

BABETTE BABICH

THE FORTUNES OF INCOMMENSURABILITY

Thoughtstyles, Paradigms, and Patrick A. Heelan's Hermeneutic of Science

PATRICK A. HEELAN, S.J. — PHYSICIST, PRIEST, PHILOSOPHER

Patrick Aidan Heelan was born, very romantically, in the *1066 and All That* sense of being romantically born, on St. Patrick's Day in Dublin in 1926, as the second son of a mathematically gifted Flemish mother, Pauline Beirens, who had been sent from her native Antwerp to a convent school in Ireland, where she met eventually met Matthew Henry Heelan, who, in addition to holding all the posts that are usually all we are told of a father's life, also had gifts that left a lasting impression on his family, including a passion for music, for sailing, and for roses, and who, together with his wife, raised his family, two sons, Louis and Patrick, and a daughter, Esther, where they all grew up in a small stone house that stood on a parcel of land that ran directly to the sea, near Sandycove, in full view of the Martello Tower, not too far in space or in time from the Dublin we tend best to know from James Joyce.

Heelan, who was to become a Jesuit earlier than some boys begin to shave, began his studies at Belvedere College and, at University College, Dublin, took courses with Erwin Schrödinger and John Synge at the Institute for Advanced Studies in Dublin. A traveling studentship would take him from Ireland to St. Louis for a first doctorate in physics and then to a stint at the School of Cosmic Physics of the Institute for Advanced Studies in Dublin along with studies for a Licentiate in Theology followed by a two-year post doc in high energy physics at Princeton, for what was for him a very influential meeting with Eugene Wigner, where he also began his association with the physics department of Fordham University at Rose Hill in the Bronx. He then taught physics and cosmology at University College Dublin, did a second doctoral degree in philosophy at Louvain in Belgium, now: Leuven, writing his first book on Husserl and Heisenberg, *Quantum Mechanics and Objectivity* (1963). He took the opportunity to return to New York's Fordham University, with a year spent as a visiting Professor in Physics at Boston University along the way, and then, in 1970, he was invited to chair the department of philosophy at the State University of New York at Stony Brook, directly overseeing its development into a leading center for continental philosophy, he took on administrative responsibilities as Vice-President for Liberal Studies, taught a certain ex-biology student one crucial course in the philosophy of science and began writing a book on *Space-Perception and the Philosophy of Science* (published in 1983)

B. E. Babich (ed.), Philosophy of Science, Van Gogh's Eyes, and God: Hermeneutic Essays in Honor of Patrick A. Heelan, S.J., 1–18.

© 2001 Kluwer Academic Publishers. Printed in the Netherlands.

– a book that was to have surprising influence in fields such as architectural design, cognitive and experimental psychology and even exo-biological research but, and this is regrettably typical, much less than its share of influence in its own focused reference to the philosophy of science. In 1983, Heelan was a Senior Fellow at the University of Pittsburgh’s Center for the Philosophy of Science. Ten years later he returned to university administration as Dean of Humanities and Fine Arts at Stony Brook. And in 1992, moved on to still more academic administrative tasks as Executive Vice-President for the Main Campus of Georgetown University. He is currently the William A. Gaston Professor of Philosophy at Georgetown University.

These are the academic milestones and accomplishments of Patrick Heelan’s life, leading to a listing in the *Encyclopédie Philosophique Universelle* – (as *Philosophe irlandais*), here translated from the French as part of the preface to this collection. But, like the above, such a prestigious account is far too spare. It fails to convey his intelligence, or his wit and Irish humor, as it has amused – and more than occasionally also disconcerted – his American colleagues. Nor can such a complex personality and the range of such a life be communicated here, to do that one needs to tell a real story, with all the resources of Ireland’s fiction. If I lack the skills to tell such a story, I know at least that it should be told. And in the “Afterword,” we shall indeed hear the tale from Heelan himself.

SCIENCE, ERROR, AND HERMENEUTICS

Ludwik Fleck, a Pole from the quintessentially rabbinical town of Lemberg (Lvov) could hardly be further from Patrick Heelan – or his Irish, Catholic Dublin roots. But they share more than one thing in common and both would endure a less than effective influence due to resistance to the associative resonances of the language they used and the very conceptual and, at the same time, very political, fortunes of incommensurability.

If Patrick Heelan invoked the phenomenological resources of Edmund Husserl in order to explore Heisenberg’s quantum mechanics, such a reference together with its associated language or conceptual terminology could not but clash with the then-contemporary scholarship (authoritative references and conceptual schemata) of the philosophy of science, which had already (as mainstream analytic philosophy in general had done) relegated Husserl to a lesser post in an hierarchic philosophical scale of clarity or fruitful philosophic expression leading not to cognizing quantum mechanics but merely to a place alongside Frege, as author of a *Logical Investigations* somewhere to be ranged behind Wittgenstein’s canonic title. It was from within the climate of analytic philosophy, the same climate that has not wavered in its dominance of professional philosophy, that Heelan first articulated his conceptual vocabulary, with talk of so many “Worlds” (of the Sportsman, of the Husband, of the physical scientist),¹ in order to express what he then called an “horizontal analysis.”² For Heelan, to talk of horizons and world, particularly the Husserlian “Life-World” enabled an approach to the paradoxical question of nothing less than the objectivity of quantum mechanics, permitting “an analysis of the intentionality structure of quantum physics”³ and including the bugbears *indeterminacy* and *complementarity* – in terms of Husserl’s correlated conception of the noetic structure of knowing. Quantum mechanics could thus be expressed as “the formal material theory whose function is to describe a World-

for-things, and the experimental observational and operational part which makes the World-for-things also a World-for-us.”⁴ For Heelan, such a twofold phenomenological *and* hermeneutic reading of the structures of objectivity internal to quantum mechanics both in theoretical and in practical expression meant that the life-world did not come to an abrupt halt at the laboratory door.⁵

Heelan’s focus on instrumentally (and thereby significantly) mediated perception provides the key to his insight into Heisenberg’s epistemology at the level of the Indeterminacy Principle, a principle which takes as its point of departure the observation that the “act of measurement,”⁶ as the critical micro-activity of physical science, “perturbs the object” of scientific inquiry, “which yields the well-known result that the object... can be known neither empirically nor formally.”⁷ Heelan’s study of complementarity, analysing the intentionality structure of quantum mechanics, argued that measurement as such includes perturbation as a complementary component of observation because “the measuring process” rather than being objectively extrinsic to the object measured is intrinsic or “essential to the definition of a physical property.”⁸ From a literally phenomenological point of view then, “the activities which take place between object and instrument in the measuring process serve no other function than to render some physical system or some property of it, accessible to a human observer by magnifying it or otherwise ‘translating’ it into a form in which it can produce a perceptible impression on a human observer.”⁹

Thus Heelan was able to argue that “quantum mechanics shares to the full the public objectivity of science.”¹⁰ In consequence, he could also argue that an expressly phenomenological or horizontal analysis of quantum mechanics was indispensable for an adequate quantum mechanical theory of knowledge,¹¹ as Werner Heisenberg’s own definition emphasises: “quantum mechanics is a science of immanent acts and objects,” describing not “nature but our knowledge of nature.”¹² Hence, for Heisenberg, “there is no place for an objective (i.e., physically objective) science of microphysical objects, except as a science of *how we know* and not of *what we know*.”¹³

In Husserlian terms, the physicist appropriates or takes over the *intentional* perspective of the instrument itself. For the scientist in this intentional relationship, a “measuring instrument” would not then itself be an isolated thing but an extension of the scientist’s intentional orientation in which the instrument “played the part of an ‘observing instrument’ which ‘felt’ and ‘observed’ reality and ‘spoke’ of its experiences to the scientist through the ‘language’ of observable physical symbols; for the new science consciously took the point of view of an instrument immersed in nature.”¹⁴ The whole of Heelan’s later concern with readable technologies is thus pre-articulated in this context which is importantly as much hermeneutic as it is phenomenological: “The measured property produces a macroscopic effect in the instrument; as for example, a pointer reading on a scale, a ‘click’ of a counter, or a track in a bubble chamber. This macroscopic effect is a material sign. A sign has a double reality: its mental reality as a pointer, sound or bubble track, and an intentional reality proper to it as a sign.”¹⁵

Heelan invoked N. R. Hanson’s “patternings” of discovery in terms of the multifariously (depending on the research context in question) “dressed” *world* of the research scientist for whom a laboratory or research center is differently experienced than it can be for a journalist or a student, or the cleaning staff (just as the man who operates a massive backhoe on an urban construction site experiences or “lives” the heavy machinery he controls as well as the site itself as a discrete world – here

understood in contrast with those passers by, who, caught up in their own “worlds,” as we say, give neither machine operator nor construction site a second glance).

Heelan’s application of Husserl’s philosophy to an expression of Heisenberg’s physics, *qua* physical philosophy illustrates the indispensable force of both a phenomenological analysis and a hermeneutics of the same objective recourse, expressed in exact opposition to the epistemological failure of the one-to-one correspondence language schematism required by logical positivism.¹⁶ For Heelan, a phenomenological analysis in the theoretical context of quantum mechanics, necessarily presupposes an hermeneutic account. And in this latter context, Heelan’s hermeneutics of scientific practice requires less the resources of pragmatism than the critical sophistication (*pace* Robert Neville below) of phenomenology. To give it a Kantian stamp, a phenomenological analysis of science without hermeneutics is blind but a hermeneutic philosophy of science without phenomenology is empty.

In his earlier and later books, the phenomenological component is key to Heelan’s thinking. As Heelan expresses it, a “scientific observation” is necessarily technologically and theoretically mediated, which is to say that it is “accomplished with the aid of instruments.”¹⁷ Scientific observation is thus a matter of mapping out or “outfitting” an entire world, presupposing a trained conversancy with the report of the instrument, or the “readability” of the instrument-measurement-laboratory environment as such.¹⁸

It is tragic but all-too routine in the fortunes of the academy, i.e., the disciplinary project of the philosophy of science as a profession and as such, that for Patrick Heelan, as for Ludwik Fleck, the cognitive dissonance inevitably to be correlated with conceptual incommensurability had to make communication within the disciplinary confines and influence of the philosophy of science effectively impossible. Thus when Lawrence Sklar was invited to comment on *Space-Perception and the Philosophy of Science* at a book session on Heelan’s work at a meeting of the American Philosophical Association,¹⁹ presided over by Heelan’s fellow Irish countryman, Ernan McMullin, the commentary was never able to overcome the shock of this same conceptual dissonance to address the substance of the book charged for critical reception.²⁰ The different styles of thought between Heelan and ordinary emphases in the philosophy of science left Sklar no access to the thought at work in Heelan’s study. But the deficiencies of Sklar’s conceptual reference were derived from Sklar’s rather than Heelan’s hermeticism. Heelan had of course sought to address the reigning tradition or received modality of the philosophy of science in his book, which not only begins with a discussion of “Phenomenology, Hermeneutics, and the Philosophy of Science”²¹ but includes a very perfectly optimistic or “bridge-building” chapter on “Hermeneutics and the History of Science.”²² Heelan’s book recommended a course avoiding the twin dangers of traditional or analytic, received views in the philosophy of science as well as traditional or routinely continental accounts of hermeneutics, which continental side has for its part been burdened since Dilthey with an exactly impoverished view of the nature of the distinction between the human and the natural sciences.²³ This was the same distinction Heelan had earlier sought to bridge via the reconstitution of the fully-fledged, carefully noetic-noematic horizon called forth in the “World of the physical scientist” (a world excluding Dilthey’s famous opposition as much as the unilateral correspondence rule and scheme of ordinary philosophy of science not only of Bridgman and Campbell but also in the historical climate or episteme of the mid-sixties:

Niels Bohr, John von Neumann, Eugene Wigner, and so on). For Heelan, both continentally-minded scholars of hermeneutics²⁴ and traditional philosophy of science exemplify the still today persistent conviction that “hermeneutics has nothing whatsoever to do with natural science.”²⁵ The entrenched tradition in the philosophy of science which Heelan opposes to the more socially and contextually lived views advanced by Fleck and Thomas Kuhn remains to be addressed to a specifically normative and practical research program of problem solving as instigated by Karl Popper and continued in Larry Laudan and onwards in received or establishment philosophy of science today.²⁶

INCOMMENSURABILITY AND STYLIZED RESISTANCE

The positive achievement of the present volume redresses the conceptual incommensurability between Patrick Heelan’s contributions to the philosophy of science and traditionally analytic philosophy of science. Repatriating the term “hermeneutics” within the conceptual armature of the philosophy of science is accordingly to take a step *beyond* both analytic and continental perspectives in philosophy. And in what I believe to be a parallel illustration of incommensurability, in the case of Ludwik Fleck, rather than hermeneutics *per se*, as the explicit adoption of a method initially conceived with reference to (religious and thence to legal) texts, to be applied to the philosophic analysis of science (as ineliminably theoretical practice), the problem is the literal question of *style*: both his express invocation of styles of a thought interior to science and the great obstacle to its reception in traditional philosophy of science which was entailed by the tacit *style* of biological science and medical research illustrations.

Fleck’s conceptual terminology centers around thoughtstyle [*Denkstil*] and the related if even more elusive conception of a thought-collective [*Denk-Kollektiv*], particularly with regard to the latter’s emergent properties. The alien implications of Fleck’s notion of thought-communities (throughout history as well as in modern scientific research practice) challenge Western liberal ideas of individualism and freedom – nor is it an accident that these ideas are more and not less crucial to contemporary images of science in the West. In opposition to the contemporary Western cultural emphasis on the individual and its correlative emphasis on individual scientific genius, Fleck’s philosophic reflection on the collective working or dynamic function of science foregrounds the ineluctable dependence of the individual upon or within any given, historical, thought-collective. In his landmark 1947 essay, “To Look, To See, To Know,” Fleck mused that “scientists, most frequently individualists, do not want to see the collective nature of thinking. What would remain of their renowned genius?”²⁷ But for Fleck, the individual cannot escape/surpass the collective: the thought-collective of any era is the presumed, taken for granted, but above all, the precisely *unreflected* culture of that same era. Not a *consciously* received or dominant perspective of thought, the collective is the world assumed in advance of a particular research tradition. A given scientific thought-collective is the perspective within which what is scientifically conceived *can be* conceived as such.²⁸ As Fleck’s later editors Cohen and Schnelle explain, “the individual member of a collective is not free to be scientifically active otherwise than on the basis of his stylized socialization. His thought must start with the propositions created by the collective.”²⁹ In Fleck’s own words,

the ordinary scientist of the day finds that “scientific truth” is a complex mental construction, inseparably connected with investigative techniques, statistical interpretations and manifold conventions. He knows that it may often be expressed only in a special jargon and be intelligible only after a prolonged professional training. In his opinion, “scientific truth” depends conjuncture, i.e., on the scientific opportunity, on the environment and on the personal influence of the author.³⁰

Maintaining that scientific truth is ineluctably social, Fleck takes the disciplinary relevance of the sociology of knowledge to be no less central to an understanding of science than history itself.

Yet to advert to the sociology of science (in particular) and to the history of science³¹ (more broadly) is only to compound the troubles of incommensurability. Sociology of science or, more radically, sociology of knowledge, particularly natural science, is far from an entrenched, much less a secure, discipline *vis-à-vis* science (witness the recent fury of the science wars, fading because the more radical proponents of sociological, anthropological, rhetorical or cultural studies of science have been chastened and are in due retreat: rendering unto science what is science’s, on the precise terms dictated by fealty to the hierarchical ideal of science as such) or the philosophy of science. Nor was this conflict between world views (think of Dilthey or Mannheim) less inchoate in Fleck’s era.³² The contributions of both John Ziman and Rom Harré below testify to a renewed sensibility to the relevance to the sociological context of and for the lived and cultural practice of science. With specific reference to the philosophy of science itself, Ragnar Fjelland’s insightful reference to the Sokal hoax and its coordinate parallels with the elusive words of both Bohr and Heisenberg, and my own more directed essay on the same hoax emphasizes critical reflection in thinking about science as such.³³

Here, in the context of social theories of scientific culture, it is important to advert to the camp quality or fairy-tale oddness of the Sokal hoax as it is one that has yet to be regarded not as the kind of joke Sokal pretends it was – but as the still unexposed ruse that took, and continues to take, science far too seriously and far too uncritically.

Throughout the past century, both critics and proponents of science alike have been invited to swallow only what certified scientists endorse. *This is the joke* in the overawed response to the authority of science, at work in the original locus or butt of the joke played upon the editors of *Social Text*, just as it was a joke played on and among friends: and therefore a double hoax effected by means of an exact “in” on the game. It is a crucial piece of evidence (or gossip)³⁴ that Alan Sokal was a friend of *Social Text* editor and meteorological enthusiast, Andrew Ross, that Sokal met his current wife at a party at Ross’s home – and that exactly not nothing follows from that. Academics who know editors should know better than to think that any kind of vetting or review – blind or expert, humanist or scientist, or what have you – would or could be at work in such friendly cases. The hoax was an academic set-up.

An overawed confidence in the ultimacy of current (no less! *pace* Laudan) physical science is at work in Sokal’s own presumption (like that of his plainly more accomplished colleague Steven Weinberg)³⁵ regarding the finished adequacy of science as critically perfect and quite literally beyond critique. This totalising confidence in science has sometimes been saddled by critics with a suffix – calling it *scientism*³⁶ – to hold it at a critical distance from more nuanced views on science. But in the context of the philosophy or sociology of science, etc., a charge of *scientism* is about as much a reproof as deism in a religious community. The same overawed joke continues to work in the wake of tiresome commentary on the story and (surprise, surprise) no one who

writes on it can be found to say anything but “how wonderful”³⁷ it was just to think how Sokal rode into town to show up the postmoderns, the Derrideans, the feminists, the anthropologists and sociologists of science, the social constructivists – at which point, in a spontaneous reflex of reflexivity, certain scholars are then to be seen busily shoring up their own legacy by severing their own work from any even putatively or remotely science-critical perspective. I name here only Bruno Latour and Ian Hacking, where other names, are, despite themselves, lesser names.³⁸ The philosopher of science, one can only be led to believe, must eschew critique if he or she is to be taken as “knowing anything” about science. Thus, in the past, one has ruled out the philosophical perspective of a Nietzsche or a Heidegger or even the later Husserl. One cannot be, and one must affirm that one has never been, a member of any such critical party.

BETWEEN THOUGHTSTYLES AND PARADIGMS

We have noted the relevance of terminological incommensurabilities in the thinking of both Heelan and Fleck to indicate the conceptual difficulties presented for the tradition of the philosophy of science.

Just how egregious these difficulties were – and in large measure remain – is best seen by noting a striking conceptual limitation of the important collection edited by Cohen and Schnelle, *Cognition and Fact: Materials on Ludwik Fleck*.³⁹ This wonderful collection includes English translations of Fleck’s own essays dating from 1934-1960 as well as profiling leading scholarly responses to Fleck both in his own time and in recent times. However, not a single essay in this nonetheless pathbreaking collection shows any comprehension of the function of the biological dimension of science or medical metaphor for Fleck, as the very key to Fleck’s philosophical thought on science and scientific knowledge. Why is this and, more importantly, what follows as a result?

This limitation both ironically and naturally proves Fleck’s thesis, because, without a fundamental background in biology, the task of getting Fleck’s points proves impossible, even in a sympathetic collection dedicated to advancing his reception. The exception that might be thought to be found in the contributions by those scholars not only biologists or physicians but Poles themselves, repeats a generally critical perspective on Fleck and his medical limitations, attributing more sensitivity to the workings of the immune system and much more wholism to medical research practice than happens in fact to be the case, a deficiency evident given the AIDS crisis and betrayed by the medical profession’s oddly uncritical support of the healing promise of genetic re-engineering chances inspired by the mere idea of decoding the human genome – which same crisis confirms Fleck’s critical charge contra the conceptual deficiencies characteristic of modern medicine itself and its search for *singular* causal agency (i.e., HIV). That is: the reigning thoughtstyle (and collective rule) of biological as medical science is increasingly physical science, and even, in the case of genetics, information science. Fleck’s biology, by contrast, was the classic microbiology of the early twentieth century.

Fleck challenges the standard story of the triumphalist emergence of modern science as a break with the fearful, uncritical but above all *unobserving* medieval point of view as an eruption into modernity effected by the plain and simple means of observation, whereby the main empirical advance in modern science corresponds to simply taking a look (through the microscope or the telescope). This standard perspective was, for

Fleck absurd. One could not, “assert that a medieval scientist did not have any positive relationship to observation.”⁴⁰ Key for Fleck was the conceptual schema within which whatever was observed or noted would be able to be observed or seen:

Looking and seeing at that time differed from the present day, but it would be a sign of naïveté to think that a man of those days was asleep, and roused himself from his sleep only during the Renaissance. ... the 16th century could find bones in the neighbourhood of cemeteries and study them, but the Middle Ages simply lacked any intellectual need of such observations; when looking at a bone one could see only what one could find in books...⁴¹

For Fleck, those very books tell the story as much of scientific discoveries as of the ruling interests and the shifts in interests that accompany discoveries:

In 17th century anatomy books we find long chapters describing and enumerating the so-called *ossa sesamoidea* which are disposed of with a few sentences in today’s textbooks: they are currently, so to speak, outside the osseous system and little of interest from the ontogenetic, morphological, or physiological viewpoint. But at that time they were important because of certain old myths according to which from one of such bones there will develop “*sicut planta ex semine*” the complete body to appear at the Last Judgment.⁴²

For a further example, the 14th or 15th century anatomist or physician simply “knew” that the male of the human species lacked a rib. They did not need to, more decisively, they could not check for themselves to “see” that this was not so (nor, as beyond Fleck, Allan Debus and Peter Dear can explain, could simply pointing out such a “fact” have been able to alter such convictions).⁴³

Fleck’s point is that *contemporary* anatomical preoccupations are just as “incorrigible” as such putatively erroneous medieval views because our own contemporary views are as invisible as and as unquestionable to us as their own medieval preoccupations were to the anatomists of past times. We only see such preoccupations *as* preoccupations *because* they are not our own. Contrasting the modern with the medieval scholar, Fleck concludes that “what is of importance to us, is for him inessential, inexplicable, alien, just as, on the contrary, his own thinking is alien to us.”⁴⁴

Fleck was not a pioneer of social studies of knowledge or scientific culture *per se*. Such disciplinary approaches to science studies, including anthropology as well as sociology, tend to be epistemically uncritical, for they themselves are articulated within specific disciplinary or “scientific frames.” And Fleck’s concern, which is what renders his thinking proper to the philosophy of science, was addressed to what he called *epistemology*, which is why he ended up by elevating the promise (not the reality) of sociology to the height of a veritable mathematics for the sake of science.⁴⁵ But Fleck’s concern with epistemology was the question of what could be said to be known, specifically as articulated in the terms of science, corresponding to what is said to be true. This epistemological concern reflects the logical limits of scientific discovery (research) and scientific justification (theory and philosophy). By reflecting on the historical question of thoughtstyles and the relevance of precisely *collective* communities of thought and the emergent properties that characterize such styles of thought interior to such communities in specific cultural and historical context, Fleck sought to show that that same history and range of cultures adumbrated nothing less ambitious than the material dialectic of science in real and ineluctably communal research practice.⁴⁶

Famously, and this is why we are able, in the insular context of English language philosophy of science, to talk about Fleck at all, it was no one less important than Thomas Kuhn himself who read Fleck’s *The Genesis and Development of a Scientific*

Fact,⁴⁷ as part of his own scholarship and as part of the preparation for his own work on *The Structure of Scientific Revolutions*. Because, as Kuhn reports in retrospect, he only read German “badly,” he was also compelled, as he emphasizes, to read and re-read Fleck.⁴⁸ But more significant than such a painstaking reading is that Kuhn happened upon Fleck’s book in an intensifying constellation of ideological circumstances then rife in an era that today seems as distant as Fleck’s vanished world, as distant as Heelan’s own then-still-Joycean Dublin has come to be from today’s prosperous, cyberneticized, cosmopolitan Dublin, regarded from the new, more than postmodern, vantage of the twenty-first century.

Kuhn’s time spanned prewar and wartime history and his crucial and formative intellectual work found expression during the era of the postwar world known as the cold war. In America proper, the climate of the cold war reflected the still virulent McCarthyism dominating everything from art to criminal justice and including the academy – or Ivory Tower as it was then unquaintly regarded. If the McCarthy era in the land of the free and the brave was not Stalinism, it nevertheless had exactly repressive parallels. Much of the social and intellectual repression characterizing the 1950s and 1960s expressed this climate of fear. The change that began to take place in the late 1960s (really into the 1970s and, alas, coming there to an unsung and unmarked dead-end) saw a world change in fashion and life-styles, if *not* in politics. But if today, we no longer hear about the socialist or communist *threat* – to use the language of the cold war – that is not because we have become more nuanced about the psychoanalytic projection implicit in the language of a “communist threat” or about the multifarious and complex dimensionality of political forms like socialism or communism but rather and because like so many of the varied biological species that have become extinct in the long course of the last century, the *threat* of socialism/communism has been vanquished. The end of the cold war corresponds to the extirpation of the political regimes of socialism and communism itself, almost like the phantasm of democracy in the US.⁴⁹ But this does not quite mean that we’ve evolved beyond the inanity of McCarthyism.

I do not think it an exaggeration to suppose that, in the then political climate and despite the manifest relevance of Fleck’s study, Kuhn *could not* cite Fleck, *even had he wished to do so in fact*. And if, more radically, one supposes, as claimed above, that Kuhn’s “paradigm” is a periphrastic construction derived from Fleck’s *Denstil/Denk-Kollektiv*, this is exactly because, and on more than one level, only such a paraphrase was, *per impossibile*, possible.⁵⁰

The problem of citing Fleck for Kuhn had nothing to do with Fleck’s “Polish” German (this is a nonsensical claim as Steve Fuller advances it in his otherwise useful book on Kuhn⁵¹ – there is no ‘false friend,’ as language teachers say, to trip one up in the translation of *Denkstil* [thoughtstyle] or *Denk-kollektiv* [thought-collective]). Not a problem of translation but the political restrictions of his era (from the forties through the fifties and early sixties) entailed that Kuhn could not adequately cite Fleck. That is, Kuhn *could not have* used such dangerously loaded terms as “thought-collectives” – or “thoughtstyles” – in his 1962 book for the perfectly banal reasons we still attribute to and name “politics.” The language of collectives or thoughtstyles would have evoked precisely *reactive* reactions in a time of the paranoia and anxieties expressed in words like brainwashing, propaganda, the Iron Curtain and the Iron State, and the inscrutable evil of Eastern Europe, of Russia and (this is all that is left today:) China. And in 1976,

the later Kuhn, writing an introduction to the belated translation of Fleck's book, remained careful to underscore his personal, exactly gut distance from these very same terms. Even after so much time, one may hear the echo of Western anxiety *vis-à-vis* the image of the "collective" as the anti-individualist, veritably mindless, socialist "horde."

Nothing but the then-times themselves gave birth to the unhappy coinage of the term *paradigm* – among whatever other reasons there may also have been for Kuhn's labile word.⁵² This same historical echo reverberates through Kuhn's own revealingly over frank, at times uncomprehending, sometimes brittle autobiographical reflections. And in *The Road Since Structure*, in Kuhn's interview with Aristides Baltas, Kostas Gavroglu, Vassili Kindi, even the most hermeneutically impoverished reader must note the relevance of Kuhn's most repeated word "anger"⁵³ – articulating his actions in terms of a fundamental, choleric impatience. This reflexive reaction to his own academic legacy does not contradict as much as it complements the contextual circumstances (or very Fleckian "genesis") of Kuhn's book, as Fuller has outlined it in his own study of Kuhn and the development of Kuhn's own influence in the broader culture of the academy itself.⁵⁴

In this context it is worth recalling the anecdote Kuhn found significant enough to dedicate a great proportion of his own brief introduction to Fleck to retelling it. Kuhn reports that his own mentor, Harvard President James Bryant Conant, who became US High Commissioner for Germany, unhappily made conversational use of the German reference to the title of Fleck's book, as Kuhn had related his discovery of Fleck on enthusiastic occasion. For Conant, the borrowed reference backfired, as such borrowed references can do in trans-cultural contexts. Conant's German associate responded to the mention of the title of Fleck's work with a spontaneous denunciation of the concept as such: recoiling from its titular proposition in a bravely, determinedly naïve positivism that is the unchanged ideal of the philosophy of science, latterly called realism, then betrayed by the stolid conviction that the-facts-are-the-facts. By definition, as Conant's Teutonic interlocutor painstakingly instructed him, and hence *contra* the concept of *Entstehung* and *Entwicklung* – the one thing *Tatsachen* or "Facts," did not as such have was anything like a "genesis" and the last thing they are able to do is "develop." Facts are just "discovered" as what they plainly are. Sidestepping such debacles, Kuhn eschewed Fleck's terminology and spoke instead of paradigms and paradigm shifts, normal and revolutionary science.⁵⁵

But this genesis explains why Kuhn himself was never able adequately to specify the meaning of or to defend or even to understand or to accept a conceptual constellation he had only first discovered in Fleck.⁵⁶ It also explains the parallel resistance to these same ideas in the philosophy of science as such. As Lothar Schäfer reflects: "keeping Kuhn's thorough-going dependency on Fleck in mind, one must draw the obvious conclusion that the key presupposition for [the concept *per se* of] *revolution* in the philosophy of science has to be found in the *ahistorical* consciousness" of the philosophy of science.⁵⁷ Only a lack of historical background sophistication (what the Germans call *Wissenschaft* with respect to history itself) internal to the discipline of the history and philosophy of science could have permitted the enduring influence of the idea of revolution as a "fact" so very contrary to the complex dynamic of the empirical history of science.⁵⁸

ERROR AND SCIENTIFIC TRUTH

In the wake of Kuhn's *The Structure of Scientific Revolutions* – that is, internal to the hermeneutic history of its reception and effects – Steve Shapin can begin his small volume on *The Scientific Revolution* with the grand declaration that there was no scientific revolution. Shapin says this non-revolutionary thing for the same historical reasons that Fleck himself could have given where the history of science outlines neither unbroken scientific advances nor patent revolutions. Thus Fleck emphasized the precisely non-cumulative (non-linear) character of scientific progress in contrast to the cumulative character of past error.

For Fleck, as, more abstrusely, for Ernst Mach, *error* was not to be condemned to the dustbin of history because from the same historical perspective it is impossible to know exactly where true error lies or in what it consists or inheres. Nor can we be sure, indeed, that the greater error would not be found in our own incorrigibly *presentist* account of what we rudely and incorrigibly (currently) regard as past errors. For Fleck, as for Mach and for Nietzsche, it is as difficult to say what error is as it is to say what truth is – however much this important and radically post-Kantian corollary may be expected to dismay latter-day Popperians.⁵⁹ Our perception of error is precisely as context-dependent as our perception of scientific truth.

For both Fleck and Mach, as scientists, exactly as practitioners and for good reasons, designing research possibilities and perspectives, the so-called “errors” of the past represented veritable resources of future scientific truth. Past errors were regarded as scientific reserves, representing realms not to be forgotten.⁶⁰ By contrast with the ideal of timelessly routine presentism,⁶¹ which is the ideal of scientific progress, Fleck's historical reading of the history of scientific truth and the facts of the same offers a sustained reading of nothing less than the erring *truth* of, for example, astrology as such. For Fleck, astrological preoccupations and assumptions articulated nothing less indispensable than that very errant context without which the scientific concept of what we today regard as the disease entity,⁶² i.e., syphilis, as such, could not have come to stand: where a precise genesis and development was requisite for the very exactly scientific discovery of what we name the modern disease-entity as such.

In this same historical context, it is relevant that the contextual question is the one question that Barbara Saunders never thinks to consider in her fine contribution below, reviewing the historical and contemporary reception of Aristotle's account of the mirror-reddening gaze of the menstruating woman. The contextual primacy of error turns upon the cultural difference that ought perhaps to be made of the historically differential matter of fact that ancient mirrors are not made of glass but polished *metal*: bronze, copper, silver, etc. And all such metals and their alloys (even without attending to the sobering limits of contemporary understanding of ancient metallurgy) have the special characteristic that from the moment they are fashioned (and it is for this reason very relevant, albeit not to Saunders, that Aristotle duly specifies a brand new mirror) such metal mirrors oxidize. Newly fashioned (like newly polished) metal mirrors tarnish upon exposure to the ambient atmosphere, an atmosphere including not only factors of climate but also relevantly in the case of personal mirrors, the humidity of respiration and perspiration, and notably the acidity in human breath – that same acidity that undergoes circadian alterations in the course of the day as well as in response to hormonal changes, the same acidity that is so little a merely theoretical construct that

its effects have closed the caves at Lascaux to visitors in recent years, and so on. That is, Saunders does not, just as a good scientist would not, raise the question of the possible locus of truth in what we are (already) persuaded to have been error or, indeed, ancient Greek, male prejudice.

But Fleck argues differently. Key for the development of the modern scientific conceptual understanding of syphilis were, for Fleck, conceptions of that disease now regarded as mistaken. These constructs, different historical schemas, work in a non-linear development of the changing scientific accounts we have of syphilis, as a precisely pleonastic disease, including a history of but not limited to therapeutic measures, an understanding of the progressive character of the disease, and advancing an ongoing inquiry into the causes of the disease. Even after Fleck, even after the decisive challenges to the agent theory of disease, we continue to identify (mistakenly on Fleck's account) syphilis with the spirochaete and to search for a singular or decisive aetiology of all diseases in our search for cures for the same. This one-size fits all scheme of illness and health persists and may well stand behind the current enthusiasms for the genome project (one gene – one disease) as the latest instauration of this causal conviction.

As a scourge from heaven, under the sign of venereal influence, syphilitic disease could be transmitted through the blood, as the sins of the fathers are visited on the sons. For this insight, the religious, astrological, mythological lore of the disease was not merely a mistaken track in the history of scientific progress towards modernity and the disease entity that we think to limit to and so to identify with the spirochaete itself (*qua* disease vector/agent) but the veritably erring path of the scientific discovery of and the emergence of scientific fact itself. Exactly this transition was required, Fleck argues – as in a similar vein the historian Lawrence Principe has retraced the similar importance of alchemy exactly for the sake of (and *not* as blocking) the development of chemistry⁶³ – in order to identify or “discover” a connection between a disease of the skin (the integument as the symptomatic locus where primary, specifically observable symptoms of syphilis are manifest) and the blood. It was the developmentally pleonastic character of the disease of syphilis made its scientific study and resolution particularly difficult – and not only, as an argument that still recurs in the context of HIV research, the taboo of sexual anxiety and prejudice.⁶⁴ More critically in terms of the development of the modern disease concept, what, asks the serologist, has a disease of the skin, the province of dermatology, to do with the blood? And what, we may and we ought still to ask, has a disease characterized by serological changes to do with the nervous system, the spinal fluid, the organism as a whole? How/why does syphilis, if untreated, progress in approximately 25% of the those affected, to the later stages of this disease? Why 25%? Or for a contemporary perspective, is there a relationship between syphilis and HIV? Such questions address the problem of infection – and immunology and public health – *in nuce*.⁶⁵ For Fleck, who was one of the first to correlate not only the elevated count of leukocytes but also (so Fleck argued, though this, significantly has not yielded a research tradition in the [accordingly inactive] science Fleck established – leukergy)⁶⁶ other characteristic changes of those same leukocytes as specific indices of immune response (notably, so Fleck argued, as adequate to differentiate between bacterial and viral infection and more), these questions remain to be posed in a precisely *complex* (to emphasize the scientific and research point Fleck insisted upon) context.

Thus it was the very erroneous idea of a scourge, of the “unscientific” (to use Fleck’s specification of the nature of the) notion of “befouled” or “bad blood” that was a requisite, indispensable element in the development of the scientific concept of the strangely unstable disease entity that Fleck himself in perfectly scientific sobriety did not finally identify with the spirochaete.⁶⁷ For Fleck, bacteriological agency alone was insufficient to explain the aetiology of the disease as such, specifically in its pleonastic character, and which we, in the wake of AIDS and the ongoing global crisis of the same, have learned to understand as an important precision, focused more upon the immune system than the concept of the disease-entity as such.⁶⁸

Fleck thus maintained (for the sake of preserving the same complexity he thought indispensable to medical research) that error was to be *conserved* (Nietzsche went further in this, as in everything, and spoke of “cultivating error as the mother womb of knowledge,”⁶⁹): preserving past error like an attic full of things out of style, or a basement collection of discarded and forgotten artifacts, broken, or bent, which are nonetheless kept out of the prudential recognition that they might turn out to be useful – once dominant sclerotic perspectives (no matter if we call them Fleckian thoughtstyles or Kuhnian paradigms) have been altered by as yet unanticipated influences.

Critically – logically, as Patrick Heelan tirelessly observes – it is only if our truths are true, only if we can be certain of our scientific point of departure, that the process of science can be a more or less ideally algorithmic and heuristically banal (Kuhn’s “ordinary”) affair (this is the contention Laudan’s progressive program eliminates by designating as “revolutionary” the everyday ideal of science, whereby revolution turns out to be a matter of normal, scientific problem solving).

Ludwik Fleck’s conviction, which he shared with Ernst Mach, was that the complex riches surrounding an abandoned path might hold a useful key for us when retracing our steps after sufficient anomalies, or encounters with the limits of the ruling paradigm (as the current scientific thoughtstyle) moves us to do so. For Heelan, a fuller, properly hermeneutic understanding of science refers us to the broader scope of the human values and the full resources of the life-world that must be incorporated, completing the balance of the scientific notion of truth.

Fordham University/Georgetown University

NOTES

¹ Patrick A. Heelan, *Quantum Mechanics and Objectivity: The Physical Philosophy of Werner Heisenberg* (The Hague: Nijhoff, 1965), 4. By noting the range of “lifeworlds” differentially constituting the “World of the physician, the World of the sportsman, the World of the husband, the World of the wife,” The reference is as specific to its era and to the perspective of the author in the early sixties, and is accordingly as dated as it is charming. Heelan lists an array (now noticeably dated) of “World” possibilities as a prelude to his focus upon “the World of the physical scientist in the twentieth century.”

² The terminology of a horizontal analysis is classically phenomenological. It refers to “an analysis of ‘horizons’ of the kind of cognitive intentionality-structure implicit in the conduct of a systematic investigation.” Heelan, *Quantum Mechanics and Objectivity*, x.

³ Heelan, *Quantum Mechanics and Objectivity*, 22.

⁴ *Ibid.*, 179.

⁵ The world of lived research of “pieces of apparatus and their behaviour” constitute “a World-for-[the physical scientist],” whereby “the world to which [the scientist] orients himself is one structured about things; it is a World-for-things.” *Ibid.*, 176.

⁶ Because the measurement process is “part of the activity whereby we contact and so observe physical reality, it has a distorting effect on reality.” *Ibid.*, xi-xii.

⁷ *Ibid.*, 62-63.

⁸ *Ibid.*, xii.

⁹ *Ibid.*, 63.

¹⁰ *Ibid.*, 99.

¹¹ It may be argued that this is so with respect to an epistemology of quantum mechanical physical theory in a way that it is not so essential for an epistemology of classical physics.

¹² Werner Heisenberg, *Physicists Conception of Nature [Das Naturbild der heutigen Physik]* A. J. Pomerans, trans. (London: Hutchinson, 1958), 25; cited in Heelan, *Quantum Mechanics and Objectivity*, 150. For a scientist, the objective description of a quantum mechanical system “is the noumenal condition of possibility of the wave packet (or objective knowledge) which accounts for the distribution of possible events linked by the wave packet ... The objective tendency or *potentia* then is the *noumenal correlate of this union of subject and object* in experience ... it [thus] bridges both the external world and the transcendental subjectivity of the knower.” *Ibid.*, 100; cf. 150.

¹³ *Ibid.*, 71. Heisenberg writes that the wave function “is objective but not real.” Heisenberg, *Neils Bohr and the Development of Physics* (New York: McGraw Hill, 1955), 27; cited in Heelan, *Quantum Mechanics and Objectivity*, 150.

¹⁴ For Heelan, further, in an extension which would be critical for the rest of his thinking on science, technology and *readability*: “The scientist translated these into mathematical symbols, in which they entered a mathematical theory which, as an intelligible whole, gave meaning to its terms. (*Ibid.*, 174.) This is the general structure of Heelan’s account of readable technologies, a hermeneutic endeavor which he now explains in Husserlian and even scholastic, Lonerganian agent-active terms (rather than with reference to Heidegger, despite the language of questioning): “The intentional reality of the sign is the noematic correlate to an act of inquiring intelligence which is not content with what it sees but looks for explanation.” (*Ibid.*) “Of itself,” to take this structure of inquiry to a Kantian level, “the instrument is ‘dumb’; it waits to be questioned by the scientist, and the form of the question structures its response.” (*Ibid.*)

¹⁵ *Ibid.*

¹⁶ Heelan, *Quantum Mechanics and Objectivity*, 175.

¹⁷ Heelan, *Quantum Mechanics and Objectivity*, 58-9.

¹⁸ To trace the hermeneutic dimension of nothing less than mathematics in its physical referentiality, C.F. von Weizsäcker, offers a wonderful image: “The properties of a physical theory are formulated in abstract mathematic language. Let us compare them with a musical score. For those who cannot read notes the musical score is dead, but the man who understands them hear ths the melody in them.” C.F. von Weizsäcker, *The World View of Physics* (London: Routledge and Kegan Paul, 1962), 135. Cited in Heelan, *Quantum Mechanics and Objectivity*, 5.

¹⁹ Meeting of the American Philosophical Association, 1984.

²⁰ As a compensatory consequence, seeking a foothold, Sklar's discussion went on through the time allotted for the commentary, as well as the time slotted for Heelan's response as well as the time for projected questions and debate – a thoroughly unprofessional display – while both Heelan and McMullin stared out into the confounded and impatient audience.

²¹ Heelan, *Space-Perception and the Philosophy of Science* (Berkeley: University of California Press, 1983), 1-23.

²² Heelan, *Space-Perception and the Philosophy of Science*, 220-246.

²³ And Dilthey's own perspective was scientific *ab initio*.

²⁴ This includes sociology of knowledge and of science, as dramatized by the recent Sokal trauma as it has affected social studies of science and as it has had, by drastic but heretofore unremarked contrast, almost no effect whatsoever in the philosophy of science proper.

²⁵ Heelan, *Space-Perception and the Philosophy of Science*, 222.

²⁶ Thus Laudan resolves the challenge of Kuhn's theoretical distinction between normal and crisis or revolutionary science: "science is *normatively* the effort to surpass old theories and research traditions by new ones of greater explanatory power." Cited in Heelan, *Space-Perception and the Philosophy of Science*, 229. Of course, it goes without saying that this redefinition of "revolution" means that the latest findings of Fermilab, much as the public relations perspective would be pleased to have it said: constitute revolutions on a par with Copernicus, Newton, Einstein, Heisenberg. This makes Weinberg and Sokal, Stephen Hawking and Carl Sagan *revolutionaries* to a man. Normal science is revolutionary science. End of debate. For note that the stultification of contemporary scientific research traditions is no obstacle from such a viewpoint but rather yields Weinberg's later and dramatic claim that physics is (as logic would indeed dictate) now "final" – as Fukuyama has claimed – "history is at an end" in a lovely parallel with the last words of the "Compulsory Preface" that is the starting point of *1066 and All That*.

²⁷ Ludwik Fleck, "To Look, to See, To Know" [1947] in Robert S. Cohen and Thomas Schnelle, eds., *Cognition and Fact: Materials on Ludwik Fleck* (Dordrecht: Reidel, 1986), 151.

²⁸ For this reason, Fleck's *critical* notion of thought-collectives, as intrinsic to the progress of science as such, contradicts the dominant, historically ideological ideal of scientific genius and the achievement of Western liberal individualism. See Rom Harré and John Ziman in this volume. See also Ziman's *Of One Mind: The Collectivization of Science* (Woodbury, NY: American Institute of Physics, 1995). But beyond this, see also Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science* (Cambridge: Cambridge University Press, 1998).

²⁹ Cohen and Schnelle, "Introduction," *Cognition and Fact*, xxxi.

³⁰ Fleck, "Crisis in Science" [1960], *Cognition and Fact*, 153.

³¹ More than anything else, new approaches to the history of the science (deriving from changes in history itself have begun to undermine the standard or received view of analytic philosophy of science. Many scholars credit Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life* (Princeton: Princeton University Press, 1985) but if such a study was one of the first to enjoy widespread attention, it did so only in the wake of a shift that had begun even before Herbert Butterfield, *The Origins of Modern Science 1300-1800* (London: Bell,

1957 [1949]) and A. C. Crombie, *Augustine to Galileo: The History of Science AD 1100-1700* (London: Heinemann, 1952) but, arguably, with Pierre Duhem, *Le système du Monde: Histoire des doctrines cosmologiques de Platon à Copernic* (Paris: Hermann, 19-13-1959) – for a discussion of Duhem and the peripheral limitations attending the reception of this study, see Babich, “Continental Philosophies of Science: Mach Duhem Bachelard” in R. Kearney, ed., *Continental Philosophy in the Twentieth Century* (London: Routledge, 1989), 175-221 — other studies include Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995), etc. For a general overview, see H. Flores Cohen, *The Scientific Revolution: A Historiographic Survey* (Chicago: University of Chicago Press, 1994) and for a specialized, more ethnographically influenced, see Golinski, *Making Natural Knowledge*. In the context of the philosophy of science, a more sober account of the history of alchemy and natural science appears to be emerging in the wake not only of scholars such as Dear but Lawrence Principe, *The Aspiring Adept: Robert Boyle and his Alchemical Quest* (Princeton: Princeton University Press, 1998), which should be read for good measure as a nuanced pendant to Shapin and Schaffer. And last but first in the order of political relevance or influence is the chapter being written even now which tells the story of the history of the philosophy of science itself in the collection edited by Giere & Richardson and now in Michael Friedmann’s still-too-positivist attempt to bridge the analytic-continental divide. See Friedman and Ronald Giere in Giere and Richardson (eds.) *Origins of Logical Empiricism*, (Minneapolis: University of Minnesota Press, 1996) and see Freideman, *Reconsidering Logical Positivism* (Cambridge: Cambridge University Press, 1999).

³² In the words of one commentator, Fleck was “too much ahead of his time.” But the sociology of science (or the sociology of knowledge), apart from distant disciplinary lions like Robert Merton, but *qua* sociological studies, *qua* quintessentially *social* science, has yet to enjoy an uncontested status *vis-à-vis* “science” proper, that is: *natural* science. But, in the wake of the science wars, sociology has been compelled to defend itself precisely as a science, in the wake of works not only promising but offering an exact sociology of “laboratory life” or the “manufacture of knowledge.” In the same spirit, we may add the increasingly maligned anthropology of science as it reflects the total distaste for cultural studies of science, including the rhetoric of science. The history of science itself remains methodological and conceptual problem precisely because, unlike other “social” studies of science, the history of science refers very precisely to nothing other than “the facts” – that most canonic of scientific notions.

³³ This is the patent sense in which Nicholas Jardine recommends in the recent (re)edition of his book *Scenes of Inquiry* (Oxford: Oxford University Press, 2000 [1991]), a book perpetuating the mainstream philosophical habitus of leaving continental philosophers like Patrick Heelan (and so many others) unnamed and hence out of account, while coopting the larger iconic names (like Gadamer’s own): an iceberg chipping strategy which leaves the realm of needed reflection and reception as unfathomed as ever before, yielding conclusions of inevitably ultimately, limited or feeble weight. Until a modest focus becomes a word not for politic exclusion and selective inclusion but integrity and pluralistic respect and sensibility, it will end up, as Jardine’s book ends up, in its earlier as in its later instaurations, drawing a conservative line in the dust, that like Toulmin’s fine recommendation at the end of his *Cosmopolis*:

The Hidden Agenda of Modernity (Chicago: University of Chicago Press, 1992), needs far more than conviction behind it but vital and critical action.

³⁴ I thank Steve Fuller for bringing this rather relevant detail to my attention. And I here underline that I take this report, a perfect piece of gossip, on perfect faith. Hearsay is hearsay.

³⁵ See for a specific discussion of this credulity and its limits, Alasdair MacIntyre, "Preface," Babich, ed., with R. S. Cohen, *Nietzsche, Epistemology, and the Philosophy of Science: Nietzsche and the Sciences* [Boston Studies in the Philosophy of Science 204] (Dordrecht: Kluwer, 1999), xv-xvii.

³⁶ See Tom Sorrel, *Scientism: Philosophy and the Infatuation with Science* (London: Routledge, 1991).

³⁷ How could it happen that scientists not be asked to *vet* an article on science – as if the evening news did not constantly present scientists happily saying speculative things about genetic engineering or evolution or even (please let's do think of Carl Sagan) astronomy *and* evolution, in one blow, unmasked or debunked by other scientists just as happily as misleading, overstated, and even erroneous.

³⁸ Ian Hacking, *The Social Construction of What?* (Cambridge: Harvard University Press, 1999); Bruno Latour, *Pandora's Hope: Essays on the Reality of Science Studies* (Cambridge: Harvard University Press, 1999).

³⁹ Cohen and Schnelle, eds., *Cognition and Fact*.

⁴⁰ Fleck, "Scientific Observation and Perception" [1935], *Cognition and Fact*, 73.

⁴¹ *Ibid.*, 74.

⁴² *Ibid.*, 76. Translation modified.

⁴³ See in particular, Peter Dear, *Discipline and Experience*, Ch. 1. For a further list, see note 31 above.

⁴⁴ *Ibid.*, 74.

⁴⁵ This is the substance of the concluding reflections of "To Look, To See, To Know."

⁴⁶ Certain sociologists have observed that Fleck's idea of collective knowledge is abrogated by the very idea, as it were, of "intellectual property" and the very observable struggle in science for the ownership of an idea and the authority and power deriving from such influential ownership. Fleck himself describes this struggle concerning the possession of "Salvarsan" or the emergence of a single fact (like the Wasserman test) in his book. But the notion of a thought-collective and its associated style for Fleck concerned the dynamic of research and discovery, the genesis, that is to say and the development of the then and thus emergent "fact." Tribal thinking or mindless identification with a research collective does not follow from Fleck's description.

⁴⁷ Ludwik Fleck, *The Genesis and Development of a Scientific Fact*, trans. F. Bradley and T. Trenn (Chicago: University of Chicago Press, 1979 [1935]).

⁴⁸ Note that Kuhn's self-deprecation indicates a language competence that itself requires a hermeneutic articulation, different as it is from the case far more routine among American philosophers today of not being able to read German at all. In the same way, we note Kuhn also reports that he likewise read and spoke French very haltingly, a facility which was likewise different from illiteracy, as his own autobiographical reflections make clear in his recollection of his time during and after World War II in France where what he, as a military expert was able to do then required just as much ability in French as he similarly disavows with a scientist's characteristic diffidence – a point of self-deprecating irony further attested by the praise of the quality

of his French which he received from Parisians.

⁴⁹ This insight is hardly a resultant of recent events, but it is now unmistakable in the wake of the dramatic demonstration of the limits of that same image of “democracy” afforded by the very events of the 2000 US presidential election and judicial decision regarding the undecided results of the same.

⁵⁰ Kuhn could not credit Fleck beyond his famous prefatory characterization of Fleck’s book as “an essay that anticipates many of my own ideas” and situating those same “ideas” in what Kuhn called “the sociology of the scientific community.” (Kuhn, *The Structure of Scientific Revolutions* (Chicago, University of Chicago Press, 1970 [1962], vii). To the great disappointment of students of the sociology of science, Kuhn ultimately failed to specify or further to extrapolate upon this allusion to the “sociology of the scientific community.” In Kuhn’s case, the author himself exemplifies the worst aspect of the so-called authorial-fallacy. Like works of art, words like *paradigm* (or *thoughtstyle* or *hermeneutics*), may be seen to have lives and fortunes of their own, apart from and often alien to their originators. Expressing, just as Kuhn maintains, the salient core of Fleck’s *The Genesis and Development of a Scientific Fact*, the notion of a “thoughtstyle” presented Kuhn with a research palimpsest, interpretive armature, or background structure for *The Structure of Scientific Revolutions*, providing an exactly deployable heuristic device for articulating the historical course of scientific change or revolution in science. And Kuhn needed such a structure just because it was otherwise unavailable, as Kuhn himself rightly emphasizes in his 1976 introduction to Ludwik Fleck, *Genesis and Development of a Scientific Fact*. See too the more comprehensive introduction by Lothar Schäfer and Thomas Schnelle in their edition of Fleck, *Entstehung und Entwicklung einer Wissenschaftliche Tatsache* (Frankfurt am Main: Suhrkamp, 1980), vii-xlvi.

⁵¹ Steve Fuller, *Thomas Kuhn: An Intellectual Biography for Our Times* (Chicago: University of Chicago Press, 2000).

⁵² Beyond political ecology but exactly due to this historical circumstance, Kuhn’s *Structure of Scientific Revolutions* fails to refer to Fleck, nor does Kuhn mention Fleck’s notion of thoughtstyle [*Denkstil*] and -thought-collectives except – and this again is my contention here – by way of a periphrasis which would have an extraordinary destiny: the very word *paradigm*.

⁵³ Aristides Baltas, Kostas Gavroglu, Vassili Kindi, “A Discussion with Thomas Kuhn” in Kuhn, *The Road Since Structure: Philosophical Essays, 1970-1993 with an Autobiographical Interview*, ed., J. Conant and J. Haugeland (Chicago: The University of Chicago Press, 2000), 255-323.

⁵⁴ Steve Fuller, *Thomas Kuhn*. Fuller follows Hart’s 1993 article discussing the meaning of “style” as it emerges in the context of the correspondence between Panofsky and Mannheim (see Fuller for reference). But see, too, for a more contextually situated discussion both of that correspondence and the scientific context in which Fleck’s term makes its appearance: Jonathan Harwood, *Styles of Scientific Thought: The German Genetic Community: 1900-1933* (Chicago: University of Chicago Press: 1993).

⁵⁵ As Fleck’s notion of thoughtstyle implies, and just as Nietzsche argues, Kuhn could find in Fleck only what he had eyes to see, or ears to listen for. Hence, what exceeded Kuhn’s capacity for understanding he simply overlooked. Kuhn’s mistake lay – and with regard to Fleck he had only one – in assuming that the history and sociology of ideas was a properly established or developed discipline with a patent and received

structure. A scientist by training and not a historian, Kuhn could not have guessed that nothing could be further from the truth in the social dynamic one author has famously dramatized as the “two cultures” and the still enduring abyss between the same.

⁵⁶ This would mean, to oppose the dominant reading, expressed by one reader reflecting on Fleck’s reception by comparison with Kuhn’s own influence, that at the very least, such a reliance shows less Kuhn’s prescience than his opportunism

⁵⁷ Lothar Schäfer thus explicates the very problem of contemporary analytic philosophy of science as the dominant thoughtstyle *possible* in and for the philosophy of science (*Wissenschaftstheorie*). For Schäfer, Kuhn is little more than Fleck’s veritable epigone. See Schäfer, “Theoriendynamische Nachlieferungen. Anmerkungen zu Kuhn-Sneed-Stegmüller,” *Zeitschrift für philosophischer Forschung* 31 (1977): 19-46.

⁵⁸ The “very idea” of the history of science was conceptually problematic – and in many ways, it continues to be so. If we are to believe Nicholas Jardine’s mild and all-too-conservative warning in his own *Scenes of Inquiry*, and we should. A recent review of the disciplinary relationship between the philosophy of science and the history of science shows that this tension remains – although it is manifestly clear that the philosophy of science can no longer insist, as a reconstructivist perspective could argue, that it is too bad for science if it does not in historical fact accord with theoretical accounts of the logic of scientific discovery or invention. And if talk of *thought-collectives* and *thoughtstyles* was problematic, to combine the former with the idea of the history (specifically, fatally, expressed as the genesis and development) of a fact was exactly shocking to the logical mindset of the philosophy of science – as, later, in Heelan’s case, it would still be for Larry Sklar, and as it continues today.

⁵⁹ I discuss Nietzsche in my study, *Nietzsche’s Philosophy of Science: Reflecting Science on the Ground of Art and Life* (Albany: SUNY, 1994). For the connection with Kant see Babich, “Nietzsche’s Critical Theory: The Culture of Science as Art” in Babich, with Cohen, ed., *Nietzsche, Epistemology, Philosophy of Science: Nietzsche and the Sciences II* (Dordrecht: Kluwer, 1999), 1-13. The critical dynamic of questioning both what we name as true and what we regard as error is the keystone of Kant’s entire philosophy of science. I discuss this in The aesthetic design of the scientific question effects its judgment power (Bxiii). Rather than a science based solely on observation (and inductive regress) which would be no science at all in the image of logic and mathematics, Kant resolves the Humean problem of induction in the Preface to the second edition of the *Critique of Pure Reason*, in the empirical practice of questioning because the question (or experiment) both concedes and exploits the epistemological limitations of reason (KdrV Bix) and experience (cf. A124-126). Just as mathematics owes its scientific integrity to the axiomatic character of its conceptual groundwork, so physics operates with axioms or defining assumptions on both theoretical and objective levels, that is both in its fundamental concepts and in its experimental processes (B241/A196; A713/B741-A727/755). Two different readings of Kant’s philosophy of science are useful here if they both remain – for different reasons – oblique to the traditional or ‘received’ account of the philosophy of science as such: the first providing an architectonic or schematic of the Kantian schema, Gerd Buchdahl, *Kant and the Dynamic of Reason* (London: Blackwell, 1992) and the second insightfully bridging Kant’s first and third critiques in Heidegger’s interpretation of Kant: Pierre Kerszberg, *Critique and Totality* (Albany: State University of New York Press, 1997). An excellent historical (and still exactly analytic) account is Michael

Friedman, *Kant and the Exact Sciences* (Cambridge, MA: Harvard University Press, 1992).

⁶⁰ This is the practical point of “going back to the drawing board” in a research context.

⁶¹ This opposes the casual presumption of one generalist in the history of philosophy, who thoughtlessly invokes the presentist viewpoint (he is not better on Nietzsche) that “what was once believed true is now known to be false, like the cases of astrology or alchemy.” Robert Pippin, “Gay Science and Corporal Knowledge,” *Nietzsche-Studien*, 29 (2000): 136. Similar references may be sought in the literature of philosophy, particularly with reference to science or truth, almost at random.

⁶² And Fleck was all for abolishing that unitary and reified perspective as obstacle to a dynamic conception of a disease.

⁶³ Principe, *The Aspiring Adept*.

⁶⁴ It is significant in this context that the larger number of sociological discussions, presumably more under the influence of Foucault and on Fleck, understand the term “social construction” of diseases to refer to a kind of parallel definition of the disease in the mind of the socius rather than having anything to do with the factual genesis and development of the scientific or medical concept. Fleck’s is an aetiological and hence more epistemological concept.

⁶⁵ See R. C. Lewontin, *Biology as Ideology: The Doctrine of DNA* (New York: Harper, 1993) for a provocative reflection on the relationship between ambient or non-microbial factors and virulent disease or epidemiology.

⁶⁶ It is relevant that the co-workers to be found for this discipline lacked the requisite background for Fleck’s conception and that Fleck himself would not cooperate in the formation of such a tradition (the method of medical research science, as he understood it, precluded any systemic or dynamically complex science as leukergy. Thus modern medicine took only the most straightforward index of a much more complex phenomenon because they lacked the conceptual framework for the diagnostic information Fleck could derive from leukergic immune response.

⁶⁷ Fleck, *Genesis and Development of a Scientific Fact*: “Syphilis is not to be formulated as ‘the disease caused by *Spirochaeta pallida*.’” 21. For Fleck such statements are problematic because “syphilis as such does not exist” (39) a claim which he makes not to deny the disease but to point to the inadequacy of the classic germ carrier theory of disease. For Fleck, anticipating today’s increased attention to the role of the immune system and the organism as a whole, the disease entity (so-called) is but one aspect, because disease requires more the presence of infectious agents: perfectly healthy individuals carry such “disease agents” without being themselves ill. Illness or sickness is a complex state rather than a state betraying the presence of an alien invading element interior to an otherwise hermetically secure or integral biological system.

⁶⁸ This same insight has been extended, in a direct line from Fleck’s work, to oncology. G. Zajicek, “Ludwik Fleck: Founder of the Philosophy of Modern Medicine,” *The Cancer Journal*, 5:6 (1992): 304-305.

⁶⁹ Friedrich Nietzsche, “Wir müssen das Irren lieben und pflegen, es ist der Mutterschooß des Erkennens.” *Kritische Studienausgabe* (Berlin: Walter de Gruyter, 1980), Vol. 9, p. 503.