

What's Kuhn got to do with it?*

PHILIP E. MIROWSKI

I. On feeling cheated by Kuhn

It is not a fate that you would not wish upon anyone: to be feted and celebrated during their lifetime as author of one of the 100 most important books of the twentieth century, and yet close on the heels of their demise, become rapidly subjected to a bout of reconsideration and rejection that borders on the vindictive, with the unabashed aim of relegating them to the trash-heap of history. Yet, singularly, that is what has happened to Thomas Kuhn and his (dare I say) paradigmatic work *The Structure of Scientific Revolutions [SSR]* [1970 (1962)]. Everyone all of a sudden seems miffed with Kuhn.¹ Stephen Weinberg accuses him of triggering the 60s 'revolt against reason' that has poisoned public understanding of science (1998); John Horgan (1996) portrays him as someone congenitally incapable of giving a coherent account of scientific progress; his students such as Kenneth Caneva (2000) bemoan the fact that it took them so long to perceive the flaws in his deceptively confident assertions about the role of criticism in theory change, or the role of history in understanding science. Participants at a symposium on 'The Legacy of Thomas Kuhn' at the Dibner Institute in November 1997 grouched about the fact that the best-known historian of science of the twentieth century didn't seem to possess the patience or inclination to work extensively with primary sources and archives. Even Steve Fuller engages in a little of this Kuhn-bashing himself, writing in one place that *SSR* has 'a philosopher's sense of sociology, an historians sense of philosophy, and a sociologist's sense of history' (p. 32).²

What seems lacking in this flurried winter of discontent is the realisation that the upsurge of dissatisfaction should not be directed at the hapless scapegoat named Kuhn, but rather more justly at ourselves. Can anyone seriously maintain that we were duped or swindled by *SSR*? After all, it was no one but us who lit upon this most unlikely portion of the 'International Encyclopaedia of Unified Science' and made it a runaway best seller. We were the ones who felt that first frisson of excitement in thinking that science was not the algorithmic sausage-grinder of the positivists, but rather something more closely resembling the quotidian workplaces with which we were all familiar. And I blush to admit it was especially we social

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Author: Philip E. Mirowski, University of Notre Dame, IN, USA

scientists who grasped so feverishly at Kuhn's account of normal science and revolutions in order to lend some credence and legitimacy to our own intellectual traditions. It was none other than our own credulousness that led us to sniff around in the new specialisation of the history of science because we apparently caught the whiff of something exhilarating in the pages of *SSR*, and we alone who managed to overlook the strange contradictions that permeated the text and our reception of it.

Steve Fuller has been the first to have had the foresight to usher us out of the sterile self-pity of disillusioned acolyte and ask the really penetrating questions concerning Kuhn's legacy; and for this alone we owe him a hearty vote of thanks. His new book on Kuhn (2000a) helps us see just how very odd a phenomenon the popularity of *SSR* really was. How did it come to pass that someone who so regularly poured cold water upon any new intellectual alliance between the history and philosophy of science could have ended up as its prophet and patron saint? Or, how did it come to be that an author who warmly preached the virtues of a hermeneutics of sympathy towards ancient superceded thinkers and their texts himself end up so roundly and thoroughly ignoring his critics and repudiating his enthusiasts? How was it that someone so openly tone-deaf and disdainful towards the social sciences could come to be celebrated as providing it with a major source of legitimation, and even regarded as assisting it in being admitted to the 'right' side of the demarcation criterion? The ironies even extend to the very specific theses of the book. How did it come to pass that a caricature called 'normal science' which surely did not exist before the late nineteenth-century German industrialisation of science (if even then), and subsequently applied to a motley of incidents in the history of science, no one of which occurred later than 1930, come to be confused with an historically sensitive account of the sociology of science in the Cold War era of Big Science? Or, coincidentally, how were we ever persuaded to focus so intently upon the 'autonomy of science' precisely in the era and geographic location of the most aggressive expansion of state management and funding of scientific research in human history?

I agree wholeheartedly with Steve Fuller that Thomas Kuhn the man was not a cause but merely a symptom of some larger deformations of the philosophy, politics and economics of science, especially those occurring in the postwar American context. That is why he compares Kuhn to the character 'Chance' in Jerzy Kosinski's *Being There*, haplessly showered with credit for things he never did nor intended. Some reviewers of Fuller such as David Hollinger (2000) seem to resent being reminded that Kuhn served more as a speculum serving back their own presuppositions than as a flag to rally the troops round. But Fuller does something even more subtle, in that he gives a reflexive 'Kuhnian' reading of the career and writings of Thomas Kuhn, showing how his thought can be rendered more comprehensible by situating it within the context of his 'masters'—and here, James Conant is central—and his followers. Imre Lakatos once jokingly paraphrased Kuhn's message as, 'Do your master's thing, or your own if you can convert others, but not before then!' (in Motterlini 1999, p. 94). It seems that Kuhn really did manage the impossible, which was to 'do his own thing' only after he had become the icon of thousands; but by that time, it was too late. But Fuller's reading is Kuhnian in an even less disciplinary sense as well. Fuller will perhaps acknowledge that his is not a definitive historian's *history* of Thomas Kuhn, lacking in much of the standard armamentarium and chronological narrative line, although it should

be said in his favor that he did spend some serious time in the Harvard archives. Rather, Fuller aims to write 'philosophical history': that is, a broad brush *explanation* of what may have otherwise appeared irrational or adventitious in standard narratives of reigning orthodoxies, with a view to prompting debate about the future of a particular line of inquiry.

I share his conviction that philosophical history is the project really worth our efforts, and so will confront his book accordingly on that level. I too worry about the present predicament and future prospects of science studies, which seems ever more intent upon withdrawing into *faux* playfulness and smug obscurity; and I fret over what has become of the profession of the history of science. The relationship of these prospects to the changing structure and functions of the university, first during the Cold War, and now in our brave new era of globalised privatisation, is incontestably the major subtext to the saga of the rise and fall of Thomas Kuhn. But then there is also the question of the role of the social sciences in all this—on this issue I will diverge rather sharply from the line in Fuller's book, wherein the social sciences are praised for their prospective protopaedetic properties. It may seem odd coming from someone whose own intellectual identity is so wrapped up with the social sciences, but I sincerely doubt that one can banish the ill effects of Kuhnian relativism by simply reversing his own clear contempt for social science. It is my conviction that an even bigger dose of history, perhaps including a dollop of the history of American social science, is needed to appreciate and assess the regime of science organisation and funding which pervades the post-Kuhnian landscape.

II. The allure of Kuhn, and its discontents

Steve Fuller expends so much effort in uncovering the dark side of Kuhnophobia, that I think he neglects to remind his audience (many of whom will by now have missed the experience altogether) of what it was like to become besotted with *SSR* a little after it appeared in 1962. While it may appear incongruous now, Kuhn's book was regarded as a liberating experience by those looking for a respite from the unalloyed scientism of the immediate post-Sputnik years. It may be worthwhile to briefly revisit some of those themes that resonated so harmoniously with other cultural currents of the time, if only to contrast them with the deleterious effects which they may have had in the ensuing years.

The overarching theme that Kuhn never failed to foreground (and which constitutes the source of so much modern dissatisfaction) is the contrast between his reliance upon communal concepts and patterns of thought as counterpoised to the reductive psychological individualism of so much Anglo-American thought. It tended to give his writing a certain Continental flair, without, of course, delving into any of the fine points of their actual treatment of social structures, much less the classic texts.³ It is this apparent wholism which draws the ire of so many Science Warriors intent upon blaming all their woes upon the excesses of the 1960s, but I would instead point out that it was a distinctly decontextualised and featureless portrait of the 'over-socialised agent', one nicely attuned to the needs and demands of the American academy. The major way one could tell that scientific thought was putatively 'socially conditioned' *a la* Kuhn was not by any appeal to tacit knowledge (that was the repressed Continental Polanyite wing of the anti-positivist reaction)

but rather through an explicit rejection of the model of algorithmic decision theory which had come to dominate analytical philosophy of science in the postwar period, by means of the expedient of tarring it with the ambivalent characterisation of 'normal science'.⁴ I believe it was this move, more than anything else, which rendered *SSR* such a breath of fresh air for so many readers. (This point is crucial for understanding Kuhn's reception, and is unaccountably neglected by Fuller.) Because in the Cold War mindset, the act of insisting something wasn't algorithmic was tantamount to suggesting it was irrational, Kuhn sought to substitute one mechanistic scheme for another: namely, he resorted to the old Germanic social science modality of lockstep 'stage theories' to provide meaning and form to something that resisted being reduced to law-governed component parts.⁵

Hence, there was a very real sense in which *SSR* was read as de-privileging the science of the 1960s, or at least the paint-by-numbers mechanical science of the postwar positivists. Sure, there were those mock-Foucauvian 'radical ruptures' versus the uniform continuity so beloved by those enamoured of the 'Unity of Science' movement; but there really were bits here and there redolent of a generalised social theory, albeit one with so few commitments as to vanish in the very moment of its conjuration. This was a social theory that would *not* resemble the imitation mechanics of decision theory and neoclassical economics, a point which will assume some significance below. Nevertheless, it was not entirely a pipe dream in the 1960s to have read Kuhn as ushering in an era where science itself could be an academic topic of study in its own right, as opposed to an unquestioned basis for a uniform general pedagogy forced down the throats of the unwitting student. It was this largely unrealised promise of a world turned upside-down, of a future transvaluation of the values of the a hierarchy of knowledge pivoting upon the natural sciences that lured historians of science in large numbers, and fostered a climate in which the 'sociology of scientific knowledge' could take root here and there in the academy.

Steve Fuller's book on Kuhn has very little patience with any of this. If I may paraphrase the argument, Fuller presents Kuhn as steeped in and shaped by a problematic first enunciated by James Conant in his multifarious roles as Harvard University president, Vannevar Bush's NDRC deputy in charge of the A-bomb, all-round governmental advisor and science manager, as well as pedagogue concerned with the future of American democracy and the threat of Soviet Communism. The burning issue which motivated Conant, and latterly Kuhn, says Fuller, was how to reconcile the both the apocalyptic products and authoritarian processes of the Big Science appearing everywhere in the twentieth century with their commitment to the superiority of democratic and egalitarian political structures; if you couldn't square that circle, then the entire Cold War appeared as one dreary hollow cynical exercise, closer to John le Carré than Alexandre Koyré.⁶ After all, one of the worries which beset the strategic community in the 1950s was the fear that Soviet society was actually better configured to produce Big Science results on an assembly-line basis than was the supposed *laissez-faire* situation predominant in the United States, and thus would defeat the latter in a technoscientific race.⁷ Fuller argues that many of Kuhn's (and Conant's) doctrines were contrived to salve those worries and forge a science pedagogy that could thrive in Cold War America.

In a nutshell, Fuller maintains that Conant and Kuhn concocted a 'double truth' doctrine tailored to the Cold War exigencies and to the political conundra it engendered. They began with the premise that Big Science was here to stay, and

that continued support for the hydra-headed monster was preferable to sweeping the postwar stables clean and trying to engineer a scientific community better suited to democratic needs and procedures. But the prescription was not to force the layperson to consciously swallow this bitter pill—to relinquish core democratic structures simply in the interests of defeating the Soviets through bigger and better Doomsday Machines was too repulsive a doctrine for any but the most Manichean to bear—but rather develop one ideology for the layperson and another, distinctly separate pedagogy for the tyro scientist. The layperson was persuaded to accept an over-socialised account of the scientists so that they might believe (however wrongly) that dissent and disagreement were absent from legitimate science, and that Science exhibited a self-organising structure and unified telos no matter what the setup. Further, it had become necessary to reify a strong pure/applied distinction in order to deny that the scientists bore any role or responsibility in the uses to which their discoveries were put; and indeed, the historical fact that military and industrial concerns were increasingly encroaching upon the prerogatives of the university since at least the 1880s were nowhere to be encountered by the hapless layperson. Instead, the average citizen was exhorted to enjoy a warm glow from the fact their tax dollars went to support who-knows-what classified research, which might eventually result in such boons to mankind as Teflon and the Internet. While the Harvard 'general education' course sought to telegraph that Science deployed essentially the same 'strategy and tactics' (Conant's terminology) from the seventeenth century to the present, and that your average student was simply too inept to successfully complete the simplest simulated table-top experiment, the pedagogy for the actual tyro scientist banished any recognisable history altogether. There the guild mentality of the postwar departmental organisation was coming to reign supreme: the budding physicist or chemist would simply take the legitimacy of what they were taught on blind faith, working the problem sets and filling in the lab notebooks, cranking the handles and crunching the numbers, dispensing with any awareness of the historical contingency or contested validity of delegated forms of scientific research. Ignorance of the cultural aspects of science would nurture docile and hard-working makers of thermonuclear bombs and Grand Unification Theories in short order, and with minimum fuss. Fuller makes much of Kuhn's Orwellian doctrine of the rewriting of the history books with each scientific revolution, but frankly, that was not so very different from the other Cold War Orwellian doctrines (such as the 'impossibility' of consistent democratic choice procedures demonstrated by Kenneth Arrow, or Thomas Schelling's strategic doctrine that you could frighten your enemy with atomic blackmail into behaving rationally) kicking around RAND and other postwar institutions of the higher learning in the same era. Certainly there would in each case be a small community who were privy to the 'real' situation—and for Kuhn, this would be the invisible college of professional historians of science—but they no longer had any critical function to fulfil, either within pedagogy or broad-based philosophical discussions of 'whither science'. Hence, Kuhn never supported the hybrid departments of the 'history *and* philosophy of science' which sprung up in his wake, and have themselves come to grief in the interim.

I think there is much truth to Fuller's characterisation of *SSR* as a 'Noble Lie' to shield science from political exposure in a democratic context and to protect its economic viability from demands for accountability; and I personally think it matters little whether Kuhn the man consciously intended to endorse the bulk of

this package or not. After all, one virtue of Fuller's book is to insist that the reader's reception of few commensurate works have ever diverged so dramatically and systematically from the intentions of their author as has happened with Kuhn's *SSR*. Anyway, we tend to forget it was the Viennese positivists and their American epigones who were by and large socialists and social planners; Kuhn fits in quite handily with anti-positivists such as Polanyi and Hayek and Merton as defenders of the special elite status of science as demanding tribute in the format of unstinting funding and unqualified testimonials of belief from outsiders.⁸ If scientists such as Stephen Weinberg and Paul Gross seek to saddle poor Kuhn with the onus for the loss of faith in this particular catechism, it can only be because their formative years were so sadly deficient in historical and philosophical training, and hence they seem incapable of appreciating that it was the end of the Cold War that reopened all dossiers and voided all the agreements concerning the relationship of science to the state and to society, and of the scientist to his brethren. The irony is that the drab world of careerist normal scientists which Kuhn described and praised was the world which has since come to pass, and it is a world which would naturally want to heap calumny on his head.

Indeed, I should like to venture further than Fuller by suggesting that Conant was even more significant for the subsequent conceptions of science than he has suggested in this book. Not only was Conant the pioneer of an essentially a-historical approach to the history of science, preaching *Realpolitik* as the only viable method to reconcile militarily regimented science with a free society, deploying simulation of great moments in science in order to impose and police the internalist/externalist distinction, and insuring the meritocratic character of science by progressively bureaucratising admissions to the university and the curriculum which would sort aspirants to a scientific career. Conant was also one of the main players in the importation and development of the discipline of 'operations research' into the US in World War II, along with fellow science managers Bush and Warren Weaver.⁹ While I agree that the pedagogical aspirations of Conant were most directly played out in the shape assumed by Kuhn's *SSR*, it was the actual managerial innovations of Conant and his comrades in arms in the guise of 'operations research' that are most salient for Fuller's diagnosis of the modern malaise of the history and philosophy of science.

III. Operations research as applied social epistemology

To a first approximation, 'operations research' (OR) comprised a set of doctrines whose purpose was to more efficiently prosecute war through the better integration of men and the novel technological devices which had been developed by scientists during and after the Second World War. It has become standard in the historical literature to point out that many of these doctrines, which look at first glance to resemble later social science, were in fact innovated largely by physicists and natural scientists. Fuller (p. 201) reports that Kuhn himself worked on radar jamming and OR during WWII, and this experience helped turn him away from physics and towards the history of science. While the actual content of OR is not so important in the present context, the fact that it presaged a concertedly *interdisciplinary* approach to the understanding of social processes and their reactions to technological change is of direct relevance. Especially through the intermediary of

the computer, it ranged from experimental studies of the cognitive behaviour of individuals to direct thermodynamic analogies treating equilibrium configuration of groups of men. For the major significance of OR for Conant, Weaver and other scientists was that it was a tentative solution to the problems thrown up by the wartime mobilisation of science by the state. Physicists wanted to be paid by the military, but not actually be *in* the military; physicists wanted to do social science for the military, but not actually to *be* social scientists; physicists wanted to give others orders, but not actually be *responsible* for the commands given. In order to enjoy these extraordinary immunities, the physicists and other natural scientists found themselves innovating new roles which would manage this awkward combination of engagement and aloofness from the chain of command. In effect, these natural scientists invented new mechanisms of control within the military hierarchy in order to better control their own agenda of research, which had itself come to be overwhelmingly funded by the military. Hence, the rise of Big Science and the innovation of OR went hand in hand in mid-century.

The fact that OR doubled as a kind of theory of science policy is best illustrated by the circumstance that the whole 'Social Planning of Science' movement in Britain, the direct lineal predecessor of British sociology of science, were essentially coterminous with the luminaries of the British OR establishment. Patrick Blackett, J.D. Bernal, Conrad Waddington and a host of others used their wartime experience to argue that science could be more rationally planned and funded for the benefit of mankind. The political overtones of this movement provoked a reaction in the form of a 'Society for the Freedom of Science' spearheaded by Michael Polanyi and Friedrich von Hayek (McGuckin 1984). Since Kuhn was frequently accused of being the American Polanyi, we can again observe the close correlation between attitudes towards the postwar promise and prospects of OR and the rise of a particular version of the history of science. The OR connection to modern science studies is further revealed by the support by Waddington for the early incarnation of the Edinburgh Science Studies Unit.¹⁰

The reason this has bearing on the story in Fuller's book is that much that happened after *SSR* is attributed to the baleful influence of Kuhn alone, when in fact, it would be more historically accurate to approach *SSR* as one small symptom situated within a much larger dynamic of the playing out of theories of science management and policy in the Cold War era of Big Science. In effect, just as there were differential British and American versions of how science operated when it was 'tacit' (Polanyi) and 'normal' (Kuhn), there was also a British and American version of OR, corresponding to the British and American variants of science funding and management. The British version rapidly devolved into a left-wing sociology of science which had little bearing on actual science management, whereas the American version got processed into Mertonian sociology of science and a neoclassical economics of technical change, both subservient to an extensive infrastructure of science managers run by the military and (later) units such as the NSF and NIH. This explains a number of phenomena which Fuller treats as nearly inexplicable, including the curious relationship to political economy:

the political economy of science has been so verbally co-opted that Kuhn's sociological followers, be they Mertonian or SSK, have been able to cast several models of scientific activity in the discourse of political economy without either making links with the larger political and economic scene that sustains Big Science or even drawing much on the empirical and explanatory resources of political science and economics. (p. 235)

Fuller has been misled by the absence of direct citations in coming to think that science studies bandies about economic terminology without touching base with economic content: the missing link is OR. Operations research inspired much of early science studies (either through advocacy or rejection), and operations research was a major instrument for the stabilisation of the American orthodoxy in economics (Mirowski 1999). Even Thomas Kuhn himself participated in a conference on technological change sponsored by RAND, whose subtext was to argue out whether innovative weapons systems could actually be planned out in detail before they were ever built (Hounshell in Hughes and Hughes 2000; Nelson 1962). Kuhn fit effortlessly into this community because, as Fuller notes, his model of normal science was eminently an industrial one, where 'success can be measured by sheer output . . . against a backdrop of a constant state of competition' (p. 199). This was the Cold War 'double truth' doctrine all over again: outsiders would be continually reassured that scientists were the freest of free spirits and that scientific inquiry could never be planned or dictated, whereas those in the know were busy reprocessing physical mathematical models into social science doctrines whose major thrust was to justify very specific interventions in the funding and organisation of science. In Britain the planning pretensions were a bit more out in the open, but were constrained by the happenstance that OR remained low-tech and disengaged from the computer: the result was an a-cognitive SSK largely decoupled from the political process. In compensation, it was philosophers of science who were fastened upon as the 'enemy', with much effort devoted to debunking the existence of a context-free scientific method. In America, by contrast, OR went high-tech and morphed into systems analysis, retreating into a technocratic conception of planning which managed to evade any socialist taint or hostility to market allocation (Hughes and Hughes 2000). Philosophers of science there were most assuredly *not* the enemy (as they were not for Kuhn himself), since they had in the interim been largely co-opted into OR and decision theory. Quine, Donald Davidson, Hans Reichenbach, Nicholas Rescher and a whole roster of others all were consulting at RAND while they were forging postwar analytic philosophy. It became a characteristic postwar trope to conflate cognition as intuitive statistics with a context-free 'scientific method' in postwar America (Gigerenzer 2000). Instead, American science studies subsequently bifurcated into the history of science (including Mertonian sociology), with all the consequences of shortsightedness and irrelevance which Fuller has denounced, and the economics of technical change, itself an adjunct of OR.

Thus, I would concur with Fuller that, 'a seemingly radical innovation that quickly acquires widespread currency probably serves some well-established interests which remain hidden' (p. 372); I would just add that Fuller himself has only just begun to scratch the surface of Kuhn's 'context of reception', and that when one digs down a little deeper, one begins to appreciate just how resonantly in tune with the *Zeitgeist* Kuhn managed to be.

IV. Older but wiser

The above sketch tries to augment some of Fuller's own efforts to contextualise Kuhn; but it also has some implications for his own project of a socialised

epistemology which he may not like. In both the last chapters of the Kuhn book and in other publications (2000b,c), Fuller has promoted the idea that the social sciences provide a dependable platform from which to launch a politically committed and socially relevant discipline of science studies, one which will address many of the drawbacks of postwar history and philosophy of science which he has devoted many pages to document.¹¹ In this quest, I believe he has overlooked the all-important historical fact that the kind of social science inflected interdisciplinary inquiry into capital-S Science which he imagines as a resource for the future has already materialised in the postwar period, and it constitutes the core of the kind of history and philosophy of science which he so deplors. Not only did operations research give rise to the social studies of science movement, but it also shaped and promoted neoclassical economics, Parsonian sociology, analytic philosophy of science, structuralist anthropology, evolutionary game theory, and even the vintage of cognitive psychology that Fuller has especially endorsed as the wave of the future for science studies. What I would suggest that he has missed is that all these social science developments were not so easily separable from the physical analogies and models which constituted their conditions of existence, and that they were part and parcel of the postwar structure of science management and planning within which they were situated. In a catch-phrase, there is little that is distinctively 'social' about postwar social science (think of the conceptual dominance of statistics, or the fascination with methodological individualism, or the mechanics of modernisation theory); and in any event, they were comprised mainly of just another one of those 'radical innovations' which serve widespread interests which he himself has warned us against. If that indeed was the case, then where does Fuller believe the Archimedean point will be located against which he can leverage his critique of Big Science?

At various points in the present book Fuller presents his program of science evaluation as bearing resemblances to that of Karl Popper (p. 394fn); but I think he neglects to recall that the Popper of 'critical realism' was also Popper the unabashed Cold Warrior, not to mention the Popper of 'situational analysis', a crude attempt to exempt neoclassical economic theory from his otherwise falsificationist precepts (Caldwell 1991). I have a sneaking suspicion that once Fuller's research agenda and policy prescriptions are fully spelled out (which they are not in this volume, nor indeed, elsewhere), his social epistemology will turn out to resemble nothing so much as the OR-inspired doctrines of the postwar period: in other words, it will be an 'economics of science' not so very different from a cognitively inspired version of neoclassical economics.¹² Maybe it will have a little 'bounded rationality' from Kahneman and Tversky or Herbert Simon, and maybe there will be stirred into the pot a little cost-benefit analysis from the consumer's point of view, and maybe there will even be room for some critical analysis of Pareto's theory of the circulation of elites or Mancur Olsen's theory of the sclerosis of bureaucratic organisations; but this is just all up-to-date OR, and all trends that one can already discern within the precincts of the orthodox economics profession.¹³ It is science planning in the name of rendering the research process more responsive and cost-effective—and since there is no longer any political constituency attached to an abstract 'societal welfare', much less the medieval idea of the university, the proposal cashes out as hired consultants for the powerful, the well-organised, and the well-heeled.

But wouldn't the ultimate irony be to discover that it wasn't just Kuhn's *SSR*, or Quine's naturalism, or Merton's norms; no, most of what passes for successful 'social theory' in the Anglophone world is largely inseparable from the postwar organisation and conceptual structure of the natural sciences; and that just because that one believes that 'most of what non-scientists need to know in order to make informed judgments about science falls under the rubric of history, philosophy and sociology of science, rather than the technical content of scientific subjects' (1998, p. 10), that this is no reliable prescription for political action, but just reprises the Cold War 'double truth' doctrine in yet another guise? Wouldn't it end up being the *SSR* for the MTV generation, or more to the point, the era of fully global privatised science? Wouldn't it be poetic justice to discover that, having established that Kuhn could not set the terms for the reception of his book, nor control its ultimate message, Fuller found out he can no more transcend his context either?

Notes

1. Popperians were always wary of Kuhn from the beginning; see, for instance, Motterlini (1999) and Jarvie (1988). But there are fewer and fewer of those around these days; so the modern disaffection should be traced to other sources.
2. All subsequent undated page references are to Fuller's (2000a).
3. In a text Fuller unaccountably neglects to cite in his book, Kuhn wrote: 'Forty years ago . . . I came upon a few pieces of the Continental literature on the methodology of social science. In particular, if memory serves, I read a couple of Max Weber's methodological essays . . . as well as some relevant chapters from Ernst Cassirer's *Essay on Man*' (1991, p. 17).
4. Indeed one can go even further, as Hacking (2000) has done, and attribute the centrality of the doctrine of 'incommensurability' in Kuhn to his characterisation of the dull algorithmic character of normal science: 'Kuhn came to expect incommensurability because he turned flexible ordinary languages into abstract structures between which mutual adaptation or translation had been engineered out'. This certainly was the case in Kuhn's post-*SSR* career.
5. Fuller (2000a, p. 258) compares Kuhn's 'revolutions' to Walt Rostow's own stage theory in economic history; I believe this provides novel insight into the types of arguments that repudiated simplistic rational-choice theories and yet still resonated with Cold War mandates. On the hallowed lineage of stage theories, see Meek (1976).
6. The biography by Hershberg (1993) renders this problematic and its consequences with greater clarity than does Fuller himself. Hershberg quotes Conant as writing: 'I do not like the atomic age or any of its consequences. To learn to adjust to these consequences with charity and sanity is the chief spiritual problem of our time' (1993, p. 572).
7. See, for instance, Armen Alchian; Kenneth Arrow and William Capron, 'An Economic Analysis of the Market for Scientists and Engineers', RAND Research Memo RM-2190-RC, 1958.
8. On Polanyi's position, see Philip Mirowski, 'On playing the Economics Trump Card in the Philosophy of Science' (1997).
9. To find descriptions of this history, as well as outlines of the content of operations research, see Fortun and Schweber (1993), Mirowski (1999), Mirowski and Sent (2002), Rau (1999).
10. Fuller briefly discusses the connection on (pp. 327–328), but does not make the links to OR.
11. Fuller even goes so far as to assert that the social sciences provide a more virtuous pedagogical model for science studies, since they 'do not launder out ideological disagreements in professional training, but rather enable those disagreements to align with, and often alter conflicts in the society at large' (p. 401). While this is not the place to make the argument in detail, I would suggest that the modern social sciences can be split into two groups: those who do their laundry in private, and those who scrub in public but have no impact on conflicts in the society at large.
12. One indication of this possibility is the passage on p. 349 that endorsed the neoconservative economist Thomas Sowell's attack on the labour theory of value from a neoclassical standpoint. He writes: 'should not STS practitioners ultimately prefer a theory of scientific value based on utility rather than labour?'
13. I am not the first to worry about this. See Steve Downes' review of Fuller's *Science* in *Philosophy of the Social Sciences*, March 2000.

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