon him directly and in a binding manner, following from ... what we call the *thought style* (*Denkstil*) (Fleck 1929, 66; emphasis Fleck's; cf. Fleck's 1929, 75; 1936, 94; and 1960, 154).<sup>63</sup>

Truth and falsity are relativized to the style in which an idea or utterance is formulated: "[T]ruth is the up-to-date stage of changes of thought-style" (Fleck 1936, 101-102). There is no neutral ground for adjudicating between claims stemming from conflicting styles of thought, and Fleck challenges those who disagree.

In 1947, Fleck acknowledged that "Many representatives of sciences still using the thought-style of classical physics, affirm that it is possible to conduct a so-called 'objective observation' of an isolated elementary fact" (Fleck 1947a, 142). He responds that "[i]n such opinions of some physicists one finds a wealth of illusion and misunderstandings characteristic of *their* style of

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<sup>63</sup>The saving factor from this unconscious and binding style of thought is that individuals migrate from one style of thought to another, depending on the social context they happen to be in. Unfortunately, this cognitive versatility does not appear to be under the control of the individual, but the social setting which obtains. Nonetheless, this does provide the opportunity for changes in the style of thought, along with conflicts between groups, as perhaps in the situation quoted from Fleck above, when one tries to transform the content of an idea into a format amenable to a different style of thought. "[E]ffects [between collectives] take place either by the individual's taking part in more than one mental community, . . . or by way of clashes between members of various collectives" (Fleck 1936, 105).

thinking" (Fleck 1947a, 142; emphasis Fleck's).<sup>64</sup> And further:

It is impossible to isolate the object of observation without assuming in advance that it possesses certain features. ... However, they do not see that this apparent arbitrariness is a necessity imposed by a specific thought-style. ... The use of an apparatus is always the expression of applying a certain developed thought-style. ... The scientific apparatus directs thinking toward the path of the scientific style of thinking: it produces a readiness to see certain forms, while removing at the same time the possibility of seeing others (Fleck 1947a, 142-144).

By Fleck's analysis there is no objective observation; there is no neutral ground for ascertaining what is and what is not the case. All such claims are relativized to a particular collective and style of thought: "In every observation, the ordinary everyday one, or the most scientific one, is a modeling, the pattern is supplied by the collective body. And there is no other possibility" (Fleck 1947a, 147).

Reasons for rejecting the classical notion of a mind-independent world surely stem in part from Fleck's own medical and scientific experience. For instance, debates concerning the objective existence of disease entities due to complications involved with the variety of their instantiations, and philosophical views rejecting the concept of an absolutely objective world emerge when Fleck writes that

an observation with the aid of a scientific apparatus is not tantamount to the co-ordination of some number which is independent of us, with a certain constant independent element. It is rather the construction of a sentence of the following type: "under the conditions of the measurement the probability that the weight of body C does not exceed the limits of 5.32587-5.32589 g amounts to

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<sup>64</sup>It is curious to see Fleck assuming a normative stance, when he otherwise would reject the legitimacy of such a stance. A person's or group's style of thought may suppress things that a different style emphasizes, so it is no one's fault for not believing in another manner of thinking. No individual or group has privileged access to a ("really") correct system of thought, so the best we can aspire to is to respect one another (Fleck 1960, 157).

about 95%". What have we found? A complex construction, an entire theory expressing the relation between a series of numbers, a set of conditions partly depending upon ourselves, a state of knowledge at a given moment and a certain element isolated by us. *From this construction one cannot deduce anything about something which is independent of us* (Fleck 1947a 145-147, emphasis Fleck's).<sup>65</sup>

Fleck believed that the scientific ideal is to reach a single, true knowledge, but he notices that the sciences "do not add up to form a consistent homogenous whole" (Fleck 1946, 113).<sup>66</sup> There is no stable core the sciences share that could sustain an isomorphic relation with an objective world. "Were it so, we would have in science a constant, unchanging part ... [but] even the most certain, basic elements undergo change" (Fleck 1946, 113). Therefore, as he sees it,

the notion of truth in its classical significance, as a value independent of the subject of cognition and of social forces, compels one to accept truth as an unattainable ideal, and the history of science teaches us that we do not approach that ideal, even asymptotically, for the development of science is not unidirectional and does not consist only in accumulating new pieces of information, but also in overthrowing the old ones (Fleck 1936, 111).

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<sup>65</sup>But, with unsurprising contrariness, Fleck also says that "[N]o science at present contains an assured portion of the objective picture of the world, *but all sciences are getting nearer and nearer to it*" (Fleck 1946, 114; emphasis mine). Here one may justifiably scratch her head, for what on earth could Fleck mean by getting "nearer and nearer" to an objective picture of the world, in light of what else he says?

<sup>66</sup>Fleck's image of science and its product is complicated and messy, coming through in early works noting the difference between science as it is presented in a journal, and a "living scientific practices" that takes place behind the scenes (Fleck 1929, 50).

One cannot look upon the sciences as being only a set of sentences or a system of thoughts. They are a complex phenomena, at onetime perhaps individual, at present collective ones, made up of separate institutions, separate actions, separate events. Written sentences, unwritten customs, one's own aims, methods, traditions, development. Preparation of the mind, cleverness of hands. A specialized organizational structure, with its hierarchy, ways of communication and co-operation, an organizational court, public opinion, press, and congress. A distinct relation to other aspects of life, to society, to the state, etc. (Fleck 1946, 118).

For another scientist's view of his own field, and his response to logical reconstructions, see Senior (1958).

Fleck has changed his approach in relating the concept of truth to reality. In essays from 1929 to 1936, he holds that there *is* an objective state of affairs, but it constantly eludes our grasp due to the effects of our intervention. Now the tie is removed because science is heterogeneous. One is prevented from mapping scientific results onto a single stable reality, because of (1) the sciences' multitude of investigative directions, (2) the historical shifts in their directions, and (3) the absence of a common, constant, unifying core. Scientific approaches both synchronously and transhistorically are not consistent. Thus the "realities" that their statements are true to must similarly be quite different.

So, contrary to Toulmin (1986), Fleck does not argue that there is a single unchanging set of facts. There is no Anderssonian emphasis on objective factors determining scientific facts, unless Andersson meant to refer to the objective factors of sociological behavior Fleck would probably want to be accurately describing. Rather, if we are inclined to believe distinct scientific fields are getting "the truth," or getting an accurate picture of reality, then there are many realities: "The thought-style *creates* reality, not in a different way from other products of culture and, at the same time, itself undergoes certain harmonious changes" (Fleck 1936, 112; emphasis Fleck's).

Much in Fleck's writings supports the description of a fact as a social creation with little to do with a non-social world. To be sure, perceptions occur without any cognitive effort. However, what the perceptions mean and how they fit together to become a picture of the world has everything to do with a community's style used to interpret them. For Fleck, without the social interpretative apparatus, there is no world.

Furthermore, styles of thought change. Without this change, one's world-picture would stagnate and new discoveries would stop. "One cannot, simply and immediately, see something new and different;" the thought-style must first change and with it "the brute force of the directed mental readiness



must cease" (Fleck 1935a, 74). The process is quasi-revolutionary:

[T]he new observation, i.e. the discovery, is carried out in such a way that, during the epoch of equilibrium, there arises a certain intellectual unrest and a tendency towards changes: a chaos of contradictory, alternate pictures. The picture, fixed up to now, disintegrates into blots which arrange themselves into different, contradictory shapes. ... At this creative moment there becomes embodied in one or more investigators the mental past and present of the given thought-collective. All physical and mental fathers are with them, all friends and enemies. Each of these factors pulls to its side, pushes or inhibits. Hence the flickering chaos (Fleck 1935a, 76-77).<sup>67</sup>

After a thought-style's renovation, its range can be extended to a new community by educating more people to live within it. Afterwards, "all the participants will see the new form directly, with their own eyes, *as if* it were the only one, everlasting truth, independent of the people" (Fleck 1935a, 77; emphasis mine). Facts, then, are not external and impersonal. Everything believed to be the case is fundamentally shaped and given content by social influences of collective life. What is the case, under Fleck's analysis, is created by us as social beings. But then why do facts seem to be external and independent? For Fleck, there are two reasons. One is pinpointed in Löwy and Merton's discussion of a social "trickle-down" fact theory. But this is only part of the story; the second source is in the specialists's domain.

Fleck frequently speaks of the strengthening of an idea into a fact due to the social relationship between professionals and laypersons. In processing from professional to layperson, scientific ideas become more obvious and absorbable into the everyday style of thought. The specialist, depending upon

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<sup>67</sup>Curiously enough, this scenario seems to be a uniquely *personal* event, something that does not apparently mesh well with what else Fleck has to say about an individual's thought. That is, the thought-style under transformation is in the individual's head: *that's* what being changed in order to foster the new "discovery." Elsewhere, Fleck presents the transformation in terms of a thought-style in its *entirety* which must change. That, as has been explained, is a *social* entity. This is another instance of what I believe is Fleck's confusion between a style of thought and an individual thinking in a particular style.

the laity for support, equips her results with already recognizable (everyday) features, or adapts "each novelty to the adopted set of opinions, and equip[s] it automatically with identical features of greater certainty and weight" (Fleck 1936, 102).

Merton (1983) and Löwy (1988a) appeal to the style of written professional literature to help make this case. Professional literature is tentative and provisional, "because the judgment about the existence or non-existence of a phenomenon belongs, in a democratic collective, to a numerous council, not to an individual" (Fleck 1936, 107).<sup>68</sup> After being reformulated for the layperson, the subjective character of the proposal changes into proven fact, "united with the entire system of science, it will henceforward be recognized and taught" (Fleck 1936, 108).<sup>69</sup> The layperson's

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<sup>68</sup>Consider, for instance, this excerpt from an interview with Robert Wilson on the topic of his and Arno Penzias' "very low-key" 1965 publication:

I guess we wrote a very understated article. . . . Some of the steady-state people were pleased by the way we had gone about things. We felt that, at least until they had a chance to think about our results, we shouldn't go out on a theoretical limb we couldn't support (Wilson, quoted in Bernstein 1984, 205).

Or, in an equally telling case, Karl Jansky wrote in 1933:

I haven't the slightest doubt that the original source of these waves, whatever it is or wherever it is, is fixed in space. My data prove that, conclusively as far as I'm concerned. Yet [Harald] Friis [Jansky's supervisor] will not let me make a definite statement to that effect, but says I must use the expressions "*apparently* fixed in space" and "*seem* to come from a fixed direction," etc., etc., so that in case somebody should find an explanation based on a terrestrial source, I would not have to go back on my statement (letter by Jansky, quoted in Sullivan 1984, 14; see also the titles to Jansky (1933a, 1933b).

<sup>69</sup> A recent example comes from the March 28, 1995 *Time* article on scientific debates concerning the age of the universe. It presents the scientific positions in simplistic terms, illustrated by pictures and graphics. There is virtually no discussion of the difficulties scientist confront in their laboratory work, nor is there mention of any calculations -- odd, considering this issue deals closely with theoretical (i.e., mathematical) physics. It may be worthwhile to consider that reporters are taught to write news articles largely for an audience assumed to have a 4th grade education. But of course, not all media articles are so geared; even with the newspaper domain, contributions from the *New York Times* news service are obviously more complicated than those from the Associated Press.

The Hubble constant, which can be written " $H = V/D$ " (where 'V'=velocity and 'D'=distance) is represented in *Time* as follows:

Astronomers have known since Hubble's heyday in the 1920s that you need only two pieces of information to deduce the age of the universe: how fast the galaxies are flying apart and how far away they are. The ratio of these two numbers tells you how fast the cosmos is expanding (a rate known as the Hubble Constant; it's expressed, for those who

social distance from the author changes the creator's mental composition into a proven object, an impersonal thing.

This does sound like what was discussed in Chapter 2. But it does not reflect the entirety of Fleck's position. Classifying facts in the simplified region of the popular domain entails that scientific facts exist only at the popular level. Yet the fact under investigation in the *Genesis*, the relation between syphilis and the Wassermann reaction, was a fact specific to Fleck's own professional circle. It was a relation that new members were trained to detect. A medical dabbler, having little proficiency with the relevant tools, could hardly attach the same significance or meaning to the medical fact Fleck describes.

There are different forms of communication; Merton and Löwy pointed out popularization. At the professional level there are "informing" statements, formulated in terms of their own technical language (Fleck 1936, 86). The more mature (i.e., developed, stable) the thought-style, the larger the number of specialized, informing statements (Fleck 1936, 87). These statements capture facts at the professional level and have their own depersonalizing characteristics.

First, as a person becomes trained within a profession, collective-particular experience becomes attached to the sense of its vocabulary. This sense is something that a layperson will not, without training (such as learning to see through a microscope) detect (Fleck 1935a, 59-61). In a very important sense, then, although the layperson may have access to the professional literature, he will not completely understand it. Fleck sees that there is much more to learning a community's method of communication

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insist on the proper terminology, in units of kilometers per second of recessional speed per megaparsec of distance (*Time*, 80).

The parenthetical remark is especially telling: "those those who *insist* on the *proper* terminology" indicates either that this scientific-speak is either obviously not of practical use in this context, or the writers wants to show that they've done their homework, or both. In either event, this formulation of the Hubble constant and the one which astronomers and astrophysicists would use are not alike.

than memorizing definitions or mimicking the words:

None of these terms can be fully replaced by a logical explanation, for the tradition of the given discipline and its historical development have surrounded it by that specific sacramental power which speaks to the members of the collective with a greater strength than the logical component (Fleck 1936, 100).

Another feature of the professional vocabulary aids in detaching "significance from the subject of cognition, hence in establishing the 'objective' meaning. In this way the object being defined becomes independent, as if possessing absolute existence" (Fleck 1936, 108). Words in a systematic style of thought, such as the scientific, are built out of pre-established roots and linguistic appendages which "prejudge in advance that the specified object has a fixed place in the system of the given science" (Fleck 1936, 109). The next step from here is signs and symbols (as for the chemical elements, or a calculus). "At this stage the objectivization of mental products is the strongest: they acquire the features of a complete independence from man" (Fleck 1936, 109); "[f]inally one will reach the most general features of the physical style: mathematics" (Fleck 1947a, 147).

There is, then, a professional parallel to amateur "fact-forming" events. For the amateur, depersonalization occurs through the simplification and integration of debated and complicated texts. At the professional level, there is a socially fixed and historically evolved vocabulary. Due to its establishment prior to an individual's awareness, it confers a sense of extra-individual weight, not invention. Furthermore, as the specialists' vocabulary becomes more complex and abstract, it develops into a formalized structure, removing all traces of personal subjective factors leaving an aura of objectivity behind.

Scientific apparatuses contribute additional apparent objectivity and certainty to thought-stylized results. These devices are, as discussed earlier in Fleck's rejection of theory-neutral experimental observation, "a realization of

some result of a definite thought-style, [directing] our thinking automatically on to the tracks of that style" (Fleck 1936, 109).<sup>70</sup> The results of experimental devices, built to check for the very stylized ontologies they were constructed to investigate, add further weight to the veracity of the style of thought being used.

Professionals are closer to the phenomena of investigation, the experimental devices, specialized procedures, and vocabulary than the layperson. The layperson is not ordinarily privy to these. He does not work in the lab, and it is unlikely that he is well-versed in the lab's vernacular. Otherwise, no simplification and translation would be needed. One may speak, as Fleck does, of sub-collectives within a larger collective: the collective of the specialists, and that of the layperson. The circulation of a fact within their respective domains contributes to the apparent objectivity of an idea in different ways:

The objectivity of scientific observations consists merely in relating them to the entire store of knowledge, experience and the traditional mental customs of the scientific collective: the outcome is independent of the passing moods of the individual and of his readiness which is given by the collective of everyday life, but, instead of forms conditioned by the style of everyday thinking, science creates at best constructions conditioned by the differing thought-style (Fleck 1947a, 147).

Since the simplification of an idea from the specialists' domain to the common-sense domain would alter its content, according to Fleck, one would

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<sup>70</sup>Although it is odd when Fleck says that "A telescope shows Saturn's rings ... Whoever can look into the telescope and think of Saturn consequently uses a certain definite thought-style. There is no other possibility for him: he *must* recognize the ring of Saturn as a reality independent of himself, and his own thought-style as the only 'good' one" (Fleck 1936, 109-110). Surely the individual may think something else, for instance, that the image of Saturn is a picture pasted on the other side of the telescope, that the image is a remarkable artefact of the device, that he (the observer) is hallucinating. Such opinions may not be taken *seriously* by others in the field, which is what I believe the crux of Fleck's point is, but the individual is free to think what he or she wants nonetheless. It is only after one factors in other variables, such as if the observer is a member of the astronomical community, or is a layperson who is supportive of scientific results and interprets the image seen on scientific authority, that any notion of necessity in that interpretation begin to enter.

think that the fact of the layperson and the fact of the specialist are quite different things. Furthermore, as the technical language increases in abstraction and complexity perhaps involving mathematics, the capacity of the layperson to comprehend a scientific result is even less.

There is the professional status of a fact, and a layperson's version of it. Both arise through the circulation of ideas; but they are not quite the same ideas nor, by implication, the same facts. However, to the degree it is agreed that the translation preserves the right sort of content, greater objectivity is attained as the relations between different styles of thought increase.<sup>71</sup> It is in this sense that Fleck would construe scientific progress, or an image of science getting closer to "the truth": in the convergence of styles of thought to one another, but not to any objective world in the classical sense.

#### *D. The Passive and Active Components of a Fact*

Surprisingly, one of the apparently major components of a fact that Fleck discusses in the *Genesis* does not readily appear in his other works. The only notable usages of the terms "passive" and "active" in his other works are:

(1) "[C]ognition is neither passive contemplation nor acquisition of the only possible insight into something given. It is an active, live interrelationship, a reshaping and being reshaped, in short, an act of creation" (Fleck 1929, 49),

(2) "The development of ideas occurs using its own paths, it has its

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<sup>71</sup>Thus, some styles of thinking tend toward one another, as do, Fleck believes, those of science and everyday life. This is the sense of getting "closer" to an "objective picture" of the world I think Fleck intends. But their component ideas are stylistically determined nonetheless. Such a collective mood, Fleck says, "may have either of two effects: it may blind or it may make clear-sighted. Working evenly for a long time, it creates science, art, technology. When it suddenly bursts out -- it causes riots and revolutions" (Fleck 1960, 157). Earlier, in 1936, Fleck also stated that "'[c]ommon sense', the personification of the thought-style of everyday life, obtains a surface appearance in the direction of the principles of the specific scientific style" (Fleck 1936, 107). That is, there is some sense of styles of thought striving toward one another, at least for everyday life and science, although how and why this would happen is not explained (presumably, it eventually would be chalked up to an historically- or socially-caused event).

own historical conditioning, not a logical one; it is -- so to say -- passive, not active" (Fleck 1936, 92), and

(3) To the early natural scientists observation was a process which ran its course between two discrete principles, the knowing subject and the object to be known; in a manner such that the first principle had a purely passive function and the second remained basically uninfluenced by this process. Any productive activity on the part of the observing subject during observation, any influence upon the object, was regarded as a falsification of pure observation. Scientists thus have considered the role of the subject ('I observe') as a kind of 'passive activity', and that of the object (the possibility of being observed) as an 'active passivity'... (Fleck 1935b, 240).

None of these usages bear significant resemblance to the passive and active components of knowledge described in the *Genesis*. This is partly because the terms are unexplicated, so even if they were intended to function the same way, the text does not encourage that conclusion; it would need to be read in. And trying to read the *Genesis* into the quotations above is difficult. In the first quotation, "passive" and "active" do not allude to sociological or historical phenomena, but apparently try to capture a lounging/exercising comparison. In the second, there is no comparison to what Fleck speaks of in the *Genesis*, since there the "active" associations pertain to historical matters, but this quote associates "passive" with "historical.". The third quotation does not mesh well with the *Genesis* either, largely because "passive" and "active" are undefined, making their sundry combinations even less obvious. With sufficient analysis, definitions could possibly lend helpful material to the views of Heelan (1986) and Rotenstreich (1986) discussed in Chapter 2. But this is questionable, as the view of scientific activity Fleck describes in the third quotation apparently targets a misconception.

Due to the absence of passive and active associations in Fleck's other texts, I maintain that the space scholars have used expounding upon the

virtues of passive associations in Fleck's concept of a fact has been essentially misguided. This is not to say that it is a useless approach. But those associations do not, on the whole, serve as a foothold for *Fleck* to insert an objective world, or act as a foundation for facts *qua* facts. Why those concepts made a significant appearance only once is not known. If they generated as many conflicting interpretations in his time as they do now (with a "passive" observer, "passive" responses of the world to "active" investigation, "passive" logical consequences following "active" assumptions), then perhaps their disappearance followed critical response sent his way. But the bottom line is that there are no passive or active concepts employed either before the *Genesis* or after. From the secondary literature they appear significant, but this is frequently because the only text the writers discussed here have read, or at least cited is Fleck's *Genesis*. Working from such a small sample of Fleck's work, their reaction is understandable.

#### *E. Conclusion*

In this thesis I have sought a consistent, and believable, account of what Fleck means by a scientific (or, for that matter, any) fact. This is complicated because Fleck's concept of a fact relies so heavily upon his concept of a thought-style and its corresponding collective. Under analysis, his foundation is not stable, which renders all that needs its support (particularly, his concept of a scientific fact) as weak. Fleck's position suffers for many reasons. First, he (a) rejects the possibility of maintaining an authoritative stance on physical reality while (b) simultaneously taking an authoritative position on social (which is not obviously different from physical) reality. If one should grant him (a), there is no compelling reason given for believing what he has to say about (b).

But if one should ignore that difficulty, Fleck then seems to be just wrong. His appeal to communities, distinguished by their differences, rapidly disintegrates because all individuals are different. He offers no obvious



mechanism for telling whether people are similar enough to belong to the same group. Without that, each person is as a collective unto him or herself. As such, the central function of societies or groups is precluded because there is no obvious way to create groups out of Fleck's method.

This sociological methodology is furthermore flawed because it is a non-starter. In other words, his theory might have a chance if it is applied *in medias res* of human social development. However, Fleck does give all indications that he explains how thought actually develops and occurs (ignoring the first difficulty discussed above); it is a process with a natural beginning. But it could not have begun. Ignoring the second difficulty discussed above, individuals can think, according to Fleck, only within a community. But to have had the very first community, we would seem to need individuals who were already thinking, and somehow, thinking alike.

A person sympathetic to Fleck may well wave his hands at these puzzles, as they are only philosophical problems. But if his account is to be an authoritative, natural account for how beliefs about the world developed, then if it did not need to work in the beginning there is no reason why it should have to work now. And if it is not purported to be an authoritative account (i.e., if it is reformulated so that it is reflexive and, subsequently, is one of many possible "harmonies of illusions"), then better reasons than Fleck's need to be given in order to render it appealing. Because if the sociological theory of this program suffers from inconsistencies and incoherencies, then so too does the conception of a fact it was developed to explain

Primarily there are three difficulties I would highlight from my interpretation of Fleck's work. (1) Fleck's work is convoluted, his theses are frequently circular, and his assertions are often contradictory. This means that alternative constructions of the concept of a fact may well be feasible on the basis of these texts. (2) On my version of Fleck's view, an individual cannot

know a fact if it has not received significant enough social circulation, leaving it questionable what to do with statements of fact such as "I am sitting here, alone, now." (3) The role of the individual is that of a puppet dangled by social and historical forces, without any considerable voice.<sup>72</sup>

Fleck's work lacks a systematicity which causes the difficulty of (1), and is not thought through well enough to answer (2) or (3). If Fleck's ideas are truly interesting and useful, then they deserve clarification and modification so that may be usefully adapted to modern concerns rather than remain the fodder for empty philosophical and sociological debates over who influenced whom.<sup>73</sup>

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<sup>72</sup>On Fleck's account, the individual's capacity for free (or at least, self-guided) action and thought is nonexistent. For those who find determinism by physical laws distasteful, Fleck adds more via history and sociology. For what reason does a person join one collective rather than another? Does an investigator, when working on her own, pursue an approach because she chooses to, or is the appearance of choice merely the effect of residual collective forces? Or perhaps the "choice" is due to hormones:

It depends on the intensity of *feeling* of the investigator whether the fact, whether the new shape will appear to him within this chaos as a symbolic vivid vision, or else as a weak hint of resistance which inhibits the free, almost discretionary choice between alternative pictures (Fleck 1935, 77; emphasis mine).

It could have been that because Fleck was writing against then-mainstream of epistemology, he felt he needed to push the importance of the social in individual cognitive activity. He acknowledged in 1947, for instance, that his view would probably be distasteful for those who uphold individuality. Scientists would disagree with Fleck, for by his account "what would become of their renowned genius?" (Fleck 1947a, 151).

But, conceding to Fleck that reductionism is not the key, individual cognitive and behavior is not simply the sum of social and historical effects coming from without, either. Fleck may have been significant in accenting the latter, but his view does not succeed in capturing a satisfactory account of what it means for an individual to think. There is very little discussion of the individual *qua* individual, which under the reductionistic and Fleck's account is starting to seem ridiculous: the person is either a set of physico-chemical processes, or is the result of social-historical forces. In either event, the self reduces to or evaporates to something not very self-like at all. Only an intuitive, personal sense of selfhood independent of chemistry or society prohibits the acceptance of either of the above hypotheses. This is a rationally shaky reason for the rejection. But it is psychologically appealing enough to motivate the desire to look, at another time, for an alternative approach.

There are occasions where Fleck alludes to the effect of individual actions upon styles of thought (as, for instance, his example of creating a new collective to translate a thought across to a different style puts an individual in the deciding role of a social creation). But these actions are largely construed as non-rational events. When the stylistic system is up and running, the individuals think in the prescribed way. When there is room for alternative interpretations, the results come across as psychological or sociological accidents.

<sup>73</sup> Here Löwy and I can agree. She believes that the search for precursors is an historical vice; Fleck and Kuhn worked in different periods and had different goals. "[I]t would be a poor service to both Kuhn and Fleck to read Fleck's work as only a 'prefiguration' of Kuhn's ideas" (Löwy 1989, 47).

Virtually every secondary article, found primarily in sociological sources, attests to Fleck's influence in virtue of a brief mention in Kuhn's preface to *The Structure of Scientific Revolutions* :

[Through a random exploration] that the Society of Fellows permits, ... I have encountered Ludwik Fleck's almost unknown monograph, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basel, 1935), an essay that anticipated many of my own ideas.... Fleck's work made me realize that those ideas might require to be set in the sociology of the scientific community. Though readers will find few references to [his work] ... I am indebted ... in more ways than I can now reconstruct or evaluate. (Kuhn 1970, vi-vii)

Substantiating such an influential link would need to be carefully handled. There may well be, for instance, some surface similarity in Kuhn's paradigms and Fleck's thought-styles.<sup>74</sup> But Kuhn states in the foreword to the English translation of the *Genesis* that at the time he first read Fleck's book he was struck by the "to me unknown and yet vaguely repulsive perspective of a sociology of the collective mind" (Kuhn 1979, ix). Furthermore, Kuhn's identifying distinction between normal science and scientific revolution, according to some authors,<sup>75</sup> is missing from Fleck's text. Thaddeus J. Trenn claims:

Fleck is able to discuss scientific change without dwelling upon the controversial concept of revolutions; indeed, Kuhn's sharp distinction between so-called normal science and revolutionary science has no direct counterpart in Fleck's theory. (Trenn 1979, xiv)<sup>76</sup>

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<sup>74</sup>Those who explore the matter a little more deeply include Wittich (1981) and (1986), Baldamus (1972) esp. 283-284; Hollender and Olszewski (1984) esp. 274-276; Pitt (1981); Jacobs (1987) esp. 266-273.

<sup>75</sup>This is articulated in Hollender and Olszewski's (1984) "On the Origin of the Term and Forming of the Notion of Scientific Revolution," although Kuhn's contribution to the concept is downplayed as well.

<sup>76</sup> I disagree with this. Fleck's discussion of overthrowing old styles of thought (Fleck 1936, 111), and "epochs of equilibrium" followed by gestalt-shifting changes in thought-styles (Fleck 1936, 76-77), closely resembles Kuhn's ideas on revolutionary gestalt switches. See also White (1991).

I suggest that the work of Paul Feyerabend more closely resembles Fleck's, since the quantity and degree of untranslatability between Fleck's styles of thought mirrors the latter's pluralistic view more than Kuhn's paradigmatic monism.<sup>77</sup>

But these tasks, should one choose to accept them, are reserved for another time. Fleck's writings offer many opportunities for exploration for scholars interested in the social sensitivity of scientific activity. However, his reliance upon social and historical determinism renders him mute on other concerns. If wondering "how ought a scientist (or I, for that matter) justify an epistemological or metaphysical claim now?" the answer could not be distilled from Fleck's texts.

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<sup>77</sup> Consider, for instance, Feyerabend's proposal for developing as many different, alternative, scientific theories as possible such that from the perspective of an alternative theory the strength of a tested theory may be determined (criticized).

Alternatives will be more effective the more radically they differ from the point of view to be investigated. It is bound to happen, then, at some stage, that the alternatives do not share a single statement with the theory they criticize. The idea of observation that we are defending here implies that they will not share a single observation statement either. To express it more clearly, each theory will possess its own experience, and there will be no overlap between these experiences (Feyerabend 1965, 214).

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