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Circles of Scientific Practice: *Regressus, Mathēsis, Denkstil*

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Abstract: Hermeneutic studies of science locate a circle at the heart of scientific practice: scientists only gain knowledge of what they, in some sense, already know. This may seem to threaten the rational validity of science, but one can argue that this circle is a virtuous rather than a vicious one. A virtuous circle is one in which research conclusions are already present in the premises, but only in an indeterminate and underdeveloped way. In order to defend the validity of science, the hermeneuticist must describe a method by which a vague and confused initial knowledge of nature gets transformed into a clear and determinate knowledge of nature. I consider three such methods. The first is *regressus demonstrativa*, favoured by the physicians of Padua during the fifteenth and sixteenth centuries. The second is *mathēsis*, introduced by Martin Heidegger in his discussion of seventeenth-century science. The third is *Denkstil*, a key concept in Ludwik Fleck's history of syphilology. I conclude by listing three desiderata for a hermeneutic science studies: that it be anti-metaphysical, historical, and sociological.

Introduction: An Existential Hermeneutics of Science

Contemporary hermeneutic studies of science typically views science in terms of practice, as something scientists *do*, rather than in terms of theory, as something scientists *think*. The origin of this practical hermeneutics is often credited to the philosopher Martin Heidegger. However, what Heidegger espoused was not, strictly speaking, a *practical* hermeneutics of science. It was, more precisely, an *existential* hermeneutics of science. He viewed modern science as a form of existence which enabled specific kinds of both thinking and doing.

This existential hermeneutics was motivated by Heidegger's phenomenological method. Phenomenology is the study of the structures of experience from a first-person perspective. The first-person perspective can be either singular or plural. Phenomenology is thus the study of the structures of both *my* experience and *our* experience. In Heidegger's view, the most fundamental first-person experience is that of existence. He argued that the basic experience of existence has a hermeneutic, or interpretative, structure. To exist as a human being is to experience oneself as making sense of, as understanding, the world.

Heidegger focussed on the kind of mundane, non-deliberative and non-linguistic understanding experienced in such everyday activities like riding a bicycle, tying one's shoes, or brushing one's teeth. He maintained that this fundamental existential structure of non-deliberative, practical understanding provides the necessary conditions for the possibility of scientific research, as such (Heidegger 1962: 408). Scientific explanations, concepts, and theories are possible only because **[84]** scientists find themselves ineluctably embedded in concrete research contexts which place continuous demands on their practical abilities. This is why Heidegger has often been interpreted as promoting a practical hermeneutics of science. But the neglected point is that these practical activities are irremediably shaped by the way scientists experience the meaning of things, as well as the meaning of their own existence in relation to those things.

In this paper, I will focus on a central difficulty faced by hermeneutic studies of science. This is the difficulty of circularity. As we will see in the examples ahead, when viewed hermeneutically, scientific research appears inherently circular. It seems to rely, in some sense, on the very knowledge is claims to establish. Yet, as Heidegger (1962: 194) observes, in a scientific proof "we may not presuppose what it is our task to provide grounds for." A scientific proof is supposed to be non-circular, and yet hermeneutic science studies insists that such a proof must rest on a foundation with an inherently circular structure. The central difficulty, for Heidegger, is not to explain how non-circular knowledge may depend on circular knowledge. The difficulty, rather, is to explain why the circular basis of scientific knowledge does not threaten its validity. This is the difficulty of demonstrating why the circle of scientific practice is a virtuous circle rather than a vicious one. As Heidegger (1962: 195) wrote, in a much cited passage, "[w]hat is decisive is not to get out of the circle but to come into it in the right way." According to Heidegger, the hermeneutic challenge is to show how scientists work within the circle of understanding in a way which produces reliable knowledge of nature.

In what follows, I will consider three attempts to address this difficulty. The first was made during the Renaissance by professors at the University of Padua. The second was made by Heidegger in his own discussion of the rise of modern science. The third was Ludwik Fleck's historical epistemology of syphilis research. My hope is that these examples will help to illustrate the diverse ways in which hermeneutic studies of science may address the circularity of scientific practice. Let us begin, then, with the Renaissance natural philosophers of Padua. **[85]**

Paduan *Regressus* and the Logic of Discovery

In a 1940 paper, historian John Herman Randall details the Renaissance development of a scientific method which, he argues, prepared the ground for the Galilean method in the early seventeenth century. This Renaissance method was the product of four centuries of intense academic discussion, predominantly among the teachers of medicine at the University of Padua. Randall's story begins in 1310, with Pietro d'Abano's distinction between two kinds of scientific procedure (Randall 1940: 185 f.). In the first instance, scientific knowledge is said to arise from the demonstration of an effect through its cause. In the second instance, the cause of an effect is discovered and defined through an investigation of the effect for which it is the cause. We have here, then, two distinct procedures: the demonstration of an effect through its cause, and the demonstration of a cause through its effect. Randall (1940: 186) argues that the great achievement of the Paduan theory of science was the transformation of the idea that a cause could be demonstrated through its effect into a full-fledged method of discovery. This insight was developed further in the fifteenth century by Hugo of Siena (Randall 1940: 190f.). He argued that in sciences like physics or medicine it is impossible to use only one procedure; both are required. Hugo thus makes the claim that a proper method in natural science will begin with the effects, look for the cause, and then explain those effects by that cause.

Here we see how the difficulty of hermeneutic circularity begins to emerge in the Paduan analysis of scientific method. That method required one to pursue knowledge of causes on the basis of knowledge of effects, and to demonstrate knowledge of effects on the basis of knowledge of causes. However, the cause is meant to be the *reason* grounding one's knowledge of the effect. Hence, if knowledge of the cause depends on prior knowledge of the effect, then causal knowledge presupposes what it is meant to explain. The Paduan method thus appears to be circular.

This difficultly was directly addressed by Paul of Venice in the fifteenth century. He explicitly defended the Paduan method against **[86]** charges of circularity (Randall 1940: 191). The crux of his argument was a distinction between two different kinds of knowledge of an effect. First, there is the knowledge *that* it is. Second, there is the knowledge of *why* it is. When we begin with the effect, and look for the cause, we are relying on knowledge *that* the effect is. When we then explain the effect by that cause, we establish knowledge of *why* the effect is. The method thus involves a transformation in the knowledge of effects from knowledge-that to knowledge-why. Hence, Paul argues that the Paduan method is not circular.

This transformation was further analysed in the fifteenth century by Jacopo da Forlì (Randall 1940: 189). He viewed it as a method of discovery, or, more specifically, as a method for the *resolution* of causes through the study of their effects. He illustrated this method of discovery through resolution with a case of medical diagnosis.

> [I]f when you have a fever you first grasp the concept of fever, you understand the fever in general and confusedly. You then *resolve* the fever into its causes, since any fever comes

> either from the heating of the humor or of the spirits or of the members; and again the heating of the humor is either of the blood or of the phlegm, etc.; until you arrive at the specific and distinct cause and knowledge of that fever. (Cited in Randall 1940: 189.)

What is meant to rescue the Paduan method from the charge of circularity is a step-wise process of resolution through which a general and confused knowledge of an effect is transformed into specific and distinct knowledge of that same effect through its cause. Because the second knowledge of the effect – knowledge-why – differs so dramatically from the first knowledge of the effect – knowledge-that – there is no vicious circle. Indeed, the Paduans instead began to refer to their procedure as a *regressus*, or regress.

Later in the sixteenth century, Jacopo Zabarella summarised the Paduan account, describing the process of resolution as a "mental consideration" of the cause (Randall 1940: 201). Like Forlì, Zabarella understood this as a process by which a general and confused knowledge is transformed into a clear and distinct knowledge. However, the **[87]** precise nature of this transformation remained a mystery. As Zabarella lamented, "what this mental consideration may be, and how it is accomplished, I have seen explained by nobody" (Randall 1940: 201).

As a consequence, Randall may have been too optimistic in his claim that "within the school of Paduan Aristotelians, there has been worked out [...] a logic of investigation and inquiry." It would perhaps be more accurate to say that Paduan natural philosophers succeeded in precisely identifying and defining a central problem in the philosophy of science, but that they were unable to afford a solution to that problem. This was the problem of articulating a logic or method of discovery. As this case shows, this problem arose in response to a more general conceptual difficulty. This was the difficulty of demonstrating that the hermeneutic circle in scientific practice does not threaten the validity of the scientific enterprise.

Hermeneutics versus Metaphysics in the History of Science

Randall (1940: 204) argued that there was but one element missing from the Paduan method. This missing element was mathematics. According to Randall, once a mathematical emphasis was combined with the Paduan methodology, the new method of early-modern science stood essentially complete.

This historical analysis was shared by the historian Alexandre Koyré. However, whereas Randall viewed the mathematisation of the Paduan method as the vital last step in a long process of intellectual development, Koyré (1943a: 400) saw it as a "spiritual upheaval" and an "utter transformation of the whole fundamental attitude of the human mind." It was, he argued, "pure unadulterated thought," rather than "experience or sense-perception," which provided the ultimate foundations for the new Galilean science (Koyré 1943b: 346).

Koyré viewed modern science as the result of a radical philosophical departure from the realm of concrete, phenomenal experience and an entrance into a metaphysical realm of purely intellectual mathematical abstraction. As a consequence, Koyré attempted not to show us **[88]** how scientists move successfully within the hermeneutic circle, but rather how they manage to successfully escape from that circle. They escape from it through the window of metaphysics, that is, through the pure exercise of abstract thought, without recourse to experience.

I do not want to criticise Koyré's account, but only to record the way in which it leads us beyond the boundaries of hermeneutic studies of science and into the equally treacherous terrain of traditional metaphysics. With this consideration in mind, let me now move forward to the second of my three examples. This is Martin Heidegger's own account of the rise of modern science.

Heidegger's *Mathēsis* and the Shape of Modern Scientific Existence

Like Koyré, Heidegger located the origin of modern science in a kind of mathematical understanding, but he did so within, rather than outwith, the framework of hermeneutic studies of science. Heidegger (1967: 68) argues that mathematical practices, in the sense of measuring and calculating, were already present in pre-modern times, and so they cannot provide the missing element which completes the modern scientific method. What is decisive for modern science is not a particular set of practices, but what, as Heidegger (ibid.) puts it, "rules and determines the basic movement of science itself." For him, then, the missing element was something which put already existing practices in a new light, thereby giving them a new and powerful meaning, as well as an impetus to develop in new and unprecedented ways. This was, above all, a change in experience, in the basic existential structure of scientific understanding. According to Heidegger, only on the basis of this existential transformation could seventeenth century natural philosophers succeed in integrating mathematical techniques into their theoretical models of nature. In order to model nature in mathematical terms, they first had to experience natural phenomena in a new way, as something inherently amenable to mathematisation. This new kind of experience was a new way of understanding nature, a new way of **[89]** making sense of natural phenomena, of rendering them intelligible. It was, in other words, a new mode of discovery in the investigation of what things are.

Heidegger called this new mode of discovery *mathēsis*, a Greek term which means "learning." This kind of learning – *mathēsis* – is, according to Heidegger (1967: 73), "a taking where he who takes only takes what he actually already has." As a mode of discovery, then, *mathēsis* is the discovery of what one, in some sense, already knows. It is, in other words, a hermeneutic process manifesting a circle. What one already knows about things, that aspect of things which one learns through *mathēsis*, is what Heidegger (1967: 74) called the "mathematical." He writes that the mathematical is "that 'about' things which we really already know. [...] [W]e do not first get it out of things, but, in a certain way, we bring it already with us" (ibid.). Hence, Heidegger (1967: 75) describes the mathematical as "the fundamental presupposition of the knowledge of things."

Recall that, for Heidegger, the basis of scientific knowledge is not to be found in theory, but rather in the basic existential structures which give fundamental shape to scientific experience. It is these existential structures which allow things to show up as intelligible for scientific enquiry, and hence also as amenable to particular kinds of theoretical articulation. The kind of theoretical articulation which Heidegger had chiefly in mind with respect to modern science was the one characteristic of mathematical physics. For him, the paradigmatic scientific theory is a theory articulated in the language of mathematics. His crucial point, however, was that such theoretical articulation becomes possible only on the basis of a specific kind of experience, a kind of experience in which natural phenomena are recognised as an appropriate subject matter for mathematics. The condition of possibility for experiencing natural phenomena as an appropriate subject matter for mathematics was, Heidegger argued, the fundamental presupposition of the knowledge of things, the mathematical. On the basis of this presupposition, natural phenomena become intelligible, or meaningful, as things which can be articulated in the language of mathematics. The mathematical thus provides the experiential basis for the discipline of mathematics as **[90]** an appropriate theoretical tool in the natural sciences. The discipline of mathematics depends on, and is made possible by, the mathematical as a fundamental form of scientific experience. Hence, Heidegger (1967: 68) can argue that "mathematics itself is only a particular formation of the mathematical."

With this argument, Heidegger allows for the possibility that experience shaped by the mathematical can also be formally articulated in other ways. Indeed, he explicitly argues that the mathematical also provides the existential basis for the modern experimental sciences. He writes: "Upon the basis of the mathematical, the *experientia* becomes the modern experiment. Modern science is experimental because of the mathematical" (Heidegger 1967: 93). In Heidegger's view, then, the fundamental mathematical presupposition structuring modern scientific experience provides the existential basis for both mathematical theory, on the one hand, and experimental practice, on the other. In this way, he presents a challenge to the subsequent historiographic claim that mathematics and experimentation represent two incommensurable traditions within early-modern science (e.g., Kuhn 1976).

The question remains of just what this very basic knowledge about things, this fundamental presupposition, actually was. Heidegger approaches this question by way of an analogy to the manufacturing arts. He describes the fundamental presupposition of the knowledge of things as the "one basic blueprint" in which the structure of each thing as well as its relation to every other thing is laid out in advance. This basic blueprint establishes the uniformity of all things according to relations of space, time, and motion (Heidegger 1967: 92). An underlying assumption of the metaphor of the basic blueprint is the idea that the world is an artefact created in accordance with a plan. It suggests the mythical image of an artificer God, a cosmic demiurge. This was, of course, an assumption common among early-modern natural philosophers (see Shapin 1996: 30ff.).

This mythical image re-introduces the tension, mentioned earlier, between metaphysics and hermeneutics as competing accounts of the origins of modern science. In the former case, one may assert, in accordance with this image, that the basis of scientific knowledge lies **[91]** beyond worldly experience, in a transcendent realm of some sort. In the latter case, one can treat the image as an historical phenomenon which circulates at the base of a broad and multifaceted epistemic tradition, of which modern science is but one expression. In my view, the first case more accurately represents the position of Koyré. The second case seems more in line with Heidegger's hermeneutics of science. Indeed, the starting point for his existential phenomenology was the historicity of human experience. That the mythical image of a cosmic demiurge should provide the fundamental existential structure of modern scientific experience was, thus, for him, a historical fact about the tradition within which modern science has developed and is sustained (cf. Heidegger 1982: 286). Against the mythological background of a cosmic demiurge, modern science has discovered that natural phenomena are uniformly governed by a set of universal parameters which fully determine their relations and operations. For Heidegger, then, the difficulty posed by the hermeneutic circle of scientific practice was not something which should be overcome by a flight into metaphysics. It was to be resolved through a recognition that this circularity is an inherent feature of science construed as an inescapably historical enterprise.

It seems appropriate, then, that my final example of hermeneutic studies of science should be a predominantly historical one. This is Ludwik Fleck's history of syphilis research.

Fleck's *Denkstil* and the Directedness of Discovery

Fleck (1979: 38) expresses a clearly hermeneutic notion of knowledge when he writes that "[w]hat is already known influences the particular method of cognition; and cognition, in turn, enlarges, renews, and gives fresh meaning to what is already known." Fleck's statement captures the central hermeneutic idea that the meaning of a natural phenomenon is always shaped by the knowledge we bring with us when we encounter that phenomenon, and that this encounter may, in turn, contribute to our existing stock of knowledge. Indeed, he argues that **[92]** "observation without assumption, which psychologically is nonsense and logically a game, can [...] be dismissed" (Fleck 1979: 92).

Fleck furthermore recapitulates the central claim of hermeneutic studies of science that scientific research includes the application of a method or technique in order to transform a vague and confused idea into a clear and distinct one. He argues that the historical development of scientific knowledge often begins with what he calls "primitive pre-ideas" which then lead continuously to modern scientific concepts. According to Fleck (1979: 100), it is thus possible to follow the historical development of, for example, causal explanations of infectious disease from the primitive pre-idea of demons, through the notion of a disease miasma, to the modern theory of the pathogenic agent. In another example, Fleck (1979: 23) argues that modern atomic theory originated from the ancient doctrine of the Greek philosopher Democritus. As a general claim about the historical contingency of scientific concepts, Fleck (1979: 20) writes that such "[c]oncepts are not spontaneously created but are determined by their 'ancestors.'"

Fleck's central example for this process of concept definition is his detailed study of the history of the modern concept of syphilis. Fleck observes that, at the end of the fifteenth century, the diagnosis of syphilis disappeared into what he calls "an undifferentiated and confused mass of information." He also calls this undifferentiated and confused knowledge of syphilis a "primitive jumble," which included descriptions of what we now recognise to be many other distinct diseases (Fleck 1979: 1). It would take four centuries before the modern concept of syphilis was drawn out from this confused conceptual jumble. I will not review Fleck's detailed account of this long historical process, but simply quote his own brief summary. He writes: "We have described a hazy idea of syphilitic changes in the blood and shown that this idea existed centuries before scientific proof was available. Emerging from a chaotic mixture of ideas, it developed over many epochs, becoming more and more substantial and precise" (Fleck 1979: 23). Fleck has given a detailed historical account of a process which had been called "resolution" and "mental consideration" by Paduan natural philosophers during the Renaissance. It is a process which Heidegger **[93]** (1962: 195) also called "articulation," and to which Fleck gives the name "crystallisation." For all of them, this was a process of discovery.

Fleck (1979: 53) emphasised that his history of syphilology must not be mistaken for a logical reconstruction, because "it involves the progress of vague and indefinable concepts which are about to crystallize." The idea seems to be that a logical reconstruction of a discovery process must locate at the beginning of that process one or more precisely defined concepts from which the final results are then deduced. Because the initial concept of syphilis was vague, confused, and indefinable, such a logical reconstruction is simply not possible in this case. However, this does not mean that the process was therefore an irrational one, that it was without any kind of governing rule. It does not mean, in other words, that the process of crystallisation lacks an identifiable, overall direction.

Identifying the basis of this directedness is one of the central difficulties for hermeneutic studies of science. This is the difficulty of determining how scientists enter into and successfully work within the hermeneutic circle. Recall Zabarella's lament, at the end of the sixteenth century, after discussing mental consideration as the Paduan solution to this difficulty. He wrote: "what this mental consideration may be, and how it is accomplished, I have seen explained by nobody." As I have suggested, an explanation of this phenomenon may move in two different directions. It may move out of the hermeneutic circle, and into the realm of traditional metaphysics, or it may move within that circle, and so stay within the realm of historical existence. Heidegger chose this second direction by emphasising the rootedness of modern scientific practice, including the process of conceptual articulation, in the existential soil of an historical tradition.

This was the direction chosen by Fleck, as well, in his historical account of the crystallisation of the modern concept of syphilis from out of a confused and mythical jumble of pre-ideas. Indeed, Fleck does a much better job than did Heidegger of elaborating the consequences of this approach for a general theory of scientific knowledge. The key element in Fleck's account was his concept of *Denkstil*, or "thought style." A thought style is an historically instantiated research tradition **[94]** which gives a distinctive direction and shape to the process of discovery. The carrier of this research tradition is what Fleck called the "thought collective." He tried to capture the effects of a thought style in the image of rivers meandering in myriad directions across the surface of the earth, only to all finally end up at the sea (Fleck 1979: 78). Viewed in two dimensions, the network of rivers seems to be without logic. The rivers appear to snake randomly across the earth's surface in all directions of the compass. However, viewed in three dimensions, one immediately recognises that all rivers move downward, that is, they are all subject to the force of gravity. Fleck (ibid.) writes that "[t]he field of gravity corresponds to the dominant directing disposition [of the thought collective], and water to the work of the entire thought collective." The thought style which gives shape and direction to the research process thus has its origin in the shared disposition, the *Stimmung*, or mood, of members of a thought collective.

Heidegger also recognises something analogous to a field of gravity which directs scientific work. He called it the mathematical. Indeed, as noted earlier, he defines the mathematical as "what rules and determines the basic movement of science." Heidegger's concept of the mathematical and Fleck's concept of thought style thus play a similar role in explaining how scientists successfully work within the circle of scientific practice. Both concepts refer to historical phenomena ineluctably rooted in the soil of a particular tradition. These phenomena provide scientists with a background of knowledge on which they depend when they seek to make sense of, when they attempt to understand some aspect of, the natural world.

Modern Science as Socially Conditioned Existence

A comparison of Fleck's concept of thought style with Heidegger's concept of the mathematical may help to shed further light on both. The first point of comparison is between the respective carriers of each. Fleck argues that a thought style is carried by a thought collective; it is the dominant directing disposition of that collective. For Heidegger, the carrier of the mathematical is existence. **[95]**

If a thought collective is viewed as a form of existence, then we may more easily avoid a potential misunderstanding which threatens Fleck's account. Fleck (1979: 102) even warns us of this potential misunderstanding, cautioning that a thought collective should be understood in functional rather than in substantive terms. It is not a fixed group or social class, but more like a field of force in physics, indeed, more like a field of gravity which gives a particular direction and meaning to the apparently unrelated constituents falling under its influence. For Heidegger, existence is likewise not a thing, but rather a fundamental field of experience which has a particular directedness, an intentional structure. It is a field in which things show up as meaningful. Heidegger's account thus allows us to interpret Fleck's concept of thought collective in more thoroughly phenomenological terms. A thought collective is not an identifiable group of people to which we can point, as it were, from the outside. It is a first-person experience, something which is not only "mine," but also "ours." It is the way we experience ourselves as existing meaningfully with others. It is, in other words, a fundamental human experience with a distinctly social dimension.

Although it may be argued that there is also an identifiable sociological aspect to Heidegger's phenomenology of experience, he nevertheless left it lamentably underdeveloped. Here, Fleck may be of service. He gave considerable attention to the sociology of scientific experience, and his discussion can help us to gain a better understanding of Heidegger's concept of the mathematical. Fleck (1979: 98) argues that thinking is a "supremely social activity which cannot by any means be completely localized within the confines of the individual." He furthermore describes scientific discovery as a "social event" (Fleck 1979: 76). Discovery is, of course, not something which scientists think, but something they do. Following Heidegger, we may view thinking and doing as different aspects of an integrated field of scientific experience. Following Fleck, we may furthermore recognise this field of experience as a social field amenable to sociological investigation. Hence, Fleck urges that thought styles, as integrated cognitive fields of thinking and doing, call for the application of sociological methods in the **[96]** pursuit of an adequate epistemology of science. For Fleck (1979: 102), this social epistemology must also include historical methods, because its main subject matter is what he describes as the "social conditioning of thinking," and also of doing. A history of the social conditioning of scientific experience might focus, for example, on the socialisation of individual scientists into established research communities, or, more broadly, on the formation of specific scientific disciplines and sub-disciplines, that is, the formation of specific fields of collective scientific experience.

Seen through the lens of Fleck's epistemology, then, Heidegger's concept of the mathematical appears not as a vague and polysemous concept in the mind of individual scientific actors, but something which exists only within the intersubjectively sustained field of modern scientific experience. This field is a social field, and it both carries, and is shaped by, the mathematical as what rules the basic movement of that field. Fleck (1979: 99) makes a useful analogy to a musical orchestra, asking "[h]ow could the performance of an orchestra be regarded as the work of only individual instruments, without allowance for the meaning and rules of cooperation?" The rules which give shape to a thought style are thus social rules, rules which coordinate and give meaning to the thinking and doing of actors within specific fields of scientific research. Heidegger's concept of the mathematical, as what rules and determines the basic movement of science, may likewise be viewed as a mythological and multifaceted manifestation of the fundamental social rules which determine the shape and direction of modern scientific existence.

Conclusion: Three Desiderata for Hermeneutic Studies of Science

I have suggested that the central difficulty of hermeneutic studies of science is to explain how scientific knowledge-making practices can be both circular and valid. Scientists justify their knowledge claims by adducing reasons from natural phenomena. It is commonly held that the reasons used to ground knowledge must be independent of that same **[97]** knowledge. The proof of that knowledge should, in other words, not be circular. Hermeneutic studies of science challenge this claim, arguing instead that the empirical discovery of evidential reasons presupposes, in some sense, the knowledge those reasons are meant to ground. Scientists do not, strictly speaking, discover those reasons in the phenomena, but, in some sense, they bring those reasons with them when they investigate those phenomena. It is clarifying the meaning of the phrase 'in some sense' which is the main difficulty for hermeneutic studies of science. How, precisely, do scientists already know what they subsequently claim to discover? In what way, in other words, can this circle of scientific practice be viewed as virtuous rather than as vicious?

I have argued that this difficulty had already been recognised in the fourteenth century by Paduan natural philosophers. They located and described the difficulty, and emphasised the importance of the process of discovery by which initially confused and general knowledge is transformed into clear and distinct knowledge. In doing so, they successfully identified why the circle of scientific practice is not a vicious one, but they were unable to explain what makes it possible.

Heidegger attempted to explain this on the basis of his existential phenomenology of scientific experience. He argued that successful scientific research is enabled by an historical tradition. It is through their participation in this tradition that scientists acquire the pre-theoretical, unarticulated background of knowledge they require in order to experience nature in the appropriate way. Scientific experience was, for Heidegger, deeply rooted in the sedimented mythologies of a particular form of historical existence, and it is this historical existence which gives shape and direction to that experience. Heidegger's explanation, I suggested, invites sociological elaboration, and Fleck's social epistemology of scientific discovery provides a useful way of answering that invitation. Combining their two views, I suggest that hermeneutic studies of science pursue a programme in the historical sociology of scientific knowledge. This would involve an organised effort to tell the history of how the thinking and doing of individual scientists become socially conditioned as the constituent parts of an integrated field of collective scientific experience. **[98]**

With this programme in mind, let me conclude by briefly stating three desiderata for hermeneutic science studies. First, it should be anti-metaphysical. It should not seek a solution to the difficulty posed by the hermeneutic circle by leaving behind experience, and fleeing into a transcendent realm of concepts and rules. Second, it should recognise that staying within this circle commits it to the history of science. Both entrance into and successful movement within the hermeneutic circle are necessarily historical achievements, and must be studied as such. Third, and finally, it should recognise that these achievements are not those of isolated individuals, but of individuals whose combined work generates and sustains an intersubjective field of shared experience and meaning. The rules governing successful scientific practice are constituted within this intersubjective field, and would be meaningless in its absence.

I do not want to suggest that these desiderata pick out sufficient or even necessary conditions for hermeneutic studies of science. This would place an unreasonable restriction on the freedom to experiment and to speculate, as well as on the creative potential of fundamental disagreement. But I do want to suggest that this is one excellent way in which to pursue hermeneutic studies of science, a way which I am most happy to recommend.

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