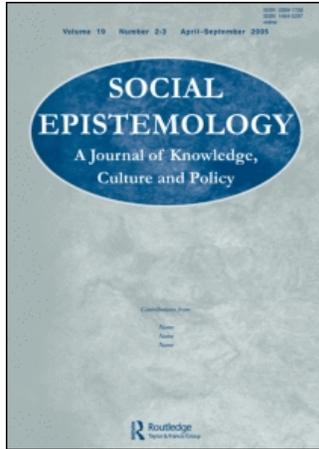


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Fuller's '18th Brumaire of Thomas K'*

PAUL A. ROTH

The title of Steve Fuller's *Thomas Kuhn: A Philosophical History for Our Times* doubly misleads. The primary title misleads because in this book I at least do not find Thomas Kuhn. There is neither the Kuhn with whom I am very well acquainted in the form of his work, and especially *The Structure of Scientific Revolutions*, nor the person with whom I was not so well acquainted but who, nonetheless, I count it a pleasure to have known. Second, what the subtitle promises is just not what Fuller provides. For the book, however interesting and provocative, offers no *philosophical* history. Developments in and nuances of arguments, their critiques and transformation from thinker to thinker, form no part of Fuller's narrative. Most significantly, Fuller never locates *Structure* in some specifically philosophical context—preferably one defined by issues current to the philosophy of science of the day—and so offers no explanation of why it there made so dramatic and enduring a mark.

Finally, I puzzle with regard to how to conceive of Fuller's book as a history. Fuller claims to provide a narrative that accounts for why a major intellectual moment—the reconceptualisation of the nature of science—took the shape it did. But we find less a narrative than a chain of associations and accusations. Fuller purports to locate its intellectual origins (in one Thomas K, as I prefer to call him, an unwitting and annoyingly witless pawn of Cold War warriors) and to document the profound yet allegedly unremittingly negative impact of *The Structure of Scientific Revolutions*. Fuller's perspective remains throughout relentlessly presentist and conspiratorial—in the 1950s James Bryant Conant and the mandarins of power successfully script institutional structures for the natural sciences, science policy, science studies and philosophy of science which endure to this day.

Yet the dystopian portraits of scholarly communities emerging from the book are to provide an incentive to work for their change, their resilience notwithstanding. For although Fuller claims to take us back to a point in historical time when issues were open, nothing in his narrative establishes that genuine alternatives ever existed, that things could have turned out otherwise. This is not because the account is so richly detailed that all appear determined, but precisely because it is so lacking in historical specifics that is impossible to discern if the events chronicled are over-determined or not. This is especially disappointing inasmuch as Fuller

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makes the task of prodding one's 'audience to convert a usable past into a viable future' his litmus test for 'good' academic work.¹

Yet, let me also say, important issues motivate and animate Fuller's book. To appreciate these requires working past Fuller's animus towards the person named in his title. But working to the book's underlying concerns also reveals just what short shrift Fuller gives the critical task of recovering a 'usable' past. My particular path through Fuller involves trying to do justice both to the Kuhn of my acquaintance and to the Thomas K. Fuller pillories. Distinguishing the two, identifying Fuller's actual target, and indicating the problems remaining constitutes no more than a propaedeutic to what is certain to be an ongoing discussion of issues this book raises. Whether to forgive the author of *Thomas Kuhn* his excesses I leave as an exercise for the reader.

I begin, then, with my own exceedingly abbreviated suggestions regarding the location of *The Structure of Scientific Revolutions* in the context of philosophy of science then (the early 1960s) and now. Why the overwhelming impact of *Structure*, since its lead ideas seemingly appear in so many precursors? Wherein lies its novelty?

By way of illustrating what I take to be the difference between *Structure* and its predecessors, and so at least partly explanatory of its perceived novelty and impact, recall two philosophically important essays that precede *Structure* by about a decade: Hempel's 'The empiricist criteria of cognitive significance' (1950) and Quine's 'Two dogmas of empiricism' (also 1950). Appreciating the relationship in which these essays stand to one another offers insight, or so I suggest, to understanding how Kuhn stands to his contemporaries and predecessors.

Hempel's essay has, quite unfortunately in my view, essentially disappeared from philosophical sight; Quine's, quite rightly I believe, remains regarded as one of the key pivots on which philosophy turns in this century. But the essays both argue for the same fundamental and fundamentally important points, though Hempel, in his own plodding and relentlessly careful way, makes them more clearly. The central point concerns how meaningful statements cannot possibly be known to be meaningful by being evaluated piecemeal. Their critiques reveal the core positivist doctrine tying meaningfulness to individual statements to be a non-starter; one unworkable for even statements taken to be models of meaningfulness.²

Why the philosophical laurels to Quine? Hempel presents the points scouted above as just one more problem for positivists to put on their agenda to solve. For Quine, these same issues become reasons to scrap the existing agenda. One sees a difficulty to be overcome within the context of business as usual, the other sees the need for doing (philosophical) business in a radically different way.

A similar relation holds between Kuhn and his predecessors, and understanding that relation provides a point of entry to the reasons for Fuller's parodic Thomas K. For, prior to Kuhn, no one had used the problems of verification (or falsification) to mount a wholesale challenge both on the rationality of theory change and the idea of scientific progress. Unlike the positions found in Popper, Feyerabend, Toulmin and Hanson (not to mention Hempel), Kuhn argues that the presence of anomalies is a standing condition for science. Additionally, and very importantly, there is no saying, on the basis of any logically precise criteria, when a theory stands or falls in the face of such anomalies.³

So, infamously in Kuhn, the basis for theory choice becomes characterised in terms of religious conversion, *Gestalt* switches, etc. In addition, and partly as a

consequence, one loses all metrics on the basis of which science can be shown to progress. Kuhn's account does not have any notion of improvement built into or guaranteed by our current best investigatory practices. As I like to note when I teach *Structure*, the title must be read ironically.⁴ For the book's message is that, *inter alia*, important scientific changes (I resist the temptation to say 'advances') have no structure, no logically defined sequence that rationally guides people from old theories to new.

Likewise, at the end of the book, as Kuhn acknowledges, what 'science' is is no longer clear. It is not an historically stable set of methods, facts, problems, concepts, or results. Seen historically, the natural sciences are not natural kinds. Kuhn thus made the history of science immediately and deeply relevant to the philosophy of science, a relevance strikingly at odds with the exclusive emphasis philosophers then placed on the logical structure of theories. Kuhn's way of presenting the history implied that theory-centric philosophers of science had grasped the wrong end of the stick for purposes of understanding scientific change and development. If history revealed a radical logical disconnect between successive theories, then efforts, for example, to elaborate the formal logical structure of physics and trace connections from Newton to Einstein were completely ill-conceived. If no logical connection exists, if anomalies just cannot play the logical role assigned them, then philosophy of science as practised needed a complete rethinking.

So the moral of my historical tale ascribes to Kuhn the place of distinction he (rightly) holds in philosophy of science relative to his contemporaries for reasons identified when distinguishing Quine from Hempel. Kuhn articulated and made explicit important implications unrecognised or unappreciated by his predecessors. Philosophy of science could not be done in the same way, or on the same assumptions, after Kuhn as it was done before.⁵ Indeed, Kuhn changed the style and substance of philosophy of science in ways which are still unfolding. For, while Kuhn receives credit (or blame) for encouraging social scientists to open the laboratory door, he provided reasons for philosophers to do the same. The past and current work of Cartwright, Hacking, Giere, Harding and many others insists that an understanding of science starts not with the details of logical structure found in theories, but with an appreciation of laboratory work.

Now by way of charting the relation between Fuller's Thomas K. and the Kuhn described above, note that *none* of what I take to have made Kuhn significant for the philosophy of science receives mention or notice in Fuller's discussion of Thomas K. Fuller ignores everything that interested philosophers of science in Kuhn because Fuller's concern is not on the structure of scientific change, but with the mechanisms of scientific stability. Fuller's Thomas K. emerges only when Kuhn's *Structure* is read as a sociological tract. The evidence here is Barry Barnes's *T.S. Kuhn and the Social Sciences* (1982), the only other work with which I am familiar where a reader could complete the book and remain unaware that Kuhn ever discussed changes in science. Fuller and Barnes ask the same basic question: what sort of community is a community constituted by paradigms, and to what effect? Only from a sociological standpoint, I suggest, does Thomas K. emerge—the Carl Schmitt of the philosophy of science, the person who rationalises the enforcement of norms in the name of community.⁶

What is so wrong with the community of science as Thomas K. is said to imagine it? I characterise Fuller's approach in terms of Marx's 'Eighteenth Brumaire' because his overriding concern is with the material and social conditions of

scientific production. Do we appreciate, Fuller worries, the public costs incurred in sustaining scientific production in its present form? Natural scientists, he maintains, aided critically by the spell cast through the work of Thomas K., have constituted themselves as a guild whose members claim and receive extensive public support. Yet this guild poses itself as the only competent judges of what they do and whether they are right to do it. What, Fuller wonders, are the hidden costs, financial and intellectual, of supporting this privileged class. To have science, must we cede it all oversight and evaluation of its own activities?

From the perspective that Fuller occupies, Mach's noble efforts to counter Planck's pernicious influence represent the revolution that was not. Recollecting this path not taken is to provide a usable past, a basis for a better future (at least with regard to science policy). Fuller portrays Thomas K. as the figurehead of a later *faux* revolution. As Fuller's story goes, circumstances elevate Thomas K. to a position to which he doubly pretends. First, he pretends to open science to wider public understanding by emphasising how it constitutes a particular type of community. But his mode of portrayal serves in fact only to insulate this community from pressures for it to serve the larger community of which it is a part and on which it financially depends. Thus Kuhn betrays the public by promoting a vision of science which ill-serves this public. But, second, although smart enough to glimpse the consequences of his positioning of science, Thomas K. the person reveals himself to be undeserving of his place on the world stage. Given a bully pulpit, he chooses instead to remain silent regarding the social disaster which he rationalised.

Fuller's characterisation of Big Science and its struggles to secure federal dollars but avoid scrutiny by or accountability to this public echo Marx's prefatory remarks to 'The Eighteenth Brumaire': the 'struggle took place only within a privileged minority . . . while the great productive mass of the population . . . formed the purely passive pedestal for these combatants'.⁷ Fuller's book too reads like a report from the barricades, and his side is losing. His words and tone are those of an angry and impassioned theorist bearing witness to the long-term duping of the public by those who pretend to know better than the people themselves where their interest truly lies. By detailing the pernicious effects of Thomas K., Fuller hopes to reveal how the dead hand of James Bryant Conant lies heavily on us all.

History repeats itself, but farce for tragedy, Kuhn for Mach, *The Structure of Scientific Revolutions* for *The Science of Mechanics*.

Mach's appeal to a 'critical' approach to the history of science as a source of dissenting voices to the orthodoxies of his day is much the one taken in this book and very much against the historiographical spirit that has followed in the wake of Kuhn's influence. . . . *Structure* unwittingly defused these critical sentiments that had emerged from science's increasing involvement in the military-industrial complex.⁸

I want to overcome *Structure's* effects on its readers. This implies reversing the value orientation that Kuhn's book has promoted in the organisation of knowledge production. Specifically, I argue that paradigms should be seen, not as the ideal form of scientific inquiry, but rather an arrested social movement in which the natural spread of knowledge is captured by a community that gains relative advantage by forcing other communities to rely on its expertise to get what they want.⁹

The farce mocks us, the academic public, for not recognising Thomas K. as a pretender, as a pawn of reactionary forces and promoter of *ersatz* scientific revolutions.

In order to move beyond worries about how Thomas K. does or does not relate to Thomas Kuhn, let's focus instead in Fuller's book on what emerge as issues of general and genuine concern: which institutional arrangements promote the best science, what ought the public role of science to be, and do current institutional arrangements hinder or promote either scientific productivity or its public uses? In order to begin to understand how Fuller answers these questions, two determinative intellectual influences, one explicit, one hidden, need to be identified. This, in turn, allows an appreciation of the problems these influences create for the task Fuller has ultimately set himself.

The explicit influence is Karl Popper, the hidden is John Stuart Mill. This is not happenstance, insofar as Fuller draws extensively from Feyerabend as well as Sir Karl. The unacknowledged Millian influence is, I will insist, the more important. For Fuller's book roundly condemns many for their lack of intellectual responsibility to the larger body politic; it proselytises ceaselessly regarding the important yet unfulfilled normative role science studies should play for science policy and scientific practice. But effectively nothing of substance is said regarding just what the content of the normative is. Only by teasing out the Millian ethos underlying Popper's open society and Feyerabend's anarchism can, in fact, Fuller's own normative commitments be dragged into the light of day. Once exposed, I shall suggest a fundamental difficulty in reconciling the twin aims Fuller endorses—the creation of socially responsible scientists and the open society model of the scientific community.

The Popperian mugging of Kuhn recorded in *Criticism and the Growth of Knowledge* indicates that at least two critical issues divide Kuhn from Popper and his tribe. One, already mentioned, is the role that could be ascribed to anomalies in accounting for theory change. But a second and equally important divide is that for Kuhn, there are two equally identifiable types of science—normal and revolutionary. However, only one can possibly progress, *viz.* normal science. For only in the context of normal science do there exist clear markers of progress—solved puzzles.¹⁰

For Popperians, Feyerabend included, normal science is no science at all; it is 'hack science', at best a simulacrum of the real thing.¹¹ Real science consists of the bold hypothesis. Real science is rarely seen. Revolutionary science is what science is, for this is the only way knowledge advances.¹²

We are now in a position to explore some of the perhaps unintended consequences of Fuller's valorisation of the Popperian conception of science. Popper as well as Feyerabend models 'real' science on the actions of an heroic individual. Part of the contrast between a Kuhnian conception and a Popperian might be put in terms of a corporatist versus an entrepreneurial account of economic growth and innovation. In this regard, Thomas K. makes scientists into organisation men, complete with all the suppression of individuality that goes with that stereotype. Big science, like big corporations or big government, stifles individuality and innovation. Loyalty and conformity are the prevailing values.

Fuller's explicit recommendation here is completely in keeping with the valorisation of the entrepreneur—get science off the government dole (what Fuller calls the 'secularisation of science'). If science is so great, his suggestion goes, people will pay for it. But on the entrepreneurial model, scientists would have to work to attract the equivalent of venture capital for their projects. No more decisions made behind closed doors by panels of self-styled experts who are

accountable only to themselves. The secularisation of science, by making all research programs rest, as accountants say, on their own bottom, would open for public debate the critical question of which science is worth paying for.

To the extent that the present institutional forms of science partake heavily of public funds while remaining insulated, in terms either of procedures or products, from public scrutiny or evaluation, Fuller's concerns deserve the most serious consideration. But here we come face to face with a tension long simmering in Fuller's own work, one engendered by the Millian celebration of the individual to which Fuller subscribes. In earlier work, Fuller worried about science as an institution from two distinct and not obviously compatible perspectives.¹³ One concerned science as a product. The thought here is that if science does not do its work by way of special method, but nonetheless produces a product that society values (in terms of engineering payoffs, if nothing else), then one question for the science policy-maker is how best to maximise scientific productivity. But there is a second, distinct concern that a policy-maker might have, *viz.*, how to make the institution of science and the scientists inhabiting it more sensitive and accountable to a wider public.

These imperatives—the productive versus the ethical—pull in different directions. Since it is more or less taken for granted (with apologies to Larry Laudan) that the notion of a perduring scientific method cannot explain how scientists do what they do, there is no *a priori* reason to believe that the institutional arrangements that promote science *qua* product will overlap with what may be thought politically desirable—e.g. public scrutiny and accountability. Complicating this picture is the fact that it is easy to imagine empirical or operational measures for the one—productivity—and very difficult to do so for the other—the normatively desirable.

Popper too wanted it both ways. His philosophy of science fits nicely with his social philosophy since, in good Millian fashion, both suggest that the royal road to human betterment lies in unceasing application of criticisms to one's views. For Mill, confidence in your views is a function of seeing how well they stand up to criticism; for Popper, confidence in scientific theory is a function of how well corroborated they are, how long they have survived efforts to falsify them. Likewise, Feyerabend, even more explicitly than Popper, ties possibilities for development and change—his very Millian notion of human progress—to constant challenges to the self.¹⁴

But does the Millian notion of self-development and the entrepreneurial model of inquiry following from it fit well with Fuller's aspirations for a normatively informed science policy? Fuller complains at length that paradigms constitute politics by other means. But moving from a corporatist to an entrepreneurial model hardly guarantees anything in the way of norms, unless the primary norm is devil take the hindmost. As James Bohman observes, entrepreneurs shape the public, not the other way around. There is no accountability *per se*, but the potential for squandering social resources even more without regulation.¹⁵

Here we confront the core problem with the proposal to recover a usable past. From whence come the norms on Fuller's normative agenda? Had Fuller read more Hayden White than *Metahistory*, he would perhaps be more cautious than he is in setting himself the task of recovering a 'usable past'. White has long maintained a basically Nietzschean view that the meaning given history is a product of the will of the historian. It is the historian who imposes order on the traces of the

past and turns it into something morally edifying or politically useful. White's conception of the role and public duties of the historian closely parallels those Fuller ascribes to scientists. White writes:

How can it be said then that the professional historian is peculiarly qualified to define the questions which one may ask of the historical record and is alone able to determine when adequate answers to the questions thus posed have been given? It is no longer self-evidently true for the intellectual community at large that the disinterested study of the past . . . is either ennobling or even illuminative of our humanity. . . . And it follows that *the burden of the historian* in our time is to reestablish the dignity of history studies on a basis that will make them consonant with the aims and purposes of the intellectual community at large, that is, transform historical studies in such a way as to allow the historian to participate positively in the liberation of the present from *the burden of history*.¹⁶

White sees history as politics by other means, just the position Fuller holds regarding scientific communities sustained by paradigms. But White finds no constraints on what historians may ultimately deliver, a view that causes him no end of trouble.

I posit, in this light, three questions/challenges for Fuller and his promise to provide a 'usable past', one compatible both with the needs of individuals *qua* individuals and the needs of communities for responsible and responsive members.¹⁷

(1) Fuller's Rawlsian problem: if science is to 'serve' the public, by what mark will we know this? Fuller, I would like to say, must lift the 'veil of ignorance' that so far occludes the process by which normative decisions are to be certified as appropriate normative demands. If, for example, he endorses democratic means, what makes this other than his *ex cathedra* pronouncement on what is morally right?

(2) Fuller's Laudanian problem: both Popper and Feyerabend have their own criteria for scientific progress. In this regard, consider the following extremely puzzling remark by Fuller: 'Popper differed from the social constructivists in his insistence on an explicit normative standard, against which such ongoing developments in science may be judged. My own social epistemology agrees with Popper . . . though I am more explicit than Popper about the political character of the means by which this normative standard is determined'.¹⁸ But to differ with Popper on this point and in this way is to deny precisely what Popper most wants to assert—there are objective criteria for verisimilitude, progress, etc. So, there is not only a hidden ethical master-narrative, there is a hidden master-narrative of scientific progress. What is it?

(3) Fuller's Platonic problem: here the problem is set by the fact that Fuller attempts to wear what I shall call a 'hermeneutic Ring of Gyges'. In the Platonic myth, the ring frees its wearer from fear of retribution by making him invisible. Fuller, so quick to identify the scriptwriters for and the puppets on the world historical stage, nonetheless renders invisible his own standpoint. But what makes Fuller the historiographic unmoved mover, somehow privileged to narrate events *wie es eigentlich gewesen?* Some hermeneutical self-appraisal is called for, not just sand in the face of the readers.

I want, at last, to conclude by coming back, albeit briefly, to Thomas Kuhn, the person. To recur yet again to Marx's 'Eighteenth Brumaire':

Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past. The tradition of all the dead generations weighs like a nightmare on the brain of the living.¹⁹

Did anyone ever doubt Kuhn made a revolution from circumstances directly encountered, given and transmitted from the past? But he also uniquely transformed that history, and that forever sets him apart from his predecessors. Equally, tradition weighed heavily on Kuhn. But given the time, how could it not? I, at least, cannot fault Kuhn for failing to be the *Zeitgeist* on horseback. As Clifford Geertz observes in his moving tribute to Kuhn, it is not unusual to find those who unleash a revolution rather wishing that things might have gone differently.²⁰ The Thomas Kuhn it was my pleasure to have known never played the role of great man; he bore with grace the considerable burden imposed by the fame bestowed on him. Offered a world stage, he refused parts for which he felt unsuited and unprepared. He contented himself with commenting only on what he knew. All this bespeaks a steady intellectual nerve and an embrace of responsibility, not their opposites. As we learned from Socrates, true wisdom begins with knowing what one does not know.

Notes

1. Fuller, p. 12. In this Fuller indicates his deep affinities with Hayden White's views of history, a point to which I return at the end of this article.
2. Both essays also explore important implications of this attack on verifiability, e.g. that the unit of empirical significance is whole theories, that theories can accommodate evidence in various ways, non-equivalent ways.
3. Fuller notices, but does not make enough of, in my opinion, parallels between what Kuhn says and Carnap's distinction between internal and external questions. For Carnap, there is no rational basis of choice between competing frameworks where the terms are different enough, or; as one might say, the terms are incommensurable.
4. Hanson, for all his emphasis on the significance of theories in providing a scientific *Gestalt*, gives us a quite unironic reading of science. His 'patterns of discovery' are theory-driven, but he draws no great historiographic lessons from this. His model science is still physics, his favored approach still the formalised account.
5. Fuller's attempt to promote Toulmin over Kuhn betrays, as I note at the end, the hidden master-narrative at work in Fuller's account. For as a review of Toulmin's work reveals—see, for example, his contribution to *Criticism and the Growth of Knowledge* edited by Imre Lakatos and Alan Musgrave (Cambridge: Cambridge University Press, 1970)—his account of scientific development is much more accretionist, much less radical than that which Kuhn later proposes. Yet if it is precisely the accretionist picture that Kuhn throws into question, his remarks on the nature of scientific communities notwithstanding, then Fuller desperately needs a way to retell the history of science from that 'lost' point of view. Nothing so far as I can see, in his remarks provides any clue of just how he would do this, or even how he imagines this might be done.
6. Thomas K., the pawn of Conant and those who would shield science from an uncomprehending public, rationalises the insulation of science from close public scrutiny by making commitments to a received research program definitive of science and the *sine qua non* of any real scientific progress. The effect of this, which Thomas K.'s puppet-master sold to business and government alike, is to make the discipline or laboratory a closed society, one where failure to toe the orthodox line leads to expulsion from the community (or, what comes to the same thing, rejection of one's grant applications). Fuller fulminates at length on the pejorative effects that follow from acceptance of 'no science without the paradigm' view of things. Indeed, he intimates that the current community structure of science, the one licensed by a book Thomas K. authored, bears responsibility for the possible demise of true scientific change.
7. Marx, 'The Eighteenth Brumaire', in Lewis A. Feuer (ed.) *Marx and Engels: Basic Writings on Politics and Philosophy* (Garden City, NY: Anchor Books), 1959, pp. 319–320.
8. Fuller, p. 35.
9. Fuller, p. 37. Perhaps the quickest way to obtain a sense of all the pernicious influences Fuller lays at the feet of Kuhn and his work is to follow out references to 'Kuhnification' in the index to Fuller's book.
10. I would now add a third difference—Kuhn does not share Popper's belief in the growth of knowledge.
11. In John Watkins's famous phrase from his contribution to *Criticism* (see note 5).

12. Significantly, Popper, Feyerabend, Toulmin and Lakatos are all comfortable, each in his own way, talking about *knowledge* and how it advances. This marks a critical difference with Kuhn, the sort of difference that marked the contrast between Hempel and Quine.
13. The problem I discuss is not new in his work, but simply takes a different form than that noted previously. See my 'The Bureaucratic Turn: Weber *contra* Hempel in Fuller's *Social Epistemology*'. *Inquiry* 34 (September 1991), 365–376.
14. See, for example, Feyerabend's remarks on this point in *Criticism*, pp. 210–211.
15. Bohman, personal correspondence.
16. Hayden White, The burden of history, in *Tropics of Discourse* (Baltimore, MD: Johns Hopkins University Press), 1978, pp. 40–41. See also his The politics of historical interpretation: discipline and de-sublimation, in *Content of the Form* (Baltimore, MD: Johns Hopkins University Press), 1987, pp. 58–81.
17. Rorty, faced with exactly this problem, opts for his much criticised bourgeois liberal solution, a sharp division between a private sphere and the demands of the public sphere. Rorty fears most how all individual rights may be overrun by appeals to some higher good.
18. Fuller, p. 394, n. 27.
19. Feuer, *Basic Writings*, p. 320.
20. Clifford Geertz, The legacy of Thomas Kuhn: the right text at the right time, in his *Available Light* (Princeton, NJ: Princeton University Press), 2000, pp. 16–17.