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## **Calling Science Pseudoscience: Fleck's Archaeologies of Fact and Latour's 'Biography of an Investigation' in AIDS Denialism and Homeopathy**

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**Abstract:** Fleck's *Genesis and Development of a Scientific Fact* foregrounds claims traditionally excluded from reception, often regarded as opposed to fact, scientific claims that are increasingly seldom discussed in connection with philosophy of science save as examples of pseudo-science. I am especially concerned with scientists who question the epidemiological link between HIV and AIDS and who are thereby discounted—no matter their credentials, no matter the cogency of their arguments, no matter the sobriety of their statistics—but also with other classic examples of so-called pseudo-science including homeopathy and other sciences, such as cold fusion. The pseudo-science version of the demarcation problem turns out to include some of the details that Latour articulates multifariously under a variety of species or kinds in his essay/interactive research project/monograph, 'Biography of an Investigation'. Given the economic constraints of the current day, especially in the academy, the growing trend in almost all disciplines is that of suppression by threat: say what everyone else says or you won't be hired (tenured/published/cited). In this way, non-citation of outlier views generates what Kuhn called normal science. Finally, a review of Lewontin's discussion of biology shows the continuing role of ideology by bringing in some of the complex issues associated with the resistant bacteria (tuberculosis, Lyme disease, syphilis) and AIDS.

### **1. Reception and Its Discontents**

Ludwik Fleck, himself an outstanding scientist, has been rather less than well-received in philosophy of science, the field of philosophy in which the primacy of the 'received view' enjoys its best legacy or dominion to this day.<sup>1</sup> For this reason, almost all essays on Fleck begin by apologizing for writing on him and most recently he has needed to be defended from Fleck scholars as well.<sup>2</sup>

Fleck's *Genesis and Development of a Scientific Fact* (Fleck 1979) was initially published in 1935. Fleck is usually characterized as a Polish Jew although, nationally, the reference can be misleading, given that Fleck was born in 1896 in Lvov/Lemberg in Galicia, then part of the Austro-Hungarian empire and today the Ukraine. Fleck specialized in medical bacteriology, which perforce included epidemiology (itself comprising a range of disciplines from history to anthropology and sociology as well as public health, and, if only for the sake of etiology, philosophy). As a practicing scientist, with notable research discoveries to his credit, including serological discoveries and even a complete field that he named leukergy,<sup>3</sup> Fleck would seem a model philosopher of science. So it should seem, but most treatments of Fleck begin, as I have now done, by informing the reader about him while lamenting his lack of reception.

Not only is Fleck unjustifiably ignored in philosophy of science proper<sup>4</sup>—disattention being the prime engine of academic suppression—but given his hermeneutic complexity (about which more below) when he is read, his work is often mischaracterized. What is certain is that Fleck counters mainstream or received philosophy of science, leading some interpreters to find themselves flailing between the Scylla of relativism (which they, misleadingly, attribute to Fleck, sometimes in a well-meaning way)<sup>5</sup> and the Charybdis of scepticism, despite the 'fact', as it were, that as a scientist, Fleck himself was always concerned to speak of 'scientific facts'.<sup>6</sup> Fleck's contributions, apart from his work in the discipline he called leukergy, are thus in philosophy of science proper, particularly epistemology, if I will also parse those contributions historically and interpretively or as I argue, along with Patrick Aidan Heelan and others, as hermeneutic and phenomenological.<sup>7</sup>

Where Fleck is conventionally introduced it can tend to be in connection with Thomas S. Kuhn, who was for his part often concerned to distance himself from Fleck after his initial reference to Fleck in the 1962 preface to his famous *The Structure of Scientific Revolutions*. Thus while Kuhn acknowledges the value of Fleck's ideas for his own work, the context and style of this acknowledgment in addition to his relative subsequent inattention to Fleck has allowed the majority of Kuhn scholars to discount Fleck's 'influence' on Kuhn.

Recollecting his three-year stint as a 'Junior Fellow of the Society of Fellows of Harvard University' (Kuhn 1962, v), Kuhn rhapsodically highlights the sheer serendipity of his encounter with Fleck, almost in a picture-book allusion to the cross-over between analytic and continental emphases, mentioning Piaget, Quine, and Whorf in the same sentence as exemplifying in Kuhn's recollection

the sort of random exploration that the Society of Fellows permits, and only through it could I have encountered Ludwik Fleck's almost unknown monograph, *Die Entstehung und Entwicklung einer wissenschaftlichen Tatsachen* (Basel, 1935), an essay that anticipates many of my own ideas. (Kuhn 1962, vi–vii)

Kuhn explains that although 'readers will find few references' to Fleck's work in his own study, the acknowledgement is a vital one inasmuch as his work remains indebted to Fleck's earlier study 'in more ways than I can now reconstruct or evaluate' (Kuhn 1962, vii). I've argued that this complex association deserves our attention (Babich 2003a, 2003b) and in *Thomas Kuhn*, a recent biography, the social thinker on science, Steve Fuller, settles the matter on Kuhn's side by setting Fleck's influence in terms of a still broader context (Fuller 2001, 59–60) and casually replicating, as Fuller does here, the hierarchy of the sciences in the process: whereby Kuhn's topics would be physics and chemistry but Fleck himself would be labouring in the at the times not-yet science of medicine, then still in the throes, according to Fuller, of 'exchanging its artisan roots for a more experimentally based future' (Fuller 2001, 60). Fuller's comment replicates the key problem of philosophy of science (*P* and not-*P* as I call it, meaning physics and not physics—where *P* does not even include chemistry, to the fair indignation of those who write on chemistry: see Babich 2010) and is quite innocent of the very experimental context of Fleck's research expertise in biochemistry and cytology. Nevertheless, it is certain that Kuhn did not quite anticipate Fleck's radicality and later sought to distance himself from that same radicality and especially from the extent to which those same radical dispositions could appear to have informed Kuhn's own *The Structure of Scientific Revolutions* (Kuhn 1962, v–vii).<sup>8</sup> The distancing strategy is still more evident in the introduction Kuhn wrote for the translation of Fleck's book (Kuhn 1979), a translation initiated not by Kuhn (who in his reflections offers a taxonomy of fields and the social and other distinctions that follow from them, as departmental demarcations haunt many academic lives, not only Kuhn's) but by the US sociologist Robert K. Merton together with the English-based Wilhelm Baldamus (1977). Hence, when Kuhn speaks as a philosopher of science among philosophers of science, that is, to Aristides Baltas and Kostas Gavroglu, his recollection of reading Fleck focuses on glossing Fleck's title while offering a hermeneutic of Kuhn's own preface, thus emphasizing the limits of Fleck's influence and so reducing Fleck to a literally titular influence:

It was I think in Reichenbach's *Experiment and Prediction* that I found a reference to a book called *Entstehung und Entwicklung einer wissenschaftliche*

*Tatsache*. I said, my God, if somebody wrote a book with that title—I have to read it. These are not things that are supposed to have ... they may have an *Entstehung* but they are not supposed to have an *Entwicklung*. I don't think I *learned* much from reading that book. (Kuhn 2000, 283)<sup>9</sup>

The more complex problem of Fleck's broader reception is beyond the scope of this paper: bridging not only (as Kuhn himself emphasizes in the taxonomic differentiation noted above) the disjoint fields of *history of science* as opposed to and distinct from *philosophy of science*—thus Kuhn points out to his interviewers after a long exchange in which he implicitly makes the same point again and again: 'although I'm the chairman of a program in "history and philosophy of science", there is no such field' (Kuhn 2000, 315)—quite in addition to the different emphases of the sociology of science and indeed, as I point out elsewhere with reference to Latour and others, the anthropology of science, to which one must also add the further hermeneutic and phenomenological, that is to say: *continental* (as opposed to analytic or mainstream) approaches to philosophy of science (Babich 2010). Nevertheless, the above themes, including their intersections and overlaps, are essential to note just because this article turns upon the issue of reception as such and its importance in philosophy of science including science studies. What thinkers are received, what ideas become influential, what counts as or is taken as truth, is a particularly important question when it comes to scientific truth, scientific fact.

## **2. On the Future of Science Studies: From Fleck's Scientific History to Latour's Lab Anthropology**

Here I read Fleck's *Genesis and Development of a Scientific Fact* in combination with claims traditionally excluded from reception, regarded as opposed to the facts, and consequently almost never discussed in connection with philosophy of science save as examples of pseudo-science and what had traditionally been called the problem of demarcation. Here, I am especially concerned with those working scientists who have questioned the epidemiological link between HIV and AIDS and are discounted in consequence and this no matter their credentials, irrespective of the cogency of their arguments or the sobriety of their statistics. Along the way I will have reference to other classic examples of so-called pseudo-science including homeopathy and, in passing, even the maligned example of cold fusion, starting below with Bruno Latour's own reflections on the contextualization of his own explorations in his recent

(autobiographical—but, as Nietzsche would say, aren't they all?) project, 'Biography of an Investigation' (Latour 2012).

Given the economic constraints of the current day, especially in the academy, the growing trend in almost all disciplines is that of suppression by threat: say what everyone else says or (and this is a real and working threat) you won't be hired, tenured, published, or indeed read. Non-citation of outlier views yields nothing less than what Kuhn called normal science: everything else is (using Charles Fort's technical term for the excluded) effectively 'damned'. In Latour's recent reflections on the 'disciplining' of anthropology (Latour 2013), the argument advanced is not only that anthropology is barred from observing the practice of the 'whites', that is, the natural sciences but that anthropology and other social sciences are in practice excluded from the greater philosophic conversation on science.

For Latour, in what began as a reflection on a book about such scientific modes of existence, had invited his own involvement with the movement in philosophy known as the 'speculative turn', if it is also true to say that Latour for his own part engaged this movement in his own fashion going beyond what some analytic (and some self-describedly) continental scholars might call 'experimental philosophy' towards what mattered for Latour and his own studies of laboratory science, namely towards the empirical. As Latour explained (and it is fair to say that he meant this literally, even if it would turn out to have disputed significance for those scholars outside his group who took his internet disseminated call seriously):<sup>10</sup>

I invite my co-investigators to help me find the guiding thread of the experience by becoming attentive to several regimes of truth, which I call *modes of existence*, after the strange book by Étienne Souriau, recently republished, that features this phrase in its title. (Latour 2012)

The Souriau text to which Latour, along with his co-editor Isabelle Stengers, refers (Souriau 2009), was itself initially published in 1943, and the exploration for Latour is part of his project of explaining his own apparently contradictory approach to anthropology of science exactly qua *philosophy of science*. In Latour's 1991 *Nous n'avons jamais été modernes. Essai d'anthropologie symétrique* (a book which may with profit be regarded as the first contribution since Pierre Duhem's otherwise little received *German Science* to a national or 'territorial' anthropology of science), he argues that we do not put ourselves as investigators in question and to this extent it may be argued that it is a pernicious lack of hermeneutics, rather more Dilthey- and Heidegger- and of course Nietzsche-style than either Ricoeur-style or Gadamer-style

hermeneutics that continues as methodological deficit to limit philosophy of science but also and this is Latour's more disciplinary claim, the field of anthropology as well.

'The classic question of philosophy, "what is the essence of technology, science, religion, and so on?"' is on Latour's reading tacked into or upon the object question favoured by the new speculative turn in so-called object-oriented ontology, which Latour then repositions in terms of his own question: 'what are the *beings* appropriate to technology, science, religion, and how have the Moderns tried to approach them?' (Latour 2012, 2).

Latour himself does not invoke hermeneutics as much as he details its practice, with reference to the Bultmann scholar and translator who was his own teacher at Dijon, the Heideggerian specialist, André Malet. Instead, cleaving more closely to the protestant Bultmann than the bastard Catholic Heidegger, Latour reflects that the science of reading, and this would include classical and theological philology itself, requires the same anthropology of reception that Pierre Hadot, in a separate instance of reflective hermeneutics, dedicated to his own account of the history of the reception of Augustine in the rise and fall or fall and rise of interpretation of philological rigour (in addition to Pierre Courcelle, on Augustine, Hadot mentions as scientific exemplars Paul Henry on Plotinus: Hadot 1995, 51). It is not my claim that Hadot is Latour's reference here, but the point is a related one in the context of the hermeneutic which is itself as a discipline also not much in vogue in Latour. Nevertheless, hermeneutics is what Latour is talking about as he explains to his readers here, 'the Biblical text finally became comprehensible, revealed as a lengthy process of transformations, inventions, glosses, and diverse rationalizations which, taken together, sketched out a layer of interpretations that played out' (Latour 2012, 3).

That Latour is telling all of us, and perhaps also himself, the story of his life, and defending that account in the process is clear: 'In the Abidjan of 1973–75, I discovered all at once the most predatory forms of capitalism, the methods of ethnography, and the puzzles of anthropology' (Latour 2012, 4). The insight into this confluence would served Latour brilliantly for the rest of his life, and yet, and this is perhaps instructive to the scholar, Latour himself never saw any of this as cautionary, but rather and to be read as revelation. And it is in this revelatory fashion that he retraces the history of the effects of this initial insight in his more recent book (Latour 2013) as a sign and invitation to his own further researches to the height of persuasion, such that the reversals or better obstacles he would encounter along the way (the name and the *locus classicus* of such loci of Princeton would have to loom large here), would remain desolations to surprise him to this day. Yet the question Latour asks is perhaps the most important for the present essay on Fleck, raising once again the same questions, almost in the same way that Nietzsche had raised them in speaking to his own colleagues his

inaugural address in Basel, in a now largely forgotten text on the so-called Homer question (for an explication in the context of philosophy of science and indeed of the relation between philology and science, see Babich 2010). For Latour, ‘why do we use the ideas of modernity, the modernizing frontier, the contrast between modern and premodern, before we even apply to those who call themselves civilizers the same methods of investigation that we apply to the “others”—those whom we claim, if not to civilize entirely, then at least to modernize a little?’ (Latour 2012, 4).

Given my current theme I will not be able to develop this point here as much as it deserves but we might apply it to the distinction to be drawn between science and pseudoscience as philosophers of science might demarcate the same, as well as to the status to be claimed or disputed for to be named a philosopher of science like Fleck or indeed Heidegger or more distally still, Nietzsche.

The Whites anthropologized the Blacks, yes, quite well, but they avoided anthropologizing themselves. Or else they did so in a falsely distant, ‘exotic’ fashion, by focusing on the most archaic aspects of their own society—communal festivals, belief in astrology, first communion meals—and not on what I was seeing with my own eyes (eyes educated, it is true, by a collective reading of *L’Anti-Oedipe*): industrial technologies, economization, ‘development’, scientific reasoning, and so on: in other words, everything that makes up the structural heart of the expanding empires. (Latour 2012, 5)

I cannot here retrace the story of Latour but he relates it as taking him from Abidjan to San Diego in search of symmetry, but it may be sufficient to note that in Powerpoint presentations of this essay I find it useful to feature a slide of Latour and Woolgar’s *Laboratory Life*, pointing not to the fatal subtitle *The Construction of Scientific Facts* with its scare word ‘construction’ as this would perturb both Ian Hacking and Latour as critics turned the term against them by using it to characterize their own work—this sensitivity engendered no fewer than two books (Hacking 2000, preceded at the same press by Latour 1999)—but to the fact and the problem of the fact that the introduction was composed by Jonas Salk himself.

Latour’s violation of the code of scientific objectivity may well have been a revelation or a sign in his own reading as the same violation took him to his on-going success (or influence) in the guise of his own study of neuropeptides qua actors qua actants, and that ‘violation’ was his attention to the scientists themselves in laboratory itself. Frank L. Baum’s wizard knew what he was doing when he warned Dorothy not to look at the man behind the curtain. All Latour’s success would crystalize as soon as his

attention could be shifted to the things themselves, the objects as such, the ‘non-human characters’. What one would *not* be discussing would be the human agency of human agents and certainly not if one dared to call them by name or take a peek at their social practices. No matter for Latour, a focus on the ‘non-human characters’ would be quite enough:

I suddenly understood that the non-human characters had their own adventures that we could track, so long as we abandoned the illusion that they were ontologically different from the human characters. The only thing that counted was their *agency*, their power to act and the diverse figurations they were given. (Latour 2012, 6–7)

We will have an opportunity to return to this question of investigation and influence in the final section of this essay. Here it is enough to presage the point in terms of Latour’s own rueful parenthesis, musing on what response he might have had had one ever indeed thought to ask him about his own ‘philosophy’:

(Not to worry: no one has ever asked me that question, since the tumultuous quarrels over relativism and the science wars have in the meantime turned me into a mere sociologist, adherent of a ‘social construction’ according to which ‘everything is equal’, objective science and magic, superstition and flying saucers ...). (Latour 2012, 13)

Rules and the roles of demarcation remain and in Latour’s little list of relativized damnations, Feyerabend’s famous damnation, astrology doesn’t rate a mention. Thus it is telling that when I presented a version of this study, speaking on Fleck to ethnographers in Heidelberg and suggesting in the course of my presentation that one might do well to take a cautionary hint from Latour in their own application of their own field to medical anthropology, be it a matter of the disease presentation of AIDS specific to Africa or else to Southeast Asia or investigating the role of witchcraft and other agencies in illness or reflecting on the different working assumptions of Aryurvedic medicine, my very kind colleagues in ethnography despite their greater knowledge of the culture in question were happy enough to subordinate non-white knowledge culture to white knowledge culture, rather as Latour had experienced it during his Abidjan sojourns some fifty years earlier. I thus found that the very parallel I sought to suggest met with significant resistance. To my surprise in that encounter, my social science colleagues had no doubts whatever regarding the ‘facts’ and when I suggested that science studies itself was at risk or in danger of a certain element of

thought control, they protested that it was not so. As we continued to speak I realized that, of course, the ruling view had never been in question and, like myself of course and obviously in ways I myself doubtless have trouble seeing, my interlocutors had, in Latour's own terms, never been modern. Their good goal, well ensconced into the university game plan was to train young anthropologists of science Harvard-style and thus to have them secure jobs and grants by landing on the only side of the debate on which jobs and grants are to be had.

### **3. Pseudoscience, Damned Science, and the Facts**

For the sober-minded philosopher of science as for mainstream students of history and sociology of science: so-called 'sciences' such as AIDS science that happens to be critical of HIV causality (in any fashion whatever) or sciences that have to do with homeopathy or cold fusion, the very idea seemingly, but most particularly as advanced from the perspective of chemistry *à la* Pons and Fleischmann, etc., are counted as so many pseudosciences, just as years ago, astrology would be quite conventionally invoked as a paradigmatic example of such for Theodor Adorno (1994) and thus, instructively, still more critically than Adorno, in Feyerabend's critical response to the 1975 statement signed by "186 Leading Scientists" in what he called "The Strange Case of Astrology" (Feyerabend 1982, 96). Subsequently, parapsychology or other occult sciences would also be counted as paradigmatic pseudosciences, 'signs' just as Adorno had divined these as such in his reflections on the Los Angeles Times Astrology Column in his essay on 'second-hand superstition' (Adorno 1959), published all these many years ago now on the 'Irrational in Culture' (see the contributions to Grimm 2000). Accordingly, to conventional wisdom, which does not depart from Adorno's position, the only respectable, i.e., rigorously philosophical way to talk about such things would be to denounce them as such, that is as pseudosciences thereby demonstrating that the investigator shares the mainstream view.

To illustrate the problems of so-called science versus so-called pseudoscience, it is worth recalling the Bronx archivist of scientific fact, Charles Fort, and to note, once again, his language of the 'damned' (Fort 1919).<sup>11</sup> Fort, a contemporary of (and kindred spirit to) Theodor Dreiser, loved facts as observed, not the facts alone but in their contextual constellation as reported, published, disseminated. Fort would spend his life observing, nicely hermeneutically or as Nietzsche would say, perspectively, that observation itself is the problem. Far from simple objective affairs, the 'facts' are reported some this way, some that way. Some reports appear in certain loci, some in others. And sheerly from a journalistic perspective, considering all periodicals as one

might, taking the archivist's view as objectively as one might desire, it turns out that from what might logically call the 'universe' of factual reports, this was the engine behind Fort's collection, *The Book of the Damned*, and above all of his style of presentation of such so-called 'facts', only some so-called 'facts' had the fortune, whether for good or for bad, of being taken up and repeated and, by being so repeated, 'verified' (to use Reichenbach's or Popper's convention) in a constation of reception, while other (seemingly) comparable reports would be doomed to vanish, 'damned' after only a single notice. *The Book of the Damned* chronicles and systematizes, in order to draw attention to and thus to give account of such 'rare' and ergo epistemically or factually 'endangered' newspaper and scientific journal reports.

The 'damnation' of inadvertence, non-mention, inattention is a working one. It ensures that reference to certain observations (*qua* observations or as such) are silenced. Where what matters in science to this day is citation, reference, repetition, acknowledgement, recognition or what is sometimes in university discourse called 'impact', *non-mention* negates certain 'facts' as facts. In this the original observations and facts simply disappear from scientific discourse, 'damned' by nothing more than mere non-citation: no review, no discussion, in other words, *as if* never reported to begin with. This is a kind of retroactive gatekeeping.

As Fort recognized, the legion of the 'damned' that he chronicled (and thereby restored to a zombie existence in his own books) reflected the scholarly excommunication characteristic of then modern scientific and journalistic (and of course we can add: sociological and anthropological and even more so, perhaps, philosophy and indeed history of science) establishments.<sup>12</sup> To this array of the efficaciously damned, we may nominate Pons and Fleischmann's precipitous announcement of their work on cold fusion<sup>13</sup> in addition to research on the viral etiology of AIDS (see Bucchi 2004, 39ff, as well as, more broadly Gallo 1991, Harden 1992, Proctor 1995, and see especially Duesberg 1995 and Duesberg, Koehnlein, and Rasnick 2003) along with almost the entire controversy on vaccination for both children and pets, debates on fluoridation (debates again and again apparently vindicated, but I would not be sure that any such vindication will end the debate)<sup>14</sup> as well as the controversies on biosolids and EPA science in general (Lewis 2014, where the author also cites an issue of the *Economist* on science from October 2013 as well as Lewis 1996; see too Reich 2011) and always and always and again, the controversies on cancer and its causes, to which we can add climate change and weather modification, such that we can count in geography and meteorology and so on.

Political and social reflections of such scholarly and 'scientific' silencing continue to have implications for the history of biological and other scientific research not to mention the intersection of science and politics and contemporary accounts

(especially of the economics) of science are beginning, gingerly, to take up the complex challenge of such discussions.

My focus here is AIDS research as this offers a patent parallel, as I read it, with Fleck's case history of syphilis but also as AIDS research can seem to be a parallel for what is called pseudoscience and even for ailments that are regarded as spurious and still more complicatedly perhaps for controversial illnesses as Lyme disease (diagnosis and treatment—the antibiotic protocol used by physicians in the US is less than half to one-quarter the potency for less than a third to a one-fourth of the duration of treatment standard for physicians in Germany: see Nau, Christen, and Eiffert 2009 and Kaiser et al. 2011).

But to all of this must be added an unpleasant dimension, that of aggressive both political and personal or *ad hominem* rhetoric. Thus, even as Seth Kalichman asserts in his book, *Denying AIDS: Conspiracy Theories, Pseudoscience, and Human Tragedy*, that he means to make his case without resorting to *ad hominem* arguments (Kalichman writes this as using 'ad homonym attacks': Kalichman 2009, xv),<sup>15</sup> his book goes on to rehearse such attacks, homing in on one of the prime defenders of what Kalichman calls 'AIDS "denialism"', namely one Peter Duesberg, a scientist whose crime has been to suggest, in effect, that more science might be called for in AIDS research. In addition to Duesberg's own oncological research and his other scientific work on retroviruses, Duesberg, himself a chemist and professor of Molecular and Cell Biology at the University of California, Berkeley, has written on AIDS and HIV and as the above list of scientific fellow-travellers would show, Duesberg's crime may have been to continue when other scholars simply gave up disputing what had become standard or received conventionality.<sup>16</sup>

Here, if I followed the standard view in science writing, especially in philosophy of science, which always tends to be more science-triumphalist or scientific than not—this spirit is illuminated by the vulgarity and even the violence of the internet meme: *I Fucking Love Science*—I would here join Kalichman in attacking AIDS denialism out of hand: no questions asked. This is the 'are you kidding?' approach: of course! AIDS! Why people have died! Of course! Everyone knows AIDS is 'caused' by the HIV virus.

Pulpit pounded, case closed, door slammed.

I would then take the same approach with respect to homeopathy and so on down the list, writing like Adorno, of 'the stars down to earth'. Riding this wave, Martin Gardner titles his own biography with an obvious reference to homeopathy: *Undiluted Hocus-pocus* (Gardner 2013). Following standard protocol, I would go on to bash these and related themes on the head for the rest of the essay to follow, brand them as such and so many pseudosciences, say why, say how, so there (e.g. Shermer 2002, Frazier 2009, Pigliucci 2010, Daempfle 2012, Gordin 2012). But everyone else writing

on AIDS and homeopathy has already done that and I'd only be repeating what other pseudo-bashers have said if I jumped on their bandwagon, even if one of the more revealing recent collections edited by Pigliucci and Baudry (and published by the University of Chicago Press, just to add the argument from authority which deployment is in turn an excellent indicator of wagon circling) retitles the issue as a whole with reference to the classic problem of demarcation: *Philosophy of Pseudoscience: Reconsidering the Demarcation Problem* (Pigliucci and Baudry 2013a).

It goes without saying—though I am not exactly sure that scholars do know this—that normal science and normal philosophy of science constitute what appears to be the Olympic sport, metaphorically speaking, of ‘synchronized bandwagon jumping’. When we condemn plagiarism, as we tell our students we do, it is only because it is not done in the proper fashion. Of course we jump on the bandwagon ourselves, of course we beat the drum in the same way as everyone else: we cite our sources (the right ones) and write in accord with (and never against) accepted convention.

To write on Fleck in the context of homeopathy and AIDS and so on rather than in the context of generic historicism is to write against convention, decidedly off the grid, off the bandwagon. What is to more: to write on such things is also philosophically and academically isolating. For scholars cite scholars who say the same things they say or things that support what they say. The result is monotone but we call it standard or normal science. It is the received view and it is by definition, what everyone knows. Say something others do not say and you are on your own. Friedrich Nietzsche and Ernst Mach and but also Max Weber point this out in different ways, as does Martin Heidegger in his own discussion of science but it is also, quite patently, what links Fleck and Feyerabend and Kuhn in their approach to philosophy of science, a philosophical approach that no one of these philosophers of science considered apart from history and that means apart from hermeneutics whatever term they may have used for their own part.

Fleck was much more radical than either Feyerabend or Kuhn and his term of choice was *Denkstil*.<sup>17</sup> Fleck, of course and to be sure, did not invent the term but borrowed it from what he thought to have been a respectable bandwagon extant in his own day, as it were, using such conventions as he found them in the writings of Durkheim, Mannheim, Lévy-Bruhl but also Wilhelm Jerusalem and so on. This is not to say that Fleck was a disciple of any of these authors (the literature is full of scholars who do not find that Fleck does not say enough about Mannheim, the same scholars who themselves have nothing to say about Lévy-Bruhl for their own part and who do not cite the authors Fleck does cite) as much as it is to say that Fleck took himself to be on board with this particular ethnographic (including sociology and anthropology but also comparative history and religious and mythological theory, as well as rhetoric and

so on and on and including philosophy) to the extent that he found, for good systematic reasons that his own research investigations had to draw on such historical, anthropological, sociological, and comparative mythological research, including the heart of philology, which is in turn all about language and word, the terms we use to characterize the world as we find it, experience it, comprehend it, and, as scientific experts, know it.

#### **4. Metaphor, Truth, and the Esoteric**

The problem of language and word in this context is metaphor.<sup>18</sup> Thus, in his own *Genesis and Development of a Scientific Fact*, Fleck observed that the metaphors in use in any given era carry what I have elsewhere called the ‘penumbra of their past’ along with them: old wine commingling with new. I think you have to be freshly attentive to overcome the attenuation of consciousness and the numbing of habit just to be able to notice such things. Indeed, this would seem to have been what Nietzsche meant when he used a metallurgical, numismatic metaphor for truth, that was to be sure an esoteric—indeed, a beautifully high esoteric—reference to ancient cynicism.

Thus Diogenes the Cynic came on bad times, leading to his exile from his home at Sinope, because his method of handling the Sinopean treasury (of which he was master), like many modern world financial organizations to this day, consisted in, as he blamed the oracle at Delphi for so instructing him, to ‘adulterate’ the coin: taking a metaphorical word, the word of the oracle at Delphi a bit too literally: Παραχάραξε το νομισμα.<sup>19</sup> Now as Plato and Thucydides remind us, the thing about oracles is that nearly everyone gets them wrong because nearly everyone takes them literally, the problem of parsing being a matter of directionality. Avoiding the wrong direction might be the point or project of philology: certainly it helps in thinking about metaphor.

I have dedicated the past few lines to Nietzsche on truth and lie and metaphor and Diogenes the cynic and the revaluation that got him ostracized from his civic and official position and his home at Sinope (see Sloterdijk 1987, Shea 2009, Hénaff 2010, Babich 2011–2012) because Diogenes himself only did deliberately to his revalued coins what is in any case, as Nietzsche observes in his extra-moral meditation on truth and lie, regarding the fate of coins in general: their value is obscured through handling or otherwise lost over time, governments change, markets crash, and numismatic enthusiasts dealing in rare coins enter the scene. Now the value is the truth (the silver or gold in the coin), but the face value is the list or market price as it were. Diogenes adjusted the face value, and as Nietzsche points out, the value stamped on anything is elided by constant handling: convention or commonality ultimately devalues

everything, including the common origin of commonality itself. Thus at issue here is not merely the common coin of our exchange or intercourse with one another but the value we place on the terms used, the claims made.

What is truth? Better to ask, as this was Fleck's point, what are our presuppositions, just what do we assume that enables us, just to cut to the empirical basis of his reflections on epistemology, and I am citing his title here, 'To Look, To See, To Know' (Fleck 1986)?

My own critical essay on looking, seeing, and knowing in science (Babich 2010) concerns what I have already mentioned as the 'not-*P* sciences': sciences that happen not to be physics, including chemistry and geology among the more respectable among the overlooked, dissed, or even the specifically 'damned' sciences. That Pons and Fleischmann were chemists did nothing to help their case to be the 'discoverers' of cold fusion, and one may well expect in the coming decade or so for a physicist (a representative to adopt Latour's metaphor of the right scientific tribe) to win a Nobel Prize for what had been their original discovery (see Biberian 2007 for one overview of current and on-going research).

And if chemistry is a science like no other, that is to say because all that means is that chemistry is not like physics, classical, relativistic, or indeed quantum (see for an overview of the philosophy of chemistry in this context Babich 2010, 361–362, including references to the work of Eric R. Scerri and Jaap van Brakel), biology likewise is a science of its own complex and diverse kind (Babich 2010, 364–366) as is medicine as Fleck is concerned to emphasize. What should be instructive is that biology and physiological science and medicine may follow in the path of physics (as Fuller argues they ought) and thus remain on the royal road towards recognized 'science' or they may fail to mirror this pattern and this where the homeopathic damning bit begins to set in (for examples see Goldacre 2007, Coghlan 2010, etc.). Obviously in the case of the social sciences, like anthropology which as physical anthropology is the closest to 'real' science, and as I recall from my student years at Stony Brook, even indistinguishable from the natural sciences (physical anthropology was part of the department of Anatomical Sciences) and thus to what we tend to think of as 'science' proper in Anglophone context, related concerns will issues for comparative anthropology, sociology, political science, etc. The problem is that when speaking of science, as Fleck argues, even the terms we use, that is to say the words we employ with respect to different scientific traditions have our stamp on them, especially in the case of the social sciences which are in some cases designated 'historical' sciences (as opposed, so one assumes, to the timeless objective sciences). To this extent, we play with the coin of scientific conventionality like so many modern day Diogeneses, only our modulations are blind by contrast with the original Diogenes who knew he was

adulterating or moderating the coin: he was doing so calculatedly—this is one of the meanings of cynically after all.

What Fleck calls the esoteric circle, includes ‘as “general” experts, scientists working on related problems—all physicists for instance’. And as corollary, as Fleck explains, the ‘exoteric circle comprises the more or less educated amateurs’ (Fleck 1986, 111). Fleck thus distinguishes expert science from popular science in the context of an elaboration of what is more commonly known, using Polanyi’s metaphor, as ‘tacit knowledge’ of this same esoteric acquisition of expertise. The point for Fleck is to distinguish between what the student or novice learns and what, in effect, and this is Polanyi’s point, cannot be taught as such. This is the Aha! phenomenon that Fleck describes using scholastic, even Jesuitical terminology: ‘The Holy Ghost, as it were, depends upon the novice, who will now be able to see what has hitherto been invisible to him. Such is the result of a thought style.’ (Fleck 1986, 104).

This same thought style is also key for the scientist’s own operative demarcation problem when it comes to discovery: ‘It is always necessary to reject or ignore many problems as trifling or meaningless. Modern science also distinguishes “real problems” from useless or “bogus problems”.’ (Fleck 1986, 104). Fleck would to this extent appear to be fully on board with today’s language of pseudoscience and the philosophical problems of demarcation so on and yet, so I will argue, he is not.

## **5. ‘AIDS Denialism’ and AIDS Science**

Peter Duesberg remains the primary target for those who write on AIDS denialism. Understandably, inevitably, this status weighs on Duesberg—so much so that when I met him a few years ago in Berkeley he joked, it was the first thing he said to me, that journalists had been comparing him to Hitler. By any standards, a macabre joke, it was an unsettling first word. And to be sure, the journalistic point of using such comparisons is designed to evoke associations with Holocaust deniers. Calumny is as effective a tool as it ever was, even in science.

For his part, Duesberg has rather a lot of evidence on his side, epidemiological (including public health analyses) and clinical but also theoretical. Even more credibly (so one might think), he also has the support of Luc Montagnier who along with Françoise Sinoussi won the Nobel Prize for discovering the HIV virus (note that Robert Gallo, himself investigated for failing to credit others, and who was also in the same department as Duesberg at Berkeley, ought to be named, despite his own disinclination to name others, among those who ‘discovered’ HIV). Montagnier supports Duesberg’s claims and in fact Gallo’s claims too accord with Duesberg’s to the extent that each one

of these specific scientists has pointed out (and there is near unanimity among scientists involved with AIDS research) that the sheer ‘having’ of the HIV virus is as such no death sentence, as just this is associated with AIDS, precisely parallel to the complex fashion in which the sheer presence in the organism of the spirochaete does not necessarily mean that one ‘has’ syphilis, just to the extent that disease, especially of the immune system, is a complicated thing (just as Fleck, a specialist in just that field, writing exactly on topic, details). And one would think that this reference to co-factors and myriad contributory issues, that is to the obviously well-known complexity of disease causality would itself be a winning hand, that one could settle the claim of pseudoscience out of journalistic court as it were, and let the scientists go on with exploring the complex aetiology of AIDS and the relation to HIV.

But one would be wrong because there is yet another contaminating factor (so to speak). For the problem is that the most famous of the AIDS scientists, Luc Montagnier does not merely which in this case is to say *only* point out that the immune system was involved in the whole question of causal agency and by no means the virus alone if indeed that, because (this is, again, the parallel with syphilis) as with many pathogens one can be what is called a *carrier* or indeed one can be *both* asymptomatic *and* utterly uncontagious and ‘have’ whatever that will mean in such cases the virus as such, in that one neither suffers from nor communicates the virus in question). What is at issue has nothing to do with HIV research as such but with what else it is that Montagnier went on to do. For after winning his Nobel Prize, Montagnier undertook to explore nothing other than the causative mechanism of homeopathy (Montagnier et al. 2009; to be sure Montagnier is hardly alone in this, see too Samal and Geckeler 2001; Elia and Niccoli 2004; Roy et al. 2005; Schneider, Klein, and Weiser 2005; Shang et al. 2005; Milgrom 2007; McAllion 2013, etc.).

The issue of chemistry and homeopathy not to mention the poetically beautiful notion of the memory of water is itself highly charged for it turns out, fairly uncritically, that science can indulge in its own witch hunts and I am grateful to Patrick A. Heelan who initially pointed out to me in conversation—he read *Nature* as regularly as he read *Science*—what seemed to him at the time to be a very odd event, when the chemist Jacques Benveniste published his original results *Nature* in the late 1980s (Davenas et al. 1988; for discussion and further references, see Babich 2010). What was so odd for Heelan was that the very same issue conspicuously featured editorial distancing and refutations.

In an independent echo, reinforcing Heelan’s surprised observation on the original occasion of Benveniste’s publication in *Nature*, Phillip Ball has recently analysed this same unusual editorial circumstances attending the publication in question:

After a lengthy review process, in which the referees insisted on seeing evidence that the effect could be duplicated in three other independent laboratories, *Nature* published the paper. The editor, John Maddox, prefaced it with an editorial comment entitled ‘When to believe the unbelievable’, which admitted: ‘There is no objective explanation of these observations.’ (Ball 2004)

As Ball also notes, the observations initially were replicated but after the fracas that followed, those same originally independent reports would themselves, not too surprisingly, never come to be published.

This ideal here is not quite the broad based testing ideal that attends the classic view of the function of scientific publication. And the upshot was to be the tragedy of his life for the late Benveniste, who was unquestionably an otherwise well-respected chemist. One does not buck the mainstream, even if one has the results to prove it, for opponents can always easily be found to challenge the same. Anyone who has worked in a lab can attest to this (as also the already cited Latour and Woolgar 1979) and a good deal of Fleck’s writing in *Genesis and Development of a Scientific Fact* tells us why.

After recalling Benveniste’s fate one might have expected that the report of Montagnier’s research (Montagnier et al. 2009) would inspire a powerful and decisive reactive response. And this was indeed the case across the board, and especially in the UK (Goldacre 2007, Coghlan 2010) and in Germany (Grill and Hackenbroch 2010).

Here the issue concerns the denomination of anything as science (this is also traditionally called the problem of demarcation). For in order for something to be science, as it turns out, and as we know, it is not enough for it to be a fact. Hence if we were to have a new Fleck for the 21st century, as it were, we might need to speak (this is the point of the reference to homeopathy as it would also be the point of referring to the scientific basis of acupuncture) of the genesis and development of a scientific mechanism.

In other words, for the purposes of today’s institutionalized scientific establishment (there is a needed connection here with Heidegger’s discussion of modern science that is beyond the current paper), modern science today requires more than a fact *qua* fact, even *qua* Fleckian fact, complete with the associated prerequisites thereof, that is to say, for those who can stand the language, the prerequisites of and for a given *Denk-Kollektiv* and associated *Denkstil*, but modern science specifically needs to know the means, the way, and the how of a thing before it can be counted today as scientific. This, to give a medical example, entails that although chiropractic, for example, may indeed help your sprained back or some other ailment it cannot be called a science and

will be lumped in with the pseudosciences, which when it comes to getting treatment covered under various health insurance plans (it could be argued, though this is not my theme here, that a good deal of the medical controversy on these questions corresponds to corporate pharmaceutical interests) will mean that you are on your own with all the other vitamin pill popping flakes. Thus and although osteopathy as a science has been around as such for quite some time, its mechanism is unclear, empirically patients get relief, but the explanations used conflict with those of conventional modern medicine.

The same is true, even more so, as already noted, for acupuncture and other kinds of Chinese medicine, empirical practices which have the evidentiary basis of application and reports of thousands of years, thousands of years that mean nothing to Western science. Paul Feyerabend himself was all too aware of both the advantages and the limitations of non-Western medicine—I corresponded and met with him many years ago in Zürich and one of the things I noted then was the patent physical consequences of his own war-time injury to his spine. For such all too ontic reasons, Feyerabend was of a mind to be ‘open minded’ but and at the same time he also knew that medicine (all medicine) has its limits. The point I took away from my many conversations and correspondence over the years with Feyerabend as from reading his published writings was that despite or perhaps because of these same limits one should avoid dogmatism. These days, historians and philosophers of science, along with the scientists themselves, it would seem, are more and more inclined to embrace it.

And if Western chiropractic and Eastern medicine may be regarded as pseudosciences, homeopathy trumps all of these when it comes to the debates on pseudoscience. At the same time, once again, the remedies themselves are routinely efficacious: they ‘work’ as is known to most people in the UK and continental Europe. Hence you might consider homeopathy in a given case yet doing so is far from easy as homeopathy involves rather a good deal even to be tried as one might, for example, simply try something. And thus the current author does not herself use homeopathy. I do not use homeopathy but that is not because I do not ‘believe’ in it—and indeed *not believing in something* would be an absurd pre-condition for testing or refusing a remedy unless one were investigating explicit placebos which is another thing altogether because it simply turns disease into a black box, ignores aetiology altogether and, throwing up its hands, offers the non-diagnosis (idiopathic disease) to match the similar non-treatment (placebo). For my part, I am sympathetic to Thomas Szasz positive definition of disease (Szasz 1974 and see too Kimsma and Van Leeuwen 2005) which to my mind accords with all the complexities of evidence demanded by Fleck. And likewise, for my own part I have great sympathy for Niels Bohr’s pragmatic philosophy of science on matters of belief and efficacy: remedies either work or they don’t and the accusatory diagnostics of superstition or claimed attribution of ‘belief’ has

nothing to do with it, one way or the other.<sup>20</sup> Thus the present author does not ‘use’ homeopathy because it is too complicated: homeopathic remedies are anything but a one-remedy-fits-all kind of treatment. Nothing like taking a course of antibiotics for a cure for a cold in—what does the joke say? Fourteen days if you treat it, or if you do nothing at all, in two weeks.

## **6. Pseudoscience or the New Demarcation Controversy and Fleck’s Symptomatology**

Like homeopathy, similar science vigilante objections have been issued with respect to ‘Chi’, the life force so called in Chinese medicine and in September and October of 2013 there was a small flair up in the blogosphere in the science and pseudoscience wars,<sup>21</sup> including the two academics cited above, Massimo Pigliucci and Maarten Baudry (2013b) bashing a colleague, Stephen T. Asma (2013), the better to get him in line over the course of a few weeks at the end of September through to 10 October in the online and justly named ‘Opinionator’ pages of *The New York Times*. Asma had upset Pigliucci and Baudry with his reminder, *en passant*, in his initial essay that Larry Laudan, who finds the focus on demarcation problematic for epistemological reasons, takes the long view when he writes in his essay ‘The Demise of Demarcation’ regarding the question of ‘what makes a belief scientific’ that the

question is both uninteresting and, judging by its checkered past, intractable. If we would stand up and be counted on the side of reason, we ought to drop terms like ‘pseudo-science’ and ‘unscientific’ from our vocabulary. (Laudan 1983, 125)

Laudan’s point is anything but convenient for philosophers of science who would like to convert pseudoscience debates into a classical demarcation problematic. For Laudan is well aware of the role of philosophers of science as

gatekeeper to the scientific estate. They are the ones who are supposed to be able to tell the difference between real science and pseudo-science. In the familiar academic scheme of things, it is specifically the theorists of knowledge and the philosophers of science who are charged with arbitrating the claims of any sect to scientific status. (Laudan 1983, 111)

Laudan's claim is both empirical—as he contends, no demarcation line has as yet won assent across the board—and normative (his more rigorous point, as I read it, is that no such line *ought* to win such assent: Laudan 1983, 112). Thus compared to Pigliucci and Baudry and others, Laudan far from sounding dogmatically old-fashioned reads like a breath of open-minded or enlightened, or I would say, scientific fresh air—very simply, as he highlights this point:

The evident epistemic heterogeneity of the activities and beliefs customarily regarded as scientific should alert us to the probable futility of seeking an epistemic version of a demarcation criterion. (Laudan 1983, 124)

Thus Pigliucci and Baudry and their fellow travellers offer a rhetorical criterion more in line, albeit indirectly, with Prelli's *A Rhetoric of Science* (Prelli 1989). And of course this all about the rhetorical. What else can it mean?—and this is, I believe, part of Laudan's point: for what else are doing when we insist on calling anything pseudo anything?

Fleck has a reply beyond as we have seen the terminology of the trivial or the bogus just where appearances come into question. For Fleck the bogus is not equivalent to the pseudo if only to the extent that the pseudo happens to be a technical term, medically speaking, precisely well defined in terms of non-specificity as such, particularly with respect to the presentation of disease. The medical world is full of the para and the pseudo, which makes the language of pseudoscience, quite accidentally, almost another word for medical science per se. Thus for Fleck, framing his point with the countervailing assertion that

if we admitted that the development of science is only a matter of time, technical possibilities and accident, we would never understand science; in the first place we would be unable to grasp why the developmental stages possess a specific style of thinking, why a phenomena which is accessible to everybody has been observed at a given moment for the first time, and even almost simultaneously by several researchers. (Fleck 1986, 40–41)

I cite this here not so much for the thought collective dynamic of research in empirical history, interesting as this is, but to point to the heart of the pleonastic as this is the characteristic that happens to be shared by both syphilis and AIDS and to be sure, a good many other diseases, such as tuberculosis and diphtheria as Fleck details this and to which list we may add (for a currently contentious disease that also happens to

involve both spirochaete and non-spirochaete forms, as well as co-factor disease vectors), Lyme disease. For Fleck,

Nowhere outside medicine does one find so many qualifications, pseudo- and para-, e.g. typhoid—para-typhoid, psoriasis—para-psoriasis, vaccine—para-vaccine, anaemia—pseudo-anaemia, paralysis pseudobulbaris, pseudo-croup, pseudo-neuritis optica, pseudoptosis, pseudo-sclerosis, pseudo-tabes; next meningitis—meningismus, Parkinson—Parkinsonism, etc. (Fleck 1986, 41)

Fleck invokes perfectly conventional terms in his day, citing an introductory text, ‘The dividing line between the physiological and the pathological event cannot be biologically drawn with any precision. It represents a whole chain of phenomena with various transitions.’ (Fleck 1979, 56). Now this range goes hand in glove with the effort of the medical establishment, especially recently in its investment in the idiopathic, to claim that a disease presentation is perhaps utterly imagined—there are such assumptions associated with chronic fatigue and fibromyalgia varieties of psoriasis as well as Lyme disease and also IBS (as recent medical reversals on allergies to gluten might also make plain). Indeed, idiopathic disease diagnosis often end with medical practitioners throwing up their hands, leaving both patients and doctors in despair for very different reasons and what is certain is that throwing up one’s hands is almost inevitably a recipe for doing harm, it blocks research and concomitantly, this is the point or the consequence of the ‘diagnosis’, it ends testing and inquiry into the cause of disease and simultaneously offers the patient no treatment at all. There is no mystery whatever in the current enthusiasm for using placebos to treat such diseases. Where medicine cannot find a cause, medicine offers a sugar pill, like the television pediatrician’s calming lollipop (a real doctor, so one should hope, knows better). In addition and because many sufferers of so-named idiopathic ailments happen to be women (and there is a parallel here with the homosexual and drug addict victims of AIDS), their claims are routinely or typically disregarded (shades of surgical diagnoses and treatments of hysteria over the decades: where it is now recognized that most hysterectomies were performed without physiological reason which leaves the question unanswered as to what ailments had been ignored in the process).

For Fleck and this is the problem *in nuce* of the proliferation of the idiopathic (and in retrospect this may prove to be is a striking legacy of the effects of the second world war on biology as a science), it was already in the 1930s quite plain (as a legacy of the late 19th century) that there was no biological unit as such, no *Einheit* to put it simply. Rather than being a hermetically sealed beaker as it were, the organism was part

of a biome, to use our modern language: part and parcel of its environment, its *Umwelt*, its *Mitwelt*.

In this sense, the language of disease we employ today whether with respect to tuberculosis or AIDS makes sense only with reference to a disease entity with very specific and specifiable agency against which precautions might be taken and after a successful siege against the organism (notice to be sure the military metaphor) and to resist which certain means might be available to vanquish or conquer the invader.

The whole of immunology is permeated with such primitive images of war. The idea originated in the myth of disease-causing demons that attack man. Such evil spirits became the causative agent; and the idea of ensuing conflict, culminating in a victory construed as the defeat of that 'cause' of disease, is still taught today. (Fleck 1979, 59–60)

The problem is, as Nietzsche would say, that there is no inside and no outside. And while we may rightly speak, with Claude Bernard, of an interior 'milieu' and so too an exterior milieu, both are of necessity commingled which commingling is of course the meaning, the functioning, the working of life.

To clarify thought styles, Fleck has recourse to the alchemical language at the base or better said, at the origins of chemistry. Fleck's point, a point repeated rather differently and to slightly different effect in Feyerabend, is that the alchemical worldview was differently peopled as it were, to quote the same text Fleck quotes as that text in turn quotes another text by William Ramsay on chemistry (so many nested incunabula):

In those days, to quote the words of Dr. Samuel Brown, the metals were suns and moons, kings, and queens, red bridegrooms and lily brides Gold was Apollo, sun of the lofty dome; silver, Diana, the fair moon of his unresting career, and chased him meekly through the celestial grove; quicksilver was the wing-footed Mercury, Herald of the Gods, new-lighted on a heaven-kissing hill; iron was the ruddy-eyed Mars, in panoply complete; lead was heavy-lidded Saturn, quiet as a stone, within the tangled forest of material forms; tin was the *Diabolus Metallorum*, a very devil among the metals, and so forth in not unmeaning mystery—There were flying birds, green dragons, and red lions. There were virginal fountains, royal baths, and waters of life. (Ramsay in Fleck 1979, 125)

As Fleck goes on to say,

Those people thought and saw differently than we do. They accepted certain symbols that to us appear fanciful and contrived. What if we could present our Symbols—the potential, or physical constants, or the gene of heredity, etc.—to thinkers of the Middle Ages? Could we expect them to be delighted with the ‘correctness’ of these symbols and instantly listen to reason? (Fleck 1979, 125)

Of course not—and making a similar point independently, Feyerabend invoked William Blake, and he might well have invoked many others, who managed to see angels in trees, and to speak to those who see demons today, as psychiatrists sometimes have such patients.

The demons, as Fleck argued in these picturesque terms, remain in science, if not quite as lively as alchemical salamanders and green Lyons then certainly persisting in the metaphors animating the thought experiments of physics, Maxwell’s demon, and whatever evil genius inspired Schrödinger to wish so much injury to cats (for more on Schrödinger’s demons see Babich 2014), with all the echoes of the laboratory use of the lives and deaths of animals as a mark of progress.

The demon is most particularly alive in the notion of the disease entity as such. For Fleck, and this is the reason for his hermeneutic ventures into the meaning of words and terms in history and context, i.e., for the adventures of metaphor: ‘The idea of the causative agent can be traced through the modern etiological stage as far back as the collective notion of a disease demon.’ (Fleck 1979, 41).

The reference is sociological. I note this here because it is part of the reason Fleck cites Durkheim and Lévy-Bruhl much rather than Mannheim, as already noted above that Steve Fuller argues Fleck might have done (Fuller 2000, 60, but cf. Heelan 1986) inasmuch as Fleck’s own reflections are themselves quite ethnographically or anthropologically directed.<sup>22</sup> Accordingly, Fleck quotes Durkheim describing

That which is produced by the activities of the collective intellect, ‘as we encounter them in language, in religious and magic beliefs, in the existence of invisible powers, and in the innumerable spirits and demons which dominate the entire course of nature and the life of the tribe, and as we meet them in customs and habits.’ (Fleck 1979, 46)

Fleck reflects on the idea or ‘concept of infectious disease’, pointing out that it

is based on the notion of the organism as a closed unit and of the hostile causative agents invading it. The causative agent produces a bad effect (attack). The organism responds with a reaction (defense). This results in a conflict, which is taken to be the essence of disease. (Fleck 1979, 59)

The point is, on the one hand, an epistemological one (see on this, again, Heelan 1986) with respect to the aetiology of the concept of syphilis and in particular as it was this that concerned Fleck regarding the practicability of the Wasserman test as a test for a particular antigen that would indicate or signify the ‘disease’ and, on the other hand, with respect to the very notion of disease per se with respect to attesting or assuring the veridicality of the same. This complicated quote summarizes these two trajectories of thought:

It is, for instance, possible to trace the development of the idea of an infectious disease from a primitive belief in demons, through the idea of a disease miasma, to the theory of the pathogenic agent. As we have already hinted, even this latter theory is already close to extinction. But while it lasted, only one solution to any given problem conformed to that style. ... Such a stylized solution, and there is always only one, is called truth. (Fleck 1979, 100)

Manifestly Fleck’s anticipatory confidence that ‘the theory of the pathogenic agent ... is already close to extinction’ was vastly overstated. The AIDS denialism debate to this day witnesses to the resistance of the same and vision of ‘the pathogenic agent’ is alive and flourishing to this day.

Fleck’s analysis not only explored the myths and prejudices and fancies of bygone times (history) or ‘primitive’ cultures but examined the influence of our own metaphorical and mythic thinking within modern medical science. In the same way, he argued that the botanical germ theory haunted early notions of genetics and evolutionary theory<sup>23</sup> as well as (just because it made more sense of) a certain vision of anatomy. Indeed, Fleck has a great discussion of the so-called sesamoid bones from which at the end of days the bones would regrow the original body to its salvific apotheosis. But the most important reflection for our interest here is the demonic representation of disease agency:

As an example of such grossly popular science, consider an illustration representing the hygienic fact of droplet infection. A man emaciated to a skeleton and with greyish purple face is sitting on a chair and coughing.

With one hand he is supporting himself wearily on the arm of the chair, with the other he presses his aching chest. The evil bacilli in the shape of little devils are flying from his open mouth. ... An unsuspecting rosycheeked child is standing next to him. One devil bacillus is very, very close to the child's mouth .... The devil has been represented bodily in this illustration half symbolically and half as a matter of belief. But he also haunts the scientific speciality to its very depths, in the conceptions of immunological theory with its images of bacterial attack and defense. (Fleck 1979, 116)

Arguably, we continue to this day to be persuaded by this invasive schematism, so much so that the notion instantiates the persistence of the standard view or normal idea of disease causality, even the contrary evidence is taken as proof of precisely what is pre-supposed. Ergo if one is exposed to the supposed disease agent (and today this means that if one 'tests' positive for this or that), be it viral or bacterial, etc., *without* becoming ill, new hypotheses are proposed to explain this, and one is held to be 'sick', without or without symptoms. Until proof of the contrary. This is quarantine and medicine has not advanced beyond this. And of course this is the iconic notion of Typhoid Mary and the demonic carrier is again a useful metaphor.

Fleck himself opposes the aetiological concept of disease, as he would of necessity, as a founder of leukergy, but he also notes that this opposition is hardly to find support:

It has been explained that the etiological concept of disease is not the only logically possible one. Nor does it just arise spontaneously in the presence of a certain quantity of knowledge. Nevertheless contemporary scientists, or most of them, are constrained by this concept and cannot think in any different way. This also affects the whole of pathology and bacteriology. The latter has become a medical science and has almost severed its connection with botany. The thought style of pathology in general and of bacteriology is therefore nonbiological, a point that manifests itself both in methodology and in the narrowness of the problem complex with its strict limitation to medical applications. (Fleck 1979, 122)

In the case of AIDS, it is useless to us (qua adherents of modern institutional medical science) to think that the 'cause' of AIDS might correspond to the whole or part of the collocation of causes Duesberg hypothesizes, causes that bear on weakened immune system, or what he calls 'lifestyle', i.e., including drugs, high-frequency sexual activity, more drugs, extreme dieting, more drugs, exercise, more drugs, nutrition, or lack of the

same (dieting), etc. We need because we simply must have a specific causal agency, like HIV, in order that we can develop a specific drug protocol or vaccine to fight it. And the pharmaceutical industry has developed on the basis of the same schema. Thus the holy grail, so it is supposed would be a vaccine or other medical treatment against HIV, i.e., either in advance, or prophylactic, or post hoc, a 'cure'.

Nevertheless, as was known to Fleck, immunization, and medical precautions and medical interventions, etc., have again and again been shown to have little or no effect on infectious disease spread, shocking as it continues to be to say such things. The Harvard biochemist Richard Lewontin makes a similar point, albeit without referring to Fleck for his own part, using the example of tuberculosis which bacillum remains as resistant to our antibiotics as ever—tuberculosis has a kind of built-in 'tank' effect, as it were, and it is almost impossible to penetrate it, making the disease extremely resistant to antibiotic protocols. This does not mean that the contracting of tuberculosis is a death sentence or indeed that it cannot be cured (although antibiotics do not tend to do it). Like syphilis, like AIDS, albeit contracted differently, tuberculosis is a likewise and in a way a 'lifestyle' disease: one is more or less vulnerable to it (some contract the disease, and without treatment, sicken and perish where some contract tuberculosis and, likewise without treatment show no symptoms, feel nothing, and only find they had the disease years later when lesions or scar tissue show up as a result of routine tests, etc.).<sup>24</sup> But of those who suffer from the disease one can nonetheless recover—and in my literary conclusion below, I return to this point with a discussion of Mann's *Magic Mountain*—but the cure, and the very German (and Swiss and Austrian) notion of a 'Kurhaus' reflects this efficacy for the complex environmental organism that is the human being as a whole (rather than a biological unit either on the level of the patient or the pathogen), remains rest, clean and fresh air, lack of stress, and good food. Same as it was more than a hundred years ago.

But we continue to find such notions strange to our idea of disease, we are, we remain, persuaded that disease is a result of an invasive agency, a given disease entity, which entity can be identified and opposed or blocked, and if an incursion into our (presumptively) hermetically sealed being-in-the-world occurs, consequently fought against. Thus Fleck writes as a physician with some chagrin of the consequence of the complexity of the theory of immunology, that is, based on his own field of leukergy, or the functioning of leukocytes and their role in health and disease:

not a single experimental proof exists that could force an unbiased observer to adopt such an idea. It is unfortunately beyond the scope of our discussion to examine all the phenomena of bacteriology and epidemiology one by one to show that the disease demon haunted the birth of modern concepts of

infection and forced itself upon research workers irrespective of all rational considerations. It must suffice here to mention the objections to this idea. (Fleck 1979, 60)

From here Fleck goes on to point out that the primary problem with the disease as conflict, that is to say as a hostile external or foreign agency against which invasion one should protect the organism, depends upon a faulty notion of organism to begin with—starting with the simplest things. Fleck's example is a lichen, as we may remember it from schoolroom botany:

A lichen is one part algae, one part fungus, so very harmoniously coordinated that the result is an organism as discrete as we please, and one can classify all kinds of different lichen. The entire science of ecology depends on this understanding as fleck traces it from the bacterial level to that of what he calls 'the forest unit'. (Fleck 1979, 60)

A whole range of such complexes may be invoked, which complexes, depending upon the purpose of the investigation, are regarded as biological individuals. For some investigations the cell itself may be considered the individual, for others it is the syncytium, for still others a symbiosis of the lichen variety Fleck invokes, or, lastly, even an ecological complex. 'It is therefore a prejudice to stress the idea of organism', in the old sense of the word, 'as a special kind of life unit, a prejudice which is unbecoming to modern biology' (Fleck 1979, 60).

Fleck cites the botanist Hans Gradmann's reflection on the human habit to take the human, indeed and merely the human beings perception of himself as a coherent whole, as the measure of everything and by analogy assume that the world is full of such 'wholes'. Intriguingly, Gradmann's point echoes Nietzsche in this regard who also argues that most scientific and even mathematical vision may be accounted a human biopsychology and even eco-physiology in his philosophy of science (Babich 1994). For Gradmann:

Man's own consciousness of himself as a self-contained whole or entity arouses in one the instinctive notion that the whole living world is divided into a certain number of such units which we call organisms. (Gradmann 1930, 641)<sup>25</sup>

The genesis of the scientific fact in Fleck's study is course the Wassermann reaction, which detects 'the syphilitic antigen in organ extracts and of syphilitic antibodies in the

blood'. But the problem besetting this reaction from the start (and to this day) is its tendency to yield false positives ... just as nearly any sort of symptom could and was ascribed to syphilis in the literature both popular and medical over the centuries, Fleck remarks that even using modern methods of detection leaves one with a great deal of precisely non-specific leeway:

antigen detection in organ extracts is difficult, and even with the best technique yields only very irregular results. Second, extracts from organs which are definitely nonsyphilitic can also fix the complement with syphilis serum. The control tests with negative results are therefore unintelligible, and the high percentage of positive results is very fortuitous. At any rate, the first experiments by Wassermann are irreproducible. (Fleck 1979, 85)

As Fleck points out: 'His basic assumptions were untenable, and his initial experiments irreproducible, yet both were of enormous heuristic value.' (Fleck 1979, 85). What is at stake is not a matter of truth or falsity but the emergence, the establishing, of a standard. *Actual results*, as we are fond of saying, *may vary*—what is perhaps most telling is that in a lab and especially between labs, particularly between international labs, including differing pedagogical and instrument practices, they *always* do.<sup>26</sup> This is part of the reason that Feyerabend argues against Mario Bunge's impatience with paranormal studies by pointing out that scientists 'have never adopted' (Feyerabend 1997, 98) the protocols Bunge insists upon because they are 'self-defeating' if what one is doing is science, arguing that insisting on such principles as Bunge does 'is disastrous for research, bad for education and scientific PR' (Feyerabend 1997, 100). Ultimately, 'Any argument that seems to work against ghosts [as against creationism, psychoanalysis, psi-fields] will hit scientific ideas of a similar generality and any move that lets such ideas survive will also save ghosts.' (Feyerabend 1997, 100). For Feyerabend, where Bunge's materialist-realist and rather 'fundamentalist' 'faith', as Feyerabend characterizes it, clashes with 'a variety of popular views' what is instructive for Feyerabend and here he accords with Fleck, 'scientific practice does not, and is in this respect much freer than any philosophical summary of it' (Feyerabend 1997, 103).

As Fleck uses the notion of thought as he understand this in his discussion of the thinking in a thought style, the knower is changed and he is in the process enabled to adapt, as it were, 'harmoniously to his acquired knowledge' (Fleck 1979, 86) And there is the basis here for an account of the very idea of normal science or the received view as such:

This situation ensures harmony within the dominant view about the origin of knowledge. Whence arises the ‘I came, I saw, I conquered’ epistemology, possibly supplemented by a mystical epistemology of intuition. (Fleck 1979, 86–87)

Myth necessarily comes into the picture, according to Fleck, as science cannot help but include elements thereof owing to experience:

The necessity of being experienced introduces into knowledge an irrational element, which cannot be logically justified. Introduction to a field of knowledge is a kind of initiation that is performed by others. It opens the door. But it is individual experience, which can only be acquired personally, that yields the capacity for active and independent cognition. The inexperienced individual merely learns but does not discern. Every experimental scientist knows just how little a single experiment can prove or convince. To establish proof, an entire system of experiments and controls is needed, set up according to an assumption or style and performed by an expert. (Fleck 1979, 95–96)

Although I do not have the space to go into this here, it is this point that will be taken up by Ian Hacking (albeit and once again without reference to Fleck) in his 1983 *Representing and Intervening* with respect to expert observation,<sup>27</sup> and accordingly offering yet another variant upon the above distinction between what Nietzsche called esoteric and exoteric knowledge. For Fleck this same distinction is key to the experimental method:

The discovery—or the invention—of the Wassermann reaction occurred during a unique historical process, which can be neither reproduced by experiment nor confirmed by logic. The reaction was worked out, in spite of many errors, through socio-psychological motives and a kind of collective experience. From this point of view the relation between the Wassermann reaction and syphilis—an undoubted fact—becomes an event in the history of thought. (Fleck 1979, 97)

## **7. Disease**

What is a disease? This is Fleck's question and the same question appears to different rhetorical purpose in Richard Lewontin's *Biology and Ideology*, reviewing as Lewontin does the standard claims of the great medical advances supposedly wrought by scientific research progress to point out that other factors are inevitably at work in almost every instance where medical science prefers to take the credit. In Lewontin's case, the issue is a matter of rhetoric as of historical context and thus public health confusions. At issue is Fleck's own concern with aetiology or disease causation. As Lewontin writes, intriguingly repeating (without to be sure referring to Fleck) the same point Fleck himself makes, albeit updated for contemporary sensibilities and therefore in terms of the contemporary scientific thought style:

Any textbook of medicine will tell us that the cause of tuberculosis is the tubercle bacillus, which gives us the disease when it infects us. Modern scientific medicine tells us that the reason we no longer die of infectious diseases is that scientific medicine, with its antibiotics, chemical agents, and high-technology methods of caring for the sick, has defeated the insidious bacterium. (Lewontin 1991, 41)

Lewontin is hardly claiming that the tuberculosis bacillum is not connected with tuberculosis any more than Laudan is claiming that everything and anything is science. Instead, Lewontin is pointing to the very problem of identifying disease as such, just as Fleck as is doing in his own study of the constellation of research practices and what becomes 'the' Wassermann test and what becomes our collective identification of syphilis, a collective identification which has parallels with contemporary studies of AIDS causality and treatment.

Heidegger's 1927 'The Concept of Phenomenon' makes a similar point converging with Fleck's own observations 'Features of the Medical Way of Thinking' published in the same year. Speaking about *Krankheitserscheinungen*, that is 'appearances' or 'symptoms of a disease', Heidegger explains that by speaking of such 'appearances'

one has in mind certain occurrences in the body which show themselves and which, in showing themselves *as* thus showing themselves, 'indicate' [*indizieren*] something which does not show itself. (Heidegger [1927] 1962 52/29)

If Heidegger goes on to emphasize that 'this showing itself, which helps to make possible, the appearing, is not the appearing itself, the point he seeks to make elaborates

a hermeneutic articulation of Husserlian phenomenology: ‘Appearing is an *announcing*-itself [das Sich-*melden*] through something that shows itself.’ (Heidegger [1927] 1962, 52/29). Thus circumstances make all the difference and the often cited illustration, Heidegger himself speaks of the paraphenomenon or pseudo-appearance, that is appearance as ‘mere semblance’:

In a certain kind of lighting someone can look as if his cheeks were flushed with red; and the redness which shows itself can be taken as an announcement of the Being-present-at-hand of a fever, which in turn indicates some disturbance in the organism. (Heidegger [1927] 1962, 54/30–31)

It is, I believe, no accident for Heidegger might well seem to have borrowed his own illustration from Thomas Mann’s *Magic Mountain* (1924). Alexander Nehamas (2000), referring of course neither to Heidegger nor to Fleck much less to Lewontin or Laudan, has examined the working of writerly irony in his comparative reading of Thomas Mann’s *Hans Castorp* and Plato’s *Euthyphro*, not to read as much between two heroes or anti-heroes as the case may be, but to look at how we as readers read, and hermeneutically this is worth attending to.

But just in order to do that we need to look at the preconditions for attention. We tend to be caught up in our prejudices, our presumptions, our assumptions. Now Nehamas, a good student of Gregory Vlastos, observes that all of us are persuaded that we know better than Euthyphro but to the extent that Nehamas pays attention to the related art of reading in his *The Art of Living*, the problem of such better-knowing turns out to be the problems that beset the art of reading, or what Nietzsche called a ‘lack of philology’ such that we miss the point that Plato perhaps painting the point with such a sophomorically evident or broad brush might have intended all along: who is the more deluded, Euthyphro or the reader who, after all, goes along entirely. In the case of the parallel example Nehamas gives us, pointing out that as readers we attend to or follow the writerly direction given us, duly, uncritically accepting, the report of Castorp’s sensibility and the injury given to propriety and hence the account of irritability and associated pomposity, we are distracted from what is also related to us as readers at the same time.

The flush which had mounted in his freshly shaven cheek [*die frisch rasierten Wangen*] did not subside, nor its accompanying warmth: his face glowed with the same dry heat as on the evening before. He had got free of it in sleep, but

the blush had made it set in again. (Mann [1924] 1943 as quoted in Nehamas 2000, 25)

As Nehamas however points out what is at work here is a ‘chillingly’, as he puts it, writerly tour de force. What is most interesting for Nehamas is what the writer induces in the reader as that is the same kind of self-duplicity evident in Castorp himself. We know better than what we might read about the appearance of illness in the passage above because we already have our own prejudicial interpretation of Hans Castorp’s appearance:

Hans is flushed because he is shocked, dismayed, and angry. It is difficult to interpret his red face as the first symptom of the consumption that keeps him on the mountain and ultimately makes him, from one point of view, just like the rest of the patients from whom, even at the end of the book, we will still be trying to distinguish him. (Nehamas 2000, 25)

For Nehamas, this will turn out to be the function or hermeneutic efficacy of prejudice and self-pleasing conviction on the part of Castorp and the reader. As we attend to Hans trying to deceive himself about his neighbours at the outset of the novel, Nehamas explains that we manage to fail to read, an ironic failure that Thomas Mann inaugurates for us and this is part of his writerly achievement:

we disregard his much more successful disregard of his tubercular symptoms. Our ignorance regarding Hans’s illness is also ignorance regarding ourselves as well. In depicting self-deception in his character, Mann induces it in the reader. (Nehamas 2000, 25)

It is not an accident that Mann’s theme is tuberculosis and as Fleck argues, and just where tuberculosis has a certain romantic appeal, syphilis horrifies us, not less because of the associations with venereal activity and with the blood (add to this the question of decadence in generation, congenital syphilis, and the pattern is complete). Yet for Fleck and qua diseases, tuberculosis and syphilis are related, not least to the extent that just as we can ask *what is syphilis?*, it also turns out that we can ask *what is tuberculosis?* And indeed *what is AIDS?*

To use a contemporary example, we might undertake ask about yet another complex and thus disputed disease (not only AIDS is disputed), controversial to the extent that diagnosis is problematic and treatment even more so, *what is Lyme disease?* Thus Lynn Margulis, who made the signal discovery of the exogenetic origination of

intracellular mitochondria, uses a parallel with Lyme disease to invoke the famous case of Nietzsche's supposed syphilis in order to draw attention to her own argument that syphilis has a variety of manifestations, seemingly replicating some of the complicated turns taken by the argument Fleck made for his own part in *The Genesis and Development of a Scientific Fact* (in Margulis et al. 2009). Evidently, and typically enough, Margulis who was seemingly (like so many we have cited) unaware of Fleck's parallel research just indeed and to be sure as she was unfamiliar with the range of Nietzsche scholarship and not just popular accounts on the related question of Nietzsche's syphilis.<sup>28</sup> The spirochaete itself, and Margulis's work on mitochondria makes her knowledge here very relevant, is an intrinsically compound and pleonastic entity, changing morphologically over time and in response to its environment. This same mutability makes diagnosis and treatment elusive, simply because the bacteria can change form, even escaping detection. Thus late-stage manifestation of both syphilis and Lyme disease, as Margulis observes, tend to have dramatically devastating effects, while taking decades to develop, i.e., what is effectively a lifetime and producing symptoms in their end-stages, e.g., arthritis and muscular and mental enfeeblement, that are hard to distinguish from the simple effects of age, and, to compound matters, and here there is a parallel to tuberculosis, even when properly diagnosed, difficult to treat, much less to cure.

Tuberculosis may be regarded, from an epidemiological perspective as a fairly classical public health problem, one that can be solved in terms of morbidity (and apparently on every level) by public health measures alone, meaning, better food, better sanitation (i.e., fresh air and clean water), details that also have to do with living conditions and quality of life, as Lewontin is at pains to point out, means that other factors are involved with morbidity and these other factors are elements, causal if one wishes to consider the fatality of disease, that are not counted into our assessment of the cause of a disease. As Lewontin writes:

The death rates from the major killers like bronchitis, pneumonia, and tuberculosis fell rather regularly during the nineteenth century, with no obvious cause. There was no observable effect on the death rate after the germ theory of disease was announced in 1876 by Robert Koch. The death rate from these infectious diseases simply continued to decline as if Koch had never lived. By the time chemical therapy was introduced for tuberculosis in the earlier part of this century, more than 90 percent of the decrease in the death rate from that disease had already occurred. (Lewontin 1991, 44)

I repeat Lewontin's first sentence as it is key: 'The death rates from the major killers like bronchitis, pneumonia, and tuberculosis fell rather regularly during the nineteenth century, with no obvious cause.' The point has to do with considering what we learn from reviewing public health records or statistics. Overall, and absent any medical treatment and even the research conditions for the possibility of medical treatment, death rates as such simply declined. Lewontin explicates this point further taking up the broad issues of disease vulnerability having to do with stress and nutrition and a host of other factors. Note that it is not Lewontin's concern to provide support for anti-vaccination movements (the folks worried that thimerosal preservative and other incidental ingredients in a vaccine may not be good for a child's health and who caution against vaccination for this and other reasons) and he begins his remarks by citing the efficacy of vaccination but goes on to point out that this is hardly as a result of or proving the success of vaccination, his point is rather the general attenuation of the morbidity of the disease as such:

At present, Canadian and American children do not often get measles—because they are vaccinated against it, but a generation ago every schoolchild had measles, yet death from measles was extremely rare. In the nineteenth century, measles was the major killer of young children, and in many African countries today it remains the highest cause of death among children. Measles is a disease that everyone used to contract, for which there is no known cure or medical treatment, and which simply stopped being fatal to children in advanced countries. (Lewontin 1991, 44)

For Lewontin, there is no question but that vaccination 'works'. His point is much rather the more complex concern regarding the virulence of the disease in question. Hence his point is not an autobiographical one when he reflects that as recently as his own childhood, measles was not a death sentence but much rather than measles it was exactly such a death sentence in the 19th century much as it continues to be a killer in non-Western countries. The point Lewontin is making is immunological and complex but no less precise for all that. As he writes, 'there have been complex social changes, resulting in increases in the real earnings of the great mass of people, reflected in part in their far better nutrition, that really lie at the basis of our increased longevity and our decreased death rate from infectious disease.' (Lewontin 1991, 45).

The desire manifest in medical science to be able to claim the credit for this lowered death rate is understandable but it is simply, and this *is* Lewontin's contention, simply inaccurate: the statistics don't support it. As an evolutionary geneticist, Lewontin's concern here is against those who hold with certain genetic causes for

cancer and the like. Part of the reason Lewontin opposes this is because as he knows as geneticist would the difference between a phenotype and a genotype and knowing the difference should alert one already to the complex of factors that influence health and disease, that is indeed as the word phenotype already tells you, how the organism appears, its aspect, its look. Our tendency to privilege the gene is for Lewontin a manifestation of today's dominant 'ideology', as he puts it, the 'transfer of causal power from social relations into inanimate agents' (Lewontin 1991, 46) and his point is that it doesn't matter whether the agent is a bacterial, viral or indeed genetic. For Lewontin, as for Fleck, 'social causes are not in the ambit of biological science, so medical students continue to be taught that the cause of tuberculosis is a bacillus' (Lewontin 1991, 45).

For Lewontin and the point in question goes to the massive amounts of money channelled in this direction, 'We are assured that if we could only find those genes that underlie alcoholism or the genes that have gone awry when we get cancer, then our problems will be over.' (Lewontin 1991, 46). For Lewontin, this conviction ignores the complex role of money and other advantages that belong in the study of society so often divorced from medical research and that other scholars (not Lewontin) have also observed to play role in directing the very same concerns and enthusiasms of medication research (this applies to pharmaceuticals, and Szasz again is unparalleled for his honesty in this regard, but of course Lewontin is responding to the genome project and we may add today's big ticket items, money wise, when it comes to nanotech, stem cell research, and of course the new biotechnology of robotics, to be distinguished from the old, cybernetics, etc.)

Where Lewontin also agrees with Fleck is in the ideological force attributed to hostile agency. And it is with this point that I return here to the reading of Fleck and his the demonic characterization of bacterial (and we may add viral) infection. War fits the conflict notion of scientific progress, a certain myth in which we remain pleased to believe to this day.

Speaking of syphilis, to summarize here, the problem for Fleck was to trace its genesis altogether—to track and to follow its history and for him the problem there is that we project the contemporary notion of the disease back into the past—a bad faith project in any case, just assuming that science might continue to progress and therefore assuming that the contemporary notion might itself turn out to be erroneous or incomplete or otherwise limited. But Fleck, writing as a physician, was also concerned to explore just how a disease that changed its diagnostic profile, as syphilis changes its presentation from a chancre—that would be the diagnostic turf or disciplinary field of the clinical dermatologist—to a disease of the blood in its putative secondary presentation—his would be Fleck's home turf of serology—to an infection of spinal and cerebral fluid that affects the brain (and of course this involves yet another set of

medical specializations and concerns). As Stig Brorson glosses Fleck's analysis of the achievement of a serological test for syphilis, such a test as the Wassermann test translates 'an ancient proto-idea of syphilis as a change in the blood (*alteratio sanguinis luetica*)' (Brorson 2006, 67).

The point for Fleck, in the prior listing of dermatology, serology, and neurology is that assumptions to such different thought collectives differ in their turn, thus a disease that crosses these disciplinary specialties can be especially pernicious inasmuch as medicine, guild-like as it remains in its professional formation, can tend to be especially blind to such things. As a pioneer in leukergy, i.e., as a specialist in the cellular mechanics of immunology, it would do, so Fleck would ultimately argue, to be quite astounded that any fact at all, but especially such as syphilis and any diagnosis of the same, would be able to come forth as a fact at all. For Fleck:

A historical connection thus arises between thought styles. In the development of ideas, primitive pre-ideas often lead continuously to modern scientific concepts. Because such ideational developments form multiple ties with one another and are always related to the entire fund of knowledge of the thought collective, their actual expression in each particular case receives the imprint of uniqueness characteristic of a historic event. (Fleck 1979, 100)

The last thing that Fleck is saying is that there is no truth (and to be sure and rather like Feyerabend, Fleck was no reader of Nietzsche and consequently thought little of him). Much more, Fleck's claim was a sociologically and historically informed ontico-epistemological claim. For Fleck,

Truth is not 'relative' and certainly not 'subjective' in the popular sense of the word. It is always, or almost 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, stylized thought constraint. (Fleck 1979, 100)

Apart from the astonishingly Heideggerian force, very akin to the moves Heidegger makes in *Being and Time* speaking about Newton and truth, of saying that truth is '(1)

in historical perspective, an event in the history of thought' and '(2) in its contemporary context', a 'stylized thought constraint', Fleck is concerned with metaphor, which as we noted at the start, takes us to the necessity for a hermeneutic, if and to be sure a hermeneutic phenomenology.

Thus, hermeneutically speaking, we may note that the term 'relative', as Fleck uses the term, is not to be conflated with relativism as such. Hence when Fleck contends that a fact, qua empirical, is 'relative' he means that it is contingent or subject to changing conditions rather than fixed or absolute.

To these epistemologists trained in the natural sciences, for instance, the so-called Vienna Circle including Schlick, Carnap, and others, human thinking—construed as an ideal, or thinking as it should be—is something fixed and absolute. An empirical fact, on the other hand, is relative. Conversely, the philosophers previously mentioned with a background in the humanities construe facts as something fixed and human thought as relative. It is characteristic that both parties relegate that which is fixed to the region with which they are unfamiliar. (Fleck 1979, 50)

Hence it is that within this framework what counts as syphilis as Fleck analyses it will undergo transformations:

The concept of a disease entity of 'certain micromycetes' is an example, as are the pure culture and the connection between disease and microbes. It is as if their 'consistent' application alone led to the discovery and no other concepts were possible. Truth is thus made into an objectively existing quality. Scientists are accordingly divided into two classes; the 'bad guys', who miss the truth, and the 'good guys', who find it. This valuation, which is a general trait of exoteric thinking, was also created by the demands of the intracollective communication of thought and subsequently reacts upon expert knowledge. (Fleck 1979, 116)

I think it goes without saying (as Margulis et al. 2009 imply) that the same claim might be made for AIDS research (connecting the dots which would allow me to retrace the steps vis-à-vis homeopathy could be done, but I bracket this here, as I have all along been bracketing cold fusion). I also think one can do the same with what we might call science and pseudoscience, philosophy and pseudophilosophy, and so on.

When Laudan argues that 'we ought to drop terms like "pseudo-science" and "unscientific" from our vocabulary' (Laudan 1983, 125), part of his point—so I propose

to read him hermeneutically with a good admixture of Nietzsche but also with a sensitivity to the history of science—is that in garden-variety ‘fact’ it can well turn out that we may learn, that we may make ‘progress’, which is to say, seen from this perspective, that the way that we currently fight disease may well from a future perspective turn out to be as wrong-headed or as primitive as the way we currently define it in terms of our positive and negative tests. Child of the enlightenment and of the scientific legacy of the 19th century as I am, no less than was Nietzsche and student of the same legacy as it continues into the 20<sup>th</sup> century with Fleck and Feyerabend, I still continue to hope for the possibility of ‘progress’, that can only be a self-critical style of scientific thinking that is for me another word for questioning in philosophy and the condition for the possibility of revolutionary discovery in science.

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## Notes

<sup>1</sup> There are a number of reasons for this, mostly historical but perhaps even more so, political reasons, including, most traumatically, the anti-Semitism that prevented his recognition in his early life and the personal suffering he endured during the war (forced to work for the Germans first in Lemberg thence to Auschwitz *and* Buchenwald), he luckily survived, but further historical (and political) reasons that contributed to a lack of recognition after the war, a lack that persists to this day. See for some biographical accounts, Cohen and Schnelle (1986) and, to be sure, almost every general article written on Fleck.

<sup>2</sup> See for example, McCullough (1981); Toulmin (1986); Löwy (1988); Van den Belt and Gremmen (1990); Wettersten (1991). And see more recently Hedfors (2007) as well as, for a counterpoint, the joint commentary offered by Amsterdamska et al. (2008). And see too my own discussion, Babich (2003b), as well as Brorson and Andersen (2001) and Koterski (2002).

<sup>3</sup> For Fleck's own account: see Fleck (1956) and, e.g. (in English), Fleck and Lill-Szyszkowicz (1956). The biochemist, Lothar Jaenicke's (2007) commemorative reflections on Fleck include a review of leukergy under the useful subtitle "Eine Tatsache ist tatsächlich keine Tatsache, sondern eine Gruppentat." [*A fact is in fact no fact but a group achievement*]. Jaenicke refers further to the cytologist Thomas Cremer (1985), noting his positive reception of Fleck's research.

<sup>4</sup> On Fleck's scientific achievements and reception, see the essay written by the medical research scientists, Sak and Pawlikowski (2012).

<sup>5</sup> Thus Carifio and Perla (2013) who, from a cognitive psychology perspective, also contend that Fleck's 'key concepts' accord with 'modern information processing models and views', and although rightly connecting Fleck with Reichenbach and Vygotsky, also characterize Fleck's epistemology as 'relativistic', a characterization that for their part also represents (this may be news to some philosophers) the increasing mainstream in the philosophy of science: 'Fleck's relativistic epistemology can clearly be viewed as foundational to the contemporary philosophy of science perspective.' Without disputing this in the least, it may be better to read Fleck's claims in the continental tradition that is associated with 'objectivity'. See on this with specific reference to Heisenberg, Heelan (1965) and Rheinberger (2010) among others with specific respect to Fleck.

<sup>6</sup> See Latour (2004)—a mainstream (if only or simply qua Stanford/Harvard) lecture instructively published, alas, in a *non-philosophy* journal. The last, quasi-Bourdieu-style

observation about the locus of publication (Latour has his own Bourdieu-style allusion when he describes himself and others as so many ‘good critics trained in the good schools’, Latour 2004, 242) accords with Latour’s concluding remark: ‘Critical theory died away long ago’ (Latour 2004, 248). While I certainly concur with Latour (always, always, I do), I would also point out, as Latour himself does not do, good academic that he was and remains, that this is largely because the professors who held posts in Frankfurt for the last 45 years did not bother to sustain critical theory, indeed actively eschewed, as did those employed at Harvard (or Stanford or Yale). I discuss some of this, including the *Social Text* set up and the science wars, in a contribution to the *Festschrift* (Babich 2002) that I edited in honour of Patrick Heelan as well as Babich (2003b). Here it is worth repeating that when I first wrote on the Sokal hoax in 1996, not one journal editor turned out to be willing to publish *any* critique of Sokal’s be it mine or anyone else’s (as it turns out) cooked or hoaxed—the editors of *Social Text* were not hoodwinked but to the contrary exactly complicit: see Babich 2003b, 101–102) hoax. My own essay on Sokal (and to date this remains the sole contravening reading of the Sokal hoax) may be seen in a reduced version in *Telos* (Babich 1996). The full version appeared in the Fall of 1997 and I am grateful to the editor of *Common Knowledge*, Jeffrey Perl, who, as he wrote to me to say, organized a special section of the journaling around my essay, featuring it as the lead (Babich 1997) and including Richard Rorty on Kuhn (Rorty 1997) and culminating with Feyerabend’s ‘It’s Not Easy to Exorcise Ghosts’ (Feyerabend 1997). Instructively, neither essays has been engaged, critically or otherwise in the literature that counts as the Sokal debate in philosophy and philosophy of science and it rarely if ever appears in bibliographies. This is, I think, not simply because I offer a continental take on the question, offering an explicitly hermeneutic reading of his pretended ‘hermeneutic’ but much rather because I take a critical approach to Sokal’s as such. For another insider’s, i.e., Duke University-style, discussion of the Sokal hoax (and Latour with a bit on Fleck), see Smith (2006). See on the very possibility of a critical philosophy of science, my more recent reflections, Babich (2010).

<sup>7</sup> I have good company here. See some of the notes to follow, and in particular Heelan (1986) among other contributors to Cohen and Schnelle (1986) as well as, more recently, Rheinberger (2010), 27ff as well as Fagan (2009).

<sup>8</sup> This is either an unremarkable claim (for some scholars, especially Fleck scholars) or it is contested as Kuhn himself appears to do, which contestation is recurs in the judgement of Kuhn scholars.

<sup>9</sup> Steve Fuller’s (2001) biography draws on and develops Kuhn’s remarks here.

<sup>10</sup> This is to be sure a relative matter but Terence Blake, an independent scholar (and very engaged and prolific blogger), sought to take Latour (2012) at his word when he invited scholars to ‘contribute’ to what appeared to be a literal enough knowledge project, the so-called ‘AIME Research Group: Inquiries into Modes of Existence’, but was dismayed to find his critiques unanswered. Most of Blake’s discussion of this appeared via blog posts, such as Blake (2014).

<sup>11</sup> See too on the amusing dimension of this critique, for those with German humour sensibilities, Kaplan (1991). Similarly German-minded in spirit, but by no means a joke, it corresponds to be sure to Fort’s attested literary style, is the attributed ‘translated from the Fortean’ (Kaplan 1991, 3) An English version exists, of course: Kaplan (1993).

<sup>12</sup> For his own part, Fort spent his life linking the politics and the practices of the two establishments. For one contemporary discussion of one aspect of this theme in science, to be sure lacking Fort’s prose stylistics, see the contemporary journalist on science, money and politics, Greenberg (2001) and see too Oreskes and Conway (2010). In additionally from the side of practicing scientists themselves, in this case, perhaps more significantly given the politicized issue of ‘climate science’ and including the politics of the academy, including the politics of journal publishing and supposed peer review along with other political interior to science itself, Orrin Pilkey’s chapter, co-authored with his daughter Linda Pilkey-Jarvis, on mathematical models (and the politics of the same) and his sole-authored chapter on shoreline erosion in Pilkey and Pilkey-Jarvis (2007), 22f and 92f respectively.

<sup>13</sup> It is important to note the continuing force of this chemical ‘challenge’, yet it is the distinction between physics and chemistry, and thereby the political order of rank between these two sciences that seems to have made all the political, theoretical difference for the scientific estimation and investigation of the first reports of cold fusion inasmuch as these reports were made by scientists who happened to be not physicists but chemists. Significantly, mainstream philosophy of science continues to regard cold fusion as an example either of pseudoscience or else as straightforward fraud. But see Biberian (2007) for an overview of the state of on-going contemporary research.

<sup>14</sup> See most recently, Grandjean and Landrigan (2014) as well as, among many, many others Fagin (2008) and Lu et al. (2000).

<sup>15</sup> In his preface, Kalichman seems to repeat his intentions to follow what would seem to have been an external reviewer’s advice (it could not be claimed that he succeeds): ‘I have tried to remain objective and balanced in my examination of what the denialists are saying and who they are. Difficult as it may be, I have tried to take these guys

seriously, even if not what they are saying then why they are saying it. I have also tried to avoid ad homonym attacks by focusing on what the denialists are saying than who they are. But that too was difficult.’ (Kalichman 2009, xv).

<sup>16</sup> Duesberg (1995) and see on Duesberg himself, see the survey essay by Cohen (1994) and the biographical account by Bialy (2004).

<sup>17</sup> Note that Trenn remarks that Kuhn encountered Fleck ‘about 1950’, citing Kuhn as writing that Fleck ‘anticipates many of my own ideas’, suggesting that Fleck was at least ‘partially instrumental’ in making him realize the relevance of ‘the sociology of the scientific community’ (Trenn 1981, 238). But as Trenn also remarks, ‘it is doubtful if Kuhn was prepared to adopt Fleck’s viewpoint in depth, for nowhere in his writings does he incorporate Fleck’s basic distinction between thought style and its carrier—the thought collective. Kuhn has come to see that his multifaceted use of the term “paradigm” has been a source of “confusion” ... and this could have been avoided had he observed Fleck’s restricted use of “paradigm” exclusively for exemplars.’ (Trenn 1981, 238). See on thoughtstyle and paradigm with further references to the literature on its many discontents, Babich (2003a).

<sup>18</sup> It always is, as Gerber (1885) argued in his studies of language and epistemology. In addition to Nietzsche’s own repetition/reception of Gerber’s reading of metaphor and knowledge in his unpublished notes, *Wahrheit und Lüge im aussermoralischen Sinne*. I discuss this in several places, see for example, Babich (2004), esp. 133–134 and see, too, Gustafson’s doctoral dissertation (Gustafson 1982) for studies that are more methodologically philological than Lakoff’s likewise valuable rhetorically and politically attuned studies (see in particular Lakoff and Johnson 2003).

<sup>19</sup> Diogenes Laertius on Diogenes the Cynic, book VI, ch. 2, I.20, see Diogenes Laertius ([1925] 2005), 22–23.

<sup>20</sup> According to several reports, Niels Bohr was famously asked why he, a man of science, would keep a superstitious marker above his barn door: surely, the incredulous question came, he did not believe in such things. To which Bohr replied that he understood that the good luck of the charm worked whether he believed in it or not. See one account of this anecdote in Pais (1988), 210.

<sup>21</sup> Tracy Strong has my thanks for calling this exchange to my attention.

<sup>22</sup> See for a more detailed discussion of Lucien Lévy-Bruhl and Émile Durkheim, Robin Horton’s chapter, ‘Lévy-Bruhl, Durkheim and the Scientific Revolution’, in his book, Horton (1993), 63–104 and see, more recently, specifically on Fleck and Lévy-Bruhl, Werner (2014).

<sup>23</sup> See Fleck (1979), 139. Even Darwin uses a version of this germ schema as he did not know (of course) the genetic basis of heredity, so he worked with a germination schema of so-called gemmas. For further references, see Babich (2010) and see, for specific discussion of Fleck, Brorson (2006).

<sup>24</sup> For one anecdotal example, close to Fleck scholarship, when fellow philosopher of science, Patrick Heelan left Ireland in the early 1940s for the US to take up a fellowship for his PhD in physics at the University of St Louis, an x-ray required for a visa revealed that he had had tuberculosis when he was younger, manifestly recovered as a young adult, he remembered no earlier diagnosis, and experienced, to this date, no ill effects.

<sup>25</sup> Gradmann (1930), 641 as cited in Fleck (1979), 174–175.

<sup>26</sup> Latour and Woolgar's *Laboratory Life* (Latour and Woolgar 1986) is classic here, a tragic example of failing to attend to the complex politics of the laboratory and also jealousy in science may be found (not that Sapp endorses this reading, to the mainstream contrary) in Sapp's account of Franz Moewus in *Where the Truth Lies*. I offer further examples, including further literature, in Babich (2010).

<sup>27</sup> Hacking (1983). See on the relation between Fleck and Hacking, Stump (1988).

<sup>28</sup> This fact may surprise future historians just because Margulis spent her career working in the biological sciences at the very same university—Boston University—that was historically so influential in foregrounding Fleck's work in the history and philosophy of science in Cohen's and Schelle's *Cognition and Fact* inasmuch as Bob Cohen was a long-time and influential director of the Center for Philosophy and History of Science at that same university. But scholars and scientists can be innocently insular and Margulis was also limited to popular knowledge in her familiarity with the debate on Nietzsche's syphilis, which she simply took to be a fact. On the last see Schain (2001) and further Babich (2011–2012), 114.