

# On Kuhn's Case: Psychoanalysis and the Paradigm

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I am not a professional Kuhn scholar. In my formative years, Kuhn's *The Structure of Scientific Revolutions* decided my direction in life and was a source of great enthusiasm followed by intense study, but in the last thirty years or so it has been more the object of rumination than research. I must confess that my interest is now both personal, one might say, personal at the heart of my own identity as an idiosyncratic historian of science, and also indirect because in attempting to sort out some themes in Kuhn's work I hope to clarify issues which were never his scholarly concerns, but are now quite recognizably mine, though ones which I hope are of larger interest. Being mine, they bear the marks of my hybrid identity as a historian of science, a hybridity that somehow never fails to protect me from the surprise of recognizing how Kuhn himself vacillated in his professional identity. A renegade physicist? A philosopher manqué? A historian of science, plain and simple, driven by the inner logic of his own writings to metamorphose into a philosopher of language? Which of these is the real Kuhn?

A preliminary draft of this paper was prepared for a colloquium at Princeton University entitled 'Model Systems, Cases, and Exemplary Narratives', 11 December 1999. I would like to thank Angela Creager, Elizabeth Lunbeck, and Norton Wise for the invitation to contribute and for the reflections of my commentator, Carl Schorske, as well as incisive comments by Anthony Grafton and the late Gerry Geison. Since then, it has benefited from the responses of Lauren Berlant, Martin Kusch, and John Burnham, to whom I'm very grateful. I learned much from a recent conference at the Department of History and Philosophy of Science, Cambridge, entitled 'Kuhn and the Sociology of Scientific Knowledge', March 2006, organized by Ipek Demir and Kusch, in particular the contributions of Paul Hoyningen-Huene, Kusch, and Simon Schaffer. In the final stages of preparation I received very useful advice from Berlant and the coeditors of *Critical Inquiry* and an extensive detailed commentary from and email exchange with Kuhn's son Nathaniel Kuhn (who also shared with me his mother's, Kay Kuhn's, memories), for which I'm extremely grateful; late and crucial clarifications I owe to Adam Phillips. The paper is dedicated to the memory of Jeanne Kassler (1951–2002), with whom I first read *The Structure of Scientific Revolutions*.

For many years I laboured under the illusion that Kuhn was a historian of science—an illusion to which my experience of taking courses with him in the Program in History and Philosophy of Science at Princeton in 1970–72 gave ample sustenance. There was ne'er a hint of philosophy in the texts we were assigned—the Descartes we read was *Le Monde*—and we received severe warnings about the dangers of crossing over to the Department of Philosophy where we were likely to be waylaid from the true path for the study of science, which was history.<sup>1</sup> To make our intellectual lives even simpler, it was an unspoken rule in the Program that no one ever mentioned *The Structure of Scientific Revolutions*. I never heard anyone transgress. On the other hand, everyone had read that book and was acutely aware of its importance. Amongst the exciting lines of enquiry leading off it for historians were the signposts towards the sociology of science, which Kuhn had clarified and emphasized in the 'Postscript—1969'. What he would later call, in the preface to *The Essential Tension*, a 'significant mistake'<sup>2</sup> was his unreflective identification and differentiation of scientific communities by subject matter in *Structure*; by 1977 he was recommending that this should be replaced with 'examining patterns of education and communication before asking which particular research problems engage each group' ('P', p. xvi), but already in 1969 he had asserted that 'scientific communities can and should be isolated without prior recourse to paradigms; the latter can then be discovered by scrutinizing the behavior of a given community's members.'<sup>3</sup> Here was a research programme that the Princeton historians could engage in—it didn't use the banned *P* word.<sup>4</sup>

1. Philip Kitcher, then a graduate student in philosophy, tried to tempt me over to the other side, knowing that I, like him, had come from Cambridge, where history and philosophy of science lived in a fruitful if jittery symbiosis rather than submit to the Separate Spheres culture of Princeton, by pointing out the riches in philosophy—with Hempel, Davidson, Nagel, and Rorty offering courses, how could one pass them up? I did venture over briefly to Rorty's seminar because of my passion for Kant (his topic that year), but the ethos of the Program (as it was always known) obliged me to commit myself instead to the required history courses with Carl Schorske and Arno J. Mayer. In this instance, fair exchange is no robbery.

2. Thomas Kuhn, preface to *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago, 1977), p. xv; hereafter abbreviated 'P'.

3. Kuhn, *The Structure of Scientific Revolutions* (1962; Chicago, 1996), p. 176; hereafter abbreviated *S*.

4. In the spirit of this recommendation, in the fall of 1971 Gerry Geison led a Princeton Program Reading Seminar, which collectively attempted to isolate the community structure (if there was one) of early evolutionary biology by means of a quantitative collation of the

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So, in a strange way, I thought Kuhn's professional identity was unproblematic, in a way that mine has never been. I positively relished the hybridity of calling myself a historian and philosopher of science. And when you add that the science which has been the principal object of my scholarly research over more than thirty years has been psychoanalysis all hell breaks loose with the boundaries of disciplinarity. So I should feel more at home with Kuhn's permanent crisis of identity than I do. My own professional identity is easier to pin down if I specify the problems I have been concerned with. There are two principal problems or objects: Freud and the case. To describe myself, or even think of myself, as a Kuhnian with two such problems has often seemed to stretch the sense of significant kinship or family resemblance between his problems and mine. Yet I have increasingly come to be haunted by the uncanny sense that my problems are more akin to his than I myself, and certain others, would immediately recognize. This paper will be devoted to exploring this uncanny sense, to finding how much hard, documentable reality underlies this sense. I should issue one word of warning, however: amongst the kinds of documents I will be drawing upon I will include my own memories of being a graduate student in Kuhn's seminars and discussing my work with him. But, in opening my paper, I should at least declare some of the themes which will be its focus: in exploring how Kuhn was and is linked to Freud and the case, I will be examining the function of Kuhn's own psychoanalysis in his work, the function of the Harvard method of case teaching in his development as a historian of science, and, more profoundly, and this is where I am obliged to join up with those who really know their Kuhn, in the implications of what I increasingly take to be Kuhn's own most important concept: the exemplar.

'I am an anxious, neurotic—I don't bite my nails but I don't know why I don't bite my nails.'<sup>5</sup> This is, according to my memory, an accurate piece of self-capturing on Kuhn's part; an essential element in its verisimilitude is the fact that it is wry—stopping just this side of being comic. A proper

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correspondence networks of biologists; the basic finding of this empirically arduous, months-long science-(paradigm)-independent study was that there was a small core group of biologists and geologists—Darwin, Lyell, Huxley, Hooker, and Gray—a finding any Darwin scholar would have accurately predicted given twenty seconds of reflection. As a group we decided we were not impressed by this avenue of research.

5. Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi, 'A Discussion with Thomas S. Kuhn', in Kuhn, *The Road since 'Structure': Philosophical Essays, 1970–1993, with an Autobiographical Interview*, ed. James Conant and John Haugeland (Chicago, 2000), p. 321; hereafter abbreviated 'D'. This interview first appeared in slightly different form as Baltas, Gavroglu, and Kindi, 'A Discussion with Thomas S. Kuhn: A Physicist Who Became a Historian for Philosophical Purposes', *Neusis* 6 (Spring–Summer 1997): 145–200. I would like to thank Gavroglu and Kindi for making a copy of the interview available to me while I was preparing this paper.

psychoanalysis might well start here with this declaration of the unconscious: Kuhn thought of himself as the sort of person who bites his nails, but for some unknown reason he didn't. What was the unknown reason he didn't bite his nails?

I start here because it is quite obvious that this is not the sort of psychoanalytically informed question one can address to Kuhn with any hope of a good answer. It might be the most important for the 'solution' of his 'case', if that's the sort of psychoanalysis one wants to engage in—the setting up and then solving of an enigma. But, not only do we lack any substantial material upon which to base any such inquiry, I wager it would not lead us in a profitable direction.

Initially, therefore, we must give up hopes of such a psychoanalytic enquiry. However, the documentation we do possess can help us answer a related question: What significance did psychoanalysis have in Kuhn's life? The answer can be given in terms of three different categories: personal and family milieu, personal experience, and the intellectual. Of his parents, Kuhn offered brief sketches: 'except for James Conant, [my father] was the brightest person I had ever known. . . . He wasn't much of an intellectual, but he had a very very sharp mind'. In contrast, his mother was not as clever, but was more of an intellectual. It seems clear that the Kuhn family was imbued with the enthusiastic American Freudianism of the 1930s. Kuhn's brother notes that, of the two, the father was more sceptical of psychoanalysis; however this scepticism may have been complicated by the fact that his own father, Kuhn's grandfather, suffered from chronic depression and had a variety of different treatments at the behest of his wife Setty, an energetic and progressive figure in the Cincinnati German-Jewish community, including, it is thought, an analysis in Baltimore with Alfred Adler.<sup>6</sup> In addition, Kuhn's paternal aunt was a Cincinnati psychologist linked with the active psychoanalytic community in both work and friendship. Kuhn's brother Roger remembered that one of the family friends when they lived in Croton-on-Hudson was the socialist intellectual Max Eastman, onetime editor of the influential magazine *The Masses* (from 1913 on).<sup>7</sup> Kuhn's

6. Nathaniel Kuhn, emails to the author, 23 June 2006.

7. An early New York Freudian, Eastman had analysis with Smith Ely Jelliffe, then A. A. Brill, and persuaded his mother and sister to have analysis with Brill. An article he wrote for *Everybody's Magazine* in 1915, a popular monthly with over 600,000 readers, described psychoanalysis as a technique for rooting out mental cancers and was laced with stories of miracle cures and, daringly enough for the time, laid great emphasis on infantile sexuality; see Max Eastman, 'Exploring the Soul and Healing the Body', *Everybody's Magazine* 32 (June 1915): 741–50. It was this article that introduced Edmund Wilson, the preeminent literary critic of America for fifty years, to his lifelong Freudianism. Eastman's chatty memoirs of 1959, full of boastful braggadocio, *Great Companions: Critical Memoirs of Some Famous Friends* (New York, 1959), include an account of Freud's 1909 American visit and of Eastman's brief contact with Freud later in the 1930s; see pp. 129–31.

mother was certainly a more straightforward psychoanalytic enthusiast than his father ever was; through her work as a professional editor, she had edited some of the books of Karen Horney, one of the best-selling popular psychoanalyst-writers of the 1940s and 1950s.<sup>8</sup> Through such cameo memories, Kuhn's brother and children give the impression of a family entirely at ease with the ethos of the 'psychological man', obeying the therapeutic imperative so graphically portrayed by Philip Rieff in *Freud: The Mind of the Moralist* and *The Therapeutic Imperative*—classic works contemporaneous with Kuhn's own most influential writings.

The psychoanalytic milieu did not dissipate when Kuhn left his parents' family. One of his closest friends at Harvard and then on the West Coast in the late 1950s was Joe Weiss, who went into medical and then psychoanalytic training and in the late 1950s was well established in San Francisco as an active member and administrative officer of the San Francisco Psychoanalytic Society. The two families met regularly. Kuhn spent some time with the analysts in San Francisco, socially. The pattern was repeated at Princeton, where Kuhn met socially with members of the Institute for Advanced Psychoanalytic Studies, and on one occasion gave a talk to the group. Finally, it should be noted that Kuhn's youngest son, Nat, became a psychiatrist with a psychodynamic orientation.

So we can safely say that Kuhn came from a family milieu that, from before his birth to after his death, was familiar with and sympathetic to psychoanalytic concepts. Between them, his mother and his son sustained a long-term quasi-professional relationship with psychoanalysis from the 1930s to the 1990s. This, then, is the background to Kuhn's own experience of analysis, as he insisted on putting it on the record in the interview first published in *Neusis* in 1996:

For those three years in the Society I was beginning to read my way into the field and establish myself; and also doing something else, which I think I *should* put on the record. I've said something yesterday about . . . until I got to Harvard not having had many friends; I was clearly a neurotic, insecure young man. It was also the case that somehow or other my parents, my mother I think in particular, worried about this: I was not having dates and that sort of thing. My relations with women were almost non-existent. But that was in some part because my environment was a male environment. The result was that I was persuaded, without a lot of difficulty, to go into psychoanalysis. I'd had some experience as a child with child psychiatry which I did not think very much

8. See Jensine Andresen, 'Crisis and Kuhn', *Isis* 90 (1999): S48; hereafter abbreviated 'CK.'

of and don't carry fond memories of. The analysis in the Harvard years was with a man I, in retrospect, hate, because I think he behaved extremely irresponsibly with me. He used to fall asleep and then when I would catch him snoring he would act as though I had no business being at all angry or upset about it. On the other hand, I'd previously read Freud's *Psychopathology of Everyday Life* [which is, I think, ex case histories]. I do not for a moment like the theoretical categories that he introduces, or feel that for me, at least, they have any force. . . . The psychoanalysis must have been mostly before I got into the Society of Fellows, because it terminated when two things happened: I got married and my psychoanalyst moved out of town. At that point I finished my thesis which was typed by my then wife. That was a marriage that went on for just about thirty years, which produced three lovely children, whom I find immensely rewarding. ['D', p. 280; bracketed phrase was cut from the *Neusis* version]

Let me leave to one side Kuhn's view of this analysis, just in order to establish the bare historical facts and their context. Note, first, that the chronology in this passage—and, it must be said, in many of the autobiographical, personal-historical accounts Kuhn gave of his life and development—is imprecise and wavering. At the beginning of this passage, he places the item he wants to put on record as occurring during the time he was at the Society of Fellows, that is, 1948–51; however, a few minutes later, he corrects this impression: the analysis took place before he entered the Harvard Society of Fellows. We can give somewhat greater precision to the dating here: In November 1948 he completed his dissertation, submitted it, and nine days later married Kathryn Muhs (see 'CK', p. S58 n. 67). The completion of his dissertation, the termination of his analysis, and his marriage thus virtually coincided.

So when did his analysis begin? His first wife Kay gave the date as 1946, when he was twenty-four, having in 1944 completed a rushed degree in physics from Harvard, having spent near on a year and a half in war work in America, England, and France, and on his return in 1945 having begun his research for a doctoral dissertation in theoretical physics. In the years 1947–48, while engaged in his doctoral research, Kuhn started work with Conant on the courses designed for the general education for nonscientists, which became the *Harvard Case Histories in Experimental Science*, having been approached by Conant in the spring of 1947. In the course of his preparation of the case study on the history of mechanics, Kuhn experienced the Aristotle epiphany in the summer of 1947 (of which more later). In January 1948, Kuhn asked Conant to initiate the Society of Fellows appoint-

ment, which would allow him to ‘retrain’ as a historian of science, and he took up his fellowship on completion of the doctorate in November 1948. To spell this out: Kuhn entered analysis as a budding theoretical physicist, gauche with women and discontented with his work in a diffuse and unfocused manner; he left analysis two years later with a doctorate in physics, married, and training to be a historian of science, having undergone a conversion on the road to Damascus which would be one of the seeds of his future method as a historian and his great contribution to the history and philosophy of science. If I were an analyst interested in the efficacy of the treatment simply in outer signs, those of fundamental shifts in the direction of a person’s life, I would say that this was an extremely effective analysis. Before his analysis, ‘Kuhn’ bears little relation to the future author of *Structure*, nor is there any sense of the resolution of the problems of his private life or a choice of career which is more than an extension of expectations induced by family and early education. After it, there is a recognizable outline of that author, a choice of career that is undoubtedly brave and personally driven; and there is no doubt that his marriage brought him considerable personal fulfilment.

So much for the framework of Kuhn’s psychoanalytic career, which is auspicious enough, but gives us little sense of the content of that analysis.<sup>9</sup> Here, you may expect me to start talking of the Oedipus complex and narcissistic identification; let me reassure you (or disappoint you, depending on your inclinations) by my complete eschewal of such an attempt. There is no evidence on which such talk can be based; nor, in the end, is such talk very useful without the nitty-gritty details of dream and fantasy. Of necessity, a psychoanalysis that excludes or occludes the many voices of the analysand and the analyst is no longer worthy of the name. So, instead of such a wild essay, I will return to two moments, glimpses that Kuhn himself gave into what this analysis counted for. In the passage quoted above, the elided passage reads as follows:

9. Kay Kuhn recalled the name of his analyst at this time—Jacob (Jake) Ellis Finesinger (1902–1959), a product of Johns Hopkins medical school in the 1920s, who had analysis while studying in Vienna in the early 1930s before returning to Harvard as a psychiatrist where he worked at Massachusetts General Hospital until he moved to the University of Maryland in late 1949; see Stanley Cobb, ‘Jacob Ellis Finesinger, 1902–1959’, *Journal of Nervous and Mental Disease* 129 (Nov. 1959): 415–16, and [www.finesinger.com](http://www.finesinger.com). At some later point in time while living in Cambridge, Massachusetts, Kuhn had another period of analysis or psychotherapy, this time with Lucie Jessner, more of a specialist in child analysis and at this time a friend and colleague of Margaret Mahler. Jessner also moved south in the mid-1950s, to North Carolina. Both of Kuhn’s analysts were prominent and orthodox Freudian analysts, well-established members of the Boston Psychoanalytic Society when it was still a small group before the enormous expansion of psychoanalysis after the Second World War.

The [psychoanalytic] *technique* of understanding people and enabling them to understand themselves better—I'm not sure that it produces real therapy of any sort—but it sure as hell is interesting. And I think myself, I'd have great trouble documenting this, but I think myself that a lot of what I started doing as a historian, or the level of my ability to do it—'to climb into other people's heads', is a phrase I used then and now—came out of my experience in psychoanalysis. So in that sense I think I owe it a tremendous debt. I think it's too bad that it is getting the very bad reputation that it's getting these days, although I think it richly earned it; but I think what gets forgotten is that there is a craft, hands-on aspect to it, that I know no other route to, and that is intellectually of vast interest. ['D', p. 280]

Let me just note that I hear Kuhn's voice in that phrase and its rhythms: 'it sure as hell is interesting.' Beyond that moment of authenticity, this passage makes a strong claim for the influence of psychoanalysis: it made Kuhn into the sort of historian he undoubtedly became—a historian who absolutely requires a one-on-one relationship of understanding with those historical texts he set out to understand, passing via the 'head' of the author or authors of the texts.<sup>10</sup> This is both an individualistic and psychologistic method, and it reminds one that, when *Structure* gave rise to sociological accounts of the development of science, Kuhn had not only conceptual misgivings about this development but methodological and, one might say, above all temperamental misgivings. As a historian (apart from his philosophical positions on rules, communities, the character of normal science, and so on), he was individualistic and intellectual. Science was first and foremost a work of the individual mind (taken in the broad sense, to include the meaningful gestures and manual activities associated with empirical science), and the task of the historian is the thinking of men's thoughts after them or, in Collingwood's phrase, 'a re-enactment in the historian's own mind'<sup>11</sup>—al-

10. Nat Kuhn recalled: 'I asked [my father] once about how his experience in analysis had influenced his work, and he gave me an answer along the lines of the *Neusis* interview' (Nat Kuhn, email to the author, 23 June 2006).

11. R. G. Collingwood, *The Idea of History* (Oxford, 1946), p. 212. Collingwood's recommended practice underpinning the history of ideas also had a psychoanalytic aspect through his own experience; see James Connelly and Alan Costall, 'R.G. Collingwood and the Idea of a Historical Psychology', *Theory and Psychology* 10, no. 2 (2000): 147–70. We might surmise that this fundamental individualistic orientation of Kuhn's underpinned his closeness with Quentin Skinner in the 1970s because Skinner's key methodological injunctions (at the time) concerning 'authorial intention' also gave a fundamentally mentalist orientation to historical understanding; this was certainly the way I myself had utilized Skinner's work in conjunction with Kuhn in 1969–70 in my undergraduate dissertation, 'A Discussion of Some Case-Histories in the Historiography of Science', which applied the ideas of Kuhn, Collingwood, and Skinner to the Merton Thesis, the internalist-externalist debate, and the Darwinian Revolution.

though this individualistic temperament and historiographical style did not prevent him from underlining the crucial function of the scientific community to the point where he could, famously, be accused by Lakatos of portraying scientific development as ‘*a matter of mob psychology*.’<sup>12</sup>

To put the claim at its crudest (and hence render it easily comprehensible and probably erroneous at the same time): Kuhn modelled his understanding of historical texts and figures on the experience of psychoanalysis. He learned how it is that one can climb into other people’s heads through psychoanalysis; and his vision of historical understanding remained that for the whole of his professional career as a historian. (I leave to one side his understanding qua philosopher of science and philosopher of language, partly because, as I said above, I find it almost impossible to shake off my view of him as primarily a historian, whatever he may have said. Though, given the crude formulation above, a further speculation would be to add that Kuhn was a historian insofar as he functioned according to his psychoanalytic model of understanding and ceased to be one, becoming something which he called a philosopher, when he used another model. I offer this thought, hang nothing on it, and will willingly withdraw it.) There are two things to be said immediately about this process of psychoanalytic understanding: first, we have what must be a record of what Kuhn meant by this process in his epiphanic moment of understanding the *Physics* of Aristotle. This experience took place in 1947 and thus right in the middle of his two years of psychoanalysis. As he often recorded it: ‘One memorable (and very hot) summer day those perplexities suddenly vanished. I all at once perceived the connected rudiments of an alternative way of reading the texts with which I had been struggling’ (‘P’, p. xi). To which we should add his daughter’s testimony: “‘The importance of the Aristotle story to my dad’s views can’t be overstated, since we heard it many times in numerous iterations” (‘CK’, p. S56).<sup>13</sup> And to remind you how Kuhn himself saw the

12. Imre Lakatos, ‘Falsification and the Methodology of Scientific Research Programmes’, in *Criticism and the Growth of Knowledge*, ed. Lakatos and Alan Musgrave (Cambridge, 1970), p. 178.

13. In a paper written in the 1980s with the title ‘What Are Scientific Revolutions?’ Kuhn gave the most graphic statement to date of the Aristotle epiphany, having already made clear in *The Essential Tension* the importance he attached to it; it was as if he couldn’t stop insisting on its importance:

The Aristotle moment: I was sitting at my desk with the text of Aristotle’s *Physics* open in front of me and with a four-colored pencil in my hand. Looking up, I gazed abstractedly out the window of my room—the visual image is one I still retain. Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I’d never dreamed possible. . . . That sort of experience—the pieces suddenly sorting themselves out and coming together in a new way—is the first general characteristic of revolutionary change that I shall be singling out. . . . Though scientific revolutions leave much piecemeal mopping up to do, the central change cannot be experienced piecemeal, one step at a time. Instead, it involves some relatively

importance of this seminal experience: 'I had wanted to write *The Structure of Scientific Revolutions* ever since the Aristotle experience. That's why I had gotten into history of science—I didn't know quite what it was going to look like, but I knew the noncumulativity; and I knew something about what I took revolutions to be' ('D', pp. 292–93). And note also the instability of temporal sequence: Kuhn got into the history of science *because* of the Aristotle experience, but why was he reading Aristotle in the first place if he wasn't already interested in the history of science?<sup>14</sup> There is a flattening out of temporality here, entirely characteristic of what Freud called *Deck-erinnerungen*, cover memories, or screen memories as they have been usually translated.

I don't think I am being particularly controversial in stating that the Aristotle epiphany marked an advance in Kuhn's conceptual development in two distinct senses:<sup>15</sup> first, in being the exemplary instance of 'climbing into someone's head' and thus in showing Kuhn that past, 'erroneous' science made sense in terms which were necessarily different from any 'absolute' standard of what it means for physics or science to make sense; and, second, in his visceral awareness that a discontinuity, leading to what he would later call an incommensurability, was evident in this process, both historically and in the historian's own process of understanding (hence one of the principal reasons for his later attachment to the model of the gestalt switch). But we should not be under any illusions about the long-term importance Kuhn attached to this particular way of reading; in the following passage from the interview, he makes it clear that this was how he differed from Conant and nearly all other historians of science and also how this particular way of doing history may have disqualified him in many other people's eyes from being a historian:

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sudden and unstructured transformation in which some part of the flux of experience sorts itself out differently and displays patterns that were not visible before. [Kuhn, 'What Are Scientific Revolutions?' (1981/1987), *The Road since 'Structure'*, pp. 16–17]

14. The answer to this question might be simply because he was flattered by Conant's request that he prepare such a case history for the general education course. Compare 'D', p. 275:

Who fed my name to Conant I'm not sure—there are various people that it could have been. But I had a reputation as the physicist who was president of the Signet Society, there were various things of that sort in my record. I was one of the two people Conant then asked to assist him. First time he gave this course out of that little book called *On Understanding Science*, which had been the Terry Lectures at Yale. I accepted with alacrity; and I've never quite forgotten that first time I met him. Here I was, not finished my physics thesis and being immune to this sort of material—I have by then read the page proofs of *Understanding Science*—being asked to go out and do a case study on history of mechanics for this course? Wow!

15. We can also specify roughly what the Aristotle epiphany consisted in; it consisted in *seeing* a pendulum as a swinging stone; see S, p. 120.

I always felt you had to do more [than Conant did in dealing with what comes before]; and that meant you had to do a stage set, within another conceptual framework, in order to get at these things. And that was what this [Aristotle experience] did for me. But the main thing is, it didn't really get me *interested* in history of science; and there are those who feel, and feel with some justice, that I never really did get to be a historian. I think in the end I did get to be a historian, but of a rather special narrow sort. I used to think—forgive me—that with the possible exception of Koyré, and maybe not with the exception of Koyré, I could read texts, get inside the heads of the people who wrote them, better than anybody else in the world. I loved doing that. I took real pride and satisfaction in doing it. So, being a historian of *that* sort was something I was quite willing to be and got a lot of kicks out of being, and did my best to teach other people to do. I'll come back to that. But my objectives in this, throughout, were to make philosophy out of it. I mean, I was perfectly willing to do the history, I needed to prepare myself more. I wasn't going to go back and try to be a philosopher, learn to do philosophy; and if I had, I'd have never been able to write that book! But my ambitions were always philosophical. ['D', p. 276]

Pay attention to this note of pride and supreme intellectual self-confidence, alongside the involuntary, very characteristic, vigilance towards his own thinking ('forgive me') that is built on the Aristotle experience—'better than anybody else in the world'. And yet this supreme virtuosity is also a form of narrowness and handicap—the *only* way he could do history.<sup>16</sup>

The second point I wish to make about the Aristotle epiphany of 'climbing inside someone's head' is to note that it is not immediately self-evident why Kuhn thought that this particular experience of understanding is one that the experience of psychoanalysis fosters. After all, on the face of it, psychoanalysis is a process in which one has only one's own thoughts to grapple with. Analysts are notoriously silent about their own mental processes.

16. To show how the rigours of historical methodology and psychoanalysis could be intertwined, let me quote from the commentary Kuhn offered me on the draft paper I submitted to him, which is discussed at length below. He wrote that the paper

is repeatedly distorted by being directed at other authors rather than at your positive points. . . . In efforts to refute, you often exaggerate (to the point of creating straw men) the position you're criticizing. . . . I suspect that it's your concern to refute and your satisfaction in having done so that leaves you feeling you've done your job on occasions when you've succeeded only in refuting (sometimes a non-existent position) without really having thought through your material yourself. This concern with setting others straight is one of the devices you use to protect yourself from the pain of fully disciplined engagement with your own material. (Excuse the amateur psychoanalysis, but I've some relevant experience.)

Kuhn's own portrait of his analyst doesn't encourage us to think that the process for him consisted of a mutual understanding, since the figure of the analyst he leaves us with is the snoring analyst, who is not going to be an obvious candidate as the Aristotle into whose thought processes Kuhn climbed in the course of his analytic sessions. No, it is much more obviously the experience of climbing into one's *own* thought processes that Kuhn must have had in mind. The psychoanalytic framework allows one to treat oneself as an other—as in Rimbaud's 'Je est un autre' that Jacques Lacan was so fond of citing. What is so interesting in psychoanalysis, then, for someone like Kuhn, is to find other selves within one that one initially approaches as alien selves which are to be understood by a process of understanding like that in his Aristotle epiphany. Discovering that there were parts of himself that were as alien and as erroneous but were finally as comprehensible as Aristotle catches something of the flavour of Kuhn's own analysis, I suggest.

However, in the recounting of his life and its development, Kuhn evoked another set of vivid scenes which I wish to contrast with the Aristotle epiphany; they share with it the visuality and imprintedness he emphasized again and again over many years. Throughout the *Neusis* interview Kuhn took the importance of the Aristotle epiphany for granted and did not feel called upon to describe it, perhaps because he had already done so in print on a number of occasions; his interlocutors fell in with this, but, as we shall see, they were eager to try and connect it with other stories he told them. The first of the three scenes I will now invoke was roughly contemporaneous with the Aristotle epiphany.

Kuhn had noted that the principal reason for starting analysis was his difficulty in relations with women—their almost total absence. One is not entirely surprised to discover that his mother, despite her enthusiasm for psychoanalysis, did not help in this area. Kuhn illustrated this point with a story of his mother and, for want of a better word, her tactlessness:

I remember when I first started going with a woman; I had not had the normal number of dates by the time I was out of graduate school, and there was a woman I saw more than occasionally. My mother, who had not met the woman, saw the two of us on a New York street and just said to me a few days later, 'I saw you and G . . . , and she's not the right person for you.' Agh!

Later in the same interview, Kuhn was asked about his relation to philosophy—which he'd explained as requiring him to pass via history. To make his point about philosophy, he told a story about G and a woman friend of hers.

After a while [G] gave a cocktail party in New York for me, to meet some of her friends. And I went, and I got to talking to a very beautiful—not so much beautiful but very striking, buxom, well-turned-out woman. I don't know what the conversation was, but suddenly as happens occasionally all the voices in the room dropped and I was heard saying (including by me), 'I just want to know what Truth is!' So, that's what it meant to me. ['D', p. 278]

Kuhn found it hard to date this scene; his interviewer then connected it with the Aristotle incident, though Kuhn was not particularly responsive to this idea.<sup>17</sup> The initial point I want to make about this scene was the sense of Kuhn watching himself—he actually remembers the scene because of the almost out-of-body quality of the recollection. (Perhaps the scene was the object of considerable work inside his then ongoing analysis, in which he positioned himself in many different ways within the scene.) The point of telling the story is to catch the young Kuhn's passion for philosophy on the wing: 'I just want to know what Truth is!' In further discussion of this with his interviewers, who implied that this was a not unusual portrait of an intelligent young physicist wanting to know the secrets of the universe, Kuhn gives a crucial clarification: 'remember, when I said that, I wasn't saying that I want to know what is true; I was saying I want to know what it is to *be* true. And that's not something that one gets to through physics' ('D', p. 278). To clarify: Kuhn didn't want to know the truth, he wanted to know what it was for a thing to be true. This question is neither metaphysics nor epistemology; he wanted neither to know the universe's essence nor be educated or reassured about the proper path to truth. Nor is it the path that leads immediately to the philosophy of language; he did not say that he wanted to know what it is for a statement to be well formed. The Aristotle epiphany lurks here: how could Aristotle's physics be 'true' and yet superficially contradict and be incommensurable with Galileo's 'truth'?

My hypothesis, then, within the chronological indeterminacy with which

17. The passage of the dialogue runs:

T. KUHN: I got to talking. . . . but suddenly as happens occasionally all the voices in the room dropped and I was heard saying (including by me), 'I just want to know what Truth is!' So, that's what it meant to me. And this may well be before I was associated with Conant. I can't date it quite that accurately, it certainly can't be long after. I may already have been in the Society of Fellows, but I think perhaps not.

A. BALTAS: It is very well connected to the Aristotle incident. They connect very well together.

T. KUHN: Yes, and it could have happened either before or after. My Aristotle experience certainly made it problematic, and I'm not sure quite what the problem had been earlier, if this was before that. So I really can't give it to you in terms that will be developmental. But from an early stage, that tells you something. ['D', p. 278]

Kuhn surrounds the story, is that this vividly remembered scene is like a failed version of the Aristotle epiphany. What lurks within this scene is potential embarrassment and exposure. Hearing oneself declare to a momentarily silenced crowded room this particular desire, to this particular desirable woman, has all the hallmarks of a dream of being naked rather than a triumph of intellectual mastery. Why, however, is the story told in an entirely different mode? Why, in particular, is the figure of the woman central? Note one thing about his description of the awkward young physics graduate student, unused to the company of women, in conversation with one of the alien creatures, in what I can only see as a Freudian self-correction in his description of her: 'a very beautiful—not so much beautiful but very striking, buxom, well-turned-out woman.' He could have just said, and it sounds like he had originally intended to say, 'a very beautiful woman.' Why did he need to change his description? Because the new description makes it clear that she is situated on a different axis from beautiful; she is a *desirable* woman, both for society at large and for him personally. This of course is the source of the lurking embarrassment within which the Thesis on Truth is embedded. And the contradictory themes in the scene—embarrassment, self-discovery, self-revelation—mean it is difficult to read in a stable and comfortable manner.

My reading may appear implausible: what connection is there between this social scene of gaucheness and intellectual eagerness and the private scene of a conceptual breakthrough which would determine an intellectual path for life? I hope to make the connection more plausible by noting a singularity in the *Neusis* interview which may help us disentangle this primal scene of Kuhn's philosophical quest. Two other scenes are recounted that also have the unusually vibrant and evocative quality of the scene with the striking woman at the party, and both these scenes figure Kuhn as confronted by his own truth through the words of wise women. The first of these dates from the mid-1960s, after Mary Hesse had written a very positive review of *Structure* in the June 1963 number of *Isis*, the most eminent American-based history of science journal. In the course of her review, she had glossed one of his book's theses as being that 'in a period of revolution there is not even a set of neutral data which can adjudicate between rival paradigms, because there is a sense in which a paradigm *determines its own data*.'<sup>18</sup> Kuhn remembered:

When I next saw her we were in England, and I remember walking with her and going into the Whipple Museum—it's another one of those im-

18. Mary Hesse, review of *The Structure of Scientific Revolutions*, by Kuhn, *Isis* 54 (Jun. 1963): 286.

printed images. She turned to me and she said, ‘Tom, the one problem is now you’ve got to say in what sense science is empirical’—or what difference observation makes. And I practically fell over; of course she was right but I wasn’t seeing it that way. [‘D’, p. 286]

The third scene dates from not long after the conversation with Hesse:

One of the people who had been invited to participate [in the 1965 conference] was Margaret Masterman—whom I’d never met, but of whom I’d heard, and what I’d heard about her was not altogether good, and it was largely that she was a madwoman. She got up at the back of the room in the discussion, strode toward the podium, turned to face the audience, put her hands in her pockets and proceeded to say, ‘In my sciences, in the social sciences’ (she was running something called the Cambridge Language Lab), ‘everybody is talking about paradigms. That’s the word.’ And she said, ‘I was recently in hospital and I went through the book and I think I found twenty-one’, twenty-three, whatever, ‘different uses of it.’ And, you know, they are there. But she went on to say, and this is the thing that people don’t know, although it’s more or less in her article, ‘And I think I know what a paradigm is.’ And she proceeded to list four or five characteristics of a paradigm. And I sat there, I said, my God, if I had talked for an hour and a half I might have gotten these all in, or I might not have. But she’s got it right! And the thing I particularly remember, and I can’t make it work quite but it’s very deeply to the point: *a paradigm is what you use when the theory isn’t there*. And she and I interacted then, during the rest of my stay, quite a lot. [‘D’, pp. 299–300; my italics]

The first scene posed Kuhn the Question of Truth; the second scene posed him the Question of Observational Data, of the Empirical; the third not only asked Kuhn what a paradigm is but also posed the Question of Theory. What is Truth? What function do empirical data have within paradigm-driven science? What function do scientific theories have within paradigm-driven science?

If the Aristotle scene is one of revelatory transformation, of triumph, these three other scenes with women are more reminiscent of baffled failure or of sudden impasse.<sup>19</sup> Yet they are tinged with unmistakable pleasure and verve, as if the failure being revealed in them is being relished. They are

19. There appear to be no other scenes of such vivacity described in the long interview, spread over a number of days. No encounters with men have quite this clarity of vision and description. There is the brief appearance of Gaston Bachelard, ‘a large burly man in his undershirt’, but the encounter that did take place lacked any spark (‘D’, p. 285).

scenes which stage a problem, give it body, as if thought itself is being concretised. They may be tributes to, or tributaries of, an unconscious structure: 'in order to be able to think, you need to let a woman get into your head'.<sup>20</sup> Of course the last two scenes, with Hesse and Masterman, are also classic illustrations of the effective function of external criticism, memorable only, one might surmise, for the fact that they fell on ears prepared, although not consciously, to hear it. And yet these are amongst the few, the precious few, imprinted scenes, scenes worthy of remembering in their fullness—or scenes seared onto the memory by forces and predicaments of which the subject of the memory is only dimly aware. However, all four scenes are marked by the cognitive style that made the gestalt switch such an attractive metaphor for Kuhn. Each scene has a transforming timeless moment at its heart; however, only the Aristotle epiphany has Kuhn coming out of the other side of the transformative moment. The three scenes with women as protagonist set Kuhn back on his heels; he is allowed a glimpse of a truth only for it to be that instant withdrawn.

One obvious reason for this difference might be that only the Aristotle epiphany offers him something entirely other; the three scenes with women present his own ideas as to the problem to be solved—Truth, Observation, Theory—so that the revelation is of a lack in himself—a problem to be solved where none existed in his consciousness before the moment of crystallisation or of switching. (Perhaps the scenes with the women conform elegantly to a Lacanian theorem, in which it is Woman, his other, who reveals to the subject his own lack.) Put in terms closer to Kuhn's own preoccupations, the subject of the Aristotle epiphany is a revolutionary scientist, revolutionary because he is capable of traversing the distance—in reverse—between Aristotle and Galileo; this revolutionary scientist is able in a blistering moment of recognition to find all the pieces falling into place in an entirely novel pattern. In contrast, the subject of the scenes with the women only recognizes problems and puzzles to be solved, which may or may not become anomalies—an admirable enough achievement, but the work of a 'normal' scientist, not a revolutionary.

A second obvious reason for the difference is that the Aristotle epiphany was essentially a historical discovery; it was the moment in which Kuhn discovered how to be a particular kind of historian of science ('better than anybody else in the world'). It was his contention, remember, that he owed to psychoanalysis the ability to be such a historian ('the level of my ability

20. I am not claiming that Kuhn's assertion of his question concerning 'Truth' in the conversation with the buxom beauty was the first occasion for such a thought, but the hushed silence and the specificity of the addressee clearly became for him a marker of a moment of crystallisation of this question as being very much his.

to do [history] . . . came out of my experience in psychoanalysis’); and the Aristotle epiphany, the moment when he succeeded in being such a historian, was this moment when he discovered how to get inside someone else’s head. It is also the exemplary instance of the exemplar, the case which teaches how thinking from case to case works. In contrast, the topics of the other scenes are philosophical—Kuhn the philosopher being taught a crucial lesson by women philosophers (the buxom beauty counts here as a philosopher without saying a word, but Masterman was a philosopher [as well as a computational linguist] and Hesse still is one).

There is still the question, Did psychoanalysis have any enduring direct intellectual influence on Kuhn, independent of the transformation in his life in the period 1946–48 which was both personal and intellectual? The answer, almost certainly, is no. Despite his continuing to move in psychoanalytic milieus in both California and Princeton, there is little evidence of an influence on him of the order of his reading of Aristotle, the classic historians of science (Koyré, Maier, and so on), or of the unquantifiable influence of Fleck or Polanyi. As an individualist and mentalist, Kuhn of course looked to psychology of some sort to back up his account of crisis and revolutionary science; but it was Piaget and Kant, Bruner and Postman rather than theorists of the unconscious to whom he turned. Yet at one point, probably in the early 1950s, Kuhn was, with the encouragement of Weiss, at least considering using Freud as one of the case histories in the Conant general education course at Harvard, but eventually he told Weiss ‘that Conant did not like Freud’ (‘CK’, p. S60). I do not think one can make much of this attempt to bring Freud within the compass of the sciences with which Kuhn was concerned, nor is there any evidence that psychoanalytic thinking played any role in the development of his own ideas, despite the fact that he would place emphasis on unconscious “preconceptions” or “prejudices” in science in the Lowell lectures of 1951 and later (‘CK’, p. S62). Psychoanalysis considered as a theoretical system or useful set of research tools peters out.

However, I do want to emphasize one aspect of Kuhn’s work which may be regarded as part of the legacy of his overall interest in psychology, including psychoanalysis. Put baldly, Kuhn always approached the problem of scientific knowledge as governed by psychological dynamics. Kuhn’s *influence* has overwhelmingly been in a very different direction—towards the sociology of scientific knowledge. But, as became notorious, he was out of sympathy for, even suspicious and critical of, these developments of his ideas. If Kuhn was no Kuhnian, one of the principal gulfs between him and his followers was his natural espousal of psychological accounts of scientific change and his lack of appreciation of the exclusively sociological turn

which, certainly in its Durkheimian versions, was vigorously antipsychological.

The 'psychological' turn of Kuhn's own mind is best known in his use of the gestalt switch to give content to the suddenness and lack of rationality underpinning a scientific revolution; normal science ultimately leads only to the recognition of anomalies and to crises. And these are terminated, not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch. Scientists then often speak of the "scales falling from the eyes" or of the "lightning flash" that "inundates" a previously obscure puzzle, enabling its components to be seen in a new way that for the first time permits its solution' (S, p. 122).

But the gestalt switch is only one of the psychological modalities that sat naturally in Kuhn's repertoire of descriptions of himself and of historical actors. He insisted on the fact, which those influenced by *Structure* found so difficult to accept, that he was an internalist historian of science, as he himself described these methods in 1968: setting aside the present-day science he knows, reading the 'textbooks and journals of the period he studies', reading journeymen practitioners before approaching innovators, trying to establish the problems which past scientists were attempting to solve, paying particular attention to 'apparent errors, not for their own sake but because they reveal far more of the mind at work than do the passages in which a scientist seems to record a result or an argument that modern science still retains.'<sup>21</sup> The goal of Kuhn's history was the revelation of the 'mind at work' in past science. His teaching in graduate seminars embodied these recommendations; the reading assigned consisted solely of papers and excerpts from technical books written by past scientific practitioners, with hardly any secondary sources ('useful background reading') assigned at all. A successful seminar presentation would be a reconstruction of the problems, the methods, and the argument found in these primary sources.<sup>22</sup>

What kind of history is this? It is 'internal' insofar as it ignores factors external to the set of problems determined by the scientific tradition: 'the practitioners of a mature science are effectively insulated from the cultural milieu in which they live their extraprofessional lives.'<sup>23</sup> But there is another sense of *internal* dominating this specific historical practice: what went on

21. Kuhn, 'The History of Science' (1968), *The Essential Tension*, p. 110.

22. A sample assignment, taken from Kuhn's graduate seminar on the history of thermodynamics for 10 February 1971: Lavoisier, *Traité élémentaire de chimie*, chap. 1; Lavoisier and Laplace, *Mémoire sur la chaleur*, pp. 7–25, 38–57, 'and look at the tables on pp. 32, 36, and 37'; Laplace, *Mécanique céleste*, bk. 12, chap. 1; S. D. Poisson, 'Sur la vitesse du son', *Annales de chimie* 23 (1823): 1–16; and Poisson, 'Sur la chaleur des gaz et des vapeurs', *Annales de chimie* 23 (1823): 337–42.

23. Kuhn, 'The History of Science', p. 119.

in the minds of the historical actors. The definition of *internal* has both sociocultural and mentalist-psychological determinations. Having made sure that it is legitimate to ignore the external conditions governing the scientific tradition being studied, the task of the internal historian of science is to reenact the mental processes of past scientists. The successful internal historian of science *becomes* the past scientist, repeats the processes of struggle, failure, and eventual apparent success. There appears to be little conventional history in this practise, if by history we mean a narrative that brings together diverse past facts into a causal account; it is simply the rethinking of men's thoughts after them so dear to Collingwood. What Kuhn adds to Collingwood is the struggle with incommensurability, for which the gestalt switch is Kuhn's personal presiding metaphor, the same great achievement that Foucault famously described: 'the thing we apprehend in one great leap, the thing that . . . is demonstrated as the exotic charm of another system of thought, is the limitation of our own, the stark impossibility of thinking *that*.'<sup>24</sup> Whereas Foucault sought to preserve the distance, the incomprehensibility of 'thinking that' (whilst analysing its conditions of possibility), Kuhn's programme was for historians to take that leap inside the heads of past scientists, to become past scientists. In Kuhn's historiography, historians are capable of changing places with scientists through the labour of overcoming historical incommensurability (the gestalt switch, the reconstruction of the internal dynamic of scientific advance).

Kuhn treated as entirely unproblematic and methodologically unexceptional, indeed advantageous, the one-way privilege of the historian who can become the past scientist. Writing in the early 1980s, some thirty-five years after he had first deployed this method, he prefaced his exposition of his classic example of revolutionary change as follows:

My account will invert historical order and describe, not what Aristotelian natural philosophers required to reach Newtonian concepts, but what I, raised as a Newtonian, required to reach those of Aristotelian natural philosophy. The route I traveled backward with the aid of written texts was, I shall simply assert, nearly enough the same one that earlier scientists had traveled forward with no text but nature to guide them.<sup>25</sup>

That key element of history, the irreversibility of time, disappears. Kuhn's historical time is Newtonian and prethermodynamic; it is reversible and

24. Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences*, trans. pub. (New York, 1970), p. xv.

25. Kuhn, 'What Are Scientific Revolutions?' p. 15.

even displays negligible hysteresis loss—yet another reason for doubting there is an objective measure of progress.

This symmetry and potential equivalence between present historian becoming past scientist and past scientist undergoing revolutionary transformation into ‘present’ scientist entails, according to Kuhn, despite the thesis of incommensurability, that a complete reconstruction of the past mental processes by which a science developed is possible. And Kuhn implies that even more is possible. The mentalist language that came so naturally to him as a reconstructive internalist historian extended from individuals to the scientific community of which they were a constitutive part. At times, particularly in the late 1950s and early 1960s, Kuhn wrote of the ‘consciousness of the scientific community.’<sup>26</sup> This was no offhand turn of phrase. This consciousness had an internal structure and dynamic, that of the transformation from normal science to the period of crisis, when divergent responses to anomalies which had been ‘pushed to the periphery of consciousness’ or ‘suppressed entirely’ precede the ‘fundamental reconceptualization’ Kuhn called a scientific revolution (‘FTE’, pp. 262, 263). The ‘one essential characteristic’ of all revolutions is the following process:

The data requisite for revolution have existed before at the fringe of scientific consciousness; the emergence of crisis brings them to the center of attention; and the revolutionary reconceptualization permits them to be seen in a new way. What was vaguely known in spite of the community’s mental equipment before the revolution is afterwards precisely known because of its mental equipment. [‘FTE’, p. 263]

Scientific communities have ‘mental equipment’, and this equipment is deployed in crucially characteristic ways in the cycle of transformation called a revolution. There are processes by which knowledge is kept at the periphery of consciousness and is then brought to full consciousness. Exploring the function of thought experiments allows Kuhn to state this paradox with maximal dialectical rigour: ‘thought experiments give the scientist access to information which is simultaneously at hand and yet somehow inaccessible’ (‘FTE’, p. 261).

There is a force, a suppressing force, at work, namely, the legitimate demands of normal science with its positive puzzle-solving imperatives. What the thought experiment can accomplish—while by definition adding nothing empirical, nothing new from nature—is the removal of this suppressing force.

26. Kuhn, ‘A Function for Thought Experiments’ (1964), *The Essential Tension*, p. 262; hereafter abbreviated ‘FTE.’

A more Freudian description of the paradoxes of consciousness and the unconscious could hardly be wished for. It is the constant easy recourse to a dynamic psychology—a psychology within which consciousness has a variable density, a complex topographic structure of ‘fringes’ and inaccessibility, of variable opacity and transparency, within which reasons and evidence are sometimes explicit, sometimes implicit, sometimes acknowledged, sometimes actively ignored—which I would claim is another heritage of Kuhn’s familiarity, both personal and intellectual, with the Freudian science. It is true that this element in the conceptual repertoire of Kuhn’s work was to fade away. Kuhn’s persistent interest, so antipathetic to many of his philosophical critics,<sup>27</sup> in the ‘sciences’ of sociology, psychology, and history of science as sources to be preferred to the neopositivist philosophy of science, was always developing and shifting; what started with the experience of the gestalt switch and incommensurability developed via the interest in Piaget’s genetic epistemology and experiments in perceptual transformations into reflections on learning (a child learning how to distinguish geese, ducks, and swans) that was Rosch’s 1970s prototype theory *avant la lettre* (bearing in mind that Rosch, like Kuhn, was very much *après Wittgenstein*).<sup>28</sup> It then shifted again towards the philosophy of language, theories of reference and meaning, towards attempting to show how scientists could live in a different world after a revolution. But at the moment of Kuhn’s maximal confrontation with his critics, in and around the famous London conference in 1965 where he was fiercely criticized by Popper and his disciples, the gauntlet Kuhn threw down to Sir Karl had the title ‘Logic of Discovery or Psychology of Research’.<sup>29</sup>

Whatever the complexities and changes over time of Kuhn’s psychological orientation, the connection I think I have established is quite sufficient: Kuhn’s most prized skill, that of ‘climbing inside heads’,<sup>30</sup> was linked to his personal psychoanalytic experience and remained his to the end.

I now turn to the other angle of my project: a Kuhnian account of psychoanalysis. But to do so immediately would make completely opaque why

27. See Kuhn, ‘Reflections on My Critics’ (1969), *The Road since Structure*, pp. 123–75; see his comments on Karl Popper, ‘Normal Science and Its Dangers’, p. 127.

28. See Kuhn, ‘Second Thoughts on Paradigms’ (1974), *The Essential Tension*, pp. 309–18; for commentary on and contextualisation of this development of Kuhn’s thinking, see Nancy J. Nersessian, ‘Kuhn, Conceptual Change, and Cognitive Science’, in *Thomas Kuhn*, ed. Thomas Nickles (Cambridge, 2003), pp. 178–211.

29. See Kuhn, ‘Logic of Discovery or Psychology of Research’ (1970), *The Essential Tension*, pp. 266–92.

30. Or “‘putting on a different kind of thinking-cap’”, as Herbert Butterfield’s expression had it, which Kuhn implies (‘P’, p. xiii) did not have an influence on him.

I think these two projects are, first, of any more than limited interest and, second, why they might be linked. To give an adequate answer to both these questions, I must engage in some autobiographical remarks concerning Kuhn.

I first read *The Structure of Scientific Revolutions* in the summer of 1968, when I was turning nineteen, a science student—my first realistic intellectual love was physical chemistry (to be even more specific, the equations of reaction kinetics in solution)—who had become somewhat disquieted with the tight and rigorous training rituals of the sciences and wished to find ways of discovering the ‘essence’ of science. So, back then, I was reading *Structure* alongside E. A. Burt’s *The Metaphysical Foundations of Modern Physical Science* and Ernest Nagel’s *The Structure of Science*.

Kuhn’s book was a revelation and not only because it endorsed some blurred intuitions of my own concerning the sciences. Like many other readers, I found it a deeply seductive book and a book speaking to its time. The theme of revolution and discontinuity certainly suited the cultural temper of the 1960s; Kuhn’s depiction of scientists as not being a race apart, of not being rational-method followers in their very bones, satisfied an iconoclastic streak that ran, and often still runs, through those drawn to the historical and sociological study of science. Kuhn, in the *Neusis* interview, specified the aporia—his word—to which *Structure* was addressed: ‘how it could be that the most rigid of all disciplines, and in certain circumstances the most authoritarian, could also be the most creative of novelty’ (‘D’, p. 308).<sup>31</sup> And the seductiveness of the book lay in both those features. It caught the ‘reality’ of day-to-day scientific work, certainly in my experience and I presume of many others, as being rigid to the point of being mind-suffocating; and it still recognized and ratified the revolutionary, world-transforming reality of the sciences, thus suppressing and preserving (as in the German verb *aufheben*) the streak of idealisation of and narcissistic admiration for scientific knowledge that runs through many if not all onetime would-be scientists. Looking back, and reading excerpts from Kuhn’s very earliest versions of *Structure*, such as the Lowell lectures of 1951, his depiction of what he later called normal science, together with his account of what in 1951 he called ‘unconscious’ ‘prejudice and preconception’ as constitutive elements of scientific practise (‘CK’, p. S62), struck chords with those familiar with the disciplined practises and thought patterns of modern science; these ‘nonrational’ elements thus explained both the heavily

31. He addressed precisely this aporia principally in the field of science education in the early paper in which he first used the term *paradigm* after which his 1970s collection of essays was named; see Kuhn, ‘The Essential Tension: Tradition and Innovation in Scientific Research’ (1959), *The Essential Tension*, pp. 225–39.

constricted character of most lab work and also why crises in science are as much akin to sectarian disputes as to rational debates. Prejudice and authoritarian practices thus explained the narrowness of science, the discontinuous character of scientific change, and the efficacy of scientific work.

So it was *Structure* that set me on course to become a historian and philosopher of science and in 1970 led me to be a graduate student at Princeton, where my Cambridge Director of Studies and mentor Bob Young thought I would acquire the discipline I was in need of.<sup>32</sup> The seminar I took with Kuhn in the spring of 1971 was on the history of thermodynamics, and he assigned me the problem of Joule's work on electrical conduction.<sup>33</sup> The paper I finally delivered was a sprawling piece of work on a quite different problem: the interrelationship between Joule's various experimental projects in electrochemistry, the condensation and rarefaction of gases, and the mechanical effects of electromagnetic engines. Kuhn thought my work was very important; he also thought it was appallingly written. He gave me, as he gave so many people, first and foremost himself, a very hard time on the fine details of argument and writing.<sup>34</sup> It took me many drafts and many months to produce a paper he was happy with; courtesy of Gerd Buchdahl's later editorial encouragement and with acute critical comments from Mary Hesse, it eventually became my first professional publication.<sup>35</sup> And when, a couple of years after writing it, I delivered it as a seminar paper in Cambridge, England, my own written commentary on it indicates I had already realized it was not just an argument about the importance of electrochemistry in the development of ideas concerning energy conservation but was

32. Though it was not a matter of destiny. I might well have gone to Harvard if I had been awarded the Kennedy scholarship for which I had also applied and been placed as 'first reserve', a setback I blamed at the time on my brush in the interview with Isaiah Berlin over eighteenth-century Newtonianism.

33. This was the seminar that Jed Buchwald, then an undergraduate, also attended; if I remember rightly his assignment was the more rebarbatively mathematical work of Thomson and Clausius. Kuhn points to Buchwald's subsequent career as a distinguished historian of science as the 'one exception' to the reflection that 'I haven't produced any children' ('D', p. 304). For Buchwald's reflections on Kuhn, see Jed Z. Buchwald and George E. Smith, 'Thomas S. Kuhn, 1922–1996', *Philosophy of Science* 64 (Jun. 1997): 361–76.

34. My papers from that period include Kuhn's four pages of single-spaced typed general comments on the monstrous first draft I handed him at the end of the seminar, and virtually every line of those eighty typed pages had comments, criticisms, and corrections; he wrote he had spent two days going over my paper; and the evidence of the detailed and general comments indicates as much. One particular comment he made may have had resonance with his own experience with criticisms of the term *paradigm*: 'I've spent two days with this paper, and I still don't know what you mean by the term [ETCH—Electrical Theory of Chemical Heat]. There are, I think, many different senses in which you use the term, and one result is to leave me with the feeling that no chemist in the nineteenth (early) century who talked of heat at all could fail to have the ECTH.'

35. See John Forrester, 'Chemistry and the Conservation of Energy: The Work of James Prescott Joule', *Studies in History and Philosophy of Science* 6 (Nov. 1975): 273–313.

an *'in vivo* illustration' of Kuhn's ideas concerning scientific change as set out in *Structure* and its 'Postscript—1969'. What I had been doing was showing Kuhn what his own ideas, according to my reading of them, looked like developed in a new setting. I had also, it should be noted, been showing him (and myself) what one version of a dynamic and by no means trouble-free relationship between professor and student might look like.

In the course of the preparation of the paper I had what I take to be a frequent, though no less foundational for that, historian's epiphany in the archives. The central idea of my paper was that Joule developed a highly individual theory of the electrochemical nature of heat to link together the various experimental setups with which he worked throughout the 1840s; this theory envisaged matter as consisting in hard atoms surrounded by rotating atmospheres of electricity. In the summer of 1971, while I was still trying to produce a paper that would satisfy Kuhn's high standards, that is, prove to him that my crude intuitions about the underlying 'logic' of Joule's work, his 'psychology of research', were correct, I visited Joule's hometown of Salford in Lancashire and got my hands on his manuscripts. By this time I was convinced that I had successfully climbed inside Joule's head and 'knew' that he thought of every new experimental situation in terms of this model. So it was a moment of triumph and vindication when I found in the manuscripts a graphical depiction of exactly what I was convinced Joule thought: a diagram that depicted atoms with strings coiled around them being pulled by some external force. For me, this was Joule's paradigm, his exemplar, with which he bound together electrochemical action in solutions, the rarefaction of gases, and the heat produced by a rotating paddle wheel immersed in water. But, in finding this manuscript diagram, I was not only proving myself right in my historical intuitions; I was proving to my own satisfaction that Kuhn's account of how scientists thought and practised was correct. This diagram demonstrated to me that Joule's science was Kuhnian science.<sup>36</sup>

What kind of Kuhnian science? The key Kuhnian term I am now em-

36. And the importance of this moment for me was the reason why I was so disappointed that in the published version of the paper the editorial staff substituted for a photocopy of the diagram in Joule's own hand a schematic, textbooklike version set up by a modern draughtsman, with neat little atoms and strings. The 'reality' of that textual evidence had become fetishized in my own mind.

It also prompts the thought that history may be just as much of a 'predictive' science as some natural scientific disciplines. My finding of the diagram was truly exhilarating because it was what I *expected* to find; it was the opposite of the archetypal archival discovery of the unexpected. For some considerations concerning the opposite of expectation and prediction, namely chance, serendipity, and luck, see the recent article by another participant in the Kuhn seminar of 1971, James E. McClellan III, 'Accident, Luck, and Serendipity in Historical Research', *Proceedings of the American Philosophical Society* 149 (Mar. 2005): 1–21.

ploying is *exemplar*—not *paradigm*, or *revolution*, or *crisis*, or even *normal science*. Kuhn's 'Postscript—1969', together with the nitty-gritty task of writing a research paper in the history of a specific science had, without my being entirely aware of it, concentrated my attention almost exclusively on that part of the concept of paradigm Kuhn now wished to call exemplar—'models, particularly grammatical models of the right way to do things' ('D', p. 298), or 'standard examples' ('P', p. xix). However, the rotational model Joule employed, which one might, on analogy with Kuhn's own pendulum exemplar of seventeenth-century mechanics, call the flywheel exemplar, was neither quite normal nor really revolutionary science; it could even have been described as a heuristic model as much as an exemplary solution ripe for extension to other such puzzles. It *became* revolutionary when it was applied to or was displaced into another domain by other scientists (William Thomson and others); yet Joule himself showed no signs of crisis or of gestalt switching, but rather of a relentless and extremely ingenious campaign of extension of his exemplar across a wide range of experimental set-ups. Joule was doing normal science, deploying to perfection his Kuhnian exemplar; but nobody else was interested in his theory. The results he had achieved with it were subversive of current scientific dogma, but not in the immediate fields he laboured in. In other words, I found that I, restricting myself to Joule's work, was doing Kuhnian history without the concept of revolution or crisis. It seemed to me that Kuhn's own best practise as a historian did not *require* these concepts, but did require the concept of the exemplar.<sup>37</sup>

It is common knowledge how contested the concept of paradigm became and how, in many ways, Kuhn bitterly regretted his use of it. The following passage from Kuhn's *Neusis* interview is extremely pertinent, revealing, and accurate:

*Paradigm* was a perfectly good word, until I messed it up. I mean, it was the right word at the point where I said, you don't have to have agree-

37. In response to Toulmin's questions concerning what counted as a revolution, Kuhn provided a survey of a number of episodes in the history of science, one of which was the following: 'the Joule-Lenz law relating the heat generated in a wire to the resistance and current was a product of normal science, for both the qualitative effects and the concepts required for quantification were in hand.' He then went on to clarify how Toulmin's question could be put to better use: 'to answer the question "normal or revolutionary?" one must first ask, "for whom?"' (Kuhn, 'Reflections on My Critics', pp. 145–46). Kuhn's paper was written in 1969 and published in 1970; there is a similar passage worrying away at the Joule-Lenz law in the 'Postscript—1969' to *Structure*; see *S*, p. 183. The problem he had asked me to address in the seminar concerned the normal science of Joule's work on the heating effect of the electric current, but I had abandoned this question in favour of the much more interesting question, For whom? Joule was a chemist, I averred, who ended up having mathematical physicists as his audience. I was not aware at the time of the Toulmin-Kuhn dialogue.

ment about the axioms. If people agree that this is the right application of the axioms whatever they are, that this is a model application, then they can disagree about the axioms; just as with logic, without its making any difference, they can disagree about the axioms, they can switch axioms and definitions quite freely back and forth, and sometimes do. Here in physics, if you switch axioms and definitions you change to some extent the nature of the field. But the notion that you could have a scientific tradition in which people agreed that this problem had been solved, although they could still disagree vehemently about whether there were atoms or not, or something of that sort. Paradigms had been traditionally models, particularly grammatical models of the right way to do things. ['D', p. 298]

When Kuhn first mobilized the term *paradigm*, then, it covered the notion of model, model application, models of the right way to do things, the right way to go on. In the 'Postscript—1969' to *Structure*, he described this aspect of paradigm: 'shared example is the central element of what I now take to be the most novel and least understood aspect of this book' (S, p. 187). Yet Kuhn also used it to cover what he would later call the disciplinary matrix: what scientists share, consisting in diverse elements of various sorts—metaphysical commitments, symbolic generalizations, foundational tautologies, values (see S, pp. 182–87). This is the sense of paradigm that many of Kuhn's readers took to be equivalent to something like a *Weltanschauung*, a worldview, that worldview in which scientists live and through which their observations make sense—a kind of mansion of theory together with agreed-upon commitments as to what the world is like.<sup>38</sup> (Later Kuhn recognized that his lax broadening of the sense of *paradigm* came to include anything scientists agreed upon, whereas his original sense showed how it was that scientists come to agree on the *specific* practises that define their field *without* requiring assent to answers to foundational questions.) And many of Kuhn's readers wanted to be reassured that they themselves already lived in such a mansion, or, if it turned out that they were just living in temporary shacks, how they could go about turning these into the mansion to which they could grant the grand name of Science. I think it is reasonably fair to say that scientists and general readers have been most drawn to this misreading of paradigm as worldview, as epistemological shelter from the storm; in stark contrast, the abiding influence of Kuhn in science studies, in the history of science, and in the sociological and anthropological study of science stems from the sense of paradigm as shared example.

38. For such an account, see Frederick Suppe, 'The Search for Philosophic Understanding of Scientific Theories', in *The Structure of Scientific Theories*, ed. Suppe, 2d ed. (Urbana, Ill., 1977), p. 125.

To cast new light on this, not original light, but light from a different source, it is worth noting that when Kuhn discusses paradigm in the overarching sense, he is drawing on his initial insights from the late 1940s concerning the discontinuous character of science, its revolutionary mode of transformation, and the ubiquity of crisis rather than rational debate in such transformations. His original exemplar of a scientific revolution had been the shift from Aristotelian to Galilean mechanics, the Copernican revolution.<sup>39</sup> The source of these ideas lay in the period of his initial reading for the preparation of the course on mechanics for Conant's curriculum in 1947—including the Aristotle epiphany. However, Kuhn had another independent, persistent, and long-standing preoccupation: the peculiarity of the scientific education of contemporary scientists and the various attempts to improve it, of which Conant's course was one. In the application Kuhn wrote for a Guggenheim fellowship in early 1955, he remembered his immediate postwar view as having been that “many of the misconceptions [concerning the sciences] could be traced to the elementary courses designed to supply future scientists with problem-solving techniques; and I had discussed at length with friends the desirable characteristics of an alternative approach for non-scientists” (‘CK’, p. S55). It is Kuhn's acuity of perception concerning those educational problem-solving techniques, quite obviously *while he himself was going through that process of education* (that is, in the period 1941–48), that would eventually grow into the view of normal science as puzzle-solving activity, into his entirely original view of the importance and mode of operation of textbooks, and thence to the concept of paradigm qua model solution, qua exemplar. Symptomatically, in the 1969 postscript, Kuhn explains what he means by ‘shared examples’ by immediately discussing ‘the problems encountered by a student in laboratories or in science texts’. The clinching argument comes from the process by which the student solves the difficult problems at the end of a chapter they have understood perfectly, a process by which the student discovers ‘a way to see his problem as *like* a problem he has already encountered. . . . The resultant ability to see a variety of situations as like each other . . . is, I think, the main thing a student acquires by doing exemplary problems’ (S, p. 189). Kuhn then turns to historical episodes, perceiving the same process at work there—with his favourite exemplar of the pendulum. We might say, in crude terms, that the perception of the exemplar as a model of appropriate similarity relations came from Kuhn's observations of his own edu-

39. As he put it later, ‘my subsequent search for best readings has often been a search for other episodes of the same sort. They are the ones that can be recognized and understood only by recapturing out-of-date ways of reading out-of-date texts’ (‘P’, p. xiii). This, again, is an apt characterisation of the incompletely conscious project I engaged in with Joule's texts.

cation as a physicist. The term *paradigm* came to him in early 1959 while he was drafting, with difficulty, the chapter 'Normal Science' for *Structure* and his first published usage of the term was, appropriately enough, in a paper prepared for an audience of educationalists at 'The Third (1959) University of Utah Research Conference on the Identification of Scientific Talent', in which he emphasized that the steady advance of science is guaranteed not by the new buzzword of 'divergent' thinking but by its supposedly stodgier and limited opposite, 'convergent' thinking, that is, tradition-bound research on problems which are 'almost always repetitions, with minor modifications, of problems that have been undertaken and partially resolved before.'<sup>40</sup> No doubt the undeniably iconoclastic Kuhn enjoyed emphasizing the prodigious creativity of the narrowly constrained, repetitious work characteristic of the sciences; but his new concept of paradigm was also designed—and this was the revolutionary feature of his account—to show how students or practitioners of science did not need to agree on axioms or even make these axioms explicit in order to know how to go on.<sup>41</sup> Hence he arrived at the admirable formulation quoted above: a paradigm is what you use when 'you don't have to have agreement about the axioms.'

What Kuhn was pointing to was the established practise of solving problems using standard methods of deriving similarity and dissimilarity relations without recourse to axioms or, often enough, theory. 'They can, that is, agree in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it' (S, p. 44). This view of practises of classification and rule following is very much akin to the *general* account offered by Wittgenstein in *Philosophical Investigations*, as has been subsequently pointed out by a number of commentators, including Kuhn himself ('I'm a little surprised that I haven't had my nose dragged through Wittgenstein's use of [the term *paradigm*]' ['D', p. 299]—though it should be remembered that Kuhn makes crucial and explicit use of Wittgenstein at a key moment in the development of the argument of *Structure*, though without any reference to his use of *paradigm*) (see S, p. 45). Kuhn's problems with paradigm prompted him to look for terms that belonged to the same family. Hence exemplar. But there is another member

40. Kuhn, 'The Essential Tension', p. 233.

41.

Then I tried to write a chapter on normal science. And I kept finding that I had to—since I was taking a relatively classical, received view approach to what a scientific theory was—I had to attribute all sorts of agreement about this, that, and the other thing, which would have appeared in the axiomatization either as axioms or as definitions. And I was enough of a historian to know that that agreement did not exist among the people who were [concerned]. And that was the crucial point at which the idea of the paradigm as model entered. Once that was in place, and that was quite late in the year, the book sort of wrote itself. ['D', p. 296]

of this family which Kuhn did not choose immediately (but which, as we will see, he recognized did belong to the same family), and that is the case.

There is a family of disciplines that work with cases. They are suitably obvious, displaying the sort of obviousness which hovers between the banal and the crucially overlooked: medicine, law, social work, management science, and the sort of psychology in which there are clients or patients. There are a number of different ways of linking them together to give them suitable ties of kinship.<sup>42</sup> One might note that these disciplines have a professional and sometimes a legal obligation to treat their objects as persons—if only in the original Roman legal sense of the public masks of citizens. Another approach might be to seek out the genealogy of these methods for treating persons, and here the most salient line leads to the law and in particular to the tradition of common law with its articulation of case and precedent. Given the original disciplinary context of the professions—traditionally law, medicine, and the church—from which ‘cases’ emerged, the term *case* signals that, from at least one party’s point of view, the form of writing or discussion in these case-based disciplines will always remain attached to a specific individual; epistemically, the case will always be nailed down to the level of the individual. It is the task of the professional community’s internal communications to tie cases together in a rational and defensible network. Cases and the networks in which they are embedded are also peculiarly vulnerable to the forces working to close and reopen cases. Closing a case requires a great deal of work.

Such cases often look even more like shared examples than the exemplars to which Kuhn referred in his ‘later’ period do. This is in part because of the epistemic nailing down referred to above; for the clinical physician, the courtroom lawyer, and the psychoanalyst, there is always a professional obligation to talk and deal with cases and not dissemble their specificity in the work of abstraction and theory into which many nonhuman scientists (that is, scientists who do not deal with human beings qua professionals) feel obliged to translate their day-to-day practises. Hence these disciplines have a different relation to theory. Theory can always be demoted in a gesture towards the real, the empirical. Yet such gestures do not always take the form of the introduction of new, previously unaccounted-for or unnoticed empirical facts, new facts of the matter. The function of appeals courts is clarifying here. The appeal is generally brought on questions of law, the facts being assumed to have been safely established by the first hearing. Thus this reopening of a case typically involves the question, Do the facts of this case

42. For an earlier attempt, see Forrester, ‘If *p*, Then What? Thinking in Cases’, *History of the Human Sciences* 9 (Aug. 1996): 1–25.

legitimately fall under precedents X, Y, or Z (or, in the realm of statutory law, of laws P, Q, or R), as asserted in the first hearing?

Innovative higher-court rulings often involve the reorganization of the facts of the matter so that they fall into a new pattern under a different law or, most interestingly, under a new interpretation of a law or precedent. *Roe v. Wade* is well-known for its innovative interpretation of the Constitution of the United States, particularly those parts relating to ‘personal, marital, familial, and sexual privacy said to be protected by the Bill of Rights or its penumbras’, as Justice Blackmun put it.<sup>43</sup> Less well-known is the manner in which it was innovative in its interpretation of the sociopolitical context of the Hippocratic school of medicine, employing the work of the distinguished historian of medicine L. Edelstein to show, in a typical example of common-law reasoning, that the Hippocratic Oath’s prohibition of abortion ‘originated in a group representing only a small segment of Greek opinion’ and was, when placed in appropriate sociohistorical context, “‘a Pythagorean manifesto and not the expression of an absolute standard of medical conduct’” (*RvW*, p. 715). Such are the surprising uses of the history of science and medicine; such are the ways in which legal argument manoeuvres cases into new proximities and distances from prior law, code, and precedent.

It is worth pausing over this interesting episode because it is a clear example of how one works a case to produce a new, in this case revolutionary result. *Roe v. Wade* involved not only a woman arguing that the Texas anti-abortion laws were unconstitutional but a physician arguing that these laws were a violation of his ‘right to practice medicine, rights he claimed were guaranteed by the First, Fourth, Fifth, Ninth, and Fourteenth Amendments’ (*RvW*, p. 711). Blackmun’s opinion foresaw that, even if the physician were granted the right to perform abortions because of his right to practise medicine untrammelled by state intervention, the physician might fall foul of the Hippocratic Oath. Hence he rendered that oath in a new local form, restricted to Pythagorean doctors and to others like them who lived and worked in a local community in which practices such as abortion were contrary to custom. In other words, he rendered that part of the oath as pertaining *only* to such communities, in the same way that a theologian or lawyer might take the Pentateuch’s commandment that forbids killing and make it applicable only to communities which *locally* had a custom for disapproving of killing people who are not enemies in war. The Court’s opinion consisted in a vigorous relocation of abortion in relation to the right to

43. Justice Harry Blackmun, ‘Opinion of the Court’, *Roe et al. v. Wade* 93 S. Ct. 714 (1973); hereafter abbreviated *RvW*.

privacy and also in relation to the customs of a community within which the ethical duties of physicians are to be relocated. Its neutralization of the Hippocratic Oath was—who could not be aware?—part of a larger strategy involving the common law's relation to the quickening of life. At each of these stages in the argument, new similarity and dissimilarity relations are created. And it is no accident, I am sure, that such modes of argument in ethico-medical situations were becoming pressing and innovative in the late 1960s and early 1970s. Blackmun's opinion is of a piece with the freeing up of ethico-medical cases as a result of the exposure of doctors to greater legal contestation and the possibilities that technological innovation, such as life-support machines, kidney dialysis, and transplant technologies, were making available to patients and doctors. Many of these new possibilities would be resolved in classic, precedent-setting cases, which settled into local communities as shared examples for guiding best practise, involving remarkable medico-scientific-legal manoeuvres and innovations concerning these similarity relations. The development of case-based disciplines has been a significant part of the means by which law and the initiatives of interest-based local groups have replaced politics—or how surprising alliances have been developed to manage the new biopolitics—in the last third of the twentieth century.<sup>44</sup>

We are all aware of how saturated our hybrid socio-legal-medico everyday lives have become with such matters. And the most cursory examination of such cases shows very clearly how reasoning in cases, so familiar to Anglo-American common-law traditions, operates in conjunction with new scientific and medical technologies, without any necessity for the development of sophisticated, freestanding theory. There may once have been a dream of an abstract and self-consistent theoretical science of law, but the courts and the lawyers carry on quite happily without any hint of such a dream becoming real. The case-based disciplines reason analogically, creating complex networks of similarity and dissimilarity relations, often nested in heterogeneous hierarchies, with no guarantee of self-consistency or of the noncontradictory character of these overlapping categories. These truly are the disciplines that work with shared examples.

If we take this path via the case-based disciplines, it has a surprising consequence for our views of Kuhn. Coming, when all is said and done, from theoretical physics, priding himself in his historical work on his mastery of the technical content of the sciences whose history he studied (being im-

44. From a vast literature, see David J. Rothman, *Strangers at the Bedside: A History of How Law and Bioethics Transformed Medical Decision Making* (New York, 1991); Albert R. Jonsen and Stephen E. Toulmin, *The Abuse of Casuistry: A History of Moral Reasoning* (Berkeley, 1988); and Jonsen, *The Birth of Bioethics* (Oxford, 1998).

mersed in the technical details is the same as being inside historical protagonists' heads), Kuhn arrived at an account of the reasoning found in *all* the natural sciences that is much more obviously the reasoning found in the case-based disciplines: reasoning and working with shared examples, establishing on a day-to-day basis key relations of similarity and dissimilarity, which, under certain circumstances, but not in the normal disciplinary run of things, can have revolutionary effect. This was surely the reason for Kuhn's presentiment that his bypassing of Wittgenstein's discussion of paradigms (and of rule-following in general) merited him having his nose dragged somewhere; Wittgenstein's account of paradigms and the map of similarity relations for such terms as *chair* or *leaf* or *game*—Kuhn's examples taken from Wittgenstein<sup>45</sup>—shows how we are absolutely fluent in the use of such terms without having antecedent definitions or rules for applying them. 'The existence of such a network sufficiently accounts for our success in identifying the corresponding object or activity', as Kuhn put it in *Structure*. Where Kuhn expected to have his nose rubbed somewhere was, I suspect, in the opening sentence of the next paragraph: 'Something of the same sort may very well hold for the various research problems and techniques that arise within a single normal-scientific tradition.' Having invoked and used the argument from Wittgenstein, Kuhn then slips easily into that entirely Wittgensteinian—or is it Kuhnian?—characterisation of science I'm here repeatedly evoking:

Scientists work from models acquired through education and through subsequent exposure to the literature often without quite knowing or needing to know what characteristics have given these models the status of community paradigms. And because they do so, they need no full set of rules. . . . Paradigms may be prior to, more binding, and more complete than any set of rules for research that could be unequivocally abstracted from them. [S, p. 47]

This account will apply equally well to case-based disciplinary workers. Indeed, this account applies much more obviously to the practises of such disciplines precisely because there is often (though not necessarily) no grand apparatus of rules, theories, formulae, or codifications erected as the official or public face of the discipline's epistemic profile. In other words, everything Kuhn said about the natural sciences applies with even greater force to these disciplines. And hence the question as to the *specificity* of reasoning and practise, of going on, within the natural sciences becomes

45. See Ludwig Wittgenstein, *Philosophical Investigations*, trans. G. E. M. Anscombe (Oxford, 1953), §§65–80.

acute for Kuhn precisely because the account of reasoning with shared examples he discovered as the heart of science applies so much more obviously elsewhere. As Kuhn's close friend Joe Weiss once remarked about *Structure*: 'it's about the way people think.'<sup>46</sup>

One reaches a similar conclusion if one approaches Kuhn's arguments from another angle. When he drew parallels, following Piaget, between the psychology of children's reasoning about problems and the psychology of scientists or, somewhat later, between how children acquire the ability to recognize ducks, geese, and swans and how scientists discover quarks, neutrinos, and bosons, he made the implicit assumption that the psychology of children and the psychology of scientists, or the ordinary-language acquisition of children and the acquisition of meaning and reference for technical terms in a scientist's lexicon, are straightforwardly comparable. This assumption—that there is an unproblematic continuum between the workings of common sense and those of science (that is, the reasoning of members of an esoteric knowledge-producing community)—is not shared by all; indeed, it is most explicitly repudiated by the philosopher of science whose emphasis on the discontinuity between scientific theories over time and between scientific theories and ordinary knowledge has often been likened to Kuhn's—Gaston Bachelard. It was precisely because, in Bachelard's view, scientific thinking developed only through the *repudiation* of common sense, through the *overcoming* of the epistemological obstacles presented to scientific advance by ordinary experience, through the *purification* of scientific thought by expelling the seductive images (complexes) and exemplary experiences (the experience of staring into a fire induces the reverie of concentrated power and thus becomes the source of all dreams of depth), that the history of science would have to be a 'psychoanalysis of objective knowledge'—his equivalent to Kuhn's (psychoanalytic) getting inside the heads of historical protagonists.<sup>47</sup> The assumption of the continuity of psy-

46. Nat Kuhn, email to author, 23 June 2006, from a conversation between Joe Weiss and Nat Kuhn.

47. See Gaston Bachelard, *La Formation de l'esprit scientifique: Contribution à une psychanalyse de la connaissance objective* (Paris, 1938). Incidentally, in this connection the footnote given in 'D', p. 284 n. 11, is almost certainly incorrect. Kuhn states: 'The only thing of his [Bachelard's] I'd read was that *Esquisse d'une Problème Physique*'—and the footnote cites *La Philosophie du non: Essai d'une philosophie du nouvel esprit scientifique* (Paris, 1940), a text in which Bachelard does discuss epistemological obstacles; the *non* of the title is explicitly about the necessity of science breaking with, saying *no* to, common sense. Kuhn had almost certainly read not this text but Bachelard's earlier *Étude sur l'évolution d'un problème de physique: La Propagation thermique dans les solides* (Paris, 1927), which is concerned with the development of heat theory and the revolution associated with Fourier's work in the early nineteenth century, which was a major historical preoccupation of Kuhn's both in his 1955, 1958, and 1961 papers on the Carnot cycle and the related but larger canvas described in his classic paper of 1959 on energy conservation; see Kuhn, 'Energy

chologies is all-pervasive in Kuhn's work, in contrast to Bachelard's dynamic tension between the unstable scientific psychology and its original source—and pathological other—common sense (including earlier scientific theories).<sup>48</sup> It is the Kuhnian paradigm that closes off the scientific community to the outside world and is also what the members of that community have in common (see *S*, p. 176). Yet why is this crucial element not open to all? The answer in the end lies in the rigorous training and hands-on induction of members of the community, a description that highlights socialization, a social-psychological process. Unlike Bachelard, Kuhn was forced to discover the difference between scientists and nonscientists in their socialization and their community habits. But are these convergent, agreement-dominated, tacit, and refined traits exclusive to scientists? One is as likely, perhaps even more likely, to find them in communities of lawyers as in chemists, in large part because, unlike pre-Kuhnian scientists, the philosophers of law had recognized and ratified the logic of case-by-case reasoning—what Kuhn was to call research conducted within a paradigm.<sup>49</sup> If this and the arguments above are correct, then Kuhn's account of the sciences ended up insufficiently targeted on the research practises of those scientific disciplines to which he devoted his principal writings. In conclusion, I want to return to Kuhn's development as a historian of science to add one further twist to the story of his relationship with paradigms, exemplars, and cases. The point may even add a further twist to a psychoanalytic view of Kuhn.

As I mentioned above, Kuhn's entry into history of science came from his contact with Conant, president of Harvard College, when Conant was setting up the program of general education for nonscientists. Leaving to one side the larger pedagogical aims of Conant's project, the method he advocated—and put into practise in *On Understanding Science* and then again in *Science and Common Sense*—was the method of the case history, which came to fruition with the *Harvard Case Histories in Experimental*

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Conservation as an Example of Simultaneous Discovery', *The Essential Tension*, pp. 66–104. It does matter which text of Bachelard's Kuhn was familiar with because Bachelard represents a revolutionary break with the Kantian tradition so unexpectedly dominant in midcentury philosophy of science—in Cassirer, Piaget, and Kuhn (whom Peter Lipton described succinctly as 'Kant on wheels' [Peter Lipton, 'Kant on Wheels', review of *The Road since 'Structure'*, by Kuhn, and *Thomas Kuhn: A Philosophical History for Our Times*, by Steve Fuller, *London Review of Books*, 19 July 2001, [www.lrb.co.uk/v23/n14/print/lipto1\\_.html](http://www.lrb.co.uk/v23/n14/print/lipto1_.html)])

48. Incidentally, at a lunch meeting in New York City on 14 July 1979, I asked Kuhn about his view of Bachelard (although I suspect I did link his name with those of Canguilhem and Foucault at the time), and he indicated, characteristically, that he had not read as much of his (or their) work as he would have liked to and perhaps should have.

49. For the still classic statement of this mode of legal reasoning, see Edward H. Levi, *An Introduction to Legal Reasoning* (Chicago, 1949).

*Science*. In the introduction to this two-volume work, Conant justified the use of the case history form as follows: ‘The purpose of the case histories presented in this series is to assist the reader in recapturing the experience of those who once participated in exciting events in scientific history. The study of a case may be to some degree the equivalent of the magical operation . . . of transporting an uninformed layman to the scene of a revolutionary advance in science.’<sup>50</sup> This piquant vision of the benefits of the case method for the general (nonscientific) reader may have been particular to Conant, but his use of the case method as the preferred pedagogic method was by no means original. Quite the opposite: if there was one man one might expect to employ such a method, it would be the president of Harvard.

The case method of teaching, first encouraged by President Eliot in the late nineteenth century at the law school, transposed to the medical school under Cannon and Cabot, established as the preferred method of teaching at the Harvard business school from its origin in 1911, was distinctively and indelibly the trademark of Harvard—and one of its most successful exports and assurances of growing preeminence amongst universities.<sup>51</sup> Conant would himself have come into contact with the case method in his years as an undergraduate at Harvard during World War I, when Kuhn’s father was his classmate. In transmitting the case method, developed in order to render jurisprudence a ‘science consisting of a body of principles to be found in adjudged cases’<sup>52</sup> but by the turn of the century already additionally described as the Socratic method by which students learned to position themselves in the dialectic of legal argument, different disciplines and knowledge sectors gave it quite different rationales and ideological authority; the historical beauty of the case method was always its ability to fit with and embody diametrically opposed and contrasting educational philosophies—from the 1880s to today. So it is not surprising that Conant’s mature use of the case method was different in explicit purpose from that of the law school or business school; he envisaged it as ideal for educating nonscientists about science, that is, educating people who couldn’t ‘really’ understand science. For Conant, the case history bridged—and defined—the gap between ‘real’

50. James Bryant Conant, introduction to Conant et al., *Harvard Case Histories in Experimental Science*, 2 vols. (Cambridge, Mass., 1957), 1:ix.

51. See Robert Stevens, *Law School: Legal Education in America from the 1850s to the 1980s* (Chapel Hill, N.C., 1983), and, for a recent review of historiography on the case method at Harvard law school, Bruce A. Kimball, ‘The Langdell Problem: Historicizing the Century of Historiography, 1906–2000’, *Law and History Review* 22 (Summer 2004), [www.historycooperative.org/journals/lhr/22.2/kimball.html](http://www.historycooperative.org/journals/lhr/22.2/kimball.html)

52. Quoted in Stevens, *Law School*, p. 56.

understanding of science (scientists' science) and what a properly educated undergraduate or layman might be able to understand.<sup>53</sup>

Kuhn's first book, *The Copernican Revolution* (1957), was the most substantial fruit of Conant's programme; Conant even contributed a lofty foreword, defending the project of the *Harvard Case Histories in Experimental Science*, which Kuhn's own preface also endorsed with the explicit aim of supplying reading for the Harvard Courses in General Education. And, in that connection, Kuhn wrote the following sentence, which can only give us, burdened by hindsight, pause: 'Since students in this General Education course do not intend to continue the study of science, the technical facts and theories that they learn function principally as paradigms rather than as intrinsically useful bits of information.'<sup>54</sup> This usage of the term *paradigms* is entirely in keeping with a view of examples as illustrating a more general truth or concept, which is the real but inaccessible object of the communicative exercise. Kuhn here appears to conform to the Conant view of paradigms and examples as pedagogically useful but secondary and incidental to the 'real' stuff of science—behind which lurks the high-positivist ideal of 'the royal road to the really real', as Clifford Geertz characterised it in his magnificent elege to Kuhn.<sup>55</sup>

Within three years of writing this, Kuhn had established 'The Priority of Paradigms' (as he called the 'Wittgenstein' chapter of *Structure*); he had established that it is the rules and higher-order theories which are secondary and incidental to the 'real' stuff of science—which consists in working with shared examples. What Kuhn had to discard between 1957 and 1959 was Conant's hierarchy of use associated with the distinction between scientist and nonscientist. He had to discard the view that the Harvard case histories—paradigms—were science for nonscientists. In doing so, he had to return to the older Harvard view of cases, one that Conant had discarded in order to make cases part of a general rather than a hands-on education. In 1953, Kuhn would only have had to visit another Harvard school, that of business, to find the case method elaborated as finding the best solutions

53. For further discussion of the importing of the Harvard case method by Conant into the history of science, see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, 1997), p. 55, and Fuller, *Thomas Kuhn: A Philosophical History for Our Times* (Chicago, 2000). Unlike Galison, Fuller fails to recognize the internal logic of the case method, while recognizing and making an interesting case for the importance of Conant's pedagogical views and strategies as transmitted to Kuhn by himself and by Nash; however, his argument culminates in the entirely insufficient formula: Kuhn's views of science are 'Nash's pedagogical technique pumped up with ontological gas' (p. 201).

54. Kuhn, *The Copernican Revolution* (Cambridge, Mass., 1957), p. ix.

55. Clifford Geertz, 'The Legacy of Thomas Kuhn: The Right Text at the Right Time', *Common Knowledge* 6 (Spring 1997): 3.

to problems for dealing with 'human relations and administration';<sup>56</sup> but this business-school usage still followed the Conant line of thought in considering case teaching to be inappropriate for serious liberal arts subjects such as literature and physics, in which only didactic methods suited to their theoretical orientation were appropriate. Kuhn had to free himself from the elite pedagogical view of paradigm and thus return to his own initial perception, derived from his entirely scientific education, that puzzles and textbook examples were not simply the pedagogic means to theoretical clarity; they were, not the ladder up which one climbed so as to be able to throw it away once one had reached the top, but the heart of both education and practice of the sciences, above all for the scientist.

Masterman had summed up Kuhn's thesis with a memorable phrase: '*a paradigm is what you use when the theory isn't there.*' This was precisely the purest, least qualified, statement of Kuhn's argument in *Structure*. A weaker version of this thesis will help us see what the stronger, Kuhnian version is. The weaker version would claim that there is an established theoretical core to science, and it is only at the frontiers of research that, by definition, theory is somehow insufficient, and therefore paradigmatic examples are needed to guide problem solutions. Kuhn's stronger version is that the extensive agreement amongst scientists indicates that they do not even have to agree on this theoretical core; science, real science, is paradigmatic through and through. Then what is theory for? Theory must then be performative rather than constative, to employ Austin's useful terms, although Kuhn never showed any interest in them. The symbolic generalizations—which Kuhn emphasized were more interesting as definitions than as laws of nature—and all the other theoretical apparatus defining a field, shared by all practitioners, were aimed at a *practice* of problem solution (action), not constituent parts of a body of knowledge (theory). It is also quite possible that Kuhn learned to see the significance of theory in a new light through his experience of psychoanalysis. There are always two parties in the psychoanalytic enterprise: the patient and the analyst. It is the analyst who is trained in psychoanalytic theory; usually the patient not only lacks all knowledge of theory but has no interest whatsoever in it, and the ideal is of an entirely theory-innocent patient. But who generates the knowledge in psychoanalysis? Is it the patient or the analyst? However one sets about answering that question, it is likely that the conclusion will be both parties. The patient develops a certain form of theory-free knowledge in collaboration with the theory-laden analyst. Many analysts would claim that the clinical function of theory is solely to facilitate the acquisition of that knowl-

56. See *The Case Method of Teaching Human Relations and Administration: An Interim Statement*, ed. Kenneth R. Andrews (Cambridge, Mass., 1953).

edge—or whatever other process it is that takes place—in the patient. Certainly, in the professional formation of analysts, it is not theory that takes pride of place but clinical seminars and, above all, clinical supervision: the discussion of and reflection upon cases. If Kuhn had prevailed on Conant to include psychoanalysis in the Harvard Courses in General Education, the result would have been *Harvard Case Histories of Case Histories*.

Two fundamental intuitions, underpinning the development of *Structure* and Kuhn's revolutionary account of science, dated from his early years when he was undergoing analysis, completing his doctorate, and retooling as a historian of science. One of these, the Aristotle epiphany, gave him the basis for his conviction of the suddenness and incommensurability of scientific change; the other was the recognition of the prosaic importance of problem-based, problem-oriented puzzle solving both in scientific education and in the vast preponderance of scientific work. Which of these is to be credited to psychoanalysis? Let us recall Kuhn's tribute:

a lot of what I started doing as a historian, or the level of my ability to do it—'to climb into other people's heads', is a phrase I used then and now—came out of my experience in psychoanalysis. So in that sense I think I owe it a tremendous debt. . . . There is a craft, hands-on aspect to [psychoanalysis], that I know no other route to, and that is intellectually of vast interest. ['D', p. 280]

Following Kuhn, I have so far accentuated the more dramatic of these two life-making achievements, the epiphany. But climbing into other people's heads does not have to be a dramatic and sudden revelatory process or experience; it can be as prosaic as any other activity of understanding based on graft and inching one's way forward into the not-yet-understood. There are always the same two sides to psychoanalytic practice: the moments of sudden illumination and almost uncanny recognition, when an old landscape becomes suddenly unrecognizable because seen in a radically new light; and then the return to the same old scenes, reworking and revisiting, the interminable process of working through. Psychoanalytic practice is nothing if not the search for problems and their unexpected solutions. The craft of psychoanalysis which Kuhn learned may well have underpinned both these kinds of 'history': both the experience of radical incommensurability when the young Newtonian in a flash finds himself thinking like Aristotle and the rethinking of men's thoughts after them, setting up 'a stage set, within another conceptual framework' ('D', p. 276), 'recapturing out-of-date ways of reading out-of-date texts' ('P', p. xiii)—securing the piecemeal identification of the historian with the past scientist which Kuhn developed as his sole method for doing the history of science and which he then endeavoured to transmit to his students. With exemplary success.