In brains we trust: How neuroeconomists stylize trust, the brain, and the social world

Klaassen, P.

Citation for published version (APA):
Klaassen, P. (2014). In brains we trust: How neuroeconomists stylize trust, the brain, and the social world.
Chapter 2

Outline of a Fleckian philosophy of (neuro-)science

Would it not be able to manage entirely without something fixed? Both thinking and facts are changeable, if only because changes in thinking manifest themselves in changed facts.

Fleck (1979, p.50)

2.1 Introduction: The continuing relevance of the Fleckian legacy

When the medical serologist Ludwik Fleck began practicing history, philosophy and sociology of science in the 1920s and 1930s, he did so in almost an intellectual vacuum, and his *Genesis and Development of a Scientific Fact* (1979, originally published 1935) fell on deaf ears. Insofar as it fell on ears at all that is: only 200 of the 640 copies published were sold (Trenn 1979, p.xviii).

The central idea to his philosophical venture was that the epistemology of science had much to gain from historical and sociological studies of science. By now this might not seem such an innovative thought, but at the time it was radically novel. In the wake of Thomas Kuhn’s *Structure of Scientific Revolutions* (1996, originally published in 1962) we have witnessed the development of what has been variously called *social studies of science, science studies* or *science and technology studies* (STS), and much of the work done within the loose boundaries of this field continues to prove to this day that sociological, anthropological and historical studies of science can have philosophical import. But Kuhn did not invent the wheel all by himself, and among the resources he had at his disposal and
used to his benefit was the work done by Fleck. Furthermore, Kuhn had the wind in his sails, so to say. By the time he wrote his magnum opus the heyday of logical positivism had to come its end.

The decline of logical positivism was due to the fact that the conclusions of both the Duhem-Quine thesis and of Wittgenstein’s philosophy of language were acknowledged ever more broadly. From the first it was concluded that a clear demarcation between scientific knowledge and anything else—be it nonsensical gibberish, metaphysics or religion—could not principally be settled. If that were indeed true, it would become impossible to justify once and for all why one thing would and another thing would not count as scientific knowledge. The realization of the impossibility of justifying the distinction logically, opened up the possibility that it could be explained historically, sociologically or psychologically—in brief: naturalistically.

Going against the stream, Popper attempted a methodological-cum-logical justification. Central to this is the logic of the *modus tollens* and the “falsificationism” in which this form of argument was granted center stage: Even if theories could not be proven to be true—as this would require a form of induction, which is never logically valid—strong cases could be made to justify decisions to consider them false. Only to theories that could, in principle, be demonstrated to be false, would Popper grant the label of scientificity. Thus, Popper saw the growth of scientific knowledge as a process involving so-called trial and error elimination. The task set for scientists, in Popper’s view, boiled down to deducing predictions from theories and testing those predictions against reality. Despite Popper’s persistent attempts, it was ever more widely recognized that hardly any science would deserve to be called scientific if measured against Popper’s strict standards (Feyerabend 1975, Putnam 1981). How science was factually practiced became an increasingly relevant issue, as opposed to how one could rationally reconstruct its conceptual or theoretical progress.

As mentioned, Wittgenstein’s *Philosophical Investigations* (1953) also played a role in this reform. Wittgenstein’s later philosophy was widely interpreted as further substantiating the empirical turn, as witnessed for instance by its influence on the philosophies of science developed by scholars such as Norwood Hanson (1958), Paul Feyerabend (1975), Thomas Kuhn (1996) and Stephen Toulmin (1953). Specifically, Wittgenstein’s views on the normativity and autonomy of the grammar pertaining to particular *forms of life* were perceived to constitute a philosophical foundation for ideas such as Kuhn’s theory of incommensurable paradigms and the, by then, widely accepted view regarding the mediated character of observation (cf. Sismondo 2004, p.12-13). More than Popper cared to admit, these views contained significant difficulties with regard to the relationship between theory and observation and the priority given to theory in Popper’s philosophy of science.
The relevant point in the present context, however, is that Fleck had argued much the same several decades earlier, even if he did so to no avail. Fleck too had conceived of science as being continuous with nonscientific endeavors, and Fleck, too, emphasized discontinuities in scientific thinking and acting and the implications of these for matters of ontology. But for Fleck the central concept was not “form of life” or “paradigm,” but rather “style.” What most distinguished this alternative approach from more orthodox ways in which philosophy engaged with science, then, was the role granted to the empirical details of actual scientific work, placing them above, first, logical reconstructions of scientific research and, second, such end products of science as facts, truths and certain knowledge.

In the wake of philosophers such as Fleck, Kuhn and Wittgenstein philosophers of science have stopped paying exclusive attention to ideas and ready-made facts and have come to also investigate the often muddy practices in which scientists are engaged. Insofar as logic figures in this alternative approach, it is the practical logic embodied in the work of actual scientists. Questions pertinent to this branch of philosophy of science concern, for example, the conditions under which specific types of knowledge are possible at all and the meaning and implication of different epistemic orders in- and outside the science(s) studied. Rather than asking why something counts as scientific knowledge, the question becomes how something becomes scientific knowledge and how it subsequently remains scientific knowledge. In other words, time is an important factor in this philosophical perspective on science.

In this dissertation the achievements of several decades of fruitful STS research will not be ignored, of course; nonetheless, I take my principal inspiration from Ludwik Fleck. The reasons for this are straightforward. To begin, Fleck’s work is very suggestive, posing as it does an invitation to examine many types of questions and tracing many actors and entities involved in the practice of science. The role of history comes to the fore, but so do the roles of technology, the social organization of science and science’s role(s) in society at large. Following up on even just some of these themes will ensure the production of descriptively rich case studies.

In addition, Fleck’s move toward the formulation of a theory of thought styles is especially apt for providing the conceptual equipment to carry out projects in “comparative epistemology” (Fleck 1979, p. 22). As this dissertation substantiates, this type of endeavor provides analysts of science with a way to evaluate scientific enterprises without somehow assuming a superior form of insight into the matter at issue in the science studied. On a lighter note, it might be said that turning to Fleck when constructing one’s theoretical framework has the advantages that, first, he is a benchmark figure in the field and, second, his conceptual armory is relatively fixed. What changes to Fleck’s armory are still possible can only come from interpreters or users, not from a scholar who is in the business of
developing increasingly fine-tuned instruments for the investigation of science. However, against the allegation that framing the present investigation along lines drawn by Fleck is thus merely an instance of opportunism, I contend that even if Fleck is a benchmark figure, too little has been done to exploit the resources his work has to offer. Thus, I consider illustrating the usefulness and lasting topicality of Fleck’s philosophy to be an important goal in itself, even if it is secondary to understanding, in this case, neuroeconomics.

Of course, the riches of Fleck’s philosophy must manifest themselves in the fruits the philosophy allows to be reaped. Here this boils down to the question of whether Fleck’s philosophy helps one gain a better understanding of neuroeconomics and its particular stylization of trust. My goal in this chapter is limited to outlining the Fleckian approach to science and to providing support to that approach in a relatively abstract manner. To that end I will introduce the historical figure of Fleck (section 2.2), review part of his major case study (section 2.3), detail the philosophical and heuristic lessons that can be drawn from this case study (section 2.4) and discuss alternative philosophies that either contrast with or complement Fleck’s conceptual armory (section 2.5). In section 2.6 I will conclude the chapter with a brief overview of how the remainder of the dissertation will be structured, relating the various chapters to the Fleckian ideas presented here.

2.2 Ludwik Fleck

Fleck was born in 1896 in the city of L’viv, now in Ukraine but, at the time, in Poland and called Lwów. Educated as a medical doctor, he worked as a microbiologist throughout his career. Even during World War II, while being held captive because he was Jewish, he practiced microbiology. Having worked out a way of procuring a typhus vaccine from the urine of patients suffering from typhus, he was put to work at a pharmaceutical company called Laokoon to produce typhus vaccines for German soldiers. After that, he was first transported to Auschwitz and, later, to Buchenwald. In Buchenwald, after first having been forced to work as a laborer, he was put to work in a laboratory responsible for, amongst other things, the production of typhus vaccines for the Nazi’s.¹

From the late 1920s onwards, however, Fleck not only worked as a microbiologist, but also engaged in philosophical reflections on science. This resulted in several articles⁶ and eventually in the aforementioned monograph Entstehung und Entwicklung einer wissenschaftlichen Tatsache (Fleck 1979, originally published 1935). Despite some reviews in what were mostly medical journals, Genesis and Development of a Scientific Fact gathered hardly any attention at the time.

Nonetheless, today Fleck is not just a well-known figure but even an icon of a particular way of practicing philosophy of science and STS. Indeed, the annual prize by one of the
two largest scholarly associations dedicated to STS, the Society for Social Studies of Science (abbreviated 4S), is named after him. Oddly, however, even though he is widely recognized as a (philosophically inclined) precursor to this field, his work is still relatively little read.

Of course, the notion of “precursor” is historiographically problematic, if not outright suspicious. However, rather than using this concept here to illumine the history of the reflection on science, I use it to note that the self-image of contemporary analysts of science is justifiably described using this term. For, in fact, many a science scholar presents Fleck as a precursor to his or her own work. This already starts with Kuhn’s *Structure of Scientific Revolutions* (1970, originally published 1962). As mentioned, this is undeniably one of the groundbreaking works with regard to the naturalization and historicization that has become a part of the philosophy of science since the 1960s. Kuhn played a major role in bringing Fleck’s work to a larger audience, not only by mentioning that it “anticipates many of my own ideas” (Kuhn 1970, ix) in the preface to his *Structure*, but also by insisting on the publication of an English translation of it. In 1979 this translation finally appeared. Moreover, the description of Fleck’s work as precursory to either one’s own work or to science studies in general continues to this day.

The wide variety of authors who mention Fleck as a forebear indicates a reason why it is problematic to unqualifiedly characterize Fleck as a predecessor to STS. It presupposes some questionable convictions: that STS is a uniform field with one clear lineage through time; that there is no such thing as interpretative flexibility; and, hence that there is but one way to capitalize on Fleck. These considerations speak against the unqualified use of the notion of precursor and provide us with a motive for introducing somewhat elaborately what I take a Fleckian approach to science to mean. However, that there is disagreement on what lessons should be drawn from Fleck’s work is not a topic of investigation here. What must become clear, though, is that Fleck is not merely a fascinating figure, an exotic outlier at the time of his writing and a curious predecessor (to stick to the term in spite of everything) to much contemporary work in science studies, but that even today his work provides a viable resource for those interested in better understanding science.

No serious interpreter of Fleck, however, can question that case studies are a *sine qua non* in the Fleckian approach to the philosophy of science. Rather than considering in general terms the dynamics or progress of science, the nature of scientific knowledge, the role of scientists and that of the objects of scientific research and experimentation from the safe haven of one’s armchair—in other words, engaging in “an epistemology of the imagination” (Fleck 1979, p.21), analysts of science are encouraged to get their boots dirty, to engage, either historically or contemporaneously and in any case comparatively, with scientific practices as actually performed (Fleck 1979, p.21). To understand what it means to do all this along Fleckian lines and to see what this yields, I will briefly go over Fleck’s
own most extensive case study before expanding on Fleck’s philosophy of science more systematically.

2.3 Fleck’s case study of syphilis

For a long time, diagnosing “the great pretender” syphilis was a tough job—especially in its later stages. It was around four hundred years after syphilis had first been distinguished as a disease in late fifteenth- or early sixteenth-century Europe, before Fritz Schaudin and Erich Hoffmann identified the spirochete bacterium *Treponema pallidum* as its causative agent in 1905 and an “easy” and, subsequently, reliable diagnostic test for the disease in all stages of its development could be developed by August von Wassermann in 1906.\(^1\)

That it took so long undoubtedly had something to do with the reasons why syphilis has come to be known by the epithet of “great pretender”: It goes through four different phases, the first and second of which show symptoms that are also indicative of a host of other disorders. Furthermore, there do not seem to be any reliable essential signs. Even the lesion, with which the first stage of the disease usually reveals itself, is reported to have not occurred by many people who present with secondary syphilis (Mullooly & Higgins 2010).

The sexually transmitted infection usually begins with a lesion developing, on average, some 21 days after infection. The infection is commonly located on the genitals, but can potentially be anywhere on the body. It can also be transmitted at birth, in which case it results in congenital syphilis.\(^1\) Next comes a stage characterized by one or more of a large variety of symptoms, including but not limited to various afflictions of the skin, the mucous membranes and the lymph glands. After this, a latent phase, commonly lasting several years, commences; during this phase people do not show any symptoms, but are still contagious. Lastly, there is tertiary syphilis, which if the disease has not been treated, starts 3–15 years after the initial infection. This noninfectious stage comes in a variety of forms and can damage the brain, nervous system, heart, blood vessels, liver, bones or joints.

Finding a standardized and reliable diagnostic instrument for a disease as intangible as syphilis was a big challenge to medical science. The Wassermann test, based on the principle of complement fixation, tested for the presence of antibodies produced by persons carrying the bacterium *Treponema pallidum*. It became one of the first medical technologies to be governmentally regulated.\(^1\) Furthermore, the fight against syphilis was also of political importance, because of the strong moral connotations the disease carried and because fighting syphilis was considered to be an important part of the battle against “degeneration,” which at the time, was at the core of eugenic programs throughout the world (cf. Löwy 2004).
A rewarding case

This introductory account of syphilis and the history of its diagnosis provides some requisite background (and contrast) to Fleck’s major case study. For it was on the basis of an investigation of the history of syphilis, of ideas about syphilis and of practices of dealing with syphilis, ultimately culminating in the discovery of the fact that the Wassermann reaction was related to syphilis, that Fleck articulated his ideas on science, knowledge, truth, facts and society.

Fleck had good reasons for putting the Wassermann reaction center stage in his major philosophical investigation, and he made these explicit in his prologue. First, he stated that standard examples of facts considered reliable and worthy of investigation in epistemology, facts from everyday life or classical physics that is, all suffer from the same weakness: they are worn out and naive, and therefore do not allow for critical investigation.

Fleck’s worry is reminiscent of one Wittgenstein expressed in §593 of his *Philosophical Investigations* (1953): “[a] main cause of philosophical disease [is] a one-sided diet: one nourishes one’s thinking with only one kind of example.” Wittgenstein battled this disease by enriching his diet of examples with all kinds of language games. All of these were so-called thought experiments and some were completely imaginary (e.g. §142). Fleck chose another, more obviously empirical route; as he stated in his prologue, “a ‘more recent fact,’ discovered not in the remote past and not yet exhausted for epistemological purposes, will conform best to the principles of unbiased investigation.” Furthermore,

> [a] medical fact, the importance and applicability of which cannot be denied, is particularly suitable, because it also appears to be very rewarding historically and phenomenologically. (Fleck 1979, p.xxviii)

The fact at issue was that the Wassermann test was related to syphilis. Before he discussed the genesis and development of this fact, however, Fleck first described the variety of conceptions of syphilis which could be identified between the start of the twentieth century and late fifteenth century—from Fleck’s present back to the oldest historical sources that recognizably concern syphilis (Fleck 1979, p.1). In this timespan he identified four different conceptions of syphilis: the mystic-ethical, the empirical-therapeutic, the pathogenic and the etiological. Each of these conceptions differed from the others in such terms as how the disease was defined, how it was diagnosed or what was considered a cure. To express this diversity, he used a term that became notorious in philosophy subsequent to Kuhn’s publication of *Structure of Scientific Revolutions* in 1962: incommensurability (Fleck 1979, pp.36, 62).

As is so common in attempts at projecting contemporary conceptions of disease (or anything else really) back into history, there are serious difficulties in doing this. The borders
of the disorder, to be sure, will have changed significantly, as will have the alleged symptoms. Indeed, further back than the end of the fifteenth century, it becomes impossible to find anything that can be identified with what we call syphilis without becoming overly charitable.

To a present-day medical doctor, it is obvious that what we now consider to be so many distinct diseases—tuberculosis, scabies, leprosy, gonorrhea, soft chancre and more—were once jumbled together. The first steps toward disentangling these diseases were taken at the close of the fifteenth century. At that time, and stimulated by a series of epidemics that took place in the wake of “wars, famine and natural disasters, such as extreme heat and floods” (ibid., p.2), syphilological thought arose.

Astrology figured centrally in Renaissance thought and fifteenth-century explanations of syphilis were stylized in conformity with this: “almost all the authors of that period hint at the sidereal origin of syphilis as the first and most important cause of the epidemic” (ibid.). Fleck quoted Bloch’s two-volume history of syphilis from 1902: “Most authors assume that the conjunction of Saturn and Jupiter under the sign of Scorpio and the House of Mars on 25.XI.1484 was the cause of the carnal scourge. Benign Jupiter was vanquished by the evil planets Saturn and Mars. The sign of Scorpio, which rules the genitals, explains why the genitals were the first place to be attacked by the new disease” (ibid.). Syphilis, we see, was construed as a venereal disease and was done so partly for astrological reasons. Being a venereal disease, it was simultaneously implicated in a specifically moral view. It was thought that syphilis was sent by God to make man abstain from the sin of fornication.

This line of thought was what Fleck called the mystic-ethical conception of syphilis, and he showed that the understanding of syphilis as a carnal scourge or Lustseuche had clear ramifications for how it was diagnosed, for instance. Using his contemporary classification of diseases, Fleck wrote that this conception of syphilis also comprised “gonorrhea, soft chancre and lymphogranuloma inguinale” (ibid., p.3), in addition to what is today understood as syphilis.

Partly coexisting with and partly succeeding this, Fleck identified the empirical-therapeutic conception of syphilis (ibid., p.5). At the core of this conception was the practice by physicians to connect syphilis to mercury, based on the skin-related character of the disease. This followed from the use of mercury for treating various skin diseases, something that was already common at the time (ibid., p.4). The connection between syphilis and mercury became so strong that mercury was used not only as a treatment but also as a diagnostic aid. Syphilis, in this conception, was the disease that responded to mercury compounds. Hence, this conception excluded the third stage of the disease, since it does not show on the skin.

Fleck also identified what he called the “pathogenic” conception of syphilis, “that is, opin-
ions about the mechanism of the pathological associations” (ibid., p.11). This conception, which relates syphilis to the blood, can be found in the oldest writings on syphilis and, in fact, can be traced back to the (ancient) theory of the humors, according to Fleck. “Foul blood” was what explained syphilis, in this view. This type of conception, according to Fleck, helped people understand the “generalized” or “constitutional” character of the disease.  

The final conception of syphilis that Fleck identified was the etiological one, which has prevailed from 1905 onwards, since Schaudinn and Hoffmann identified the pale spirochete as the causative agent of syphilis. The rise of this conception almost coincided with Wassermann’s achievement of finding a blood test for syphilis—which Fleck conceived of as realizing “the ancient wish of the collective: the demonstration of syphilitic blood” (Fleck 1979, p.70). Fleck claimed that ideas identifiable in older conceptions of syphilis, conceptions which cannot be said to be properly scientific by today’s standards, could still be shown to be effective when Wassermann did his work at the start of the twentieth century. “Urideen” or “proto-ideas,” Fleck called such ideas. Fleck’s clearest example of this was syphilis being a condition of the blood—“bad blood.”

2.4 How to think about science?

This was how Fleck narrated the history of syphilis. But what kind of history is this? Or, perhaps more to the point, what precisely is it a history of?

The account I have just outlined obviously involved ideas about syphilis. Fleck showed that these ideas changed through time and he showed how and in response to which events they changed. Moral, religious, experimental-scientific and technological aspects all had a role to play. In short, in the fifteenth century syphilis was thought of as something intrinsically tied up with morality and religion as well as the constellations of the stars, but by the early twentieth century it was thought of as a bacterial disease. As is also apparent from Fleck’s exposition, some things remained (relatively) constant throughout this history. Specifically, the moral connotation syphilis had stayed with it, as did the idea that it was connected to “foul blood.”

But Fleck’s history is not only a history of ideas; it is also a history of a fact, namely “the fact that the so-called Wassermann reaction is related to syphilis” (Fleck 1979, p.xxviii). Fleck recounted in detail how Wassermann and his fellow researchers discovered this. So far, Fleck’s account is harmless with respect to the standard view of science. But Fleck’s view of history and its relevance to a proper understanding of science starts to deviate from the standard view if we consider that, in the latter the history of facts is irrelevant to their existence. Indeed, in the standard view of science, facts are understood as precisely the sort
of thing that elude historicity. In this view facts in and of themselves are timeless, and the only historical tales that can be told about them concern their discovery.

In the Fleckian view, however, facts essentially involve history, and not merely accidentally. In Fleck’s own words, “a fact always occurs in the context of the history of thought and is always the result of definite thought style” (Fleck 1979, p.95). Facts, thus, result from constraints on thought which are encapsulated by Fleck with the notion of style. Thus, it follows that it

is not possible to legitimize the ‘existence’ of syphilis in any other than a historical way. To avoid unnecessary and traditional mysticism it is thus appropriate to use the term ‘existence’ restrictively as only a thinking aid and convenient shortcut. (Fleck 1979, p.23)

Fleck refrained from speaking of existence as something unqualified and absolute. To understand why this was the case, consider his view on the four different stylizations of syphilis we encountered before and the lessons he drew from his observation that there were all these four stylizations. At the root is the observation that “[a]ll these points of view are based upon observation, perhaps even upon experiments, and none can simply be declared wrong” (Fleck 1979, p.8). Whatever definition one adheres to, it will determine how one investigates the disease—with which questions in mind, on the basis of which certainties and by using which technologies. Eventually, it will lead one to some and not to other conclusions regarding the nature, diagnosis and treatment of syphilis, because for each of the styles, syphilis is a different type of entity. “It is only after the choice has been made that the associations produced by it are seen as necessary” (Fleck 1979, p.8).

Fleck put this in more systematic form by identifying a two-part relationship between so-called active and passive linkages or associations which are always implicated in epistemic processes. Active linkages encompass everything with which researchers come equipped. For instance, in Fleck’s history of syphilis “the generic concept of carnal scourge was thus an active association of the phenomena, explained in terms of cultural history” (Fleck 1979, p.10). Passive linkages, on the other hand, are all those signs of resistance that subsequently seem to exist independently from the active linkages. But Fleck did not fail to emphasize that what follows passively, in fact, cannot be formulated or identified if it were not for the active elements in cognition. Again an illustration from the history briefly reported above:

a restriction of the curative effect of mercury in the sentence […] asserting that ‘sometimes mercury does not cure the carnal scourge but makes it even worse’ represents a passive association with respect to the act of cognition. (Fleck 1979, p.10)

This identification of active and passive linkages in knowledge and in the procedures for acquiring knowledge may appear to show affinity with the philosophy of science position
defended by Fleck’s better-known Wiener Kreis contemporaries. For they too emphasized that empirical statements are somehow relative to systems of representation or conceptual frames that are only conventionally subscribed to, and which can only be judged on grounds such as simplicity.

Fleck parted from the moderate conventionalism of the logical empiricists on two grounds. First, he denied liberty in regard to choosing what principles or active linkages to use. Rather than being up for grabs by any individual or community of researchers, the active linkages one works with are, as it were, handed over by tradition. Through training they are instilled in members of epistemic communities, which Fleck calls *thought collectives*. Moreover, such principles are materialized in the research technologies these communities use—something also argued by Pierre Duhem, for instance (1982, originally published 1906). History and the sequence of things matter, so Fleck underscored.

Fleck also parted company with the empiricists in regard to their insistence that the growth of knowledge entails only an increase in what follows passively, not in what relates to this actively. Thus, as empiricists would put it, the ratio between what is empirical and what is conventional changes with the progress of science. An ever larger part of what we say we know derives from our experience rather than from anything else, in this view. According to Fleck, however, it does not even make sense to speak of a reality of passive facts in isolation from the active linkages at work in our attempts to get at them. Any increase in passive linkages will only come about if sufficient investments are made (i.e., if the number of active linkages increases too).

The distinction Fleck made between active and passive linkages, then, did not simply follow the demarcation empiricists adhered to between what is given through the senses and what is not. In Fleck’s view observation does not start with what is given to the senses. We have no grounds for arguing for the existence of syphilis, for example, independent from our historically developed and culturally colored ways of knowing.

This view of knowledge, to which history and development are so crucial, also involves the idea that knowledge cannot be understood as the property of isolated individuals. In opposition to the individualist idea, Fleck suggested that knowledge is intrinsically a social achievement:

> Every epistemological theory is trivial that does not take this sociological dependence of all cognition into account in a fundamental and detailed manner. 
> (Fleck 1979, p.43)

This does not mean that knowledge is solely a social achievement, something which is clarified in its entirety once its social construction is laid bare. Any account of knowledge must also bear a relation to what it is knowledge of. Nor does Fleck’s view constitute a threat to the objectivity of knowledge. Quite the contrary! Fleck’s aim was to understand
how objective knowledge is possible, and this led him to describe the social organization
of science as a condition of the possibility of knowledge. As he put it:

those who consider social dependence a necessary evil and an unfortunate
human inadequacy which ought to be overcome fail to realize that without
social conditioning no cognition is even possible. Indeed, the very word
‘cognition’ acquires meaning only in connection with a thought collective.
(Fleck 1979, p.43)

Claims to knowledge without reference to a collective beyond the knowing individual
at issue are simply incomplete assertions, on Fleck’s view. Thus, stating that someone
knows this or that or recognizes such-and-such “demands some such supplement as […]
‘as a member of a certain cultural environment,’ [or] ‘in a particular thought style, in a
particular thought collective’” (Fleck 1979, p.39).

Thus, there can be no knowledge in the absence of thought collectives. But what are those
thought collectives that Fleck proposes? A thought collective is “a community of persons
mutually exchanging ideas or maintaining intellectual interaction” (1979, p.39) and carrying
what Fleck calls a thought style. Vital to science is the communicative interaction be-
tween the individuals making up the relevant collective. This is, in part, because of the
training through which newcomers, by necessity, internalize the external constraints of
the thought style—“we look with our own eyes, but we see with the eyes of a collective
body” (Fleck 1986c, p.134), as Fleck expressively described the outcome of this process of
disciplining. In addition, it is because the communicative interactions taking place within
a collective have a major part to play in the generation, stabilization and change of knowl-
edge over time.

Fleck identified several characteristic aspects of the structure of thought collectives. Most
central is the division, present in any thought collective, between an esoteric circle and an
exoteric circle which relate to each other much like elites and masses do. The esoteric
circle of modern science is divided into a core of specialists, with a somewhat wider circle
of general experts around it; then, outside the circle of general experts, the exoteric circle
is populated by laymen. Anyone can be a member of various exoteric circles, but only of
(very) few esoteric circles, if any at all.

In each of these circles, scientific knowledge is dealt with differently. In the exoteric circle
where popular science circulates, the contents of scientific accounts are purged of all the
details, complexities, worries and controversies which are visible in the esoteric circle. Ac-
counts of science in the exoteric circle do not aim to convey specific scientific details nor
to spur debate. Instead, they present a broad and general outlook in which what is debated
in the esoteric circle is taken for granted, and all that springs from there is beyond doubt.
This, however, does not mean that only the esoteric circle can influence the exoteric one,
without any chance for the exoteric circle to have an impact on the esoteric one. As the history of syphilis shows, the idea of foul blood and the moral connotation of syphilis existed no less in the esoteric circle than in the exoteric one, and purportedly both circles had their role to play in Wassermann’s focus on this disease. In fact, Wassermann was directed to focus on syphilis (rather than, for instance, on the much more harmful disease tuberculosis) by Friedrich Althoff, head of the ministry of educational and medical affairs, who feared that the French would be the first to find an accurate diagnostic instrument for it (Fleck 1979, p.68). Moreover, if it had not been for the insistent clamor of public opinion for a blood test, the experiments of Wassermann would never have enjoyed the social response that was absolutely essential to the development of the reaction, to its ‘technical perfection,’ and to the gathering of collective experience. (Fleck 1979, p.77)

What happens in the exoteric circle, then, is just as likely to determine what happens in the esoteric circle as is the other way around.

Furthermore, both circles know specific literary technologies used for communication in and between these different circles. For instance, moving from the core of the esoteric circle outwards, one first finds articles published in specialized scientific journals, next in textbooks meant for those who are working their way from the exoteric circle inwards, then in handbooks for general experts and, finally, in popular science accounts of all forms. The further one moves away from journal articles, the firmer the grip of style and the easier claims to factuality are made without referring to the context of their creation.

As to the relation between the thought collective and the thought style, Fleck stated that the thought collective

provides the special “carrier” for the historical development of any field of thought, as well as for the given stock of knowledge [...] This we have designated thought style. (Fleck 1979, p.39)

Thus thought collectives are groups of mutually interacting people who share a thought style—that is, who share a common background or disposition which drives, constrains and makes possible cognition. Once again in Fleck’s words:

We can [...] define thought style as [the readiness for] directed perception, with corresponding mental and objective assimilation of what has been so perceived. It is characterized by common features in the problems of interest to a thought collective, by the judgment which the thought collective considers [obvious], 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 knowledge. (Fleck 1979, p.99)
Fleck brings together a heterogeneous mixture of elements in his concept of thought style, to be sure: What is *stylized* or *conforming to style* comprises much more than thinking alone. Everything from the choice of objects of investigation to the ways in which the results of research are written and published is covered by the concept, including all that happens in between. Therefore, people who belong to the same (thought) collective will generally not disagree on which methods and which research technologies to use for arriving at the truth about the objects of interest—better still, the material, technological and hands-on, practical experience of those educated in or socialized into a particular collective are all aspects of style too. Because thought style and thought collective encompass so much beyond thought or thinking, instead of strictly sticking with Fleck’s terminology, in the remainder of this dissertation I simply speak of style and collective when I employ these concepts. Style and collective are, thus, meant to have the same meaning as Fleck’s thought style and thought collective, but style and collective express Fleck’s intended meanings without, as Fleck did, confusingly emphasizing the importance of thought.

**Style: Epistemology and ontology, history and sociology**

In order to firmly grasp the remaining part of Fleck’s philosophy of science, it is important that, in addition to the historical sequence narrated in section 2.3, I also provide some details of Fleck’s analysis of the genesis of the central fact at issue in his *Genesis and Development*—that the Wassermann reaction is related to syphilis. As I mentioned before, Fleck considered the Wassermann test to be a realization of an “ancient wish of the collective” (Fleck 1979, p.70), namely, the proto-idea that syphilis was a disease that involved foul blood. More valuable philosophical lessons can be drawn from this case, however. Fleck described what was involved in the process leading up to the discovery of the Wassermann reaction as follows:

Skills, experience in the field, and ideas whether ‘wrong’ or ‘right’ passed from hand to hand and from brain to brain. These ideas certainly underwent substantive change in passing through any one person’s mind, as well as from person to person, because of the difficulty of fully understanding transmitted knowledge. In the end an edifice of knowledge was erected that nobody had really foreseen or intended. Indeed, it stood in opposition to the anticipations and intentions of the individuals who had helped build it. For Wassermann and his co-workers shared a fate in common with Columbus. […] They wanted evidence for an antigen or an amboceptor. Instead, they fulfilled the ancient wish of the collective: the demonstration of syphilitic blood. (Fleck 1979, pp.69-70)
What at one point in time had been seen as a crucial part of the research (i.e., antigens in organ extracts), left the stage and made way for antibodies in the serum. How the thought collective around Wassermann came up with a working test, one that had a sufficiently high success rate, was impossible to reconstruct. It took a whole lot of tinkering, as Fleck vividly describes (Fleck 1979, pp.72-73). The tinkering was done in conformity to the style dominant in medicine at the time, where style dictates both methods used and ontology enacted. Following the reconstruction of Fleck’s arguments by Henk Van den Belt and Bart Gremmen’s (1990), the style at work in Wassermann’s research is best interpreted as the *style of specificity*, because the idea of specificity aptly covers the “stylistic bond [connecting] many, if not all concepts of [the] period” (Fleck 1979, p.9). Thus in serology, for instance, the relation between antigen and antibody was assumed to be specific; in nosology, the style of specificity appeared in the form of an ontological as opposed to a physiological conception—classifying disease in terms of distinct disease entities (rather than in terms of the loss of balance, for instance, which was central to traditional Hippocratic medicine); furthermore, each disease entity was supposed to correspond to a specific etiology (Van den Belt & Gremmen 1990, p.468-469).

As Wassermann brought syphilis into the laboratory, the active linkages became even more various and numerous than they were before. They now included the enormous amount of skilled labor, informed by extensive (tacit) knowledge, as well as the instruments and solvents used, the theories that guided the choices made, and so on. As Fleck indicated, nonscientific factors also entered the picture (i.e., factors which were not exclusive property of the esoteric circle). Thus, when Wassermann began to study syphilis, not only the disease entered the laboratory, proto-ideas shared throughout the more exoteric quarters of the collective followed. The “prescientific” view about foul blood is one example, as is the moralistic connotation of syphilis. As we saw previously, the latter played a vital role in the instigation of this research in the first place (Fleck 1979, p.77).

Moreover, Fleck recounted that some of the beliefs on which “Wassermann and his co-workers” acted were “completely mistaken” (Fleck 1979, p.74). Significantly, they built on the false idea that “a specific antigen-antibody reaction […] had been demonstrated” (ibid.). Thus, Fleck determined that, in the case of the Wasserman test, false assumptions led to true knowledge, and Fleck showed how this could happen. What is required, is that enough people invest sufficient amounts of time, labor, skill and technologies. Indeed, as Fleck beautifully illustrates, it is no coincidence or miracle that, if those conditions are met, false assumptions can lead to truth:

> 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. (Fleck 1979, p. 78)

Replace the manner in which style directs perception and intellectual interest with “grav-
ity” and all the work supplied by the collective with “water,” and the pieces fall into place:
a “true” finding arises. What such a true finding is, however, cannot be understood in
terms of the age-old metaphor, according to which scientific knowledge mirrors nature (cf.
Rorty 1979a)—the metaphor that is either implicitly or explicitly at play in a lot of work
in the philosophy of science (cf. chapter 1, section 1.3). This is not because Fleck ascribed
to a radically relativistic view of truth, even if it was radically “relationalist”:

‘a stylized solution, and there is always only one, is called truth. Truth is not
“relative” and certainly not “subjective” in the popular sense of the word. It
is always […] completely determined within a thought style. 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 will just not be the
same thought! It must either be unclear to, or be understood differently by,
one of them. Truth is not a convention, but rather (1) in historical perspective,
an event in the history of thought, (2) in its contemporary context, [a] stylized
thought constraint. (Fleck 1979, p. 100)

This epistemology does not allow for a conception of timeless, universal truths. In this
view there is no neutral place outside history from which such truths can be identified.
But nonetheless, Fleck’s epistemology is not relativistic. Given a style, it follows what is
and is not true. This is not subverted by the fact that there is always a way in which the
truth of a statement has to do with all the relations the statement is engaged in, with all the
active and passive linkages connecting up with it. That statements can only exist in such
complex webs of linkages is why I use the concept of relationism; the notion of “truth”
 can be of value, even if truth is (only) an event taking place in a network that is always in
the process of change.

As to this change, it deserves emphasis that Fleck viewed the identities of the individual
elements caught up in a network as being dependent on their place in that network. Thus,

[e]very change and every discovery has an effect on a terrain that is virtually
limitless. It is characteristic of advanced knowledge, matured into a coherent
system, that each new fact harmoniously—though ever so slightly—changes
all earlier facts. Here every discovery is actually a recreation of the whole
world as construed by a thought collective. (Fleck 1979, p. 102; my italics)
Similar to the ideas on knowledge and reality from such pragmatist thinkers as John Dewey, William James and Bruno Latour, Fleck attached little importance to the difference between what the world is and what we come to know it as—for the straightforward (anti-Kantian) reason that there is no way of telling what the world is like in itself, as contrasted with how it is known. Or, as Dewey expressed it:

Knowledge and science, as a work of art, like any other work of art, confers upon things traits and potentialities which did not previously belong to them. Objections from the side of alleged realism to this statement springs from a confusion of tenses. Knowledge is not a distortion or a perversion which confers upon its subject-matter traits which do not belong to it, but is an act which confers upon non-cognitive materials traits which did not belong to it. (Dewey 1929, p.381)

With the passing of time and with sufficient investments made towards the creation of knowledge, facts are allowed to emerge. And the emergence of facts entails that nothing stays the same—neither some supposedly timeless subject of knowledge nor its objective equivalent. The world emerges as having other traits than previously presumed and “that, so to speak, offers you a (partially) new mind endowed with a (partially) new objectivity” (Latour 2008, p.92). Thus:

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. The less interconnected the system of knowledge, the more magical it appears and the less stable and more miracle-prone is its reality, always in accordance with the thought style of the collective. (Fleck 1979, p.102; my italics)

The concepts of style and collective elaborated above are as central to my Fleckian view as are those of active and passive linkages. Altogether, a Fleckian perspective emphasizes the networked character of science and reality, according to which what we call truth and what we call reality is in fact a “network in continuous fluctuation” (Fleck 1979, p.79). Style, as conceived of by Fleck, is necessarily carried by a collective, and as such the concept is closely tied to specific scientific practices. This coupling to concrete practices is also borne out by the fact that styles encompass, as “active linkages,” very heterogeneous sets of (f)actors involved in scientific practice. We have seen or will see shortly that these linkages include all the research technologies they comprise, their underlying assumptions and implicated theories, the skilled labor required to complete experiments, the models at work in the construction of experimental setups and in the interpretation of data as well as the inscriptions and transcriptions inevitably involved in the process, from first sketching
out an experiment to publishing the facts the results allegedly render real. Moreover, styles exert their influence on and are influenced by not only those working in the esoteric circle of the collective carrying that style, but also potentially by all who are in the exoteric circle of the collective carrying that style.

As a final note, then, I will highlight that Fleck’s concept of style ensures that in analyzing science, first, epistemological issues are not divorced from ontological issues and, second, no a priori distinction is made between what is “internal” to science and what “external.” The first point is evident in the conception of active and passive linkages. Active linkages are those connectors or associations that lead us to reality as it is perceived objectively and independently from such linkages—that is, active linkages are to be understood in contradiction to passive linkages, which correspond to the “constrained results [. . . ] and constitute that which is experienced as objective reality” (Fleck 1979, p.40). In this view facts are passive resistances to active linkages and are not capable of being conceived without such stylized linkages. Their emergence in the process of inquiry is determined by the harmonious styles that direct perception and is in accordance with the objects and beliefs that are assimilated. Facts therefore come, inevitably and independently, from the active linkages, but only because of the “mutually supporting ideas [, technologies, practices, models,] values [and the like] that constitute a thought-style” (Fagan 2009, p.273).

The second point is evident in the the transience of styles, an understanding of which involves the study not only of present scientific practice, but also of history. Given the nature of the linkages that styles encompass, such history may involve social and cultural histories, technology histories, and so on. No advance assumptions are made regarding what sorts of factors should or should not be included to promote the understanding of this history.

Before I employ this philosophical approach to science in chapters 3 through 7, I will briefly examine how it relates to some of its intellectual kin.

2.5 Fleck’s style compared to some related concepts

Readers of Thomas Kuhn and Michel Foucault might think that the concept of style used here closely resembles the concepts of *episteme* (cf. Foucault 1994) and *paradigm* (cf. Kuhn 1996), respectively. Both are better-known concepts used to conceptualize cognitive organization, and all three concepts share a commitment to the tangle of ways of knowing and things known. The very existence of the objects of research, so these notions help understand, cannot be disconnected from the range of material and institutional conditions which, each in their own way, the concepts of episteme, paradigm and style identify. Adding to this is philosopher of science Ian Hacking, who has developed his own
concept of style in his philosophical analyses of science. Without indulging in too much “philosophology” (i.e., the study of philosophical positions for the sake of itself), I will briefly explain how style, as I use it here, relates to each of these three comparable concepts; in the process, I will articulate what is distinctive about Fleck’s concept. Moreover, at the end of this section I will add some complementary instruments to Fleck’s conceptual toolkit, which I borrowed from Hans-Jörg Rheinberger’s work in historical epistemology.

Style vs. Episteme

To begin with Foucault’s concept of episteme, we find that Foucault used this to comprehend the cognitive unity shared over a wide variety of disciplinary fields and throughout long stretches of time. Investigating the modern sciences of economics, biology and linguistics and their prescientific counterparts of the Renaissance (±1500–1650) and the Classical Age (±1650–1800), Foucault used the idea of episteme to direct attention to similarities discernible in the “depth” beneath each of these fields throughout each of these periods. The similarities Foucault encompasses with his notion of episteme concern how order is experienced, how knowledge is thought of and how (linguistic) signs are presumed to represent what they stand for. Foucault claims that between the Renaissance and end of the twentieth century, the interpretation of each of these—order, knowledge and signs—has radically changed twice. Each of the three epistemes Foucault identified, then, is characterized by a particular way in which order was experienced, knowledge conceived of and the nature of signs and their interpretation were conceptualized, no matter what field of study one looks at.

For instance, if one looks at the depth structure of Renaissance knowledge practices, one sees that resemblance is the main ordering principle and also the sine qua non for understanding knowledge and signs. As Foucault put it, “the experience of language belongs to the same archaeological network as the knowledge of things and nature. To know those things was to bring to light the system of resemblances” (Foucault 1994, p.41). Thus, in looking at classical investigations of wealth, nature and language, according to Foucault, one will find a similar unity. These, too, all share the same principles of order and the same preconceptions regarding the nature of knowledge and of signs. In this case, representation rather than resemblance is the keyword. The relationship between signs and the world is one of representation, as is the relationship between knowledge and the world.

Whereas epistemes in Foucault’s scheme cover large spans of time and very divergent fields of research, the notion of style is more restrictive in both cases. Furthermore, the concept of episteme is more essentially, and perhaps also more exclusively, tied to abstract ideas concerning the nature of knowledge and the nature of (linguistic) signs than is style. Style, as I use it here, is a more clearly “materialist” notion, tied as it is to circumscribed
practices, essentially involving technologies, theories, models, and skills. The fabric styles are made of is much more concrete, more closely connected to the specifics of particular scientific investigations than is that of epistemes. Accordingly, the concept of style requires that attention is directed more toward the continuous change of scientific knowledge, emphasizing variety at least as much as similarity.

**Style vs. Paradigm**

In light of this, Kuhn’s concept of paradigm might appear to be closer to Fleck’s concept of style than Foucault’s concept of episteme does, and indeed the comparison has been made a number of times. As Kuhn construed the concept, paradigms comprise exemplary instances of a “mature” science, which are used for teaching purposes and for illustrating the consensus in the field, as well as the field’s key theories, instruments, values and assumptions.

By definition, “normal science” is done within such paradigms, and all who work within a particular paradigm share a set of questions, agree on how to answer these and make similar appraisals regarding the difficulty of the “puzzles” contained in the paradigm. This means, for instance, that the fate of a paradigm and that of a theory implicated in that paradigm are inextricably bound together: if the theory is proven false, this is a serious blow to the paradigm, potentially leading to the state of crisis, which according to Kuhn constitutes the possible onset of a paradigm change or scientific revolution.

Overall, the concept of paradigm is a rather restrictive one; as a rule, it pertains only to one particular discipline or even to one subdiscipline. This is immediately visible in Kuhn’s specification of (part of) his formulation of a paradigm as a “disciplinary matrix.” Whereas Foucault’s episteme was broader, more oriented toward ideas and less localized than Fleck’s style, Kuhn’s paradigm is narrower and more localized. That is to say, style is more encompassing in regard to both periods of time and investigative fields than is Kuhn’s notion of paradigm.

Indeed, the Fleckian notion of style owes part of its value to how it is distinguished from the category of scientific disciplines, while paradigm, in contrast, can almost be seen as a reformulation of what, at any one point in time, constitutes a discipline. In other words, key to understanding style is that two different disciplines might share in the same style; while simultaneously multiple styles may be discernible within a particular discipline. Kuhn’s concept of paradigm does not share the same conceptual openness to different scientific realities. Also, only scientists, in the strictest sense of the word, can share in a paradigm. Fleck’s notion of style, on the other hand, goes hand in hand with the concept of collectives that are composed not only of scientists but also of laymen. Both scientists in the esoteric circle of a collective and laymen in the exoteric circle influence and are influenced
by a style.

**Fleck’s style vs. Hacking’s style**

It also seems natural to compare Fleck’s concept of style to the one Ian Hacking engages in his philosophical writings on science (see e.g. Hacking 1990, Hacking 1992). Hacking builds on the work of historian of science Alistair Crombie who, in his colossal volumes (1994), developed the idea that the entire history of Western science can be understood in terms of the progressive development of a relatively small set of styles of scientific thinking. According to Crombie there are six such styles: (1) mathematical postulation; (2) experimentation to control postulation and exploration by way of observation and measurement; (3) construction of analogical models; (4) ordering and comparing through taxonomy; (5) statistical analysis; and (6) historical derivation. These styles are not strictly tied to particular fields of research but make their way into all kinds of scientific disciplines; indeed, they show a great affinity to all sorts of methods used by different scientists throughout the intellectual spectrum.

Hacking parts from Crombie by adding a seventh style—the laboratory style—that is not merely in the business of representing but also in the business of intervening. Another issue on which Hacking deviates from Crombie concerns the former’s choice to give more emphasis to the practical and public nature of the scientific endeavor by speaking of *styles of reasoning* rather than of *styles of scientific thinking*, as Crombie does (Hacking 2002, p.180-181). More crucial than the differences between Crombie and Hacking, though, is the fact that they both use the notion of style to identify several ways in which scientists can reason towards their results; style in their senses of the word sets the standards of what objectivity is. Moreover, each of the styles they identify introduced “a great many novelties including new types of:

- objects
- evidence
- sentences, new ways of being a candidate for truth or falsehood
- laws, or at any rate modalities
- possibilities.” (Hacking 2002, p.189)

While styles are historical entities in some sense of the word, Hacking treats them as such almost exclusively with regard to their genesis. Once having been created, Hacking states that “[e]very style has become independent of its own history. [...] Each style has become what we think of as a rather timeless canon of objectivity, a standard or model of what it is to be reasonable about this or that type of subject matter” (Hacking 2002, p.188).
The similarities and differences between Fleck’s concept of style and the concept as drafted by Hacking are easily identified. As with episteme, paradigm, and Fleck’s concept of style, also Hacking uses the concept of style to historicize such interrelated items as objectivity, knowledge, objects of knowledge and science and to counter simplistic forms of empiricism. But, as Hacking notes himself, Fleck’s concept of style is “less sweeping than [that of] Crombie” and his own (Hacking 2002, p.180), because Fleck’s notion of style is much more tied to local and material circumstances, to particular fields of research and to comparably small historical episodes than are those of Crombie and Hacking. Indeed, the historical and dynamic nature of styles is much more central to how Fleck conceives of them than it is to how Hacking does.

Put differently, in arguing for the autonomy of styles as he conceived of them, Hacking wittingly or unwittingly engages as an analyst the dichotomy between what is internal to science, and what is external. External is what happens to explain the coming about of a style, and by engaging this in his philosophical investigations, Hacking can be said to undertake projects in “historical epistemology.” Internal, on the other hand, is the style itself, the types of objects it allows to be investigated and the types of evidence it admits. Hacking’s focus tends to be on this latter part of the story, after first separating it from the former. Making such distinctions in the first place, however, is at odds with the relationist position that Fleck endorses. What is internal to science and what is external is surely something scientists themselves have a say in, and investigating this helps us to understand how collectives come to and continue to uphold particular styles: by including or excluding particular methodologies, objects of research and types of evidence, scientists simultaneously generate novel facts while structuring the collective they are part of.37

Like paradigm, episteme and Hacking’s style, Fleck’s style helps us understand that for any scientist at any one time, only some options are available for consideration as one’s object of research as well as for tackling the problems posed and for stating the facts concerning those objects. All concepts, in other words, suggest that truth always occurs within a particular *regime of truth*. The question is how to individuate such regimes. One’s answer to that question, of course, will vary depending on what it is one is interested in, and hence my present promotion of the Fleckian concept of style does not include a definitive rejection of the alternatives discussed. More important than establishing the superiority of Fleck’s concept of style is emphasizing what distinguishes the Fleckian concept from the others. This is the way in which it steers away from, on the one hand, the sweeping abstractions from the particularities of scientific practice which are characteristic of both Foucault’s concept of episteme and Hacking’s notion of style and, on the other hand, from the trap of amounting to little more than a re-description of something which does not require re-description, namely discipline. Moreover, unlike some of its alternatives, the concept of
style as I use it does not come with a preconception of what does and what does not belong to science. It is a building block of a relationist view of science and reality, one whose explicit aim it is to avoid getting stuck in conventional (philosophical) dichotomies that are well-known for the obstruction they pose to an accurate understanding of science. Rather than further expounding on Fleck’s concepts in abstract terms, however, I will rather illustrate it in chapters 3 through 7. In those chapters, however, Fleck will not be my only source for philosophical concepts.

**More affiliates: Active and passive linkages vis-à-vis epistemic and technical “things”**

Having briefly discussed how Fleck’s notion of style relates to episteme, paradigm and Hacking’s concept of style, it is worthwhile to introduce two other concepts kindred to Fleck’s conceptual toolkit—Rheinberger’s notions of “epistemic things” and “technical things.” Rheinberger developed these concepts in the context of his so-called historical-epistemological investigations regarding molecular genetics. Epistemic things may at first sight seem to be simply “things” in the ordinary sense of the word, given that the epistemic things Rheinberger discusses include, for example, physical structures, chemical reactions and biological functions. However, all such things are epistemic things only insofar as they are defined by what could be called a constitutive vagueness. Epistemic things are the objects of scientific research and, as such, they “embody what one does not yet know. Scientific objects have the precarious status of being absent in their experimental presence” (Rheinberger 1997, p.28). In this they differ from the things of our ordinary life and experience, just as they differ from the mundane things of everyday life in that they are constituted by what Rheinberger calls “experimental systems.” An experimental system, according to Rheinberger, is the smallest unit in terms of which scientific research can be understood, according to Rheinberger. Experimental systems consist of technologies, techniques, tacit as well as explicit knowledge, assumptions, theories, skills and so on. As Rheinberger points out elsewhere, what he denotes with the idea of the experimental system, closely resembles the Fleckian concept of style (Rheinberger 2010b, p.31).

Something that is an epistemic thing at one point in time and in one context, may, in another context, be just an ordinary thing. Just think of temperature: even if its precise quantitative measurement once gave rise to hot scientific debate, we now casually refer to it when deciding on what clothes to wear or to take with us on vacation (cf. Chang 2004). Or think of trust. Insofar as trust figures in neuroeconomics it often—though not always—does so as an epistemic thing. However, it has a different status when, for instance, the concept is used in daily life, when its lack is included in a political scientific analysis of democratic deficits in contemporary politics, or when the attitude of laymen toward scientific expertise is described in the context of biomedical controversies, such as those relating to vaccination...
for cervical cancer. Thus, with his conception of the epistemic thing, Rheinberger picks out a functional role rather than a structural definition.

What was once an epistemic thing can come to function as a so-called “technical thing.” This can be the case when an epistemic thing has become sufficiently stable and thus becomes part of the armory of an experimental system. Temperature is a good example again: once a matter of much scientific controversy, its stabilized and standardized measurement later became a routine part of all sorts of experimental systems.

Once an epistemic thing is sufficiently stabilized it is no longer an “unknown.” Instead, it has become something about which facts can be stated. When an epistemic thing ceases to be an epistemic thing, it turns into what Fleck would call a passive linkage. Something characterized as an epistemic thing cannot concurrently be classified as a passive linkage, since passive linkages essentially lack the constitutive vagueness of epistemic things. However, given that Fleck defines active and passive linkages functionally rather than statically, once something is established beyond doubt and has become a passive linkage in the network of science, it can potentially function as an active linkage in further investigations (i.e., as technical thing). Some active linkages, thus, are legitimately described as technical things in Rheinberger’s sense of the word. This is not to say that all active linkages are former passive linkages, or stabilized epistemic things, of course; also non-corroborated assumptions, technological artifacts, (tacit) skills and many other such “things” can function as active linkages as Fleck defines them.

This is where it becomes clear why I have reason to add Rheinberger’s pair of concepts to the toolkit I will draw from: technical things form an interesting subset of a set of active linkages. When we identify something as a technical thing, we not only know that it has the same function active linkages have in regard to the production of new knowledge—bringing forth passive linkages, or clarifying, explaining or identifying epistemic things—we also know it is itself an outcome of the process called science. Thus, Rheinberger’s concepts of epistemic things and technical things show affinity to the concepts that Fleck developed—active and passive linkages. As will become clear, however, at certain points in my analysis Rheinberger’s concepts of epistemic and technical things are more convenient.

2.6 Conclusion: Where to go from here?

In an article Fleck published in 1929 in the German journal *Die Naturwissenschaften*, six years ahead of the publication of his *Entwicklung und Entstehung einer wissenschaftlichen Tatsache*, Fleck came close to giving a précis of his view on science and the implications this view has for how philosophy of science should be practiced. In this article he enumerates
the “three systems of factors” which have to be taken into consideration in any epistemological analysis; these are “the burden of Tradition, the weight of Education, and the effect of the Sequence of the acts of cognition” (Fleck 1986a, p. 47). I will address these factors in much of my dissertation, taking seriously also Fleck’s view concerning the strict interrelatedness of epistemology and ontology. Hence, like Fleck but while analyzing neuroeconomics, I will look for the style that has been brought forth by tradition and that is in turn co-responsible for this, the style that has been reproduced in education and that has amounted to a decisive force with respect to the sequence of the acts of cognition.

Accordingly, I will discuss “the Sequence of the acts of cognition” most explicitly in chapter 3 in which I scrutinize the history and philosophy of the idea that psychological functions are localizable in particular parts of the brain. Subsequently, in chapter 4 I will explore the history of neuroeconomics a.k.a. the “burden of Tradition,” as well as a topical variation on the theme of “Education” via analyses of literary forms specific to where one is placed relative to the esoteric circle of neuroeconomics. Specifically, I will first focus on the disciplinary history of neuroeconomics, and then conduct a case study concerning the issue of how general experts are drawn into the esoteric circle of neuroeconomics.

Chapter 4 will show how neuroeconomics’ disciplinary history plays a significant epistemological role itself, as it helps stylize and demarcate objects of research. Furthermore, it helps to acquaint us with neuroeconomic research on trust and with various stylistic features at work in it.

In chapter 5 then, I will provide an overview of the state of the art in the neuroeconomics of trust and bring into view all that passively follows. After this, chapter 6 takes the form of an anatomy of science, in which I will describe and scrutinize a large variety of active linkages and otherwise stylistic characteristics of neuroeconomics. This is where the piles in the swamp are identified and analyzed. I will highlight the relevance of each of Fleck’s three factors in the concluding chapter 7.