SCIENCE BOUGHT AND SOLD: ESSAYS IN THE ECONOMICS OF SCIENCE. EDITED BY PHILIP MIROWSKI AND ESTHER-MIRJAM SENT. CHICAGO: UNIVERSITY OF CHICAGO PRESS, 2002.

There exists a modest but steadily growing literature on the economics of science. Much of it concerns the funding of research and the reaping of societal benefits therefrom, but one also sees increasing interest in applying economic concepts to the conduct of research itself, extending even to matters traditionally falling within philosophy of science. The publication of *Science Bought and Sold: Essays in the Economics of Science* provides an opportunity to take stock of these efforts. The editors, Philip Mirowski and Esther-Mirjam Sent, have assembled 19 essays from a diverse array of authors with intellectual roots in economics, in philosophy and sociology of science, and in the sciences themselves. Some of these entries were written especially for this volume, others reprinted or excerpted from elsewhere. The book is seriously flawed, as important voices have been shut out and the editors' 60-page introduction itself is virtually unusable. Nevertheless, the result is a telling portrait of inquiry into this field.

Mirowski and Sent identify "three very distinct regimes of science funding and organization in the United States in the twentieth century:" the "protoindustrial regime" up to 1940, followed by the "Cold War regime" and, since about 1980, the "globalized privatization regime." They argue that "each regime comprised a distinct set of structures that have in practice summoned quite differing versions of an 'economics of science' to justify and account for their regularities" (p. 12). They "regard funding structures and theoretical accounts of their efficacy as inextricably interlinked" (p. 12), and they aim to "situate some of what are generally conceded to be the classic texts representing these alternative approaches within the [regimes they have identified]" (pp. 12–13). With the caveat that situating a work historically is no substitute for evaluating its merits, the editors would appear to have a plausible thesis and an interesting theme around which to discuss their selections.

Regrettably, their presentation is so replete with innuendo, sweeping generalizations, rhetorical affectation, and unsubstantiated assertions that it is difficult to extract anything of value. The following illustrative passage is quoted at length:

> [Economists'] initial temptation has been to treat science as just another commodity, on a social and epistemic par with poetry and pushpin. This is the first reaction of anyone who asserts that science is just a special case of the greater "marketplace of ideas;" since the market is thought to allocate resources in an

> optimal manner, there is no need for anything as pretentious as an "economics of science." The free operation of "Open Science" and individual competition for the applause of peers is all that is required, and it follows that the idea of science policy is utterly otiose. This conviction, practically second nature for a neoclassical economist, has been a snare and a pitfall for those who have turned their attentions to science. Quite baldly, economists are not free to treat science like putty clay or pancakes. (pp. 32–33)

Let us dissect this straw man from the inside out. In mainstream neoclassical economics, the fundamental assumption is that each individual seeks to maximize his or her utility. It is a matter for further investigation to assess whether the interaction of such individuals in a free market leads to an optimal (in some sense) allocation of resources. Far from assuming that markets are optimal, most purveyors of the neoclassical paradigm (including contributors to this volume) conclude that they fail, at least to some degree and in some circumstances. Far from dismissing the need for an economics of science, a number of economists (both professional and avocational) have been working to develop one. Far from treating science as "just another commodity," they appear quite interested in the consequences that flow from its distinct features.

And surely, a market-oriented economist who regarded science policy as otiose would *not* be among those presuming to mold science "like putty clay." It is rather the aspiring policy wonks themselves who can be expected to harbor such ambitions.

Nevertheless, Mirowski and Sent are quite correct that mainstream economics is inadequate as an analytical perspective on science. To see why, we shall have to proceed on our own to examine some of the essays in this volume.

First, however, let us take note of a viewpoint that has been excluded. On the first page of their introduction, the editors say that "nothing would please us more than to have natural scientists venture beyond stereotypic philippics about the utter folly of not funding their own favorite research agenda to the hilt" (p. 1). Well, biochemist Terence Kealey filled this bill and more, questioning the entire rationale for government funding in his (1996) book, *The Economic Laws of Scientific Research*. You will not find an excerpt of Kealey's writings in this volume, only a gratuitous dismissal on page 33 (and on page 320 in the essay by John Ziman). Joseph P. Martino (1992) applied public choice theory to demonstrate that various pathologies which have emerged in the research funding system are inevitable consequences of government largesse. His work, too, is absent. The editors have reprinted Arrow's and Nelson's vintage (c. 1960) "market failure" arguments for government funding, but not Demsetz's (1969) devastating critique. In the entire 550-page volume, not a single voice challenges the presumed necessity of massive government support for scientific research.

Let us turn, then, to "The Simple Economics of Scientific Research," by Richard R. Nelson and "Economic Welfare and the Allocation of Resources for Invention," by Kenneth J. Arrow. Arrow argues that "we expect a free enterprise economy to underinvest in invention and research (as compared with an ideal) because it is risky, because the product can be appropriated only to a limited extent, and because of increasing returns in use" (p. 175). Nelson reasons that

> when the marginal value of a "good" to society exceeds the marginal value of the good to the individual who pays for it, the allocation of resources that maximizes private profits will not be optimal. . . . Therefore, it is in the interests of society collectively to support production of that good. (pp. 152–53)

Something is missing here, for otherwise we have perfectly good arguments for jumping from the frying pan to the fire. What is missing is a demonstration that "collective support"—presumably involving the coercive transfer of resources from non-scientists to scientists—*will* result in an optimal allocation. Public choice theory suggests strongly that it will not, because political and bureaucratic incentives favor the general welfare no better than those of the marketplace, and perhaps worse. Arrow does allow that "problems arise whenever the government finds it necessary to engage in economic activities," (p. 179) but the issue of whether government can improve on the market is not substantively addressed by presumptuous rhetoric (e.g., "finds it necessary").

The main case against leaving science to the free market appears to be that firms have insufficient incentive to engage in basic research unless they can establish property rights in the results or keep them secret, which would lead either to underutilization of the information or to massive inefficiencies of duplicated research. As Nelson puts it, "if the results of research cannot be quickly patented and are not kept secret, other firms . . . will be free to use the results" (pp. 159–60). But, as Kealey (1996) pointed out, in order to possess the expertise to benefit from others' front-line research, a firm needs to employ its own cadre of front-line scientists. How does it attract them into its employment and judge their continued currency in the field? Perhaps by supporting their research in an open, academic-style environment?

Nelson worries that "the long time lag that very often occurs between the initiation of a basic research project and the creation of something of marketable value may cause firms . . . to place less value on basic-research projects than does society" (p. 160). But a long time lag cuts two ways. Perhaps the resources (particularly intellectual talent) that have been lavished on scientific exotica, from quasars to quarks, might have been better utilized in building a wealthier, technologically more capable society in which industrial and philanthropically sponsored research achieve the same advances somewhat later, but still in time to apply them (if indeed they will find application). Kealey (1996) pointed out that wealthy countries became wealthy before their governments began to support research on a large scale, whereas massive government science funding did not lead to prosperity in the Soviet Union. The common thread among progressive economies is capitalism, not state sponsorship of research.

Partha Dasgupta and Paul A. David seek to improve on Arrow and Nelson via their efforts "Toward a New Economics of Science" by bringing in game theory and Mertonian sociology of science. They have made little progress toward supplying the comparative institutional analysis of tax-based versus private funding that was missing from those earlier treatments, however, and they have muddied the waters in at least one important respect. Traditionally, science is understood as pursuing knowledge of nature, while technology has to do with solving practical problems—a distinction that would be as appropriate on Crusoe's island as in a populous society. Dasgupta and David propose rather to distinguish "Technology" from the "Republic of Science" (their capitalization) by their "socio-political arrangements and . . . reward mechanisms" (p. 228). "Loosely speaking," they say, "we associate the latter with the world of academic science, whereas Technology refers to the world of industrial and military research and development activities." In particular, Technology is characterized by secrecy, while science is defined by its "openness," by the "public" character of its results.

The problem with defining things this way is that the authors' endorsement of "adequate public patronage" for science then reduces to an appeal for subsidy and preservation of a particular set of institutional arrangements. Surely, the real issue has to do more directly with the effective acquisition and application of scientific knowledge. While arguing with some effect that the institutions of academic science conduce to progress in research, the authors have demonstrated neither that those institutions are necessary nor that similar institutions would be insufficiently funded by a combination of private sources in the absence of government largesse. Given the historical record, as indicated above, the burden of demonstration is theirs.

Ironically, Dasgupta and David give the academic reward system perhaps less credit than it deserves, citing concerns such as the following: (1) the importance of priority in discovery sets up a race in which the losers may be insufficiently rewarded for their efforts, making research too risky an endeavor and discouraging entry into the field; (2) the existence of competing research programs may waste resources; (3) there is an incentive to keep useful information secret from competing researchers. Notice that the first two concerns are contradictory. The rule of priority may in fact serve well to limit the number of parallel assaults on the most "glamorous" problems by discouraging all entrants save those with a significant chance of success. Yet, research does not typically have the character of a winner-take-all sweepstakes. Where one person or team achieves a fundamental advance, there is room for others to make solid contributions: replicating, refining, and extending the result and applying it in other projects. The reality of most successful academic research careers is the building up of one's professional reputation through a steady stream of published contributions in one or a few specialized areas. This reality, too, is far more conducive of rapid disclosure than of secrecy (the authors' other concern). Nevertheless, secrecy itself is not an unvarnished evil. The possibility of holding off one's competitors long enough to confirm and "milk" one's results may be part of the incentive to embark on a bold project in the first place, and it may help stave off premature release of faulty data and mistaken conclusions that mislead other researchers before being proved incorrect.

The editors have included Charles Sanders Peirce's 1879 "Note on the Theory of the Economy of Research," along with a glowing hermeneutic essay by James R. Wible. Peirce sums up his own approach as follows:

> The doctrine of Economy in general treats of the relations between utility and cost. That branch of it which relates to research considers the relations between the utility and the cost of diminishing the probable error of our knowledge. Its main problem is how with a given expenditure of money, time, and energy, to obtain the most valuable addition to our knowledge. (p. 183)

Peirce proceeds to develop a mathematical model representing choice in the allocation of resources to various research projects. For each project, utility and cost are functions of the probable error to be achieved. The object is to maximize the total utility for a given total cost. The optimal distribution of resources occurs where marginal utility per marginal cost is equal for all the projects. "When new and promising problems arise," Peirce concludes "they should receive our attention to the exclusion of the old ones, until their urgency [i.e., ratio of marginal utility to marginal cost] becomes no greater than that of the others" (p. 185).

Wible calls Peirce's essay "truly extraordinary," (p. 215) its argumentation "so rigorous that it could be the first truly modern scientific piece in all of economics," (p. 197) "a method of argument which any contemporary economist would recognize" (p. 198). He finds it "extremely unfortunate that [Peirce's essay] has been thoroughly neglected by the economics profession for over a century" (p. 215). I would suggest to the contrary that Peirce's "Note" adumbrates much of what is wrong with the neoclassical mainstream approach, particularly when the attempt is made to apply it to science. Peirce's modeling is rigorous, indeed, in the sense that numerical outputs would follow rigorously from numerical inputs, but the inputs required are simply unavailable. There is no common measure by which to compare "the error of our knowledge" in diverse lines of scientific research. Nor can we ascertain the error itself without knowing the very thing that we will not have learned prior to having completed our researches—namely, the truth toward which those researches are presumably converging. Even if we can estimate the cost and utility of making certain well-defined measurements and carrying out certain well-defined calculations, for the most part we cannot foresee what advances science might make and thus we cannot know their costs or benefits.

Peirce's advice amounts to no better than this: given an array of existing and proposed research programs, allocate each additional dollar to where it will advance our knowledge the most. Without mathematical modeling, we could have thought of that. Yet, we might also have given more consideration to just how "new and promising problems" do arise and how their discovery might best be encouraged. And we might have resisted the preposterous suggestion that scientists drop everything they are doing whenever such a new problem arises, abandoning the techniques and instruments that have been built up to deal with existing problems, returning to those problems only when the marginal utility of the new one has been sufficiently beat down. (In other words, we might have recognized that intellectual capital is neither homogeneous nor infinitely malleable.)

In short, Peirce offers a computational Tinkertoy, rooted in an anemic vision of scientific progress, requiring inputs that are unavailable and unquantifiable, yielding results which, to the extent that they are valid or meaningful, are in any event available from ordinary verbal reasoning. Similar examples are to be found in several essays presented here, and the foibles inherent in such thinking infect much of the volume. Mathematics too easily displaces critical reasoning. Displays of technical virtuosity lend credence to superficial or misguided claims and policy recommendations. Focusing on optimal allocation among existing alternatives distracts attention from the generation of new alternatives (reflecting the neoclassical mainstream's failure to come to terms with entrepreneurship) and encourages interventionism—for, if the market strays from that utility-maximizing path dictated so precisely by the equations, cannot government apply the needed corrections?

In "The Organization of Cognitive Labor," Philip Kitcher addresses "the impacts that various systems of social and individual decisions have on the growth of science" (p. 251). What are the implications of different assumptions about the goals and choice strategies of individual scientists and the social conditions in which they operate? "Instead of thinking about how best to achieve a cognitive goal," (p. 251) Kitcher suggests, "we can consider whether one type of social arrangement avoids a pit into which another falls" (p. 251). Toward this end, he deploys a series of mathematical models based on utility functions and probabilities of success that are unknown and largely unknowable, all the while repeatedly acknowledging the inadequacies of his approach ("My toy scientists do not behave like real scientists"). What is to be gained from such models, he believes, is "precision," and even though "precision is bought at the cost of realism," it "is important for both identifying consequences and disclosing previously hidden assumptions" (p. 251).

But surely, one cannot expect the output of mathematical calculations to exceed the input in precision. And if an implausible result sends us back to revising our assumptions, is that truly a *benefit* of the formal approach? Perhaps the time spent generating mathematical constructs might better have been devoted to reasoning more directly about the assumptions and their implications.

In one scenario, Kitcher assumes that a researcher is dedicated to solving a particular problem within a certain limited quantity of resources. The problem has two parts: obtaining a piece of information, then using it to complete the research. The piece of information can be acquired by the researcher directly, expending some of the available resources, or it can be borrowed from another researcher, which may save resources but may also involve greater uncertainty as to whether the information is correct. Kitcher formulates the scenario as a mathematical expression for the overall probability of success, in terms of costs and conditional probabilities, and deduces the following implication: if completing the research eats up enough resources, it will pay to borrow the input information from even extremely unreliable sources. This conclusion is obvious, for if using the information to complete the research will entirely deplete one's resources, then one has no choice but to borrow the information itself. But it is also preposterous, for no one would expend substantial resources carrying out research based on unreliable inputs. The author undoubtedly realizes this, but the point is that nothing has been contributed to our understanding by translating our assumptions into mathematics and translating the mathematical output into conclusions, if those conclusions rise or fall by comparison with the results of ordinary verbal reasoning which we might have employed in the first place.

Kitcher aims to demonstrate that scientists need not have "epistemically pure" motives. Science may be more effectively advanced by "epistemically sullied" researchers who seek not only to solve a problem but to achieve recognition as the first to solve it. Their "entrepreneurial" scheming for advantage leads them to explore diverse approaches, while the epistemically pure community tends toward cognitive uniformity. There is an appealing *invisible hand* flavor to this harnessing of self-interest to the general good, but little relevance can be attached to the contrast between pure and sullied motives. Kitcher's epistemically pure researchers are in fact strict followers of methodology, oblivious to the redundancy of their own and their colleagues' efforts. Surely, even researchers whose only intent was that problems be solved quickly, regardless of who solves them, could be expected to adjust their approaches to avoid excessive redundancy. And if an alternative approach should result in early solution of a problem, would it not be just as though the successful researcher had won a race for priority? What is essential is that individuals are free to choose, not that their individual motivations conform to some ideal.

I found it difficult to take seriously John Ziman's "The Microeconomics of Academic Science" after I came across the passage in which he identifies, as "an essential characteristic of a 'market' transaction," that the parties "have both come to put the same valuation on what is exchanged" (p. 324). It seems the author has failed to absorb one of the most basic observations of economics, namely that free trade takes place only because the parties to a transaction place *different values* on the items traded. If each party values what is given as highly as what is received, then why bother to trade at all? And, having traded, why not trade back again?

For Ziman, "to represent scientific activity in quasi-economic terms" (p. 319) is largely "no more than an elaborate metaphor." Nevertheless, he is among those authors who entertain the idea that peer recognition for research accomplishments serves as a kind of currency in academic science. The resulting reputational quasi-market, he thinks, coexists uncomfortably with the commercial market, in which the results of research may be patented or kept confidential. Indeed, he finds a troubling "innate contradiction" between a traditional academic career and a research career in industry. But just what is uncomfortable or contradictory about the existence of diverse career options for scientists?

With regard to "making the reputational status of scientists and scientific institutions more transparent," Ziman cautions that "[t]he expert judgment of peers is not necessarily unreliable, but it needs to be backed up with information on the actual contributions of researchers to the literature of their specialties" (pp. 322–23). Unfortunately, the only way to obtain such information contemporaneously is through the judgment of peers, either directly or perhaps via a citation index.

Paula E. Stephan and Sharon G. Levin are economists who have elsewhere written perceptively in favor of a comprehensive economics of scientific research (see, for example, Stephan 1996). Nevertheless, it seems to me that their contribution to this volume, "The Importance of Implicit Contracts in Collaborative Research," is misguided in key respects. In an academic research group where a professor directs several graduate students, it is understood that the students have an opportunity to build their own professional reputations, both through coauthorship of papers when their contributions suffice, and through the professor's recommendation for future positions, commensurate with their performance. Stephan and Levin make an unwarranted leap to the assertion that there is some sort of implicit contract between the graduate students and a broader academic research "system," to the effect that there *will* be a place in academic research as a principal investigator for each of them who performs well. They see this contract breaking down in recent decades, first with the proliferation of postdoctoral positions, then with the "widespread hiring of scientists on soft money in academic institutions." But what Stephan and Levin are talking about is not a contract; it is a delusion. A principal investigator directing several graduate students can turn out a new Ph.D. every year or two, over a career spanning decades. If all those Ph.D.s were to become principal investigators in academia, the result would be exponential growth with a vengeance. How could any intelligent graduate student, trained in science, expect such a patently Malthusian system to persist?

The authors also seek to draw a parallel between scientists and firms. "Market entry" for the scientist-firm occurs in graduate school. The attrition of many new firms before reaching "a critical size" is mirrored in the difficulty of surviving as an academic scientist to reach the status of principal investigator, where one becomes an "entrepreneur whose job it is to procure resources to sustain the lab—and to recruit talent to work in the lab" (p. 413). This idea of scientist *qua* firm appears incoherent, for Stephan and Levin also portray graduate students and postdocs as employees of the firm, at the same time that they are supposed to be fledgling firms themselves. The idea is circular as well, because the black box that serves as a firm in neoclassical mainstream economics is in effect itself just an individual pursuing a certain narrowly-defined goal, such as maximizing profit.

In "The Sociology of Scientific Knowledge: Some Thoughts on the Possibilities," D. Wade Hands takes note of several cases in which authors writing on scientific research from a sociological perspective clearly employ economic concepts and arguments, even while they fail to notice or acknowledge that that is what they are doing. But Hands' conclusion, that the economics of science needs to pay close attention to sociology, seems to me to have it precisely backward. It is the sociologists who, having entered the realm of economic analysis, would do well to study and acknowledge the literature of that field. Ostensible sociological critiques of economics, such as I have encountered, typically address straw-man caricatures of economic thinking, offering up instead glosses on history and current trends that suffer in various degrees from methodological collectivism, egalitarian fundamentalism, whimsical or pretentious jargon, and a tendency to recognize only base motives for economic action. (See, for examples, Michael Callon: "From Science as an Economic Activity to Socioeconomics of Scientific Research" and Steve Fuller: "The Road Not Taken: Revisiting the Original New Deal," in the volume under review.)

Hands believes that the "reflexivity problem," which afflicts the sociology of scientific knowledge (particularly the so-called "strong program" thereof), must also impact the economics of science. The reflexivity problem is this: to the extent that sociologists claim that the evolving scientific consensus is determined by social and institutional factors rather than by "nature" or "the facts," they become vulnerable to the counterclaim that the results of their own "social scientific" researches are themselves so determined. By the same token, Hands argues:

> Suppose that one is engaged in a public choice-type analysis of science; for example, suppose that we view individual scientists as acting in their own rational self-interest given the market for professional credibility. This public choice (scientific choice) analysis could just as easily be applied to practicing economists—in fact, it could even be applied to the specific public choice economist that was examining the behavior of scientists. The circularity or reflexivity problem thus occurs in the economics of science just as it does in the sociology of science. (pp. 539–40)

The key term here, I believe, is "self-interest." Hands cannot be faulted for using a term that is so often appealed to by economists themselves, but "self-interest" conveys a sense of crassness or acquisitiveness that does not always or solely motivate human action. It is rather more accurate to say that humans make choices and take action in pursuit of their individual goals and purposes. Whether the goal is wealth, acclaim, or the joy of helping to develop an intellectually satisfying new theory, it can in principle serve as the basis for an economic explanation, and such explanations apply as well to economists as to scientists. Reflexivity is only a problem if one is limited to positing goals that are unrelated to—or even call into question—the merit of one's efforts.

In "The Republic of Science: Its Political and Economic Theory," Michael Polanyi notes that "scientists, freely making their own choice of problems and pursuing them in the light of their own personal judgment, are in fact cooperating as members of a closely knit organization" (p. 465). Isolated from each other, they would accomplish little. The independent initiatives of scientists are coordinated, according to Polanyi, by a process of "mutual adjustment," in which "each takes into account all the other initiatives operating within the same system" (p. 466). Such "self-coordination" requires no external direction. Indeed, appealing by analogy to a group of people trying to solve a jigsaw puzzle, Polanyi notes that the very independence of individual initiatives permits each step to be "decided upon by the person most competent to do so" (p. 467). He concludes that "any authority which would undertake to direct the work of the scientist centrally would bring the progress of science virtually to a standstill" (p. 467).

Polanyi offers an insight into how the unity of science is maintained despite the increasingly narrow specialization of individual scientists.

> [W]hile scientists can admittedly exercise competent judgment only over a small part of science, they can usually judge an area adjoining their own special studies that is broad enough to include some fields on which other scientists have specialized. We thus have a considerable degree of overlapping between the areas over which a scientist can exercise a sound critical judgment . . . so that the whole of science will be covered by chains and networks of overlapping neighborhoods. (p. 471)

Scientists can thereby trust the authority of their colleagues in distant fields about which they personally know little, while "cranks and dabblers" are excluded. Polanyi thought that this web of overlapping specialties would enforce a coherent opinion regarding not only the validity of scientific results, but also the relative *importance* of problems in far-flung fields of scientific research. In this he was clearly mistaken. Scientist A may be in a position to judge the relative importance of problems 1 and 2; B might judge 2 and 3; C might judge 3 and 4. But there is no common measure and no mechanism by which these local assessments are transmitted to scientists in unrelated specialties.

Polanyi saw a similarity between the unmanaged coordination of scientific efforts and "the self-coordination achieved by producers and consumers operating in a market" (p. 467), even invoking Adam Smith's "invisible hand." In both cases, the participants' activities leave a record of essential information to guide the efforts of others. "In the case of science, adjustment takes place by taking note of the published results of other scientists; while in the case of the market, mutual adjustment is mediated by a system of prices broadcasting current exchange relations" (p. 468). The parallel is nevertheless strictly limited:

> But the system of prices ruling the market not only transmits information in the light of which economic agents can mutually adjust their actions, it also provides them with an incentive to exercise economy in terms of money. We shall see that, by contrast, the scientist responding directly to the intellectual situation created by the published results of other scientists is motivated by current professional standards. (p. 468)

The "contrast" intended by Polanyi is stark indeed: unlike ordinary people who respond to individual incentives, Polanyi's scientists seek only the collective advancement of knowledge. The decisions of each scientist "are designed to produce the highest possible result by the use of a limited stock of intellectual and material resources. The scientist fulfills this purpose by choosing a problem that is neither too hard nor too easy for him" (p. 468). Professional standards serve as a gauge by which the scientist "assesses the depth of a problem and the importance of its prospective solution" (p. 468). No hint is to be found here that scientists seek recognition and compete for priority, yet Polanyi could not have been unaware of the existence and power of such incentives, and an economics of scientific research is unlikely to get very far without taking them into account.

But economic modeling is clearly not Polanyi's object. Rather, he seems most concerned with the maintenance of no-strings public funding for basic research.

> [O]nly a strong and united scientific opinion imposing the intrinsic value of scientific progress on society at large can elicit the support of scientific inquiry by the general public. Only by securing popular respect for its own authority can scientific opinion safeguard the complete independence of mature scientists. (p. 473)

Might this respect for scientific authority be undermined by the acknowledgment of self-interested behavior among scientists?

In "Scientists as Agents," Stephen Turner notes that scientists devote much of their time not directly to research but to various evaluative activities, from reviewing papers for journals to screening job applicants, judging promotion cases, and grading student work. The result of such evaluation frequently is to bestow some kind of certification on scientists or their work, such as publication in a reputable journal, promotion to a higher academic rank, or the awarding of a Ph.D. degree. Turner finds that "there is a market for evaluators and evaluations, and the existence of this market is critical to understanding science" (p. 372). This market involves competition and risk. For example, a journal that publishes a faulty paper stands to lose its reputation for reliability in comparison with journals that adhere to more rigorous standards; on the other hand, an over-conservatism which fails to recognize merit in new ideas and unexpected observations allows other journals to capture the market for cutting-edge research.

Turner's essay is elaborated with appeals to such concepts as "information asymmetry," "bonding," and "agency relations," but these contribute only marginally to the basic lesson, which is the efficacy of choice in a competitive marketplace. Market transactions leave a record of useful evaluative information—whether in the form of prevailing prices, say, or in various forms of recognition and certification—by which participants may be guided in the future.

Turner asserts that the market for certification actually "produces the distinctive norms of science described by [Robert] Merton" (p. 379). The sociologist Robert Merton had posited the existence of a collective ethos—a moral code—governing the professional behavior of researchers in a famous 1942 paper, "The Normative Structure of Science." Turner directly addresses the Mertonian norms in a passage that is far too brief to do justice to the significance of his claim, but he does take time to deflate a rather silly concern that was raised by Merton and has persisted in the sociologically-oriented literature. On Merton's account, it is a moral imperative that ideas be judged solely on their intrinsic merit, not by their source. Therefore, something is wrong with the fact that more attention and credence tend to be given to the work of scientists who already enjoy a higher stature, based on their previous accumulation of recognition and awards, with the result that those scientists possess an advantage in the competition for additional recognition. This cumulative advantage, or so-called "Matthew Effect," violates the Mertonian norm of "universalism."

As Turner points out, however,

> the total value of the "product" in question, the science, is not only the ideas, the intrinsic value, but the guarantees that come along with it, in the form of risk-bearing actions taken by editors, hiring departments, and prize givers. . . . The accumulation of advantage is thus like the accumulation of cosigners to a loan. (p. 374)

It is a false morality that would deny scientists the opportunity to focus their attention on the work of those fellow researchers whose prior accomplishments indicate that their ideas are most likely to possess merit.

In his conclusion, Turner expresses concern about the "corporatization of science." He fears that the growing importance of corporate funding may threaten the "autonomy of science" that was fostered by government largesse and thereby "kill the goose that laid the golden eggs" (p. 383). He does not address the fact that the autonomy of science conducted with tax dollars is purchased at the expense of the autonomy of individual citizens who might have chosen to spend their own money differently. If government-funded academic science makes a breakthrough in elementary particle physics or cosmology, does this count as a golden egg on everyone's scale of values? Turner's very argument for the felicitous functioning of an evaluative market in academic science appeals by analogy to institutions and practices that evolved in the commercial marketplace; yet, even when he pauses to recount "some ways in which the academic-governmental system itself has sometimes gone wrong or been accused of going wrong," (p. 381) he concludes only that a closer connection with private enterprise may make things worse. Why not better?

Where Arrow and Nelson had claimed that private funding for science would be too little, Turner and others writing in this volume now fret that it is becoming too much. Does all this amount to a conservative heel-dragging against institutional change, perhaps exemplifying Mirowski and Sent's thesis that funding regimes generate self-justifying versions of an economics of science? Or are we just seeing garden-variety examples of collectivist bias in academia?

My conclusion to this long review can be quite brief. Altogether, the essays assembled here convey a single, clear message, namely, that the economics of science at present lacks a viable shared basis of inquiry. The optimization paradigm of neoclassical mainstream economics offers very little return on its investments in mathematical modeling. Sociology, whatever its merits as a window on human affairs, is no substitute for economic thinking. Most philosophers appear content to hew the neoclassical line; some delve into sociological and historicist speculation. I bring this review to an Austrian audience because science is a realm of human action that is particularly ripe for Austrian analysis, in which other approaches are not yet entrenched and have, indeed, proved ineffectual.

Scientists, like other individuals, make choices in pursuit of their goals. Working out the consequences of such purposeful choosing is the essence of the Austrian paradigm. As a scientist with an avocational interest in economics and philosophy of science, I have elsewhere (Walstad 2002) attempted to make a start on an economics of science from the Austrian perspective. The field cries out for the attention of professionals.

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