The Role of Skill in Experimentation: Reading Ludwik Fleck's Study of the Wasserman Reaction as an Example of Ian Hacking's Experimental Realism.

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Ludwik Fleck's *Genesis and Development of a Scientific Fact* is an extended argument for the inclusion of social factors in the history and philosophy of science. Fleck uses the history of syphilis as his case study, which falls into two parts. In the first two chapters of his book, Fleck presents a history of the change of the concept of syphilis as a "carnal scourge with strong moralistic connotations" in the middle ages to the modern concept of syphilis as an infectious disease (1979, 77). In the second part of the book, he presents a detailed account of the development of a serum test for syphilis, the Wasserman reaction. Fleck also argues that perception is theory-laden, for example, in his comparative study of anatomical drawings, and for the importance of social pressures on the development of science, for example, in his discussion of the rivalry between nations to first develop a cure for syphilis, as demanded by politicians (1979, 133 ff. and 68). I will examine one central argument from the second part of Fleck's book and the epistemological conclusions Fleck draws from his account of the development of the Wasserman reaction.

Fleck describes the development of the Wasserman reaction as an *invention,* not a discovery. As we will see, Fleck concludes that facts have a history, from which comes the provocative title of his book, and concludes that this creation is an irrational process dependent on social and historical factors.

> The epistemologically most important turning point [in the development of the Wasserman reaction test] occurred with the detection of syphilitic antibodies (amboceptor detection). During the initial experiments it produced barely 15-20 percent positive results in cases of confirmed syphilis. How could it then increase to the 70-90 percent found in later statistics? This turning point represented the actual invention of the Wassermann reaction as a useful test. The theory of the reaction as well as the historical and psychological circumstances surrounding its conception are of less practical importance. If the relation of the Wassermann reaction to syphilis is a fact, it became a fact only because of its extreme utility owing to the high probability of success in concrete cases. The moment when this decisive turn occurred cannot be accurately determined. No authors can be specified who consciously brought it about. We cannot state exactly when it occurred nor explain logically how it happened (Fleck 1979, 72, emphasis removed).

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The central argument for his provocative claim that facts have a history is that skill is needed to perform serological tests. The irrational element, and, indeed, the social element of knowledge involved in the creation of this test for syphilis turn out to be the laboratory skill involved in perfecting and reproducing the test.

> Wassermann's reports about his reaction contain only the description of the rela-tion between syphilis and a property of the blood. But this is not the most impor-tant element. What is crucial is the experience acquired by him, by his pupils and in turn by theirs, in the practical application and effectiveness of serology. With-out this experience both the Wasserman reaction and many other serological methods would not have become reproducible and practical. Such a state of experience became general only slowly and had to be practically acquired by each initiated individual. ... even today, anybody performing the Wassermann reaction on his own must first have acquired comprehensive experience before he can obtain reliable results. Only through this experience will he participate in the thought style, and it is experience alone that enables him to perceive the relation between syphilis and blood as a definite form.
>
> We might also mention some cases where such experience involving the irrational "serological touch" is specifically needed (Fleck 1979, 96-7, emphasis removed).

Fleck does present some convincing evidence that there are problems with the view that science passively discovers truths about nature, and with the view that science is simply progressive (1979, 64). First, during his initial experiments, Wasserman's basic assumptions were wrong. He was not looking for primarily for amboceptors (antibodies) in syphilitic blood, but rather for antigen. It turned out, however, that detection of amboceptors became a practical test for detection of syphilis, while antigen detection is not reliable. Furthermore, Wasserman's first experiments showed the opposite. His results were far too accurate in antigen detection, and they are not reproducible (Fleck 1979, 70-71). At the same time, Wasserman's results in amboceptor detection were quite poor, leading him to downplay their importance (Fleck 1979, 71, also cf. citations on 74-75). Wasserman's first experiments were, however, of enormous heuristic value, leading others to perfect a serum test for syphilis using amboceptor detection (Fleck 1979, 85).

While I do not dispute the importance of Fleck's case study for the philosophy and sociology of science, nor his general conclusions that observation is theory-laden, that the development of science involves social factors, and that science is not simply progressive, I do dispute Fleck's argument that the skill involved in perfecting the Wasserman reaction leads to the conclusion that knowledge is culturally condition in the sense that it depends on a particular historical thought-style, where 'thought-style' is Fleck's a catch phrase for methods, ontologies, and theoretical commitments which change historically, much like Kuhn's (1974) 'disciplinary matrix' and Laudan's (1978) 'research traditions.' In a summary of his work written in 1960, Fleck argues as follows:

> Although preliminary experimental results were not reproducible, the Wasserman reaction became accepted because it soon proved to be extremely useful. The very difficulty of reproducibility ... demonstrates the social dependence of all knowledge (1979, 155).

Fleck assumes there is no difference between theories, social pressures, and acquired skills. We see this when we consider that Fleck's term 'thought-style' is broad enough to include all three and no attempt is made to distinguish the epistemological significance of these different aspects of thought-styles.[1] W. Baldamus makes a similar point in criticizing the relations between the first and second parts of Fleck's book. Fleck assumes that the same consequences for a theory of knowledge follow from the independence of theory and experiment and the necessity of skill in the case of the Wassermann reaction, and the "normative and mythical preconceptions in the earlier history of medicine," which Fleck describes in the first part of his book (Baldamus 1977, 140-141).[2] Fleck has not clearly distinguished the various parts of his argument, all of which he sees as leading to the conclusion that social factors are crucial for an understanding of science (1979, 76). I will use some work by Ian Hacking to argue that the skill involved in experimental work does not lead to the conclusion Fleck expects, and that a different analysis of 'thought-collectives' could lead to a solution to one of the main problems in Fleck's work, how to define an objective element of knowledge.

In his recent work Hacking emphasizes the importance of interacting with the world through experiment and downplays the importance of theory (1983 and 1984). In effect, Hacking has argued for the separation of theory and skill in science, which allows skill to be seen as a less irrational element because it does not depend on a particular theory. Indeed, Hacking goes so far as to adopt the language of creation, rather than discovery, of phenomena in science, but argues that the creation of scientific phenomena provides an argument for scientific realism, a position he calls "experimental realism" (1984, 119).

Hacking argues that regular and repeatable phenomena are rare in nature. The scientific revolution coincided with the development of techniques to create stable phenomena. Hacking has us consider the objects that science studies:

> it may seem that the objects of the sciences, although described by changing systems of categories, are not themselves historically constituted. Yet what are the objects? Do they include such phenomena as the deflection of a magnetic needle by a steady electric current, or Faraday's more ingenious devices, the electric generator and the electric dynamo? These are not eternal items in the inventory of the universe, but came into existence at very specific times. Nor am I content to say that the inventions are dated, while the phenomena and laws of nature that they employ are eternal. I have been urging for some time that one of the chief activities of the experimenter in the physical sciences is quite literally to create phenomena that did not exist before (1984, 119).

Thus, Hacking argues that we create effects just as we have created the machines that produced them. Even though these effects come into existence at a specific time, they are not completely dependent on a particular theory, nor even on a historical thought-collective, what Hacking calls a "style of reasoning."[3] If we invent something on the basis of one theory, and then change to another theory, the invention goes on working just the same. Hacking says that effects are theory resilient (1984, 118). A simple example is the Leyden Jar. It was invented with a specific view of electricity in mind–that electricity is a fluid (Roller and Roller 1948, 593 ff.) The jar was seen as a way to catch the fluid and store it, although the jar itself was found to be unnecessary soon after the invention, for example being replaceable by glass plates. We still have the same effect but we no longer believe that electricity is a fluid. We might say that the apparatus only received a new name, that is, 'capacitor', while the theory that explains the effect changed in a more substantial way. When we changed our theories, we developed a new ontology that explains the effect. Instead of a fluid, we now say that we are dealing with a charge, which is more than a change in name, it is a change in the basic ontology of our theory.

Hacking's argument also applies to the Wasserman reaction, which Fleck presents as a compelling case history of the creation of a stable and repeatable scientific phenomenon. The Wasserman reaction is not a simple discovery that could be reduced to an observation sentence. However, it is repeatable, even though it was discovered with an incorrect theory. If we reject the foundationalist view that new phenomena are discoveries of part of the 'real world,' totally independent of our activity, there seem to be three possible ways to interpret such cases of new scientific phenomena. We could argue, like Fleck, that laboratory skill is part of a thought-collective (to use his term) and that skill forms the irrational part of this collective. The phenomenon would then be a creation of the collective in the sense that it could not be objective or independent of the collective. Alternatively, we could argue that skill is part of theory, that any training involved in learning how to experiment is simply the learning of more theory. Kuhn argues against the second view by criticizing the application of strict rules to account for skill and by arguing that there is an irrational element involved in training (1974, 318). Here I will simply point out that the view that there is nothing more involved in learning a skill than the learning of strict rules which could be interpreted as part of theory is a thesis which needs to be proven. Hacking presents us with a third alternative, that experiment has a life of its own, independent of theory, an alternative which is exemplified by the fact that Wasserman's basic assumptions were wrong. Experiment sometimes proceeds independently of theory, indeed, experiment sometimes even advances with a basically incorrect theory. Hacking presents other examples of experimentation proceeding independently of theory in the second half of his book *Representing and Intervening* (1983, esp. ch. 9).

How we interpret the role of skill in the creation of phenomena has consequences for Fleck's view of science. First, we can eliminate the contentious language of Fleck's title. 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. Secondly, Hacking's interpretation of the role of skill in the creation of phenomena could lead to a solution to one of the main problems in Fleck's work, how to define an objective element of knowledge. It is clear from Fleck's definition of a scientific fact that he thinks a fact is "thought -stylized," and thus somehow dependent on thought-collective, but he also says that facts cannot be totally accounted for from the collective point of view (1979, 83). Fleck claims that some elements of science are not creations of a thought collective, and he calls these the "passive" elements in knowledge. The distinction between active and passive elements of knowledge is useless for Fleck, however, because he says that "the passive and the active elements cannot be separated from each other completely either logically or historically" (1979, 95; cf. Anderson 1984, 29-30). Therefore, Fleck's dependence on a passive element to account for the objectivity of scientific theory is no better than the view that correspondence with an inaccessible noumena makes our theories true. The passive element is inaccessible because Fleck rejects the view that knowledge rests on a foundation of neutral observations. Specifically, he rejects Carnap's *Aufbau*, and instead holds that all observation is theory laden (or thought-style laden) (Fleck 1979, 49-51).

It is easy to see how someone defending the view that there is an objective element in physical theory can be forced into a dilemma. We might say that there are facts in the world which make our sentences true, if we suppose that the data we are to check to see if our theories are true must be independent of our style of reasoning, but we then face the problem of skepticism. Fleck denies that there is any accessible neutral element in knowledge. On the other hand, if 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. Fleck wavers back and forth between relativistic language and objective language, but it is not possible for him to define the objective element of knowledge as a passive element because his own position is that it is impossible to separate passive and active elements of our knowledge.

Hacking grants the antirealist much and still makes his case for realism. He is not committed to neutral observations as a foundation of scientific theories. He thinks that some neutral observations exist, but he argues that observation is not as important to science as our Logical Positivist tradition would have us believe (Hacking 1983, 167). Instead, Hacking argues that the creation of stable effects is crucial to science. The naive realist would have us believe that there is an independent reality that science discovers, something which is independent of styles of reasoning, and cuts across all styles. Hacking holds that science is about effects and that effects do not go away when we change theories, but he is willing to admit these effects are not eternal.

> I assert a seemingly more radical step, literal belief in the creation of phenomena, shows why the objects of the sciences, although brought into being at moments of time, are not historically constituted. They are phenomena thereafter, regardless of what happens. I call this 'experimental realism' (Hacking 1984, 119).

I do not want to get into a dispute over Hacking's version of realism.[4] The aspect of Hacking's 'experimental realism' that I want to emphasize here is that even without a universal, ahistorical element, there are theory resilient effects which cross theories as long as we keep using them. Since these effects are the main objects which modern science studies, theory resilient effects are enough to argue for the objectivity of science. Effects are relative in the sense that they are bound to our ability to recreate them, they depend on our skills, but they still cross theories and styles of reasoning. Thus, theory resilient effects show that inventions are not a creation of our theories, even if they are a creation of our skill. The fact that Wasserman's theory was all wrong, or that the advances in the test procedure were carried out by anonymous laboratory workers does not show, as Fleck sometimes says, that a fact "is always the result of a definite thought-style" (1979, 95). Both Fleck and Hacking have shown how experimental practice and theory can act separately from one another. This justifies Hacking's separation of experimental practice and theory, and shows that Flecks conception of thought styles covers up an important distinction by making it appear as though experimental practice and theory are always indistinguishably part of a single thought style.

Hacking's description of theory resilient effects provides a way of replacing inaccessible noumena with something concrete. We do not need to postulate an ahistorical, universal element which we are suppose to passively discover, but which turns out to be inaccessible, in order to explain how empirical data can be decisive in science. Instead, we can look to the creation of stable phenomena. Stable phenomena are objective in the sense that they are independent of particular theories, but not in an absolute or foundationalist sense because they depend on our skill. Thus, we can still be active and creative in science without losing objectivity.[5] Hacking's experimental realism provides a way for interpreting Fleck's study of the development of a serum test for syphilis which avoids the contentious language of the development of facts.

Notes

[1]Commentators blur the distinction too. For example, Thaddeus Trenn makes it appear that the Wasserman reaction, Fleck's main example of a created fact, should be interpreted as conceptual by interpreting Fleck as claiming that "conceptual creations of science ... become accepted as fact", but that takes away the element of laboratory practise which Fleck emphasizes (1979, xiii).

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[2]Also cf. Thomas Kuhn for a general criticism of arguments that normative elements determine scientific practice (1977, xxi-xxii).

[3]With some reservation, Hacking (1982, 50-51) takes from A.C. Crombie the term 'styles of reasoning' as his catch phrase for methods, ontologies, and theoretical commitments which change historically. I argue that 'styles of reasoning' is narrower that Fleck's 'thought-style,' because it does not include experimental practice.

[4]I take it that Hacking is committed to some form of realism which allows us to speak about the same object while working in different theories, but it is unclear what his realism is, especially since he presents three rather different sorts of arguments for it in *Representing and Intervening*. Arthur Fine questions Hacking's commitment to realism and proposes that Hacking may be realist in name more than spirit (1986, 176, n. 22).

[5]There is an alternative solution to the dilemma of those trying to defend an objective element in science, the 'no-theory' of truth advocated by Davidson, Arthur Fine, and Michael Williams. If the no-theory analysis of truth holds up, the above will have been shown to be a pseudo-dilemma. "Nothing makes a sentence true," so there is no need for an explanation (Davidson 1986, 310).

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