

**Cracks, Hidden Passageways and False Bottoms:
the Economics of Science
and
Social Studies of Economics**

**Introduction to:
The Effortless Economy of Science?
forthcoming, Duke University Press**

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final version 2.0
April 2003

* I should like to express my gratitude to the University of Trento for a friendly and accommodating atmosphere in which to compose this introduction. The University of Notre Dame provided no support or encouragement in this endeavor. I should like to thank the following for conversations concerning these issues: Esther-Mirjam Sent, Steve Fuller, Diana Hicks, Chris Kelty, and the participants in the HPS discussion group on “Science, Truth and Democracy” at Notre Dame.

He once remarked that mistaking science for technology deprived the nonscientist one of the greatest sources of awe, replacing it with a diet as filling as Tantalus's fruit. I had only to hear the man talk for fifteen minutes to realize that he believed science had no purpose. The purpose of science, if one must, was the purpose of being alive: not efficiency or mastery, but of the revival of appropriate surprise...[If he] lamented the commercialization of science, he despaired even more over the science of commerce."

Richard Powers, *The Gold Bug Variations*

I. Living in the Cracks

This is a collection of papers, each of which in one way or another have fallen into those in-between spaces where prudent academics fear to tread. The reader is undoubtedly aware there exist established disciplinary academic formations of hallowed lineage, such as the fields called "economics" and "philosophy," not to mention "physics" and "statistics"; and then, there are also the disciplinary groupings of rather more recent and tenuous purchase, such as the "social studies of science", the "history and philosophy of science", "science policy," "metrology", and the "history of economic thought"; but protracted experience has now convinced me that almost none of my contemporaries' mental maps has commodious space set aside for the social study of the history of economics, the economics of the philosophy of science, the history of the economics of science, the political history of analytical philosophy of science, or the comparative philosophy of quantification in physics and economics. I have encountered people in the course of my travels who apparently felt that critical/historical inquiry into the political and economic underpinnings of science would be tantamount to blaspheming the bible, or defiling the music of the spheres, not unlike the attitudes of the dyspeptic character in Richard Powers' *Gold Bug Variations*. Mark Blaug (2001), among others, has pronounced the social study of economics an anathema; and below we shall take note of various scientists who have deemed it inconceivable that scientific numbers could constitute anything other than Nature braying in a very loud voice. Philip Kitcher (whom we shall encounter repeatedly below) has waxed nostalgic that "Mertonian sociology of science was the first (and last) serious sociology of science" (2000, p,S38), and bewailed the fact that from his perspective, everything else since has been a disaster. People do seem inclined to batten down their mental hatches when they detect someone venturing to explore the history of confused comparisons of science to an economic process, or else

engaging in comparative cross-disciplinary empirical investigation of the relative stability of quantitative scientific measurement, or asking whether science could possibly have underwritten the widely revered ‘laws of supply and demand’ which supposedly direct our modern scientific endeavors, or questioning whether a statistical tradition like econometrics really deserves the empiricist reputation which it appears to enjoy. In such instances, one is flagged with the warning: You can’t get there from here.

Of course, no one is obliged to rejoice in any such combinatorial explosion of mix-and-match hybrid inquiry, just as no one can be expected to voluntarily sing the praises of hybrid corn or chi-chi designer cocktails. And perhaps most people are simply hewing to the prudent maxim that strong walls and well-insulated buffer zones make for friendly neighbors. Nevertheless, it has struck me forcibly that there is something distinctly odd about some of those interstitial spaces on the academic disciplinary map marked “There Be Dragons”, the crawl spaces and half-basements and hidden stairwells that surround the conventional academic disciplines. As Steve Fuller has put it in a different context, “The intensification of analytic philosophy in the United States from the end of WWII to the end of the Cold War may have unwittingly instilled what the social critic Thorstein Veblen would have called a ‘learned incapacity’ to reflect on the social conditions of knowledge production” (2002, p.171). Consequently, the papers in this volume have set out to explore that no-man’s land at the borders of science and economics with attendant emotions pitched somewhere beyond the thrill of spelunking and the delectation of trespass. It has often been forcibly brought home to me that most people really do not want to entertain the idea that science and economics are inextricably intertwined in something beyond ritual obeisance to a vacuous ‘marketplace of ideas’, or at least have avoided it until very recently.

The fear and loathing accompanying the areas reconnoitered by the papers herein has an identifiable source, something that has only recently been accorded serious attention by some perspicuous authors such as Wade Hands (2001) and Don Howard (2003). Contrary to popular impressions, political economy and what Hands calls “science theory” have enjoyed a protracted and intimate liaison; it is just that the affair has been kept surreptitious, perhaps a bit seedy, and maybe even equipped with plausible deniability, especially after the 1950s, as Howard makes clear. Philosophers of science prior to WWII had unabashedly committed to

normative doctrines of what science was ‘good for’ – think of John Dewey, or Otto Neurath, or Pierre Duhem or F.S.C. Northrop -- and openly linked them to their political and economic enthusiasms; for their part, economists have always had direct access to favorite images of ‘science’ not only to justify their own methodologies but also to provide templates for the very content of their models (Mirowski, 1989); policymakers have always had to deal with the conundrum that he who would take the prince’s coin will perforce sing the prince’s tune; those who design and build scientific measurement devices have always understood that the very act of measurement was costly, not to mention disruptive, and therefore measurement was always corrigible and calibrated relative to a larger budgetary framework. Earlier generations understood that scientific research was not ‘naturally’ oriented towards any single goal or terminus. There is nothing new in all this. What was novel, and accounts for much of the modern disdain which greets the hybrid inquiries mentioned above, was the fervor with which all these things were descried and denatured in the second half of the 20th century. It is true that the movement for a “value-free science” began in Germany in the early 20th century (Proctor, 1991), but it flourished with a lag in the United States. Analytic philosophy starting with Reichenbach (1938) declared that the context of discovery was irrelevant for the context of justification; pundits repeated *ad nauseam* that scientists could never be held responsible for however their discoveries were put to use;¹ thinkers such as Friedrich Hayek and Michael Polanyi insisted that “The attempt to rearrange science or society with some explicit theory of rationality in mind would disturb the delicate balance of thought, emotion, imagination and the historical conditions under which they were applied and would create chaos, not perfection” (Feyerabend, 1978, p.7). Not coincidentally, during the period of the largest expansion of military funding of all the sciences in history, public figures such as Vannevar Bush and Warren Weaver were trumpeting the virtues of “free science”; nominally free, that is, from the dictates and directions of its paymasters, with the supplemental ‘linear model’ of pure science ⇒ applied science ⇒ technology appended to placate the taxpayers. For their part, American neoclassical economists busily began banishing the history of economics from their curricula, the better to maintain the fiction that theirs was a timeless doctrine of disembodied “rational choice”, cut adrift from all

¹ Or, in the words of that other famous philosopher, Tom Lehrer, “The rockets go up/ Who cares where

institutional specificity or, conveniently, direct inspiration of the physical sciences. Because it suited their own purposes, they also subscribed with alacrity to the ‘linear model’. Politically pugnacious economists such as Milton Friedman and Kenneth Arrow threw their weight behind the supposed “positive/ normative distinction” in their theories, which putatively left the decisions as in what welfare consisted to their clients. Philosophers, together with statisticians and economists, began to pretend that elaborate statistical algorithms incorporating garbled components of Neyman/Pearson and Bayesian techniques, perhaps fortified with game theory as in the case of sequential analysis, could somehow provide solutions to the value-relativity of measurement and quantification, not to mention age-old problems of induction. Somewhere along the way, “science” itself was getting conflated with mechanical induction, a conflation made more acceptable by the hypnotic flashing lights of those imposing mainframe computers upon which the algorithms were played out. All and sundry further tended to treat some generic thing called ‘mathematics’ as if it was capable of cutting the Gordian knot binding science and the economy all by itself.

This is not to assert that everyone with a modicum of intellectual pretensions withdrew altogether from the hybrid concerns located at the intersection of the *Geisteswissenschaften* and the *Naturwissenschaften*; indeed, we identify and discuss many of the polymaths who influenced the 20th century science/economy problematic in this book. It is rather that the previously pertinent disciplines (Philosophy, Economics, science policy,...) in the American academy were thoroughly reconstituted in the post-WWII period so as to define these concerns as lying primarily outside of their ambit. The postwar phenomenon of “analytic philosophy” provides one instance of this pattern (Howard, 2003; McCumber, 2001); the rise of American Neoclassical economics is another (Mirowski, 2002); the postwar construction of politics was a third (Purcell, 1973). The heretofore neglected role of “Operations Research” in channeling trained natural scientists into a generic but well-funded theory of the command and control of social organizations provides yet another exemplar (Hughes & Hughes, 2000; Kirby, 2000). The rise of Mertonian sociology of science, constituted as a study of science purely from the outside looking in, as if through bulletproof glass and one-way mirrors, constitutes a fourth (Hollinger, 1996; Wang, 1999; Turner, 1999).

The fact that exfoliating science/economy hybrids were rooted out and rendered sterile across the academic board suggests that this was a systemic phenomenon, linked to the postwar state/military organization and funding of science in the American context, and not some mere artifact of this or that disciplinary peccadillo.

In this book, we do not plan to waste time bemoaning the fact that the science/economics nexus was brusquely shoved out to the margins of discourse after WWII, but instead set out to explain and understand the ensuing state of affairs. After all, the relationship of economics to the republic of science continues to be fraught with pitfalls, and ironies abound. The scientific status of economics is often a matter for ridicule and disdain; but anyone who is familiar with the history of economics knows that just about every economic theory which has ever gained a modicum of adherents has of necessity attached itself to some very specific conceptions of science, both in method and content. Conversely, there is no natural science which has achieved a rude minimum of results without keeping at least one eye trained on the economic foundations of its activities, both in a practical sense (Who pays for research? *Cui bono?*) and in such a manner that resonates with the economic common sense of its era (variational principles, conservation principles as no free lunch rules, 'fitness' as individual success, Nature as rationally comprehensible). One polarity of the distinction does not merely 'inform' the other, but instead, they have always been mutually constituted. Therefore, it will not get us far to try and pin the blame for this artificial quarantine on a particular school of economics, or the ideas of one or two politicians, or the ambitions of this physicist or that biologist. Since the papers in this volume often argue that the Social and Natural are often intertwined, prudence would dictate that some caution is also called for when posing the question, What are the forces which argue for and militate against the kinds of research contained in this volume?

There are a few hopeful developments which suggest that the Cold War deep freeze of any protracted examination of the science/economics nexus may be starting to thaw. These trends fall under three broad categories: [1] recent profound changes in the social structure and organization of science, which one can regard as the post-Cold War regime change; [2] the recognition, epitomized by Hands (2001), that older notions of 'economic methodology' are moribund, and that the intellectual future resides in an amalgamation of natural science

and economics themes into a general research program of the political economy of science in action; and [3], the increasingly strenuous competition between the budding specialty of ‘science studies’ and a certain branch of American philosophers of science to claim expertise over inquiries such as those found in this volume. After a brief glance at these developments, I shall give some indications of how the papers herein can be read as contributions to a field of inquiry which has yet to find a name, but may eventually find a home within science studies.

2. How Science Theory is being Forced to Come to Terms with Economics

[1] *Privatized science*. The initial impetus behind this collection, as well its companion volume (Mirowski & Sent, 2002) is a conviction that a number of profound alterations have been happening to science in the last two decades, and that we who are caught up in the groundswell possess a very inadequate vocabulary with which to understand it. Very roughly, as explained in that companion volume, we are situated in the midst of a top-to-bottom transformation of science organization and funding in the Western countries, starting from a Cold War regime to something we call a regime of globalized privatization. In the rarified precincts of science policy this is neither an altogether novel nor controversial thesis;² and yet, being so ubiquitously all-pervasive, reaching into every nook and cranny of laboratory life and encompassing such far-flung diverse phenomena as --: the dissolution of the individual scientific author as lynchpin of inquiry and bearer of ‘credit’, the legal and conceptual reconstruction of intellectual property, the re-engineering of the institution of the university and the reconfiguration of previous disciplinary prerogatives, the outbreak of the Science Wars and the consequent loss of any sense of the mutual reinforcement of the natural and social sciences and the humanities, the unreflective treatment of information as a commodified ‘thing’ and its obverse phenomenon of a contempt for the history of inquiry, and the ascendancy of postwar American neoclassical economics as the *lingua franca* of public justification of any and all of the above – the high fences and minefields between the relevant disciplines have stopped being merely a nuisance and become downright

debilitating, rendering us incapable of appreciating the breadth and scope of the changes happening all around us.

The attempt to wean the university off its 20th century military patrons and turn it into a cornucopia of ‘technology transfer’ for economic growth and regional development has had the unintended consequence of reviving the entire question of the legitimacy of an “economics of science”, for both economists and for philosophers (Fuller, 2002). Whereas previously “pure science” was deemed situated beyond the purview of economic considerations, with the possible exception of Arrovian appeals for no-strings funding to subsidize the ‘public good’ of inadequately appropriated knowledge, now it has dawned on any number of pivotal constituencies that the privatization of funding and the vast expansion of intellectual property might entail revisiting the question of the economic underpinnings of science. Predictably, some economists have jumped into the fray with assertions that their same old ‘tools’ could be applied to ‘science’ in the same ways that they have been applied in the past to hamburgers and welfare checks (Stephan, 1996; Shi, 2001, Jensen & Thursby, 2001; Leonard, 2002), and some philosophers and science policy analysts have followed suit (Goldman, Kitcher, and Ziman in Mirowski & Sent, 2002; Schmidt, 1994; Zamora, 2002). However, most natural scientists themselves have been notably reticent to permit themselves to be situated on a par with temporary laborers, salesmen and entrepreneurs, and have consequently been inclined to cast the net wider for what they consider to be a better way to understand their modern predicament. Perhaps this is not just a matter of the inevitable invidious pecking order between the natural and social scientists, or a misguided belief in class distinctions, but should be taken more seriously as one symptom of a structural weakness of recent forays into a “new economics of science.”

There are a number of ways to question the relevance of postwar American neoclassical economics in helping to understand the vast reorganization of science occurring in the 21st century. Some have questioned the simple presupposition that “credit is seen as a substitute for money in scientific exchange”(Shi, 2001, p.28; see chapter 7 below); others complain that all the various neoclassical variations upon a decision-theoretic framework totally misapprehends the cognitive attributes of the successful scientist and has no plausible scientific basis in psychology (see chapters 6 and 11 below); while still others point out that the idea of a ‘free market’ for

² See, for instance, (Ziman, 1994; Gibbons et al, 1994; Lessig, 2001; McSherry, 2001) and of course

ideas functions as a purely metaphorical construct with no objective correlatives which compare to the full panoply of legal and social rules and norms of actual markets. Whatever the merit of each of these complaints, I believe there are two primary obstacles which will inevitably obstruct the pursuit of a vibrant and useful neoclassical economics of science. The first is the observation that neoclassical economics was itself patterned upon 19th century energy mechanics (Mirowski, 1989), and even in its modern incarnations maintains a rather outdated notion of certain key scientific terms such as ‘equilibrium’, ‘frictions’, and even ‘model’; beginning in the second half of the 20th century *avant garde* natural scientists noticed this when they became acquainted with neoclassical economics, and this fact was often sufficient for them to lose all respect for the scientific ambitions of economics (Mirowski, 2002, chap.3; Waldrop, 1992) Even modern attempts to import more *au courant* techniques into the economics of science such as spin glass models (Brock & Durlauf in Mirowski & Sent 2002) rarely overcome this aversion, since most physicists are not noticeably favorably impressed with their quotidian research struggles being compared to the behavior of a molecule on a lattice in a magnet. (Unlike economists, they only need to see the mathematics to understand its provenance.) In short, while portrayals of agents as natural physical phenomena may go a long way in conjuring valid scientific credentials and solid scientific legitimacy in the minds of the general public, it tends to be much less effective in recruiting and placating the very actors who actually construct and deploy those same mathematical models of natural physical phenomena on a daily basis.

Whatever one might think of the prospects of a rational mechanics of the social structures of science, it will turn out to be the second obstacle which most insistently prevents the application of orthodox neoclassical ideas to the contemporary problems of the interanimation of economics and science. For the hallmark of the neoclassical tradition is to search for the timeless, abstract and fundamental laws of economic behavior, irrespective of cultural, geographical and structural idiosyncrasies of human interaction. This accounts for the central place that such doctrines as utility maximization, the dominance of calculation of self-interest in human nature and Nash equilibrium hold in the neoclassical canon. At first glance, such universal ambitions would seem to resonate with the idea of globalization in the economy and in science management, enforcing as it does greater uniformity in commodities, production and

(Mirowski & Sent, 2002).

retail trade. And yet, it is this very renunciation of all differences which make a difference which ultimately renders the tradition incapable of adequately dealing with historical change, and in particular, with the precept that changing scientific organization and theories will eventually result as an unintended consequence in a transformed economics. There is something curiously poignant about an imperialistic economics, always looking forward to new frontiers to conquer, which stridently insists upon the essential stasis of its subject matter, when in fact its own conditions of existence are persistently being revised in tandem with those of the process of scientific research.³ For instance, recent avatars of a “new economics of science” such as Paul David have applied their generic game theoretic model indifferently to the Scientific Revolution of the 16th century and to late 20th century science (David, 1998; David & Dasgupta, 1994) to explain the ‘efficiency’ of Western science. His vision of utopia is situated squarely back in the Ike Age, when Stanford grew from mediocrity to eminence through mass infusions of military funding and science management. Another paper of this ilk attempts to argue there exists only one uniquely efficient way to set up intellectual property rights in a university context for all time (Jensen & Thursby, 2001), conveniently ignoring the fact that earlier neoclassicals had equally glibly defended the obverse Cold War separation of university scientific research from commercial exploitation as ‘efficient’ when it was politically expedient (Hounshell in Hughes & Hughes, 2000). In these and numerous other cases, neoclassical economics is primarily serving as a weapon of mass distraction, diverting attention from the causes and consequences of the vast contemporary reorganization of the scientific process, and not providing tools for understanding and discussion of the emergent nature of science in the 21st century. It is instead a misleading caricature of an “effortless economy of science”—modern science as a set of self-sufficient and efficient social structures, perduring throughout human history, patterned upon the neoclassical image of the market.

Perhaps my attitude is a little too redolent of Pollyanna (although I must aver that I am not often accused of this personality trait), but it does seem to me that most scientists will eventually come to realize that the so-called ‘new economy of science’ is serving as little more than the emperor’s new drycleaner, and that confident appeals to the effortless economy of

³ Hands (2001, p.373) gives this an especially wicked caricature: “Take a game theoretic model from industrial organization theory; change the firms or players to ‘scientists’; add the adjective ‘epistemic’ in a few places... and suddenly you have a philosophical model of scientific knowledge.”

science in our brave new world -- of wrenching career restructuring, of offices of technology transfer and research governance boards, of offshore outsourcing of laboratory work to needy Second World countries, of interminably stretched-out employment periods as university unfaculty, not to mention legal agreements over intellectual property the length of *War and Peace* – this notion of an effortless economy of science will come to appear a quaint aberration in the annals of intellectual discourse. Management consultants are already turning this neoclassical notion of a new economy of science into a chalice of hemlock: for instance, contrasting universities with McDonalds franchises, invidiously comparing the former “dumb organizations” to the more market-saavy “smart organizations” (Stewart, 1997). Instead, people who are seriously concerned with the health and wellbeing of the scientific enterprise will eventually want to know how it was that we arrived at the modern state of affairs, which means they will want to know something about the set of conditions which gave rise to the temporary configuration of the post-WWII structure and conduct of university science, as well as the dubious ways in which science was then portrayed as transcending market operations, which suggests in turn that they will be interested in the papers included below in the section on “Science as an Economic Process”.

[2] *Revamped relationship of philosophy to economics.*

Once upon a time economics knew its place, and philosophy conspired to keep it there. This era, as already hinted, was located in the latter half of the 20th century. During this Bronze Age, economists were very worried that they be highly regarded a science; philosophers deigned to assist them in their ambitions by pointing to the scriptures of the scientific method, generally extracted from the annals of their favorite science, physics. This service was performed out of sheer public-mindedness, an altruism thoroughly removed from the rough-and-tumble of political considerations or considerations of social context.⁴ Economists, deferring to their betters, sought to incorporate all the ritual behaviors of axiomatization, covering-law models, quantification, quarantined contexts of discovery, inductive inference, falsification, normal science, hard cores and protective belts, and all the latest accoutrements. Some might complain

⁴ Ron Giere has glossed this postwar beneficence as: “Don’t think about the fact I am a German immigrant or speak with an accent, just consider the validity of my ideas.” (quoted in Howard, 2003). I discuss the strange

that their recital of the catechism was less than fervid, and their ritual performance a tad mechanical, but nevertheless, philosophers hastened the balm of scientific reason on its downhill course, and the economists in their inferior location dutifully scooped it up.

One of the benefits of recent scholarship in the history of the philosophy of science is that we now can see that there was little about this parable that actually rang true; and therefore, it really has become imperative to rethink the very idea of a professional field of “economic methodology”. Wade Hands (2001) has made this case with great aplomb, so we need not explore the present predicament of the self-styled methodologist here. However, a quick glance at the content of the ways philosophers have used economics in the past does bear some relevance for the papers collected herein. In particular, the philosophical Holy Trinity of Popper, Kuhn and Lakatos have recently come under intense scrutiny with a secular trio of intellectual biographies (Hachohen, 2000; Fuller, 2000; Kadvany, 2001). While they differ quite substantially in their historiographic stances and intentions, one thing that stands out quite starkly in all three is the insistence by the biographers on the central importance of the ‘political economy’ (in the older sense of that term) of each philosopher of science in governing much of their respective doctrines concerning the prosecution of science, even though it also seems that each philosopher took serious pains to suppress or disguise those motivations from becoming too apparent in their published texts. It is worth dallying a moment over each protagonist, if only to set the stage for a different sort of relationship between philosophy, economics and science advocated in this volume.

Let us start with the case of the neoclassical economists’ favorite philosopher, Karl Popper. Whatever his supposed contributions to the philosophy of physics (given that it was frequently conflated with the doctrines of the logical positivists that he said he opposed; and in any event, physicists did not pay much attention beyond perfunctory praise of falsification), it appears clear in retrospect that Popper’s work is saturated with both Marxism and neoclassical economics. Popper was not shy of admitting it in certain contexts, like his intellectual biography (1976, p.113): “it had been in part a criticism of Marxism that had started me in 1919 on my way to *Logik der Forschung*.” The service that Hachohen provides is to situate it as the central defining characteristic of his early career. He reminds us that Popper’s initial notoriety as a

history of the meanings of the “social” in 20th century philosophy of science in my forthcoming “The Scientific

philosopher was due primarily to his two books *The Poverty of Historicism* and *The Open Society and its Enemies*, which can be read as extensions of the famed *Methodenstreit* between German historicists and Austrian subjectivist economists of the late 19th century (Hacohen, 2000, p.468). He demonstrates that Popper started his career as some version of interwar socialist, but upon encountering Friedrich Hayek during the war, and beholden to him for his being rescued from the Antipodes to the LSE, moved rightward with alacrity and became the Cold Warrior which was his major claim to fame in the postwar period. It is important to remember that, “when he bemoaned the sorry state of the social sciences in *The Poverty of Historicism* he excluded economics” (p.284); and that his later “situational analysis” was, as he conceded, “an attempt to generalize the method of economic theory (marginal utility analysis) so as to become applicable to the other social sciences” (1976, pp.117-118). So the postwar philosopher that the intelligentsia hated to love was an Austrian philosopher of science trumpeting the virtues of neoclassical economics and denouncing the vices of Marxism, all under the rubric of the extension of the methods of the natural sciences to the social sciences. The Hayek connection is especially significant, as Thomas Uebel (2000) points out, since their shared attack on the Viennese positivist Otto Neurath and their loathing for his ‘scientific socialism’ tended to wash out all their other substantial bilateral intellectual differences, such as conflicting evaluations of the ultimate wisdom of a ‘social physics’, or the indispensable role of ‘psychologism’ in the social sciences. The fact that his biographer suggests Popper was not personally familiar with much actual social science writing, nor indeed had much patience with describing actual social structures, given that he “paid no attention to institutions... When he spoke of observation, experiment and refutation, he made no distinction between the individual scientist and the community... He presupposed a Republic of Science but did not formulate a vision of it” (2000, p.513) only renders the thesis more salient that what tended to set Popper apart from the general run of philosophers was his particular vision of the political economy of science, and not any specific analytical innovations or perceptive descriptions of the day-to-day operations of science. Indeed, the supposed prophet of critical rationalism characteristically never even considered whether it was possible for laypersons outside of the science in question to criticize the agenda of the research program pursued by scientific leaders (Jarvie, 2001). However much he sought to

distance himself from the logical positivist movement, this basic image of a scientific method floating free of all social instantiation, ineffable and yet incongruously inaccessible to ‘totalitarian’ societies, was the key to understanding the relevance of Popper to the Cold War world.

Even more fascinating is John Kadvany’s account of how a Marxist-inspired Hegelianism disguised as an improvement upon Popper came to supplant Popperian philosophy of science on its home ground at the LSE, as well as in the hearts and minds of many Europeans. Imre Lakatos (Lakatos) began his career in Hungary as an avid Stalinist and (apparently) a strong externalist in the philosophy of the natural sciences, but fled after the disastrous Hungarian Revolution in November 1956 to England. There Lakatos was fascinated by the strategies by which ideas successfully come to power, and realized that Popper and Hegel shared substantially more than the former would admit, such as their antifoundationalism and their appreciation of error and its role in the progress of human knowledge. He came to realize, as Kadvany explains, “That same kind of historicism is implicit in the ‘negative’ progress made via the critical elimination of theories is, for Lakatos, Popper’s Achilles Heel. This leaves open the technically sweet philosophical problem of historicizing Popper” (2001, p.12). There he set out to implement his insight in his Ph.D. thesis, *Proofs and Refutations*, a stunning display of virtuosity which argued that the complex process of mathematical proof cannot be modeled in a fixed formal language, at least in part because history had to be intentionally falsified for both pedagogical and philosophical purposes.

What at first seemed an elaboration of Popper’s critical rationalism rapidly turned into a scathing critique of the master, however. Lakatos had a wicked sense of humor, and later characterized Popper’s three main contributions as: “(1) a step backward from Duhem (the falsification criterion), (2) a step backward from Hume (the supposed solution to the problem of induction), and (3) *The Open Society* by one of its enemies” (in Motterlini, 1999, pp.89-90). But more to the point, Lakatos was also skeptical of Popper’s latent neoclassical economics: “Popper... never discusses the ceteris paribus clause problem seriously. Anybody who has read anything about the methodology of economics knows that this clause is always discussed”

(p.88).⁵ However, as Kadvany argues, the Lakatosian Methodology of Scientific Research Programmes is itself a projection of a different branch of political economy: “Lakatos’ covert Hegelianism within Popper’s world demonstrates the virtuous role of intellectual dissemblance and cunning in the Hungarian Revolution” (2001, p.301).

Lakatos, much more than Popper (and in contrast to Kuhn) was concerned with the vexed question: what it is that prevents reason from descent into demagoguery? As his friend and interlocutor Paul Feyerabend understood, “The so-called authority of the sciences, however, i.e., the use of research results as barriers to further research, relies on decisions whose correctness can only be checked by what the decisions eliminate – a typical feature of totalitarian thought” (1987, p.158). As opposed to an evocation of a Land of Cockaigne where the personal fallout from disputes were magically banished, science bequeathed its beneficent supporters only more marvelous consumer goods, and markets always were efficient, Lakatos was concerned with a rather more drab social existence predicated upon powerful bureaucracies and Machiavellian agents and the monopolist’s problem of overproduction—indeed, as Kadvany argues, something more closely representing the Hungary which he had fled. It was a world of staged falsifications in the public quest for truth, elites and personality cults, dissembling and betrayal. In this world, pathologies could sometimes become virtues: histories could be falsified, elites were a necessary evil, it was sometimes prudent for criticism to be covert, and deliberate lies were not always straightforwardly mendacious (as in ‘rational reconstructions’). In contrast to earlier postwar stories, Lakatos urged a more skeptical political economy of science: “Scientific autonomy is not equivalent to the unconstrained funding and support of any scientific research” (2001, p.224). Perhaps he was joking – and then again, perhaps not—when he said: “we are currently facing both an academic and a publication explosion. We therefore need a demarcation criterion in order to be able to ‘burn’ the right people and the right books” (in Motterlini, 1999, p.96).

This political economy of the commissar proved altogether too bleak and downbeat for American tastes; and therefore Lakatos complained that the new paymasters opted for what he called “a rather ad hoc footnote to Polanyi and Merton” (p.30), namely, the characterization of normal science found in Thomas Kuhn. Early on, Paul Feyerabend recognized that Kuhn’s *Structure of Scientific Revolutions* was an “ingenious defense of financial support without

⁵ See also (Motterlini, 1999, p.107) where Lakatos cites Latsis as showing that neoclassical economics is a

corresponding obligations” (1978, p.99); but it has been the lively book by Steve Fuller (2000) which has brought the political economy of the Kuhnian portrayal of science to widespread attention. Since this work of Fuller is considered at length below in chapter 5, it may suffice here to note that the political economy of Kuhn was in practice just as authoritarian as that of Lakatos: “I was trying to explain 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” (Kuhn, 2000, p.308). Fuller juxtaposes Kuhn’s revolutions to Pareto’s theory of the circulation of elites (2000, p.166), but I think that particular comparison is a little misleading. After all, Pareto’s sociology was intended to account for the irrational surd of society left over after neoclassical rational choice had explained the fundamentals. Kuhn’s aim was instead to assert the unintentional benefits of leaving normal science to its own devices, once the paychecks were cashed. But stripped of Lakatos’ Hegelian historicism and bereft of any acknowledgment of the deadly politics of large bureaucracies, Kuhn’s centerpiece of normal science corresponds most nearly to the rational choice image of the operations researcher, something he himself spent the war engaged in.⁶

Operations research was a new field growing out of WWII, and was the major conduit bringing physical scientists into what would be considered the social sciences in the second half of the 20th century, and had intimate contacts with the rise of the American neoclassical orthodoxy in that time period (Mirowski, 2002, chap. 4). It was the nascent science of command, control, communications and information processing *part excellence*. It was initially applied to problems of military organization, which in the post-WWII context also meant the problems of science management, since the military had become the pre-eminent patron of scientific research first in America, and later, elsewhere. Later on it became applied equally to corporate settings (Kirby, 2001), and had profound implications for fields seemingly as far apart as analytic philosophy, artificial intelligence, organizational theory, politics and computer science. While

‘degenerating program’ in his terminology.

⁶ “I was assigned to the Ninth Bomb Division in Rheims as an advisor on radar countermeasures. That was a... what did you call it... industrial engineering group?—that’s not quite what it was called. These people who applied mathematics and science to strategic and other problems in a rather unsystematic way. “ (Kuhn, 2000, p.271) Kuhn’s absent-mindedness on his participation in ‘operations research’ should not be taken as evidence on its impact upon such matters as his career choice upon return from the war, as this interview makes clear. Also, one should not be misled into thinking that Kuhn was somehow *more* of an historian than Lakatos; as he admits in the

the explicit history of operations research [OR] is not discussed as such in this volume (but see Mirowski, 1999), in retrospect it nevertheless constitutes one of the threads that connects most of the topics of the papers in this volume. For instance, the crusading aura of the operations researcher was that he would bring the bracing tonic of quantification to fields and questions which had previously been sorely wanting in that dimension; and as such, he helped to foster the mistaken belief that quantification and precision measurement were methodologically stabilizing influences bestowing scientific credibility in their own right, entirely removed from any social dimension to science. The papers below in the section on “Rigorous Quantitative Measurement” were written with an intention to counteract such superficial understandings of the role of numbers in scientific models. Another calling card of postwar OR was a certain facility with statistical models, which was itself an artifact of the spread of probability concepts throughout physics in the first half of the 20th century. Without such a broad-based proselytization across the academic board for the empiricist virtues of statistical procedures on the part of OR, it is indeed doubtful that econometrics would have had quite the impact that it did in the second half of the 20th century. The papers in the section below on Econometrics were written in order to demonstrate that statistical algorithms did not really solve many of the thorny problems of inductive inference as much as it shoved them under the rug, especially (but not exclusively) in postwar economics. Finally, OR was often retailed as the application of a generic “Scientific Method” to issues of war (and later peace); the reason, as explained in (Mirowski, 2002), was that physicists and other natural scientists wanted to be consulted on issues of command and control, but not to *become* social scientists; the myth of a portable scientific method served their interests well in that regard. It was their conjuration of a generic expertise in models of command and control which also underwrote their arrogation of special status for themselves as scientists to be situated above and beyond the reach of conventional social hierarchies and external demands for accountability for their own activities. (During WWII, American Operations Researchers enjoyed a unique and envied status of officer rank outside of the command structure of the individual services.) And this brings us back full circle to the political economy portrayals of science considered in the section on “Science as an Economic Process”.

same interview: “... 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 an historian” (p.276).

The historical protagonist (as opposed to the philosopher's ally) Thomas Kuhn assumes a different hue when situated in this context. It was becoming apparent that earlier attempts by figures such as Michael Polanyi to argue for the monastic quarantine of science from society couched in a discourse of faith and tacit knowledge would just not do the job in a postwar world predicated upon the military funding and organization of scientific research, based as it was on large impersonal laboratories and the new phenomenon of science managers.⁷ That job fell to a short book in which, notably, *none* of the examples were drawn from any science later than 1920 (Fuller, 2000, p.74). The *Structure of Scientific Revolutions* is a book which argues that science has always operated in the same manner from the 16th century to the present, that cognitive competencies are pretty nearly on a par everywhere in science so that there disagreement is fairly rare, that normal scientists voluntarily relinquish their intellectual fates to their authoritarian masters, that accountability equals control over the ability to rewrite your own collective history, that the optimal strategy in scientific research is to not rock the boat (but if you must, make sure beforehand you have plenty of the right kind of allies), with 'incommensurability' promoted up front as a warning for the layman to not even think of becoming embroiled in paradigmatic commitments he could never hope to come to comprehend. Therein lay the gist of Feyerabend's pointed diagnosis of "an ingenious defense of financial support." The true genius of the book is that it was written with sufficient recourse to the idiom of 'revolution' to appeal to its 1960s-era contemporaries, and in a style so guileless that it was assigned to subsequent generations of undergraduates.

It does not matter whether Thomas Kuhn the human being ever seriously set out to write an *apologia* for Big Science in the military modality; it served that function nonetheless. Further, the above doctrines resonated rather conveniently with the operations researcher's approach to the corporate organization, again intentionally or not. Scientists' individual preconceptions could be described by the fledgling field of cognitive science (Kuhn's duck-rabbit example); they made optimal calculations of their own individual self-interest and engaged in game-playing which might be described as strategic in the novel idiom of game theory; normal science was an authoritarian hierarchy, rather like an army or a corporation; 'revolution'

⁷ The new world of Big Science and its managers is discussed in (Galison & Hevly, 1992; Galison, 1997; Kay, 1993; Kohler, 1991; McGrath, 2002). Polanyi's own anti-Bush version of the social benefits of science is discussed in chapter 3 below.

was something to be harnessed and pacified (like those operations researchers in the seething Vietnam conflict); historical narratives were little better than propaganda subordinate to considerations of power and influence; science produced output for its own sake in a context of generalized individual competition. Perhaps the only aspects of postwar OR that were missing were a penchant for quantification of paradigms (although Kuhn did write elsewhere on ‘quantification’ itself as a scientific stratagem) and a recourse to probability and statistics to summarize the bold generalizations from individual case studies (that option was left to the Mertonians and their ‘scientometrics’, a pale imitation of econometrics).

Contrary to popular convictions, the philosophy of science was absolutely saturated with the language and concerns of the dominant political economy in the postwar period; it is only now, with some historical distance and in the wake of the Fall of the Wall, that we can begin to perceive the extent of its influence. This has serious implications for the ambitions of a new generation of philosophers who seek to promote their own version of “social epistemology” (Schmidt, 1994; Goldman, 1999) as an antidote to the creeping painful irrelevance of contemporary philosophy of science (considered in the next section below); but it also has direct importance for what those less infatuated with academic philosophy can hope for in a seriously engaged economics of science. The papers anthologized herein call for a much clearer picture of what real science is capable of doing right now and how it operates. Contrary to self-styled defenders of science, the world does not particularly need yet another reified picture of the imaginary operation of a perfect marketplace of ideas. Recent scholarship helps us understand that a useful economics of science will tend to live “in the cracks” between a self-confident economics, the pedagogical methodological self-image promulgated within the formal natural sciences, and a righteously prescriptive philosophy of science. The reason for this exile is that very tendencies we have identified above: where economists revel in atemporal natural science-inspired models of social activity, philosophers of science tend to surreptitiously retail thinly disguised political economy notions to each other and the larger populace; and the natural scientists sometimes fall back onto their supposed monopoly of a generic scientific method in order to better control their own nagging dependencies upon various constituencies within the university and upon the larger political economy, the better to be permitted to carry on their own research in ways they see fit. It goes without saying that in recent times all three tend to treat

history after the fashion of Big Brother, and as if that weren't bad enough, treat each other with open contempt. The denizens of the Technology Transfer Office and their economist allies think they have found a better way out: simply privatize the process of research and let the best epistemologists win! The claims and counter-claims of each of these constituencies have thus turned out to be extremely opaque and unhelpful when it comes to understanding the patterns of the contemporary privatization of science identified in section [1] above. Unless one is to be permitted to shift idiomatic bases with relative abandon, and to bring to bear the insights of one tradition upon the presuppositions of another, without holding inquiry hostage to the half-submerged value systems of any one of these groups, I seriously doubt we shall breach the impasse of modern science theory. That is the guiding principle of any inquiry which has to replace the older moribund notion of 'economic methodology,' at least if there is to be something more than crass opportunism and pedantic special pleading for existing academic disciplinary boundaries. That is the standard that the papers in this volume try to adhere to.

[3] *Possible alliances with science studies.*

The question still stands as to where such hybrid inquiries can find a home, and whom, if anyone, will take them in and nurture them. This is a life or death issue for the history of economics in general (Weintraub, 2002), as well as the hybrid studies contained herein. If we hew to our own precepts, it follows that, at least in part, this will turn out to be a function of when and how the modern university gets restructured.⁸ While there do exist alternative sources of support to contemplate the place of science in society, such as the popular media, politically motivated think tanks, contract research houses and so on, it does nevertheless seem rather far-fetched to argue that the varieties of expertise herein envisioned for the studies championed herein could actually thrive outside of a university setting. For instance, it would result in cognitive dissonance, not to mention existential nausea, for a specialist in science theory to hang out their shingle offering themselves for hire as possessors of generic expertise in pronouncing upon the goals of scientific inquiry in a democratic society, since that would inescapably embroil them in becoming an advocate for one particular constituency, a role the science studies avatar

⁸ This is not the venue to go into the possible geographical bifurcations between the American academy and the European and Asian situations, something which shows up pronouncedly in the Weintraub volume. Science studies as an academic formation itself appears to be more institutionally robust in the European academic context.

might assume under only the most dire of circumstances.⁹ The whole point of insisting upon scientists and non-scientists as inextricably enmeshed in mutual determination is to come to comprehend which particular research programs get stabilized through processes of negotiation, recruitment, purchase and realignment; and hence eventually to appreciate that their publicly stated objectives are frequently as much a *post hoc* rationalization of events as they were causal forces in bringing them to fruition. In such instances, legitimacy is always an ongoing construction project; and the university is one of the few places in society where (in the past) tolerance of messy construction sites and this sort of ambiguity could be reasonably presumed.

There are a few niches in the university open to providing a haven for this style of thought: sometimes they are dubbed ‘science studies units’, sometimes centers for the sociology and history of science, less frequently programs in the interface of business and technology. Thus it would seem pertinent to ask whether the studies in this volume could be reasonably supposed to fit comfortably under any of those rubrics. One problem with posing the question in this manner is that the nascent specialization of science studies has weathered some severe storms lately in the guise of the so-called Science Wars¹⁰; and the prognosis for its own place, especially within the American university, remains profoundly uncertain. This is not the appropriate venue to replay those old battles nor indeed rewrite the history of the conflict of the faculties; our only interest at present is to evaluate some modern tensions in science studies writ large, and try and situate this book within that agonistic field. Another problem with our rhetorical question is the implicit need to define and characterize modern “science studies” for the newcomer; this is something which cannot be done justice in the space of the present Introduction.¹¹ If I may be granted the indulgence, I propose instead to meditate for a moment on what I conceive to be a misguided conception of the field of science studies, namely, that associated with the name of Philip Kitcher.

There seem to be two major rivals within the broad church of science studies seeking to provide forums for the discussion of the political/economic forces and their impact upon the

⁹ It is already provoking comment within the science studies community; see (Scott et al., 1990). The anguish is especially evident in the ‘bioethics for hire’ profession, one of the few growth areas for philosophy of science/ethics in the era of privatized science. See (Elliott, 2002).

¹⁰ For some representative texts see (Gross & Leavitt, 1994; Koertge, 1998; Kitcher, 2000; Labinger & Collins, 2002)

¹¹ Some useful introductions include (Biagioli, 2000; MacKenzie, 1996; Mirowski & Sent, 2002).

actual output of scientific research. The first is a subset of the scholars congregating within the ‘social studies of science’ tent primarily concerned with the regulation and arbitration of science and the disputes that tend to arise when other social actors get involved. Authors here would include Donald MacKenzie, Sheila Jasanoff, Steve Shapin, Eveleen Richards, Corynne McSherry, Steve Turner, Trevor Pinch, and Andrew Pickering. These writers tend to engage in historical research, wear the badge of empiricists while acknowledging the pitfalls of selectivity of evidence, seek to explain processes of dispute resolution in terms of attributions of ‘interests’ of parties involved, and maintain an insistence that different social alignments can produce different scientific outcomes.¹² The second contender, and sworn opponent of the first camp, is the philosopher of science Philip Kitcher, although at various junctures the movement might be broadened to include those who march under the banner of “social epistemology”, such as Alvin Goldman, David Hull, and Miriam Solomon.¹³ This phalanx is united by the belief that one can provide abstract philosophical solutions to the political and economic woes which beset science, primarily by mounting a defense of what they view as the obvious unique success of science as a social formation. Their characteristic trope is to render some phenomenon which at first glance appears ‘irrational’ as its opposite when repositioned through the instrumentality of some compact yet stylized ‘model’ of behavior. By inclination, they lean toward the rationalist end of the spectrum, although they do make use of some historical materials taken from others to justify their positions.

The intrepid reader of this volume will notice that a few of the papers below seek to define their objectives by contrasting them with those of Philip Kitcher. From one perspective, this stands as a tribute to his work, since he is the *fin de siècle* philosopher who perceived long before his peers that philosophy of science was inescapably a political undertaking, and as such, he has not hesitated to enter into controversies over issues that really mattered to contemporaries, such as the teaching of creationism, the legitimacy of sociobiology, the justification of the

¹² I expressly decline to discuss the ‘actor-network’ French school of the sociology of science here, even though they have some explicit things to say about economics and science. Some indications why I resort to this perilous pass might be found in Latour (1999).

¹³ The inventor of the term ‘social epistemology’, Steve Fuller, does not fit easily into either of these camps. He has been known in the past to have made extensive use of the work of the social studies camp, although lately he has taken to criticize such icons of the group as Bruno Latour and Michel Callon. His original training was as a philosopher, but he sets himself in opposition to the Kitcher style of philosophy of science. His latest attempt to

Human Genome Project, and so forth. But equally it serves as a warning that something about the makeup of the modern philosophy profession yearns to render it fundamentally conservative, in that Kitcher needs to be understood as one more recognizable member in the sequence of philosophers identified in the previous section as retailing neoclassical economics under elaborate disguise because they conceive it an adequate characterization of the political economy of science. Once this is fully appreciated, then it follows that the papers in this volume (and not just the chapter explicitly concerned with Kitcher's writing) constitute one long argument that the way forward lies with the social studies of science camp, and not with the Kitcher-style social epistemologists.

Kitcher, like so many other Science Warriors, seems to think that the not-so-hidden agenda behind science studies is to debunk, demystify and delegitimize science, and perhaps Reason in general. This notion would be risible, were it not so grossly misleading: most scientists instinctively understand that they would have nothing to fear from any group prosecuting such an outlandish and silly agenda; while most science studies scholars are smart enough to realize that spending most of their waking hours studying science could not be justified under such motivations. Instead, Kitcher misses what seems to be the main division between science studies scholars and philosophers of science: the former wish to enumerate and explore all the ways in which science in action is imbricated in the societies in which it is embedded, whereas Kitcher has always wanted to assert some form of analytical separation between science and society;¹⁴ in other words, Kitcher wants to shore up the tradition represented by Polanyi, Popper, and Kuhn indicated above. Kitcher's quest for a knockdown 'demarcation principle' has assumed various formats over the course of his career, but has only been rendered relatively explicit in his latest book, *Science, Truth and Democracy* (2001). There he restates his position as acknowledging that social structure has *something* to do with the way scientific research is carried out, but that in the majority of cases, the criteria for the success of science are effectively independent and separable from the value criteria by which individuals and societies gauge the conformity of science to their values and aspirations (p.124). This is nothing more than an 'invisible hand' argument, as many have noted; science purportedly serves

spell out a third way is (Fuller, 2002). He is discussed further in chapter 5 below. Another exceptional figure is Brad Wray (2000).

transcendent ends which are not and need not be the ends of the individuals engaged in their idiosyncratic pursuits. Furthermore, these ends are intrinsic goods in and of themselves, decoupled from the system of incentives and restrictions imposed by the paymasters of science. It is significant that Kitcher has never been able to definitively settle on the identity of these transcendent goods over the course of his career: they started out looking like formal notions of “truth”, and then were mutated to the identification of putative ‘natural kinds’, later that became transmogrified into the ‘unification of explanation’, and as of the present installment, he seems to have retreated back to the logical empiricist default position of providing accurate predictions. But no matter: philosophers will forever wrangle about the true nature of science as prelude to their dream of the final knockdown argument which will silence all doubt and opposition to their own favorite utopia. Kitcher’s very real conundrum is that almost nobody today believes any longer that these abstract philosophical criteria of the success of science are sufficient to underwrite a separate, self-governing and independent cloister of scientists supported by unlimited public funds: in other words, Golden Age philosophy of science is dead. Here is where our first thesis comes back into play. During the Cold War, when most science was primarily funded and organized by the military and the academic system was expanding, stories like those told by Polanyi, Popper, Kuhn (and maybe Lakatos) were eminently *useful*. They asserted that the public should simply acquiesce in the kinds of science it got in return for its investment, because scientists were the only legitimate judges of their own activity, and in any event, the public could never fully come to comprehend how science worked. Scientists should not set out to challenge research programs in their own areas of specialization, but rather forge their identities and chart their personal progress within their preset parameters. The myriad ways in which this doctrine dovetailed with such outside structural impositions as security classifications, open vs. closed publication outlets, activist science managers, handpicked peer review panels, and so forth should be obvious. Now, in the new regime of globalized privatization and flexible research specialization, all of that insistence upon the independent pristine constitution of the scientific sphere seems more than a little threadbare and outdated. Indeed, Kitcher’s pressing problem is to somehow argue that the modern research structures which are increasingly results-oriented in the shorter term, stress bureaucratic accountability to outsiders and are less infatuated

¹⁴ More recently, in (Kitcher, 2002), he denies this characterization; but just like his interlocutor in that

with the disciplinary power of professionalization are nonetheless insignificant in their impact upon the true transcendental goals of science.

That is how Kitcher and the social epistemologists arrive at the seemingly discordant positions that modern science as a generic entity automatically serves a set of transcendental values and objectives independent of those imposed by society or held by individuals, and yet, incongruously, also insist that something must be done to protect science from the inappropriate encroachments and corrupting demands of a societal nature. Kitcher phrases his conundrum as follows: “What exactly is the goal of scientific inquiry in a democratic society? (Or, in economic terms, what precisely are we hoping that science will maximize?)” (2001, p.145).¹⁵ The answer to both questions from our present perspective is that they are both so ill-posed as to be meaningless. The relationship of science to democracy is one of the most vexed and contested issues in the history of the 20th century (Hollinger, 1996; Purcell, 1973; Ezrahi, 1990; Wang, 1999); the lesson seems to be that science is neither inherently democratic, nor does it necessarily thrive only in a democratic society. Furthermore, there is nothing palpable which science as a social formation exists to ‘maximize’. However, these questions do reveal that Kitcher can only manage to conceptualize the interplay of science and society in quasi-economic, and moreover, strictly neoclassical terms. Hence, far from being an innovative expansion of philosophy of science into novel questions of social organization and away from context-independent epistemic propositions of Legendary philosophy of science (as Kitcher sometimes likes to paint it), what we have instead is an awkward throwback to the crypto-neoclassical political economy models of science which have pervaded post-WWII philosophy, as described above.

Kitcher’s riposte to the science studies community can be found summarized in his latest book. Mimicking their language, he also avows science is driven by ‘interests’ (and who would want to deny it in the present atmosphere of corporate science?), but these interests are merely individual biases which must be modeled as ‘preferences’.¹⁶ Preferences are not systematic or

instance, Helen Longino, I find his protestations of innocence unconvincing.

¹⁵ Dewey and the Pragmatists had a lot to say about this, before they were sidelined by Kitcher’s forebears, the analytical philosophers of science.

¹⁶ “We do better to deploy the notion of aims in its most natural home, referring to the aims of individual agents rather than to some abstraction” (2001, p.87). It seems astounding that a philosopher can so blithely dictate that the ‘individual’ is not an abstraction, or that homes are innately ‘natural’. Perhaps this philosopher forgot one of his own earlier incarnations (1996, p.282): “Grand rhetoric about human freedom seduces us into thinking that we

structural; and moreover, they merely serve to focus our attention. Hence interests just affect where we look for answers, not how we do research or what it is that we find there. Thus every question about possible pathologies of scientific research boils down in one way or another to questions about ‘preferences’ and what to do about them. Here Kitcher becomes all tangled up in the most awkward contradictions, exacerbated by the fact he believes he can incorporate political, moral and economic theory into philosophy of science without actually consulting their dauntingly voluminous literatures. (History as something you actually had to document was abandoned long ago.¹⁷) Initially, Kitcher tries to equate collective social welfare with the aggregation of individual preferences under a democratic regime: surely he is not completely ignorant of the infamous Arrow impossibility results (1951) which suggest that the two are incompatible? Then Kitcher likes to engage in talk about ‘freedom’ and ‘morality’, but deep down (like Polanyi, like Hayek, like Kuhn) he harbors a paralyzing fear of what he calls “vulgar democracy’, and perhaps religion as well.¹⁸ His solution to this unbearable tension is to change the ‘preferences’ of those individuals whom he deems untrustworthy, converting (brainwashing?) them into possession of what he calls ‘tutored preferences’. So much for the shibboleth of freedom of thought. In case the carefully tutored citizens still do not qualify as sufficiently pliable for science, Kitcher then proceeds to invent an elaborate Shangri-La, fortuitously fully equipped an agora of ideal forms of deliberation concerning prospective scientific research programs, voting on which to enjoy support (with what nature of franchise?) and a Supreme Court of experts to sort things out when they get a little testy. Curiously in one normally extolled as a clear writer, Kitcher then appears to think that piling on the neoclassical jargon will aid and assist rendering this flight of fancy more plausible:

[W]ith respect to each budgetary level, one identifies the set of possible distributions of resources among scientific projects compatible with the moral constraints on which the ideal deliberators agree, and picks from this set the option yielding maximum expected

must, quite literally, make ourselves... this conception was always incoherent. If the self that allegedly makes itself is already fully formed, then it does not, after all, *make* itself; if it is not fully formed, then *it* does not make itself. To find our freedom, we have to start acknowledging that we are the people we are because of events that are beyond our control, even beyond our understanding.” This doesn’t sound like utilitarianism, Toto.

¹⁷ One of Kitcher’s most irritating ploys is to present himself as mediator between historicists and analytical philosophers, all the while betraying vast ignorance about the history of arguments over the relationship of science to democracy (Hollinger, 1996; Purcell, 1973) or indeed the relationship between the history of utilitarian models of science and the philosophy of science.

¹⁸ “Behind the often evangelical rhetoric about the value of knowledge stands a serious theology, an unexamined faith that pursuit of inquiry will be good for us” (2001, p.166).

utility, where the utilities are generated from the collective wish list and the probabilities obtained from the experts. (p.121)

If this sounds akin to the kind of verbiage that was broadcast from RAND in the 1960s concerning the optimal choice of weapons systems by the Pentagon, then one would not be led too far astray concerning its intellectual lineage. Indeed, decision theoretic terms applied to technoscientific options was one of the prime specialties of Operations Research in that era (Hounshell in Hughes & Hughes, 2000) and designedly so: it draped a patina of political accountability over a process which was clearly spinning out of control and rife with conflicting interests, Machiavellian manipulation and backroom politics. As we have witnessed, philosophy of science itself has projected a pale reflection of the world according to the Operations Researcher since WWII; Kitcher's tragic flaw is that his ingrained disdain for history renders him incapable of realizing it is equally true of his own work. Kitcher, for all his vaunted ambition to be *au courant*, innovating new *raison d'être* and fresh topics for the philosophy of science, ends up being just another Kuhn, another Popper.¹⁹

Of course, if qualifying as being *a la mode* were the main virtue of science studies, then Kitcher's fall from grace would be but a minor transgression; and indeed, the papers in this volume would be equally culpable. The mortal sins of Kitcher's version of social epistemology are that it ends up being both irrelevant and tautologous. It is irrelevant in that his ideal world of "well-ordered science" has nothing whatsoever to do with the present predicaments of science – the runaway revisions of intellectual property, frequent resort to litigation to solve scientific problems, the crisis of scientific publication, the disenfranchisement of university faculty from academic governance, the bankruptcy of ritual mechanized induction, the over-emphasis on arcane mathematical expertise to validate relevance, the rationalization of teaching and lab work into temp McJobs, numerous popular crises of confidence in scientists as political actors – all this and more might as well be happening in another galaxy far, far away from Kitcher's ideal republic. Doubly vexed, Kitcher's social epistemology is also tautologous because of its circular notion of what it means to be a 'tutored' legitimate agent participating in deliberations about science. If 'having a legitimate voice' means getting a gold-plated Ph.D. in the relevant

¹⁹ "the fundamentals of human reasoning are pretty much everywhere the same... So the ways in which arguments are justified (and discovered) in the human sciences should be no different than those employed in the natural sciences" (2001, p.175).

discipline, then we are back to Polanyi's and Kuhn's original prescription that only holders of valid union cards (and then, only those who publicly submit to the authority of the master) get to cast their vote for whether we get the Human Genome Project or the Texas Supercollider or a fortified Social Security system. Of course, by then your 'tutored' vote is more or less a foregone conclusion. If, per contra, getting 'tutored' only means you need to know *something* about the sciences in question, then the prescription is unavailing, since there can be no settled prerequisite of everything one needs to know in order to participate.

If Kitcher could have been bothered to consult the philosophical literature on utilitarianism, he would have discovered that this idea of "cleansed" or suitably informed preferences is the Achilles Heel of left-liberal neoclassical economics (Cowen, 1993). The fundamental commitment of neoclassical models is to treat "preferences" as the independent unmoved movers of changes in market bundles. The mid-20th century revision of the economic agent towards greater resemblance to an information processor (Mirowski, 2002) raised the disturbing spectre that if agents took action to change their own preferences through experience, learning and interpersonal interaction, then the vaunted determinacy of economic equilibrium would evaporate. A whole sequence of attempts to stem the tide, from Nash equilibrium to schemes of mechanical induction to so-called "rational expectations", have sought to endow the economic agent with some semblance of invariance, but Kitcher does not seem aware that his ideal Republic of Tutees doesn't even begin to address the problem. Not only do we lack any guarantee that *ex ante* intentions need not map into *ex post* evaluations, but the very notion of 'democracy' as giving the people what they want loses all cogency. As Cowen writes, "Fully informed preferences do not offer an Archimedean point for value theory in a world of imperfect information" (1993, p.266).

This paradox, it seems to me, is one reason for opting for the tenuous existence between the disciplines that characterizes most of the papers contained within this volume. Science studies does not aim to preserve and protect Reason, Progress or all those other philosophical abstractions from the dark forces of subversion, safely storing them in some cool dark Platonic cave. Neither does it argue that "social science" can provide us with all the pre-assigned answers we need in order to participate in an engaged political economy of science. If anything, the science studies community has been noticeably reticent in endorsing any brand of social theory

as the philosopher's stone for decoding the behavior of scientists, their patrons and their clients. Instead, they follow the precepts that a fair degree of familiarity with the science in question, combined with an appreciation for its history, a keen eye for the context in which it operates, participant observation of its protagonists, tempered with a suspension of judgment over the final significance of any given theory or empirical finding, serve to adequately organize research into science in action and its repercussions in the larger society. Science studies is not out to found a City on a Hill; it would just like to find out if Celera really did give a boost to the public genome project, or merely was a cynical move to profit from it. Likewise, the papers within don't try to decide if neoclassical economics really is the application of "the scientific method" to the economy; but rather ask if the 'laws of supply and demand' are capable of adequately organizing the set of social influences that bequeathed to us the Particle Data Group, the linear regression package for your PC, or indeed —the 'laws' of supply and demand.

3. Negotiating the two-way crawl spaces

I would not be entirely candid in this introduction if I did not mention that the science studies community has been more than a little wary of welcoming an economist into their midst. Given their sensitivity to the diversity of the sciences and their imperialistic tendencies, they suspect that the only thing worse than economists pronouncing upon the folkways of science out of bone ignorance and boundless hubris is economists offering to 'help out' science studies with their usual aplomb. Although it cuts against my own self-interest, in my saner moments I applaud their caution. The proof of the contributions of a political economy of science will come in the specifics, and not in any grand programmatic statements. Thus it may pay some dividends to briefly indicate the ways in which the papers in the final three sections of this volume intersect with themes found prevalent in the science studies literature.

[1] Quantification

One of the most important conceptual issues in science studies is the fact that many sciences seek to set themselves apart from the general run of human knowledge by their dependence upon mathematical expression, and especially, in their reliance upon quantification in the process of empirical research. Justifications of the superior efficacy of quantification have been notoriously thin within the sciences, ranging from testimonials of faith in Platonism to

Eugene Wigner's mystical evocation of an 'unreasonable effectiveness of mathematics'. Within science studies there has been some research into quantification and its usefulness in fending off challengers to the legitimacy of certain research programs (Porter, 1995), and some very interesting work on the meanings of 'proof' as it is used by mathematicians (MacKenzie, 2002), but very little on the social structures responsible for validating and revising the bedrock numbers which are asserted to anchor the major empirical/mathematical theories of our time. The purposes of chapters 8 and 9 below were to provide a concerted social account of how formal mathematics and the attribution of error interact to stabilize the numerical values of the physical constants, with the model of the process being compared explicitly to the operation of arbitrage in the stabilization of prices. This is a version of the 'economics of science' pitched far beyond the purview of anyone previously cited in this introduction, and perhaps beyond the ken of any previous philosopher of science, with the possible exceptions of Emile Meyerson and Charles Saunders Peirce.

Starting with a modicum of familiarity with mathematics, one rapidly comes to realize that the actual values of many physical quantities would not exhibit fixed invariant numerical values: many are determinate only up to a monotonic (or other) transformation, are varying functions of an array of other values, and/or are artifacts of various scales. Chapter 8 discusses the history of the realization of this fact in empirical physics, and the attempt to save the Platonist thesis by recourse to a small set of key 'dimensionless constants': the only quantitative evidence one might cite of Nature speaking in a loud and clear voice. What is an unending source of fascination to me is how scientists in action (in this case, physicists) have behaved in a much more methodologically relativist fashion than conventionally portrayed by their supposed defenders in the Science Wars. That chapter reveals that the constancy and integrity of the rock-hard constants have continually been challenged and entertained by physicists in good standing; without deleterious consequences for quantification. One intellectual source of strength of physics, contrary to Kuhn et al., is that physics has somehow maintained the possibility of doubting some of the most fundamental tenets of its received theories *within a certain framework of speculation*. Some more instances of this openness to rethinking the invariance of the invariants have popped up since the paper was published.

One particularly fertile locus of sustained skepticism about the ultimate finality of quantification is astrophysics and cosmology. In an instance of recent vintage, a team of scientists led by John K. Webb was led to speculate that some of the most fundamental physical constants, such as the speed of light (whose numerical value is assigned by definition, as explained below in chapter 9) might be changing slowly in magnitude along with the evolution of the universe (Glanz & Overbye, 2001). Speculations like this are not as rare as one might think, as the chapters reveal. What is noteworthy is that the investigators were not immediately shouted down as heretics, even though the implications for the relationship between certain cherished subsets of physics might be dire, and further, were not explored by the original authors. Indeed, physicists have proven not only to have a capacity to entertain invariance with a light heart, but also to seriously propose inversions of the standard hierarchy of the various component physical theories (relativity, quantum mechanics, solid state physics) which underwrite the quantitative invariants. Perhaps the fervor which seems to be invested in quantitative invariance is merely an artifact of a misplaced drive to find the unified Theory of Everything which physicists have taken as their holy grail for so long. One recent incident was described as follows in the *New York Times*:

Last year Dr. [Robert] Laughlin and David Pines ... published a manifesto declaring the 'science of the past,' which seeks to distill the richness of reality into a few single equations governing subatomic particles, was coming to an impasse. Many complex systems...appear to be irreducible... Carrying the idea even further, some solid-state physicists are trying to show that the laws of relativity, long considered part of the very bedrock of the physical world, are not platonic truths since time began. They may have emerged from the roiling of the vacuum of space, much as supply-and-demand and other 'laws' of economics emerge from the bustle of the marketplace. (Johnson, 2001, D1, D5)

(Here we see journalists jumping the gun drawing morals for the laws of supply and demand from the latest hot topic in physics: more on this anon.) The article goes on to describe the work of Shoucheng Zhang, who developed a model of elementary particles bubbling up from a vacuum, suggesting the bedrock of reality is not invariance, but chaos. The implications of this line of thought have been broached in chapter 8 below: the quantitative invariants are not some solid evidence of our access to the world, but rather artifacts of the way we have modeled the phenomenon.

The implications of this worldview are then subsequently elaborated upon in chapter 9. There we follow the basic science studies precept to resist resting assured that the fundamental constants are indeed constant, but instead take an unprejudiced look, and then ask who and what are responsible for their presumed constancy. In that chapter we become acquainted with the severely underappreciated field of metrology, and discover that early 20th century worries over social lacuna in enforcing the constancy of the physical constants provoked the designation of special agents and institutions whose job it was to enforce the constancy of the constants through explicit acknowledgment of the mathematical nature of their interdependence, and to impose the consistency of that interdependence through reassignment of error estimates throughout the network structure of the constants. Furthermore, that chapter shows we can deploy some of the analytical techniques of the metrologists to develop a comparative study of the treatment of quantification across various sciences. There we learn, among other things, that economics fails at stabilizing its supposed quantitative invariants to a degree orders of magnitude worse than physics, or indeed, psychology. These papers give explicit examples of how one can move between the disciplines and undertake empirical research without pledging allegiance to any single one, as advocated in this introduction.

It seems to me that this social approach to the activity of quantification would appeal to the science studies community as broadly resonant with their goals and objectives. In support of that prognosis, it is beginning to be put to use by science studies scholars like Trevor Pinch to counter ungrounded assertions that sociological factors never influence "solid" scientific results (in Labinger & Collins, 2001, pp.223, 291). Yet it also has profound implications for economics. It raises the question: why does one science give markedly different results than another one? Below, I suggest that some of the main differences between physics and economics are not traceable (as the hoary chestnut has it) to ontological differences in their respective subject matters, but to quantifiable differences in their social structures. Physics (up till the very recent past) was an extremely self-assured science which left a fair amount of latitude for individuals to question what were taken as fundamental models and assumptions. If someone took the trouble to make a coherent argument, that was often taken as sufficient justification for publication (witness the acceptance rate for articles of *Physical Reviews* in the 80+% range, compared with

15-20% for the *American Economic Review*.)²⁰ Of course controversy would exist, but special social structures – in this instance, the Particle Data Group and its Handbook of Constants—were set up to adjudicate differences. Economics, on the other hand, was rife with Cold War distrust of the competence of its own membership, and yet preternaturally opposed (by its theory of markets) to the idea that explicit social structures should be set up to adjudicate differences (in chapter 9, differences in statistical estimates). So instead, discipline was *imposed* in supposedly impartial but functionally arbitrary fashion, through restrictions on publications in a few sanctioned ‘orthodox’ journal outlets. A few editors and their chosen referees arbitrarily decide what constitutes a good number, hiding behind the opaque operation of the editorial process. The result has been much less stability and integrity in the quality of the numbers produced by the economics discipline.

[2] *Statistics and econometrics*

The spread of statistics has been an especially active area of science studies over the last two decades (Daston, 1988; Porter, 1986; Hacking, 1990; Mirowski, 1997). Primarily, the interest has centered on the diverse understandings of probability and randomness in the 18th and 19th centuries, although some very perceptive work has also been done on the conflicting philosophical definitions of probability (Fine, 1973). The papers collected together in Part IV below tend to be more concerned with the ways in which probability and statistics were used in the 20th century to help define scientific discourse, with heightened concern with the phenomenon of econometrics in postwar economics.

Chapter 10 is an extended meditation upon the widespread conviction that historical research would be rendered “scientific” through the deployment of techniques like linear regression analysis and more direct recourse to hypothesis testing. When I first embarked upon my career as an economist, figures such as Robert Fogel, Douglas North and Jeffrey Williamson were notorious for making outlandish claims about how ‘Cliometrics’ (their neologism) would sweep old-fashioned historians into the dustbin of history. Some reflection on intervening events reveals that their predictions were not borne out (although it did garner the first two a Nobel

²⁰ The precept that there is no such thing as a generic scientific publication, but that one must combine economic, technological, disciplinary and social factors into account to understand how both print and electronic journals function in their specific disciplinary settings, is the subject of (Scheiding, forthcoming).

Prize); and indeed, the bloom is off the Cliometric rose in most modern neoclassical economics departments. It should have been apparent that history and neoclassical economics were like oil and water, or better yet, Courvoisier and Coca-Cola, but hindsight was always more self-assured than prediction. In any event, I became concerned to work out an historical exercise which would show precisely where Professor Fogel's magic elixir became little better than whitewash. Chapter 10 tells the story of an intriguing set of wagers by British brewers in the early 19th century twice: once in the standard Cliometric idiom, and then once more as old-fashioned historical narrative. The point of the exercise was to isolate how even something so seemingly straightforward as empirical predictive success (remember: Kitcher's transcendental desideratum of science!) can look wildly different depending upon the explanatory framework.

Chapter 11 bears an even more direct connection to science studies, although that community may be forgiven having overlooked it in its somewhat *outré* outlet. The paper started out as an attempt to draw out some of the strategic implications which lay obscured in Harry Collins classic, *Changing Order*. Collins had argued that pure replication was extremely rare in science as practiced, but that much of what passed for empirical support was in practice extension of some empirical protocol into different domains, materials, etc. My co-author Steve Sklivas and I decided (with tongues planted firmly in cheek) to play up the conflict inherent in the situation between an originator of an empirical claim and the researchers who sought to confirm or contest it. To that end, we portray the 'choice' whether to challenge an empirical finding as a game, with major policy variables the 'amount' of tacit information which the originator opts to share with the outside researcher, the rising costs of complete replication in all respects, the level of hostility perceived by the originator when approached by the supplicant, and the level of disinterest of scientific outlets in publishing successful replications. We showed that one of the most important regularities uncovered by science studies – the paucity of pure replication in science – could be regarded as having a social component, which would be very difficult to expunge from science in action. Furthermore, we suggest that science harbors an inbuilt bias towards fostering 'extensions' of empirical exercises, rather than replications, which contrasts with the conventional wisdom that replication is one of the foundation stones of the 'scientific method'. But far from constituting yet another instance of the 'economics of science' as a simple projection of the neoclassical orthodoxy, we then turn around and reveal that

attempts by the *Journal of Political Economy* to alter this state of affairs was itself unavailing; if anything, neoclassical economics suffers from a more debilitating case of replication aversion, at least when it comes to econometrics, than the other sciences.

Chapters 12 and 13 recount one significant challenge to the legitimacy of econometrics, as it were, from within the citadel of formal probability theory. In the 1960s, Benoit Mandelbrot set out to challenge the standard characterization of the stochastic character of time series of prices as consisting of linear combinations of Gaussian processes, a major presupposition underlying much of the justification of linear regression analysis in econometrics. Mandelbrot insisted that distributions of changes of prices had ‘fat tails’ inconsistent with Gaussian or ‘Normal’ (cute Orwellian name) distributions. The reactions to Mandelbrot by economists over the last three decades have been very illustrative of the ways in which certain sciences maintain their commitments to certain restricted sets of empirical practices, especially after Mandelbrot himself went on to become a celebrity for his discovery and popularization of ‘fractals’ (which chapter 13 argues owed something to this brush with economics). Here again physics and economics remain inextricably entwined. Contrary to popular impressions, probability theory has never totally succeeded in taming randomness. Randomness refuses to be corralled so tidily. However, as in so much of history, the story does not sport a linear plot line or a simple dénouement. In the interim, “fat tails” and chaotic models exist in a sort of demimonde of financial economics, especially popular with the exiled physicists who tend to reside in the high-tech research departments of banks, whereas Mandelbrot himself has backed away from his prior insistence that Levy stable distribution theory was both simpler and a more natural stochastic description of economic data. The challenge to quotidian econometric inference, however, seems to have gotten lost in the shuffle; nothing is harder to expose to doubt than something which has earned the sobriquet “Normal”.

Chapters 14-16 at first glance may strike the median denizen of science studies as old-fashioned history of economic thought, and therefore she may be inclined to give them a wide berth; I would hope that one consequence of this volume would be to induce her to suppress that inclination. Those papers belong in this volume because they synthesize many of the themes pioneered in the previously included papers having to do with science, quantification and econometric empiricism. For instance, one prerequisite of a belief in science as a ‘marketplace of

ideas' is the presumption that all markets operate essentially alike; by contrast, one objective of these chapters is to call that commonplace wisdom into question. Indeed, part of the deep structure of the neoclassical mindset is the presumption that the Walrasian and Marshallian models are in some unspecified sense consistent and hence mutually supportive of the conviction that there subsist generic laws of The Market. This creed represses the historical evidence that the theoretical tradition of "Supply and Demand" explanations and the parallel tradition of neoclassical utility maximization were for a very long time *rivals*. This fact is brought out in Chapter 14 by tracing the antecedents to the supply and demand tradition, and pointing out that natural scientists such as Antoine-Augustin Cournot and Fleeming Jenkin promoted it as superior to the utilitarian school. The controversy over the scientific pretensions of Supply and Demand came to a head with the insufficiently appreciated work of William Thomas Thornton, who first argued the case that there could be no such thing as generic laws of The Market, but only regularities of certain classes of market formats. Further, far from being a toothless observation that diversity mattered, Thornton also used it as a political defense of the legitimacy of trades unions, a move that made most of the political economy community sit up and take notice.²¹

Chapter 15 reveals that most of the major players in the establishment of British neoclassical economics developed their doctrines, at least in part, in direct response to Thornton. This had the further side-effect of fostering that curious precept that the reprocessed energy physics which was the vinculum of the neoclassical model (Mirowski, 1989) somehow underwrote the use of geometric supply and demand diagrams. So that which had previously been separate and opposed was forced to cohabit, especially in the textbook of Alfred Marshall. Since Marshall set much of the pattern for neoclassical introductory textbook pedagogy down to the present, there we find the origins of the widespread belief in "laws of supply and demand", even amongst those for whom economic textbooks are an anathema.

Just because a doctrine attains textbook status doesn't mean it is correct, however. For instance, it was the early 20th century American community which felt that Marshall had neglected fortifying supply and demand by forging the downstream links to quantitative empiricism, as well as adequate upstream links to the formal model of neoclassical utility optimization. This story continues with Henry Ludwell Moore in chapter 16, who was one of the

²¹ This paper has already provoked reactions from those who insist that Thornton could not have been such

first to apply the nascent Pearsonian theory of statistical inference to the fitting of demand curves to real data. Again defying the conventional wisdom, Moore was not trying to fortify the Marshallian program with his empirical forays, but rather regarded his work as critique. Far from being an unalloyed success, his faltering project was bequeathed to two of the major players in the stabilization of the American neoclassical orthodoxy (see Mirowski, 2002, chap. 5), Henry Schultz and Harold Hotelling.

The point of these chapters is not to ridicule the protagonists, but rather to demonstrate how very hard it was to carry out the project of rendering the ‘laws of supply and demand’ as something a bit more solid than mere *facons de parler*, and fortifying them with all the trappings of scientific legitimacy which the protagonists felt were the *sine qua non* of their efforts. It wasn’t enough that one could inscribe a geometric shape on a blackboard, or write down a few bits of mathematics which supposedly legitimated them. That may have been good enough to browbeat students, but not sufficient to win the allegiance of the scientifically-trained. In the early 20th century, the imperative to quantify was taken as the hallmark of true science; but eventually that didn’t prove to be sufficient either. Much effort was then poured into the construction of specialized statistical techniques which were purpose-built to capture the answers to questions about the relationship of supply and demand to the neoclassical organon; but they didn’t produce the sought-after parameter estimates, either. The reason was that the Walrasian model and the tradition of supply and demand were analytically uncoupled – or to put this slightly more formally, the Sonnenschein/ Mantel/Debreu theorems developed in the 1970s revealed that Walrasian systems placed almost no restrictions on excess demand functions – so that the presumed dependence of the ‘laws of supply and demand’ on the underlying ‘deep parameters’ of individual utility maximization was a forlorn ambition. What this implies for the cogency of the supposed ‘laws’ is an exercise left for the reader.

We end the volume with a paper which may seem to rest uneasily under the rubric of the history of supply and demand of the last section, or worse, seem incongruous in both style and substance in this volume as a whole. Again pleading with you, dear reader, to temporarily withhold judgment so that its place in the larger *oeuvre* might become clear. Like so many other papers in this volume, its original motivation is to understand the strengths and weaknesses of

a pivotal figure because he was essentially ‘atheoretical’. See, for instance, (Ekelund & Thornton, 2001).

the neoclassical school through an exploration of the social processes which buttress its use and abuse. In this particular instance, I wanted to address the commonplace (but I believe doomed) predisposition to dispute the neoclassical model as too ‘selfish’ a portrayal of human nature. In that paper I make use of some themes found in Jacques Derrida to argue that the attempt to counterpoise debased market motives with the “gift” and selflessness is a one-way ticket to intellectual dissolution. Those skeptical of the neoclassical program are misled in thinking the major point at issue is the cognitive or moral philosophy of the agent—neoclassical arguments are too slippery to be pinned down there, and anyway, I would insist that its fundamental strength derives rather from its attempts to embody popular images of scientific inquiry and content into what purports to be a description of social interaction. Although it is not explicitly spelled out in that paper, this thesis has direct relevance to science studies as well. In the 1960s, the sociologist of science Warren Hagstrom (1965) attempted to counter marketplace metaphors for science by positing instead that science constituted a “gift economy”, not so very different from the groups cited within chapter 18. For precisely the same reasons enumerated therein, the attempt to render science ‘special’ by supposing that it constitutes a gift economy and therefore is the polar opposite of market exchange only serves to open the door to the unconstrained application of rational choice models; once again we discover that the undecidability of selfishness works in favor of the neoclassical school, only this time in attempts to describe science as a social process. This goes some distance in explaining the weakness of the older Mertonian sociology of science in providing an alternative social account of science in action, and leaves us with the suggestion that this can only be provided by an economics of science which does not keep other disciplines (and their theoretical contents) at arm’s length.

The papers in this volume seek to create a space where we can get past the grand abstractions about Science, Truth and Democracy and begin to talk about the ways that scientists live their lives on the ground today. If we are successful, we may just measure up to the standard already set by authors of fiction like Carter Scholz, who in his novel *Radiance* has found a language to evoke what it feels like to work in a weapons lab run by the government which is being exhorted to convert to “dual use”. Here he imagines a conversation between two weapons scientists:

- Think your man Leonardo didn't have to hold his gorge every day?
- These are our allies boy.
- They're thugs Dan. They're enemies of reason.
- Common cause, Leo. You don't have to share a pew with them.
- Common cause? What cause?
- Power, money, influence. Commonest causes there are. Who gives a shit what they believe?
- You remember when Schott won his Nobel? A year later he was pushing master race eugenics.
- What are you getting at, Leo?
- Just because you're smart don't think that you can't be stupid.

References

- Agassi, Joseph. 1995. "Contemporary Philosophy of Science as Thinly Masked Antidemocratic Apologetics," in K. Gavroglu et al, eds., **Physics, Philosophy and the Scientific Community**. Dordrecht: Kluwer.
- Biagioli, Mario, ed. 2000. **The Science Studies Reader**. London: Routledge.
- Blaug, Mark. 2001. "No History of Ideas, Please, We're Economists," *Journal of Economic Perspectives*, (13):145-164.
- Cowen, Tyler. 1993. "The Scope and Limits of Preference Sovereignty," *Economics and Philosophy*, (9): 253-69.
- Daston, Lorraine. 1988. **Classical Probability in the age of the enlightenment**. Princeton: Princeton University Press.
- David, Paul. 1998. "Common Agency Contracting and the Rise of Open Science Institutions," *American Economic Review Papers and Proceedings*, (88):15-21.
- David, Paul & Dasgupta, Partha. 1994. "Towards a New Economics of Science," *Research Policy*, (23):487-521.
- Ekelund, Robert & Thornton, Mark. 2001. "William Thornton and Nineteenth Century Economic Policy," *Journal of the History of Economic Thought*, (23):513-531.

- Elliott, Carl. 2002. "Diary," *London Review of Books*, 28 November: 36-7.
- Ezrahi, Y. 1990. **The Descent of Icarus**. Cambridge: Harvard University Press.
- Feyerabend, Paul. 1978. **Science in a Free Society**. London: New Left Books.
- Feyerabend, Paul. 1987. **Farewell to Reason**. London: Verso.
- Fine, Terrence. 1973. **Theories of Probability**. New York: Academic Press.
- Fuller, Steve. 2000. **Thomas Kuhn: A philosophical history for our time**. Chicago: University of Chicago Press.
- Fuller, Steve. 2002a. **Knowledge Management Foundations**. Oxford: Butterworth-Heinemann.
- Fuller, Steve. 2002b. "Prolegomena to a Sociology of Philosophy in the 20th Century English-Speaking World," *Philosophy of the Social Sciences*, (32):151-177.
- Galison, Peter. 1997. *Image and Logic*. Chicago: University of Chicago Press.
- Galison, Peter & Hevly, Bruce. eds. 1992. *Big Science*. Stanford: Stanford University Press.
- Gibbons, Michael; Nowotny, Camille; Schwartzman, Simon; Scott, Peter; and Martin Trow. 1994. **The New Production of Knowledge**. London: Sage.
- Glanz, James & Overbye, Dennis. 2001. "Cosmic Laws Like the Speed of Light Might be Changing," *New York Times*, 14 August.
- Goldman, Alvin. 1999. **Knowledge in a Social World**. New York: Oxford University Press.
- Gross, Paul & Leavitt, Norman. 1994. **Higher Superstition?** Baltimore: Johns Hopkins University Press.
- Hacking, Ian. 1990. **The Taming of Chance**. New York: Cambridge University Press.
- Hacohen, Malachi. 2000. **Karl Popper: the formative years 1902-45**. Cambridge: Cambridge University Press.
- Hagstrom, W.O. 1965. **The Scientific Community**. New York: Basic Books.
- Hands, Wade. 2001. **Reflection without Rules**. New York: Cambridge University Press.
- Hollinger, David. 1996. **Science, Jews and Culture**. Princeton: Princeton University Press.

Howard, Don. 2003. "Two Left Turns Make a Right: On the Curious Political Career of North American Philosophy of Science at Mid-Century," in Gary Hardcastle & Alan Richardson, eds., **Logical Empiricism in North America**, Minneapolis: University of Minnesota Press.

Hughes, Thomas & Hughes, Agatha, eds. 2000. **Systems, Experts and Computers**. Cambridge: MIT Press.

Jarvie, I.C. 2001. "Science in a Democratic Republic," *Philosophy of Science*, (68):545-564.

Jensen, Richard & Thursby, Marie. 2001. "Proofs and Prototypes for Sale: The Licensing of University Inventions," *American Economic Review*, (91): 240-59.

Johnson, George. 2001. "New Contenders for a Theory of Everything," *New York Times*, December 4, D1, D5.

Kadvany, John. 2001. **Imre Lakatos and the Guises of Reason**. Durham: Duke University Press.

Kay, Lily. 1993. **The Molecular vision of Life**. Oxford: Oxford University Press.

Kay, Lily. 1997. "Rethinking Institutions: Philanthropy as a Problem of Knowledge and Power," *Minerva*, (35):283-293.

Kirby, Maurice. 2000. "Operations Research Trajectories: Anglo-American Experience, 1940-1990," *Operations Research*, (48):661-70.

Kitcher, Philip. 1993. **The Advancement of Science**. New York: Oxford University Press.

Kitcher, Philip. 1996. **The Lives to come**. New York: Simon & Schuster.

Kitcher, Philip. 2000. "Reviving the Sociology of Science," *PSA Proceedings, Philosophy of Science*, (67): S33-S44.

Kitcher, Philip. 2001. **Science, Truth and Democracy**. New York: Oxford University Press.

Kitcher, Philip. 2002. "The Third Way: Reflections on Helen Longino," *PSA Proceedings, Philosophy of Science*, (69):549-59;569-72.

Koertge, Noretta, ed.. 1998. **House built on Sand**. New York: Oxford University Press.

Kohler, Robert. 1991. **Partners in Science**. Chicago: University of Chicago Press.

Kuhn, Thomas. 2000. **The Road since Structure**. Chicago: University of Chicago Press.

- Labinger, Jay & Collins, Harry, eds. 2001. **The One Culture?** Chicago: University of Chicago Press.
- Latour, Bruno. 1999. "On Recalling ANT," in John Law, ed.,
- Leonard, Thomas. 2002. "Reflection on Rules in Science: an invisible hand perspective," *Journal of Economic Methodology*, (9):141-168.
- Lessig, Lawrence. 2001. **The Future of Ideas**. New York: Random House.
- Longino, Helen. 2002a. **The Fate of Knowledge**. Princeton: Princeton University Press.
- Longino, Helen. 2002b. "Science and the Common Good: Thoughts on Kitcher's *Science, Truth and Democracy*", *PSA Proceedings, Philosophy of Science*. (69): 560-68.
- Lyons, Gene. 1969. **The Uneasy Partnership**. New York: Russell Sage.
- MacKenzie, Donald. 1990. **Inventing Accuracy**. Cambridge: MIT Press.
- MacKenzie, Donald. 1996. **Knowing Machines**. Cambridge: MIT Press.
- MacKenzie, Donald. 2001. "Physics and Finance", *Science, Technology and Human Values*, (26):115-144.
- MacKenzie, Donald. 2002. **Mechanizing Proof**. Cambridge: MIT Press.
- Maki, Uskali. 1999. "Science as a Free Market: A reflexivity Test in an economics of economics," *Perspectives on Science*, (7):486-509.
- McCumber, John. 2001. **Time in the Ditch: American philosophy in the McCarthy era**. Evanston: Northwestern University Press.
- McGrath, Peter. 2002. **Scientists, Business and the State, 1890-1960**. Chapel Hill: University of North Carolina Press.
- McSherry, Corynne. 2001. **Who Owns Academic Work?** Cambridge: Harvard University Press.
- Mirowski, Philip. 1989. **More Heat than Light**. New York: Cambridge University Press.
- Mirowski, Philip. 1997. "The History of Classical and Frequentist Theories of Probability," in Jim Henderson, ed. **The State of the History of Economics**. London: Routledge.
- Mirowski, Philip. 2002. **Machine Dreams: Economics becomes a cyborg science**. New York: Cambridge University Press.

- Mirowski, Philip & Sent, Esther-Mirjam, eds. 2002. **Science Bought and Sold**. Chicago: University of Chicago Press.
- Motterlini, Matteo, ed. 1999. **For and Against Method**. Chicago: University of Chicago Press.
- Porter, Theodore. 1986. **The Rise of Statistical Thinking**. Princeton: Princeton University Press.
- Porter, Theodore. 1995. **Trust in Numbers**. Princeton: Princeton University Press.
- Preston, John; Munevar, Gonzalo; Lamb, David, eds. 2000. **The Worst Enemy of Science?** New York: Oxford University Press.
- Purcell, Edward. 1973. **The Crisis of Democratic Theory: Scientific Naturalism and the Problem of Value**. Lexington: University of Kentucky Press.
- Proctor, Robert. 1991. **Value Free Science?** Cambridge: Harvard University Press.
- Reichenbach, Hans. 1938. **Experience and Prediction**. Chicago: University of Chicago Press.
- Richardson, Alan. 2002. "Engineering Philosophy of Science," *PSA Proceedings, Philosophy of Science*, (69):S36-S47.
- Scheiding, Tom. forthcoming. "Publish *and* Perish: Electronic Publication and the Serials Crisis," Ph.D. thesis, University of Notre Dame.
- Schmidt, Frederick, ed. 1994. **Socializing Epistemology**. Lanham: Rowman & Littlefield.
- Scholtz, Carter. 2002. **Radiance**. New York: Picador.
- Scott, Pam; Richards, Eveleen & Martin, Brian. 1990. "Captives of Controversy: the myth of the neutral social researcher in contemporary scientific controversies," *Science, Technology & Human Values*. (26):229-44.
- Shi, Yanfei. 2001. **The Economics of Scientific Knowledge**. Cheltenham: Edward Elgar.
- Smith, Helen Lawton, ed. 2002. **The Regulation of Science and Technology**. Basingstoke: Palgrave.
- Stewart, Thomas. 1997. **Intellectual Capital: the new world of organizations**. X: Nicholas Brealey.

Stephan, Paula. 1996. "The Economics of Science," *Journal of Economic Literature*, (34): 1199-1235.

Turner, Stephen. 1999. "Does Funding Produce its Effects?" in T. Richardson & D. Fisher, eds., **The Development of the Social Sciences in the US and Canada: the role of philanthropy**. Stamford: Ablex.

Turner, Stephen. 2001. "What is the Problem with Experts?" *Social Studies of Science*, (31): 123-150.

Uebel, Thomas. 2000. "Some Scientism, Some Historicism, Some Critics: Hayek's and Popper's critiques revisited," pp.151-173 in M. Stone & J. Wolff, eds., **The Proper Ambition of Science**. London: Routledge.

Waldrop, Mitchell. 1992. **Complexity**. New York: Simon & Schuster.

Wang, Jessica. 1999. "Merton's Shadow: perspectives on science and democracy since 1940," *Historical Studies in the Physical Sciences*, (30:1):279-306.

Weintraub, E. Roy. ed. 2002. **The Future of the History of Economics**. Durham: Duke University Press.

Wray, K. Brad. 2000. "Invisible Hands and the Success of Science," *Philosophy of Science*, (67): 163-175.

Zamora Bonilla, J.P. 2002. "Scientific inference and the pursuit of fame," *Philosophy of Science*, (69):300-323.

Ziman, John. 1994. **Prometheus Bound**. Cambridge: Cambridge University Press.