

The Economics of Science

Arthur M. Diamond, Jr.

Increasing the “truth per dollar” of money spent on science is one legitimate long-run goal of the economics of science. But before this goal can be achieved, we need to increase our knowledge of the successes and failures of past and current reward structures of science. This essay reviews what economists have learned about the behavior of scientists and the reward structure of science. One important use of such knowledge will be to help policy-makers create a reward structure that is more efficient in the future.

Introduction

George Stigler has noted (1982a:112) that the scientific study of scientists has been mainly undertaken by sociologists rather than by economists—there is an organized subdiscipline of sociology called “the sociology of science” (Mulkay, 1980), but no organized subdiscipline of economics called “the economics of science.” The scarcity of work on this topic is surprising since the tools of economics might contribute to the understanding of the behavior of scientists in several ways.

The primary thesis of this review essay will be that the economist’s utility maximizing model of human behavior and econometric tools for analyzing data are necessary for anyone who hopes to have a complete understanding of the advance of science and the behavior of scientists. Although I hope that this essay will be useful to economists, it is written to be accessible to non-economists. The essay will have several related goals. One will be to survey the literature and to examine how far economists have come in understanding science. A second will be to integrate what non-economists and economists have learned about the reward structure of science (including my own research on the economic value of citations and on the career consequences to scientists of having chosen a mistaken research project). A third will be to identify the areas where work remains to be done.

Following Milton Friedman’s (1966) famous distinction between “positive” and “normative” economics, this research will focus mainly on the

“positive” side. Positive economics is the part of economics that focuses on descriptive and causal knowledge as opposed to explicit policy prescriptions. Stigler sometimes justified a focus on positive economics by saying that whether you are a firefighter or an incendiary, you still need to know what causes fire. The justification for the focus on positive economics in this essay is partly that it allows the project to be narrowed sufficiently so that it is tractable. But, mainly, the justification is my belief that only limited progress can be made on normative or prescriptive issues until a firmer analytic and descriptive foundation has been laid. More concretely, we are more likely to effectively reform scientific institutions (or methodologies) when we have a clearer understanding of which institutions (or methodologies) have been successful in the past. The case studies that are discussed by the historian, the anthropologist, and by some sociologists, are important for laying a firm positive foundation. But the economist’s models of individual and institutional behavior and the economist’s tools of systematic data analysis also have something important to contribute.

Another interpretation of “economics of science” that will largely be absent from the current research is what might be called “economic impact analysis” of particular (and usually controversial) scientific or technical developments. A primary exemplar of a study that makes use of economics in this way is reported by Busch and his colleagues in *Plants, Power, and Profit* (1991). While such work is undeniably valuable, it is different from what I propose to undertake here. Busch is using economics to understand the impact of some “scientific results,” while we are using economics to understand “scientists” and “scientific” institutions.

The review essay begins with a survey of the existing literature on economic explanations of the behavior of scientists and scientific institutions. The human capital and implicit contracts literatures of the behavior of scientists are discussed, the latter elaborated in terms of the issue of tenure. The most common theoretical economic analysis of the university is the view that it is best thought of as a nonprofit organization. Variants of this view are discussed, with special attention to the literature on rent-seeking in academe.

In the second broad section of the essay, the econometric literature on the economics of science is discussed in the areas of scientific institutions, scientific production and earnings functions, and the earnings and status of minority scientists.

The best known and appreciated research in the economics of science concerns issues of how science contributes to technology and economic growth. Theory, case studies and systematic econometric analysis have all been brought to bear on these issues. Although we will sketch some of the highlights of this literature in the third broad section of the essay, we will not do the literature justice. To fully and fairly survey this literature is not possible because of the space it would require, and perhaps not necessary because the literature is already visible.

Economic Theories of Science and Scientists

1. What Is the Scientist After?

The psychic returns from advancing science have long been identified as one of the primary components in the compensation of scientists. Although Adam Smith was a cynical critic of the scientist's devotion to teaching, he was an idealist when it comes to the scientist's devotion to advancing the frontiers of knowledge (1976b:124):

Mathematicians, . . . , who may have the most perfect assurance, both of the truth and of the importance of their discoveries, are frequently very indifferent about the reception which they may meet with from the public. . . . The great work of Sir Isaac Newton, his *Mathematical Principles of Natural Philosophy*, I have been told, was for several years neglected by the public. The tranquility of that great man, it is probable, never suffered, upon that account, the interruption of a single quarter of an hour.

Even though we now know that Smith was wrong about Newton's alleged indifference to public recognition (see Hall, 1980, *passim*; Westfall 1980:698–780), Smith may still be correct in his more general claim that scientists receive psychic returns from the belief that their research is true and important.

More recently Tullock (1966:34–36) has agreed with Smith that curiosity is a primary motivation of some scientists (though many other scientists are paid to be curious, and hence have income as their primary goal). Usually, however, the economist assumes that the scientist is after the same things as everyone else: fame and fortune (Levy, 1988). The assumption is more due to methodological parsimony than to temperamental cynicism. Rather than resort immediately to different values to explain differences in human behavior, the economist proceeds to see how much can be explained by differences in constraints. If some differences persist in resisting explanation in this way, then ultimately it may be concluded that scientists are really different from the rest of us (perhaps in intellectual curiosity, or love of knowledge).

Of fame and fortune, economists have undoubtedly emphasized fortune most. Ghiselin (1987:271) has exaggerated this focus when he says that economic studies of science “ . . . have all but ignored the non-pecuniary aspects.” In part the emphasis on fortune may be because economists see a higher cognitive return in modelling what they hope to ultimately measure. Fortune is more obviously measurable than fame. (The Merton/Price school of sociology of science may have influenced economic studies of science more by showing that fame is measurable [through number of publications and, more strongly, through number of citations] than by functional or case-study arguments for “priority of discovery” as a motivation for the scientist.)

Other “non-pecuniary aspects,” such as Hagstrom’s emphasis of “gift-giving” (1965:19) may be less congenial to the economic approach. But even here, Posner (1988:174–176) has given an economic explanation of gift-giving as a form of insurance against risk.

2. Human Capital

Much of the modern interest in the economics of knowledge began with Gary Becker’s analysis of *Human Capital* (1st edition, 1964). Becker argued that investment in human skills and knowledge was just as important a determinant of economic growth and prosperity as investment in physical capital. He showed how education and on-the-job training were important determinants of differences in earnings both between persons and for the same person at different points in the life-cycle. The theory and evidence were elegantly developed in Mincer’s *Schooling, Experience, and Earnings* (1974). These works and a few others initiated a major change in the field of labor economics, transforming it from a field concerned mainly with institutional description into one concerned with the elaboration and testing of models of human behavior.

A survey of some of the traditional economics of education literature based on the human capital concept can be found in Psacharopoulos (1987). This literature usually did not explicitly model the behavior of universities and scientists, but did serve to make the economics of education a legitimate topic of scientific research.

An exception, one of the more extended applications of human capital theory to explain the behavior of scientists, is due to McDowell (1982). McDowell used data in professional directories and in sources such as *Physics Abstracts* to estimate rates of depreciation for knowledge in various fields. One use of his estimates is to test the hypothesis that in mid- or late career, the opportunity cost¹ of becoming a department chair is lower for scientists in fields that are advancing rapidly (and hence where knowledge is depreciating rapidly). He also used his estimates to examine gender differences in choice of academic discipline and in research productivity. McDowell argues that, since some women anticipate at least partial withdrawal from academic research for a part of their child-bearing years, they are likely to choose disciplines, such as the humanities, where their investments in human capital are less likely to depreciate rapidly. Using very rough evidence in the form of age-productivity profiles, McDowell believes that he sees convincing evidence that the average research productivity of female scientists temporarily declines between the ages of 28 and 31 (763–764).

A very different application of human capital theory to the behavior of scientists appears in “Science as a Rational Enterprise” (Diamond 1978, 1988c). Diamond develops a utility maximizing model of the behavior of scientists as a response to Kuhn’s claim (1970) that the acceptance or rejection of theories is an irrational process. Diamond claims that scien-

tists maximize a function that includes as arguments the scope and elegance of scientific theories. Because of different investments in human capital (in the current theory), scientists may differ in when they accept a new theory even if the scientists are all rational in the strong sense of sharing the same values (in this case the scope and elegance of theories).

Human capital theory has also been applied to predict the scientist's optimal life-cycle investment in human capital and life-cycle productivity in terms of quantity and quality of research. These issues have been addressed both theoretically (Diamond 1984, 1987) and empirically (Diamond 1986a; Simonton 1988a, 1988b; Stephan and Levin 1988, 1992).

Human capital theory is not without its critics among economists. For example, not all economists agree that the main function of college is to increase the skills and knowledge (i.e., human capital) of college students. Some have suggested (Spence 1973; see also Johnson 1973:33–34) that the main function of college is to provide a screen that determines which students will be able to obtain high-paying, challenging jobs.

Wolpin (1977) made a clever test of the screening hypothesis. He compared the level of education of those self-employed in an occupation, who presumably would have no screening motive to obtain a college education, with the level of those in the same occupation who were employed by a firm. He found that both groups obtained roughly the same level of education. From this he inferred that the main value of the education consisted in the skills obtained, an inference that supports the standard human capital account.

Garner has applied the screening model to the case of scientists being screened for initial employment and subsequent tenure and promotion. He claims that the common practice of screening on the basis of quantity of publications "... may bias research decisions toward orthodox, low-risk projects, denying science the bold hypotheses and vigorous competition necessary for significant advance" (1979:575).

Sharing enthusiasm for the screening hypothesis are Holub and his co-authors (1991), who hypothesize that most publishing is done in order to pass a "publish or perish" screen. Based on citation, and other evidence, they propose an "iron law of articles," which states that the number of "important" articles is equal to the square root of the total number of articles. They conclude that very few published articles provide new and useful information for readers.

Stigler (1982b) also agrees that many articles do not contain substantive new knowledge. He suggests that rather than serving as a screen, the research makes the researcher a better teacher by keeping her up-to-date in the latest methods and substantive developments in the field.

3. Economic Theories of University Behavior

Scientists may be funded by universities, by government, by non-profit foundations, or may be self-funded out of the scientist's own leisure.

David Colander (1989a:229) made a rough estimate, looking only at assigned time for research, that U.S. university funding of economics in 1987 was roughly \$250 million. This was approximately 20 times the NSF support for economics in that year. So it is understandable that those seeking to understand (and improve) the reward structure for science, have spent considerable effort studying the behavior and incentive-system of universities.

When W.W. Bartley wrote (1990: 114) that universities resemble "... fiefdoms, guilds, cartels, and mutual-protection rackets ..." he might have expected that Adam Smith, the founder of economics, would have approved of his remarks. Smith believed that because universities such as Oxford had large endowments, the professors' income was not related to how well they taught students. In the modern literature, Hansmann has returned to the question of why universities have endowments (1990). He concludes that endowments do not improve intergenerational equity, but may serve as a buffer against financial adversity and may help protect the university's intellectual freedom and reputation.

Smith might have argued in response that "intellectual freedom" is often a guise for pedagogical indolence. He believed that universities would be more efficient if they were run like for-profit fencing academies (1976a:760, 764). In modern times something very like the Smithian vision of higher education has been advocated by economist Ben Rogge and philanthropist Pierre Goodrich (Rogge and Goodrich, 1973). They advocate ending all government subsidies of university education and propose the abolition of tenure on efficiency grounds. Smith makes some assumptions that can be disputed. One is that good teaching is readily measurable and appreciated by the students. Another is that teaching, and not research, is the sole output of the university. A third is that professors do not receive direct utility from teaching, but perform it only to the extent that they are paid for it.

Sherwin Rosen (1987) has returned to Smith's critique of higher education to ask if we would be wise to follow Smith's advice that an academic's salary should be more directly provided from student fees. Rosen concludes that Smith might be right if the only output of the university was teaching, but not necessarily right if research is also an important output.

The usual argument is that if research is not directly subsidized, or is not jointly produced with some directly demanded good, such as teaching, then it will be produced below the "socially optimal level." The problem is that it is difficult to devise institutions that permit the researcher to receive compensation for the ultimate practical benefits of her research. This "public good" aspect of the production of science has been noted early and often (e.g., Nelson 1959:302; and Arrow 1962).

The problem in financing research has been compared by Harry Johnson with the problem of rewarding those who stock a pond (1972:17; see also 1973). Some fish will never be caught. Even for those that are caught, it is very difficult to determine how much of the value is contributed by the

fisherman, how much by those who maintained the pond and how much by those who originally stocked it. Similarly, pure research produces ideas that may or may not produce practically useful effects.

Adam Smith may have been the first to focus on possible inefficiencies in university education due to the reward structure set up by the university's status as a non-profit organization. Surveys of recent work on the economics of non-profit organizations can be found in Weisbrod (1988, 1989) and Holtmann (1988).

Although economists are not unanimous in their analysis of the profit maximizing firm (see, e.g., Galbraith 1985; and Williamson 1967), there tends to be a consensus on the general model that is apt to be most useful. Unfortunately, no consensus exists on the most fruitful model for non-profit organizations. This may be because work on non-profits is at the early stage preceding the development of a general model. Or it may be because non-profits differ sufficiently among themselves, so that no general account will fruitfully apply to them all.

Most early accounts, and some recent accounts, emphasize the inefficiency of non-profit organizations. Some more recent accounts, although admitting that non-profits may have less incentive to reduce costs, argue that by responding to various sorts of "market failure" they may be the most efficient form of organization under some situations.

For example, in some activities output may be very hard to measure, at least by the consumer of the service. Profit-making organizations may over-provide measurable aspects of their output as opposed to those aspects that are not measurable. Of course this problem would also apply to non-profits under models that do not have the managers caring directly about the mission of the organization. Some have argued (Ault et al., 1979, 148; see also W. Becker 1979), for instance, that universities may over-support research relative to teaching because research is easier to measure.

Firms are usually assumed to be profit-maximizers. But some economists, perhaps most notably John Kenneth Galbraith and Oliver E. Williamson, have argued that the diffusion of stock ownership allows some managers to pursue objectives besides profits. Following Southwick (1967), Kesselring and Strein have applied this model of the firm to the behavior of universities (1986; see also Rothman and Strein 1982). In their model, university administrators maximize a function of two variables: the quality of their university and "hierarchical expense," where "hierarchical expense" means "leisure or discretionary power" (104). These utility-maximizing models of the university could be interpreted as being elaborations of Culyer's claim (1970) that universities are most fruitfully viewed as clubs on the Buchanan model (Buchanan 1965; see also Sandler and Tschirhart 1980).

Another view of universities, often casually expressed, but not yet fully explored by economists, is that universities are best viewed as labor-managed firms, or as a form of partnership. This view may be what

Clark Kerr had in mind when he facetiously defined a "university" as "a series of individual faculty entrepreneurs held together by a common grievance over parking" (Honan, 1994:16).

4. Implicit Contract Theories

The traditional economic theory of labor markets suggested that, in any period, workers would receive the value of their productive output (in technical language: their "marginal revenue product"). Actual compensation in real-world labor markets often has not seemed to correspond with what has been predicted in the traditional theory. It is often observed, for instance, that the variation in productivity of scientists far exceeds the variation in their compensation. To account for such anomalies, some economists have contributed to a growing literature sometimes loosely called "implicit contracts theory." The basic ideas in implicit contracts theory have been well-summarized in Rosen's (1985) survey article.

The implicit contracts literature generally argues that on average, over their life-cycle, workers are paid the value of their marginal product, although in any single period the worker may be paid more or less. The models differ in the explanation they offer. One common explanation is that workers are risk-averse and that the firm provides earnings insurance by paying the worker more than productivity merits during periods of low worker productivity and less than productivity merits during periods of high worker productivity. Smith Freeman (1977) specializes this model to the case of research scientists.

Freeman suggests that scientists are paid higher than their average productivity early in their careers because their productivity is uncertain, and they are being insured against the possibility that their productivity is low. Frank (1984, 1985) has an alternative explanation based on a compensating differentials argument. He argues that scientists (and others) receive psychic satisfaction from being at the top of the pay distribution, and receive psychic pain from being at the bottom. In order to accept employment, the university must offer the "little fish" added compensation. Conversely, the "big fish" will accept employment at a lower salary than indicated by productivity alone, because she is receiving psychic compensation from knowing that she is "big."

Ransom (1993) offers yet another explanation for the phenomenon. He argues that because of low mobility (high moving costs) the university is able to act as a monopsonist (sole hirer of the scientists). In accord with the well-known economic theory of monopsony, this implies that the senior scientists would be paid less than if the scientific labor market was competitive. Newly minted scientists, with lower moving costs, face a more competitive labor market.

Kahn, Landsburg and Stockman (1996; 1992:510–513) and Landsburg (1993) assume that scientists are of two qualities, talented and untalented

(or "good" and "bad," as Landsburg calls them in his book). They claim that "... the talented type i scientists, knowing that they are often successful, might be willing to risk making novel predictions, while the less talented type j scientists would elect the safer course of looking first." (1992:510) Since scientific institutions are ignorant of whether a scientist is talented or untalented, the institutions induce the scientists to reveal their talent. Less prestigious institutions will offer flat salaries that overpay for the social value of research. Untalented scientists will *choose* these institutions. More prestigious institutions will offer a contingent fee based on the success of the research. Talented scientists will choose these institutions. The authors are confident that "... this solution bears some resemblance to our casual observations of the real world" (1992:513). Other researchers whose observations are not quite so casual have observed, contrary to Kahn and co-authors, that scarce slots at the prestigious institutions are *not* allocated on the basis of a scientist's "choice": which is to say that at the offered rate of compensation, there is a surplus of scientists willing to fill the limited number of slots. Alvin Roth (1994) and Aloysius Siow (1993) have discussed these features of the "real world" in academic and other labor markets.

Lazear (1996) has specialized some of the implicit contract models that he and others have developed, in order to find the optimal criteria for an agency such as the NSF to use in funding research. A primary result of his analysis is that the NSF should provide a small number of large grants (as opposed to a large number of small grants), so that the more able scientists have sufficient incentive to invest heavily in the proposal process. Another result is that the NSF should reduce the weight it places on past accomplishment, if it wants to increase the effort put forward by the more able. Finally, Lazear argues (based on the Black-Scholes model of option pricing from finance) that for a given expected level of research, the field with the higher variance of research outcomes should be funded in preference to the field with lower variance. (Yes, he identifies economics as a field with high variance.)

Lazear's advocacy of large grants to the able few may not be fully consistent with his argument elsewhere (1989) that when team production allows the opportunity for some members to sabotage the work of others, the incentive to sabotage is reduced by compressing salary differentials. Accepting this argument of Lazear's, Ehrenberg and co-authors conclude (1990) that work environments where sabotage exists are "not conducive to tournament-type pay structures" (1323). (Discussions of academic "sabotage" can be found in the sections on "Tenure" and on "Rent-seeking" that follow.)

Many models of the scientific labor market (e.g., Freeman, Siow, Lazear), assume differing quality of scientists, initial ignorance of a scientist's quality by the scientific institution, and gradual revelation of quality as a scientist's research outcomes become known. All recognize, at least as a casual caveat, that the information in a positive or negative outcome

depends on how much the outcome was due to ability (or effort) and how much it was due to chance. Camerer and co-authors (1989:1245–1246) suggest that, *ex post*, we systematically exaggerate the predictability of outcomes. They base this view on their experimental evidence for what psychologist Baruch Fischhoff calls “hindsight bias.” If hindsight bias is an important factor, it would seem to imply that scientific institutions misallocate resources by underinvesting in information about the quality of scientists (i.e., by reaching judgments about quality before quality has been accurately revealed).

In addition to the authors discussed above, a growing number of economists in recent years have modeled aspects of the labor market experience of academic scientists (W. Becker 1975, 1979; Harris and Weiss 1984; Weiss and Lillard 1982; Diamond 1984; Carmichael 1988; Ito and Kahn 1986; Diamond 1987; Siow 1991, 1993; Diamond 1993b). More general implicit labor contract models may possibly be specialized to explain some of the features of the labor market for scientists (e.g., Lazear and Rosen 1981; Harris and Holmstrom 1982).

5. Tenure and Related Issues

In 1958 Armen Alchian was one of the first economists to try to explain academic tenure. He argued that in non-profit organizations (including many universities) the management has little incentive to reduce costs, since the savings will not result in higher management salaries (at least not to the same extent as in for-profit firms). As a result, Alchian expects that non-profits will invest more in aspects of the work environment that make the work environment more pleasant. One of these is job security, that in the academic world takes the extreme form of tenure.²

As an outgrowth of the implicit contracts literature, several labor economists in the past several years also have begun to develop models of the tenure phenomenon in the academic labor market. Abba Schwartz (1988), for instance, examines the possibility that tenure is a kind of “efficiency wage,” defined as a higher-than-usual wage that is paid in order to increase the worker’s incentive to be productive.

In a different implicit contracts model of tenure, Carmichael (1988) argues that faculty are much better informed than is the administration about the quality of new job candidates. If the size of the current compensation pie is fixed, then the current faculty will suffer salary cuts if they correctly identify able new hires. Tenure, in the Carmichael account, provides current faculty a degree of job and salary security that increases the likelihood that they will correctly identify the best new hires.

Waldman observes (1990) that the awarding of tenure serves as a signal to outside institutions of the ability of the scientist. The main value to the scientist is not the salary increase that directly accompanies tenure, which is often small, but rather the increased salary and mobility that results from outside offers in response to the signal.

Also related to the tenure issue, is Siow's (1991) intriguing model that implies that if a scientist is unlucky in the quality of her first published paper, other scientists will not read the scientist's later papers because the expected return will be higher for reading new papers by other recent Ph.D.'s.

In more recent work, Siow (1993) attempts to give a unified account of peer review, tenure, up-or-out rules, and the lower-than-expected salaries for senior professors. He claims that universities function to match high quality students to high quality faculty. According to Siow, neither students nor universities value research directly, but only use it as a screen to distinguish the quality of faculty. In Siow's account, without tenure, faculty would spend more time on research (and less on teaching) in order to maintain competitiveness in a labor market that mainly hires on the basis of research productivity. With guaranteed employment, the faculty member is more willing to concentrate on teaching, which is what the universities and students value.

6. Rent-Seeking in Science

Rent-seeking is a socially unproductive activity that seeks a higher reward for goods and services than would be required for the producer of the goods and services to produce them. Rent-seeking has been distinguished from the socially productive activity of rent-creating in a market system. Rent-creating occurs when a firm or entrepreneur innovates and thus creates short-term rents (also known as economic profits) that will last until other firms imitate the innovation. A traditional example of a rent-seeking activity would be a firm that seeks an exclusive license from the government in order to create a monopoly that will permit the firm to charge higher than competitive prices.

Since Gordon Tullock first introduced the idea of rent-seeking in 1967, several papers have suggested that rent-seeking is an important phenomenon in academic life. Although many discussions of academic life have examined rent-seeking activities without explicitly identifying them as such, Richard McKenzie may have been the first in print to explicitly apply rent-seeking to the academic world (1979).

McKenzie argues that the amount of increases in salary available to a department is a fixed sum that is unrelated to the department's productivity. (Economists will recognize this claim as analogous to the classical doctrine that there is a "wages fund".) Within a department some attempt is made to divide the fixed sum in accordance with the relative productivity of the members of the department. A faculty member thus has two methods for increasing her salary: she can increase her own productivity, or she can decrease the productivity of others, perhaps, for example, by the creation of unnecessary committee work.

Brennan and Tollison (1980) follow McKenzie in assuming a wages fund at the departmental level (348). They assume that department heads

decide salary increments and that the department head is seeking to maximize the “academic worth” of the department. In the Brennan-Tollison set-up, the academic worth of a faculty member is equal to the highest salary offer that the faculty member could generate from any other university (the “reservation wage”). In their model, a department head with perfect information would pay each faculty member exactly that member’s reservation wage. In this way, with a fixed budget, the department head can maximize the academic worth of the department. Since the department head does not have perfect information on the faculty member’s reservation wage, the rent-seeking faculty member has an incentive to make the reservation wage seem higher than it actually is. This can be done, for example, by exaggerating the attractiveness of outside offers or the unpleasantness of the current location.

The ideas in the McKenzie and the Brennan and Tollison papers have been extended and given more formal presentation in a pair of brief papers by Stolen and Gleason (1984; 1986). A distinct rent-seeking account is provided by Grubel and Boland (1986), who have documented and criticized the long-run rise in the use of sophisticated mathematics in the economics profession. The authors give a rent-seeking account of why formalization has gone beyond the epistemologically optimal level. They suggest that applied economists have a higher opportunity cost of their time because applied economists are more in demand (than are mathematical economists) for outside consulting. As a result, mathematical economists are more likely to serve (and participate actively) on important committees. Therefore, the mathematical economists have a greater influence on professional standards (and hence on hiring, promotion and salary decisions) than would be suggested by their numbers alone. Grubel and Boland do not explain, however, why historians of economic thought, who also have low opportunity cost of time, have not been equally successful in influencing professional standards.

Murphy and co-authors (1991:524–525) proxy the level of rent-seeking in a society by the proportion of college majors in law. They find strong evidence that the growth of gross domestic product (GDP) is positively related to the proportion of college majors in engineering and weaker evidence that the growth of GDP is *negatively* related to the proportion of college majors in law.

7. Fads, Herds, and Informational Cascades

According to John Maynard Keynes (1936:156–158): “Worldly wisdom teaches that it is better for reputation to fail conventionally than to succeed unconventionally.” While not going quite so far as Keynes, recent literature in economics on fads, herd behavior and “informational cascades” does at least seem to indicate that it is better for the reputation to fail with the crowd than to fail by oneself.

In a key paper in the recent literature, Scharfstein and Stein (1990)

argue that investment managers will more likely be blamed if they lose money in unpopular investments than in popular investments. This creates what the authors call "herd behavior." The same phenomena has been identified by Michael Lewis in his best-selling *Liar's Poker* (1989). Lewis claims that investors who are unconstrained by this reputation effect have an opportunity to make significant amounts of money.

The development of the herd model both in Banerjee (1992) and in Bikhchandani et al. (1992) suggest that rational agents will take account of their own information, plus the information assumed to underlie the actions of others. This may result in what Bikhchandani and co-authors call "informational cascades," in which additional affirmations of a proposition are no longer based on any additional information. In a disturbing result, they show that under plausible assumptions, a false informational cascade can result. The authors specifically apply this to the case of a journal referee rejecting an article on the basis of knowledge of past rejections. As a second illustration from academia, they mention that in the market for newly minted academics, interviews and job offers come much more rapidly after the first, especially if the first comes early.

Other possible examples come readily to mind. For example, a foundation program officer may find it easier to defend the funding of a failed research project if the project was on a mainstream topic, using mainstream methodology, done by a scientist from a top school. Of course, scientists themselves would not be immune to the herd effect. Those more likely to resist the herd would be those who are less constrained by concerns about reputation (those with tenure?, those who are independently wealthy?, those who are self-confident or optimistic?).

Econometric Studies of Science and Scientists

1. Supply and Demand of Scientists

Economists have a fairly long, and mainly empirical, literature on whether there are "too few" scientists (Blank and Stigler 1957; Arrow and Capron 1959) or "too many" (Stephan and Levin 1992:168–169) or "too many now, but too few in the near future" (Bowen and Sosa 1989). An NSF-sponsored projection to the year 2005 of the equilibrium number of scientists and engineers resulted in a set of plausible projections ranging from a growth rate of 9 percent to a growth rate of 54 percent (Braddock 1991).

Simon and Warner (1992) have applied a Jovanovic job matching model to data on non-academic scientists and engineers. They find that scientists who are hired because of an "old boy" contact at the firm will have: higher starting salaries, lower subsequent wage growth, and a longer period of employment with the firm.

The reasons for mandatory retirement, and the effects of its elimination on the supply of scientists, have been discussed by several econo-

mists. Weiler (1987:213) suggests that Lazear's theory (1979) is "... compelling as an explanation of mandatory retirement in higher education" As applied to the case of scientists, Lazear's argument would be that scientists are overpaid relative to productivity when they are young, and overpaid when they are older. For the salary schedule to be sustainable, mandatory retirement is necessary as a means of putting a limit on the number of years that the scientist is overpaid. The application of Lazear's model to the case of academics is not plausible, however, if we accept the evidence of Ransom (1993) that there is a *negative* effect of age on earnings. Ransom's evidence undercuts Lazear's assumption that older academics are *overpaid* relative to productivity, indicating instead that they are *underpaid*.

Smith (1991; see also Rees and Smith 1991) has estimated the effect of the elimination of mandatory retirement for tenured faculty that went into effect on January 1, 1994. She concludes that the effects will be small, since most faculty will choose to retire at the same time as before. The exceptions will occur mainly among research faculty who have light teaching loads and able students.

One of the most highly-cited models of the interaction of supply and demand in the labor market for scientists and other professionals is the "cobweb" model of Richard Freeman (1975). This model, originally developed for agriculture and also applied by Freeman to lawyers, suggests that a would-be scientist commits herself to become a Ph.D. scientist based on the salaries for scientists at the beginning of the scientist's years of training. Thus, supply responds to price, only with a lag. This results in equilibria that can bounce around over time in a pattern that has been compared to a "cobweb." Freeman empirically supports his model with data from the market for physicists.

In an attempt to clear away the cobwebs, Siow (1984) applies a rational expectations argument to suggest that scientists will do a better job of predicting future salary than suggested by the Freeman model. Although Siow uses data on lawyers to confirm his model, he suggests that the model is equally applicable to other professions, presumably including scientists.

2. *Production and Earnings Functions for Scientists*

Scientists are often viewed as jointly producing two goods: research and teaching. Whether these goods are complements or substitutes is still a live topic of dispute. Research on the production of these goods has usually been done separately for each good. Lovell (1973), for instance, was one of the first to estimate production functions with measures of research as the output. Laband (1986) has followed in this tradition by estimating a production function where the output was the number of citations that an article received. Others have estimated production functions using some measure of teaching effectiveness as a measure of out-

put (Walstad and Soper 1988; Heath 1989; Hanushek 1986; Watts and Lynch 1989; Diamond and Medewitz 1990).

Feigenbaum and Levy (1993b) have replicated a substantial number of empirical articles in economics and have constructed an index of empirical accuracy. They also have collected biographical and career data on the economists who wrote the articles. Inspired by human capital and life-cycle career path considerations, they ask whether we can understand why some economists produce empirical work that is more accurate than the work produced by others.

Most scientists probably believe that the clarity of writing style is (or at least should be) an important determinant of the success of the writing. Diamond and Levy (1994) have used the Grammatik computer program to measure the writing quality of about one hundred addresses of presidents of the American Economic Association. They have also collected data on the career paths, number of publications and number of citations of the presidents. The authors find that the more an economist uses the active voice, the greater the economist's impact on the profession, as measured by citations.

In the theory survey I mentioned that human capital theory had a major impact on research in labor economics. One of the topics high on the new research agenda was to explain the determination of earnings in different sectors of the economy. Many empirical studies looked at earnings as a function of education, on-the-job training and other variables, often including age, race, gender and the like. Other empirical studies looked at earnings as a function of productivity. Standard economic analysis implied that workers of all types should receive earnings equal to the value of their marginal product, which is a technical way of saying that they should receive the value of their contribution to the value of the output. Some of these studies have looked specifically at the salaries of university professors (e.g., Holtmann and Bayer 1970; Bailey and Schotta 1972; Cohn 1973; Katz 1973; Koch and Chizmar 1973; Siegfried and White 1973; Johnson and Stafford 1974; Tuckman and Leahey 1975; Tuckman and Hagemann 1976; Tuckman 1976; Tuckman et al. 1977; Hansen et al. 1978; Lillard and Weiss 1979; Hamermesh et al. 1982; Hansen 1985; Diamond 1986b; Sauer 1988; Hamermesh 1989; and Kenny and Studley 1995).

Although benefit compensation could reasonably be included in earnings for the purpose of estimating earnings regressions, it is general practice not to do so, because of the high cost of obtaining accurate data. If benefit compensation was a substitute for salary compensation, then salary alone would be a poor proxy for total compensation. Reassurance that the general practice is defensible, however, is found in the evidence of Browne and Trieschmann (1991) that salary and benefit compensation are positively correlated.

Bayless has looked (1982) to see if the salaries necessary to retain academics at unattractive locations are higher than those that are necessary to retain them at attractive locations. The underlying theory is an ex-

ample of the “compensating differentials” argument that was first exemplified in this review essay by Frank’s theory of salary compression. In his version, Bayless finds that academics are willing to accept lower salaries in order to be in a densely populated location, and must be paid higher salaries to work in environments that are polluted, hot or humid.

Much of the work in the estimation of salary functions implicitly assumes that we have good measures of the research productivity of scientists and that these measures consist of the quantity (number of articles) and the quality (number of citations) of a scientist’s work. Within the sociology of science discipline, this assumption is controversial. It is defended by Robert Merton (1973), who has argued that one of the primary norms of science is that it is a meritocracy: success depends mainly on the quality of one’s work. The concrete measure of a scientist’s success, in Merton’s account, is the quantity of citations that the scientist receives from her peers. Some empirical studies have defended the Merton thesis (e.g., Zuckerman and Merton, 1971; Cole and Cole, 1973; Zuckerman and Cole, 1975; Gaston, 1978), while others have (explicitly or implicitly) criticized it (e.g., Koren, 1986, 1987; Peters and Ceci, 1982; Cole, Cole, and Simon, 1981; Mulkay, 1976, 1980).

Accepting the Merton thesis, Stigler and Friedland were the first to use citations as a measure of quality in economics (1982a). Later, with support from Stigler’s Walgreen funds, Diamond compiled a longitudinal data set on salaries, publications and citations for scientists in order to answer three questions. The first (Diamond, 1985) was how the order of authorship on an academic paper affects the compensation that a scientist receives. The second (Diamond, 1986b) was whether the academic labor market rewards the number of citations that other scientists make to a scientist’s published work. The third (Diamond 1986a) was whether a scientist’s productivity declines with age. The data set is now being used (Diamond, 1993b) to examine which of two implicit contract models is most consistent with the evidence.

Citations can be used, not only to measure the magnitude of scientific impact, but also the nature of the impact. Stigler (1982b) has used citation analysis to see if his refutation of the kinked oligopoly demand curve reduced later use of the curve (he found that it did not). Stigler and Friedland (1982b) also used citation analysis to learn whether schools of thought play an important role in economics. Diamond (1988a) has used citation analysis to see if theoretical work in the last few decades in general equilibrium analysis has had a major impact on empirical work in economics (he found that it did not). Finally, citations can be used in order to identify which topics are currently the most intense focus of research (Diamond, 1989).

Some previous work has focused on the earnings or status of female scientists and black scientists, although almost no work has been published on the status of those of foreign birth or citizenship. Using 1964 National Science Foundation data for scientists (economists were not in-

cluded, but psychologists were), Holtmann and Bayer (1970) found that, *ceteris paribus*, male faculty earned \$2074 (in 1964 dollars) more per year than did female faculty (415). The Holtmann and Bayer study is noteworthy because it makes use of a data set that is especially rich in variables, such as IQ and citations, that can serve as plausible controls for ability and research productivity. When studies without such controls find that minorities are paid less, interpretation is difficult since earnings differences due to discrimination cannot be distinguished from those due to differences in productivity or to other personal characteristics, such as labor force attachment.

Blank (1991) reports on a test by the *American Economic Review* on the effects of blind refereeing. She finds that rejection rates for males and females are about the same under both blind and non-blind regimes. She does find some effect of blind refereeing in that acceptance rates for those at "near-top-ranked universities" and non-academic locations were lower with blind refereeing than with non-blind refereeing. Broder (1993) looked at male and female reviewers of NSF proposals and found that male reviewers were about equally stringent in evaluating proposals by men or women. But women reviewers evaluated other women *more stringently* than they evaluated men.

Lindley and co-authors (1992), using data from the University of Alabama to explain salary differences, find that females have a higher return to productivity variables such as research, but a lower return for the unmeasured characteristics (that are captured by the gender "dummy" variable).

3. Scientific Institutions

In recent years economists have increasingly attempted to measure various aspects of scientific institutions. Some of this literature is usefully surveyed in Colander (1989b). Although some of the papers (e.g., Levy, 1988; Diamond and Haurin, 1993; Diamond and Levy, 1994; Feigenbaum and Levy, 1993b) relate the measures to theoretical models, many other papers simply report rankings of journals, universities or individual scientists based on various measures of productivity, such as the number of publications or the number of times the published work has been cited by others.

Stigler (1965) was one of the pioneers in the empirical study of scientific institutions. His paper addressed several issues, including how the economics discipline changed when professional economists replaced amateur economists in the discipline. Continuing the study of professionalism in economics, Diamond and Haurin (1994) examine what determines whether an economist will join and remain in the American Economic Association. The authors find, for instance, that membership is positively related to research productivity.

Liebowitz attempts (1985) to explain the increase in price discrimina-

tion by academic journals in the 1960s and 1970s as due to the increased inelasticity of demand by institutions that resulted from the introduction of the Xerox 914 copying machine in 1959. The underlying assumption is that institutional demand will be less responsive to price if the institution's customers value the journal more (because they can copy articles from it). Liebowitz finds weak evidence that the ratio of institutional price to individual price is positively related to the journal's popularity, as measured by citations per page. When Joyce (1990) replicated the Liebowitz study with a larger sample, and additional proxy variables for popularity, the evidence of the effect of journal popularity on the extent of price discrimination was strongly mixed.

Brenner (1987:115–118) looks more broadly at scientific institutions and concludes that frequently the creative scientific innovator is an “outsider” to the institutions. A corollary is that once the innovation has been made, the institutions frequently resist its acceptance.

4. Mobility of Scientists

Perhaps the first study of the geographic mobility of economists was made by Skeels and Fairbanks (1968/1969). They found that economists who publish are more likely to move than economists who do not publish. Ault, Rutman and Stevenson (1979, 1982) also have studied the inter-university mobility of economists as a function mainly of research productivity. They focus on research productivity because they believe that it is the most marketable aspect of academic productivity (more so than teaching and service) and that it is the most easily measurable. The authors conclude that increased publications will increase a scientist's likelihood of upward mobility, but not by much (1979:152). Evidence on the relative mobility of male and female academics can be found in a paper by the sociologist Rachel Rosenfeld (1981).

Reinforcing the finding of limited mobility, Ehrenberg and co-authors (1991) examined data on annual faculty turnover from the 1971 school year through the 1988 school year. They found retention rates stable across years and types of institution for each rank of professor (roughly 85 percent for assistant professors and 92 percent for associate and full professors). They also found that higher salaries increased retention rates for assistant and associate professors, but not for full professors.

Reinforcing the positive effect of research on mobility, Weiler (1991) estimates separately the probability of an academic searching for a new job and, for the subset who search, the probability of accepting a new job. He finds that the probability of searching is positively related to the number of previous academic jobs, the number of children, a primary interest in research (as opposed to teaching), the number of books published and the number of articles published. He finds that the probability of searching is negatively related to age. Similar results generally hold for the job acceptance equation, as well.

Albert Rees (1993) used an extensive data set on Ph.D.'s in science and engineering to learn the effect on salary of mobility. He found that there was a positive return for academics who moved to another academic institution, and a larger positive return for academics who left academics for nonacademic employment. Similarly, he found that there was a negative return for those initially employed in the nonacademic sector who moved to an academic institution. He concludes that nonacademic scientists receive a compensating differential in exchange for foregoing the security of tenure and the relative autonomy of professors in academia.

Biddle and Roberts (1994) focus on the mobility of nonacademic scientists and engineers between technical and managerial jobs. They present evidence that scientists and engineers are sorted on the basis of technical ability, and that the most technically able are promoted to managerial jobs, due to a positive correlation between technical ability and managerial ability.

5. Choice of Subfield and of Research Projects

Merton (1938) has suggested that the choice of problem in science may be influenced by economic considerations, but that such influence need not extend to the substantive content of the science produced. Stigler (1982a) would accept that the choice of problems is influenced by demand, but goes further to say that the actual substantive content may be influenced as well.

Evidence abounds that the funders of science have some influence on the topics studied and the methods used. In economics, Stigler (1965) has argued that when economics was done by part-time gentlemen scholars, the work was usually applied and policy-oriented. As economics came to be done by professional academics, holding endowed chairs, it became less responsive to policy demands from the outside world and more responsive to internal theoretical puzzles. Friedman (1981) has argued that the National Science Foundation has directed the economics profession toward a highly technical mathematical method. In natural science, Biagioli (1993, esp. 162–163) has documented how the problems studied by Galileo were influenced by the patronage system of the time.

In analysis congenial to the economic approach, Kohler (although not an economist) has examined how micro-constraints of academic life influence the choice of research projects. In biology he argues (1993) that the fruit fly *Drosophila* has been used as the subject for so many experiments “ . . . because it was adaptable generally to academic life” (309). He points out, in particular, that the fruit fly can be bred quickly and successfully by low-paid, seasonal labor (undergraduates). Since the fruit fly is hardy, and eats almost anything, the equipment for breeding and maintaining populations is relatively inexpensive.

In physics Kohler argues (1990:658–660) that late-nineteenth-century American physicists concentrated on careful fact-gathering because they

lacked the concentrated time, and intense collegial interaction of their better-funded European peers. In this account, the comparative advantage of the Americans was fact-gathering, while the comparative advantage of the Europeans was problem-solving theory development.

The structure of academic institutions may also have *intended* consequences on the problems studied and the methods applied. Gordon Tullock was the founder and, for twenty-five years, the editor of the journal *Public Choice*. He candidly admits to an editorial practice that is endemic:

From time to time I have decided that some particular subject should be encouraged; hence, I have lowered my standards for that particular subject with the idea of making it obvious to bright young assistant professors that this is a particularly easy place to do research which will be published. And then, as I get more articles, I raise my cutting level again but have meanwhile changed the total structure of the discipline. (1991:138)

Other editors would deny that they lower the price-to-publish as an incentive to change their disciplines, but many a rejection has been written saying that the paper has been well done, but on a topic that is "uninteresting."

Diamond (1993a; see also Diamond, 1988b and 1994a) has attempted to learn whether a scientist's choice of research projects affects either the scientist's professional recognition, as measured by citations, or the scientist's institutional affiliation. Working with a unique data set on the polywater episode in chemistry, he finds that the effect of working on a mistaken research project (polywater) was relatively small in terms of citations and nonexistent in terms of institutional affiliation.

In empirical research on issues of subfield choice, Diamond and Haurin, in a pair of papers, have exploited the directories of the American Economic Association to answer a set of related questions about the productivity and reward structure of academic economics. In one paper (1995) the authors document the changing popularity over the last seventy years of the various subfields of economics. In a second paper (1993) they examine whether the changes in popularity of a subfield appear first at the elite graduate schools and then later at the rank-and-file schools. In related work, Diamond and Feigenbaum (1993) are examining whether there are differences by gender in responses to changing demand for various subfields.

The Contribution of Science to Technological Change and Economic Growth

1. The Impact of Science on Technology and Economic Growth

Thomas Gieryn (1987; 1988) has argued that science does not benefit the public, but only achieves support by effective (and manipulative) advertising. The counterpoint to Gieryn's argument (Diamond, 1988d, 1988e) is that science has contributed to important technological advances that have improved the length and quality of human life. Casual examples would be advances in telecommunications and medicine.

Some of the most visible contributions by economists to science studies have been on the extent to which scientific research has contributed to technological change and economic growth. In recent decades several economists, and some noneconomists too, have attempted to learn more about the relationship between pure science, technology, and economic growth.

Technical innovation was an early topic of interest to economists. In the *Wealth of Nations* (Vol. 1:20–21), Smith made much of the importance of specialization in explaining innovation. He gives the example of the small boy who must work a lever in a mindless repetitive way, who discovers a way to do it mechanically, thus freeing himself to play with his friends. In such an account pure science (a.k.a. basic research) has no role to play in development of technology. When Smith elsewhere (1976b, 1980, 1983) discusses the reasons for doing pure science, he claims that the value of science is purely intellectual—to satisfy our curiosity about how the world works.

Although economic growth was the main concern of Adam Smith, economists soon focused most of their attention on the optimal allocation of resources in a situation of static equilibrium. Only after World War II was substantial attention again turned toward issues of economic development and growth (see Metcalfe, 1987). Kuznets and others paved the way by developing measures of such economic aggregates as national output. Using the aggregates, economists attempted to learn how much of the aggregate increase in output was due to increases in input quantities and how much was left over as a residual. Papers by Abramovitz (1956), Solow (1957) and Kendrick (1973) presented evidence that no more than 25 percent of the increase in output was due to increases in inputs. Researchers casually associated the substantial residuals with technical innovation. They began a research program to learn the causes of technical innovation and to more rigorously measure the impact of technical innovation on economic growth.

Although most economists prior to the 1950s had ignored technical innovation, some earlier economists had discussed technical innovation as the product of entrepreneurs. Knight (1921) and Schumpeter (1934; 1976) were exemplars of this approach, and Kirzner (1973; 1979; 1985) is a

contemporary working in the same tradition. Some (e.g., Heertje, 1987:265; and Metcalfe, 1987:618) view Schumpeter's contribution to the literature as having been seminal. Many, for instance, follow Schumpeter in distinguishing "invention," which is exogenous and unexploited technical advance, and "innovation," through which entrepreneurs make use of some inventions to bring new products (and processes) to the consumer. A notable example that makes use of the Schumpeterian distinction is Dosi's useful survey article on the economics of innovation (1988).

In a separate line of research on innovation, Schmookler (1966, 1972) used patent data to show that the frequency of invention was related to changes in demand. Von Hippell also emphasizes demand (1988). Others have stressed the importance of supply in the sense that some areas may be more "ripe" for invention, perhaps due to the advance of relevant science.

2. *The Impact of Science on Technology*

Before the 1960s, few economists gave much attention to the role of science as a condition for technical innovation. Instead, they treated science as though it were either irrelevant to technical innovation, or else was an exogenously given (and not very important) input into technical innovation. Rosenberg (1982) has criticized the economics profession for treating the phenomenon of technological advance as though it were a "black box." He says: "... the economics profession has adhered rather strictly to a self-imposed ordinance not to enquire too seriously into what transpires inside the box" (1982:vii).

Mowery and Rosenberg (1989:11-14) have also criticized the distinction between pure and applied research, citing several cases in which those seeking "practical" results achieved theoretical advances. The distinction is not always clear, in the sense that work initiated with an applied purpose often has "pure" implications and vice versa. Munevar (1990) argues that science advances by exposure to new phenomena. Advanced technology permits us to observe very small and very large phenomena that would be unavailable with lower levels of technology. With observation of the new phenomena, the robustness of old theories can be tested over new ranges of experience. Ackermann (1985) goes so far as to argue that new data made possible by new instruments are the main means by which theoretical disagreements in science are settled. Although many scholars agree that technology sometimes promotes pure science and sometimes pure science promotes technology, the consensus is that the causal relation, at least in the last century, is more commonly from pure science to technology than the other way around. The consensus is supported, for example, by Huffman and Evenson's detailed empirical study of *Science for Agriculture* (1993). One elaboration of the consensus that has recently received discussion and limited support is that science "recharges" the inventive productivity (see Kortum, 1993; Evanson,

1993; Adams, 1993).

Although the literature is not large, several contemporary economists have attempted to answer the question of whether science contributes to technical advance. Part of the increased attention to this issue may be due to the increasing number of examples in which science does seem to have contributed to technology. The studies of this issue have made use of three broad methods.

One way to study the issue is to examine the details of particular episodes to learn whether science seems to have made a contribution to the technological advance. This kind of study has been notably carried out by Griliches (1988) in his detailed investigations of agricultural innovation and by Nathan Rosenberg (1982). Dosi (1988:1136) also provides references for three specific modern cases in which scientific breakthroughs led to significant technological advances: synthetic chemistry, the transistor, and bioengineering. Of these, the transistor is one of the earliest and best documented cases (Nelson, 1962; and Dosi, 1984).

Landes's (1969) reading of economic history leads him to conclude that Western science is inherently applied in its objectives, as suggested by the recurring story of Faust. He suggests that: "... it was precisely the applicability of scientific knowledge to the environment that was the test of its validity" (1969:25). On the basis of a broad canvas of cases, Mansfield (1968a) and Mokyr (1990:167-170) conclude that until the middle of the 1800s, the relationship between science and technology was loose. Mansfield claims (1968a:44) that during earlier periods "on balance, science was far more indebted to technology than technology was to science." But with the growth of commercial laboratories at the end of the 1800s, the relationship became closer, with science more frequently leading the way. This account, by the way, would vindicate Adam Smith, since it says that at the time he wrote pure science had not yet become an important contributor to technological advance.

A second sort of evidence that can be brought to bear is survey evidence. Nelson's (1986) survey of business research managers found that many of the managers believed that science was relevant to "technical change in their lines of business" (187). Computer science, metallurgy, material science and chemistry were viewed as relevant by managers in the widest range of industries, while biology was viewed as particularly relevant (albeit by a narrower range of industries).

Mansfield (1991; 1992) has surveyed 76 large firms in seven manufacturing industries to see how many of their product and process innovations could not have been made without academic science research performed in the fifteen years before the innovation. For products, he finds that, on average, for the seven industries, 11 percent of the new products could not have been developed without recent academic research. The variation between industries is substantial, ranging from a low of 1 percent in the oil industry to a high of 27 percent in the drug industry.

A third sort of study attempts to use systematic statistical methods to

relate the growth of science to particular measures of technology, such as patents. Adam B. Jaffe uses this approach in his paper on "The Real Effects of Academic Research" (1989). Jaffe uses pooled cross-section data, aggregated by state, to estimate a production function for patents. As inputs he includes the dollars spent, by state, on industrial R&D and the dollars spent, by state, on university R&D. Using several different estimation methods, Jaffe almost always finds a statistically significant effect of university R&D on the number of patents. Additional evidence for Jaffe's spillover effect has been provided by Acs, Audretsch and Feldman (1992).

Universities are increasingly attempting to appropriate some of the "spillover" effects for themselves by encouraging professors to pursue patentable research, and to obtain patents, owned by the university, when the research is successful. Parker and Zilberman (1993) document the growth, level and possible pitfalls of increased university patenting. Most notable among the pitfalls is the conflict between the secretiveness of producing patentable research and the scientific norm of openness that has been emphasized by Merton (1973) and others.³ The main concern is that the increased incentives for scientists to produce patentable research will reduce scientists' incentives to discover and communicate basic science, thus reducing the positive externalities from science as a public good, and muddying the university's claim for government or charitable support.

Jaffe, Trajtenberg and Henderson (1993) have looked at whether new patents disproportionately cite earlier patents filed by firms or universities that are geographically close. They find a strong effect of geographical proximity, especially at the fairly small level of the Standard Metropolitan Statistical Area (SMSA). They infer that the geographic localization of spillover from technology to technology also applies to the spillovers from academic science to technology.

Link (1982; see also Link and Long 1981) looked at the determinants of the level of a firm's investment in basic research. He found that the level of basic research was strongly, and positively, related to the firm's size-adjusted profitability and extent of product diversification. He also found a weaker, and negative, relationship between the firm's level of basic research and the concentration in the firm's industry and the size-adjusted amount of federal R&D funds received by the firm.

Lichtenberg and Siegel (1991) examined confidential company-level data from the Census for the years 1972-1985 and found that companies receive positive returns from the company-level investment in R&D. Especially germane to the importance of science in advancing technology, they also find that a firm receives a productivity "premium" for investments in pure research. Consistent with earlier studies, they find that the returns to R&D are higher for larger firms (presumably because larger firms are more likely to be able to internalize the otherwise external benefits of pure research).

The studies that I have just briefly surveyed have focused on the relationship between science and technology. The assumption of these studies is that science influences technology and technology influences economic productivity and growth. I turn next to studies that look more directly at the relation between science and economic growth.

3. The Impact of Science on Economic Productivity and Growth

The causes of the industrial revolution are much debated. The growth in science in Western Europe preceded only a short time the growth in economic output. Some would view this as a coincidence or as an example of some third cause having both science and economic growth as effects. But Rosenberg and Birdzell (1990), using mainly the case study method, argue that science was the main cause of the growth. Other studies have looked at the impact of science on some measure of economic output or growth (on the assumption that any relationship will also be evidence of an unmeasured technology link between science and output). Griliches (1986) augmented a firm-level NSF data set to examine the effect of R&D in general and "basic research" in particular, on the firm's "value-added." He found that the coefficient on basic research was large, sometimes statistically significant, and growing over the period of his study.⁴

Perhaps the most ambitious recent contribution to this literature is Adams's article in *The Journal of Political Economy* (1990). Adams estimates an industry-level production function that includes as inputs the stock of scientific knowledge in each of nine fields of science, with each stock weighted by the number of scientists in the field who are employed by the industry. The estimation technique also allows estimation of the spillover of knowledge from one sector to another. The spillovers may occur both between various industries and between each industry and nine nonindustry sectors (such as governments and universities) that employ scientists. Adams finds that academic science is a major contributor to productivity growth and that there is approximately a twenty-year lag between the publication of scientific research and the exploitation of that research by industry.

The Adams study may be the most ambitious currently available. As such it may provide a clue as to why empirical research on these issues is not more common. The first two methods of empirical research, case studies and surveys, are viewed as methodologically suspect within the economics profession. On the other hand, the problem with applying systematic statistical methods to these issues is that, if Adams is right that there is a twenty-year lag, then we need a long time series of measures of pure science. In the past such time series have been very hard to come by. I would conjecture that the future for this line of research is bright as we accumulate citation and publication counts for longer periods of time.

A complementary line of research, that is more theoretical, is associated with some of the recent research on growth theory by Romer and Rivera-Batiz (Romer, 1990; Rivera-Batiz and Romer, 1991a; 1991b). In these models, economic growth is determined mainly by technological advance, which is itself determined by the level of research. (As a result of the abstractness of the models, basic research, which we might associate with "science" is not distinguished from applied research.) Because of spillover effects, and increasing returns to scale in research activity, the models imply that research should be subsidized to achieve the optimal level, and that free trade will result in higher levels of research. The higher levels of research result in higher world-wide economic growth.

Prospects for the Economics of Science

1. Economics as a Complement to Other Science Studies Disciplines

Among the social sciences, sociologists have been most active in studying science. Many observers, both from within and from outside, have perceived a crisis in the sociology profession. Coughlin, in an article on sociology in *The Chronicle of Higher Education* (1992), contends that sociologists are facing "... nagging questions about what has become of the discipline and where it is heading" (A6). The nature of the crisis would be described in many ways, but a clear symptom of it is the continued and growing diversity of perspectives from which sociology is pursued. Sociologist Richard F. Hamilton has noted that: "The field has, in a way, fallen apart into a bunch of little segments that are independent, or semi-autonomous" (as quoted in Coughlin, A8). Not only is there diversity, but there is perceived to be too little fruitful dialogue between the various points of view, and little hope of converging toward a consensus on what is fruitful and sound. Among the pessimistic, this lack of hope sometimes leads to doubts about the discipline's progress, and perhaps even survival. Steve Fuller, for instance, notes "... an unprecedented amount of soul-searching about whether 'sociology' is any longer a recognizable discipline" (1994:1).

In a symposium on the status and future of the sociology discipline, sociologist Howard Schuman writes almost wistfully about economics:

Talking with economists, one is struck by their confidence in dealing with any issue that they think can be monetized, with their common technical language, and with their sense of their discipline as concerned with problems widely recognized as central to the public good. (1994:30)

One view, by no means the only one, is that the best hope for progress in sociology is for the discipline to adopt methods of the analysis of rationality, some of which may be adapted from economics.

An advocate of this view is James Coleman, frequently mentioned as one of the nation's leading sociologists. Coleman has founded a journal called *Rationality and Society* that has economists on its editorial board and among its contributors. One of the "forthcoming" articles promised in the initial mailings for subscriptions to the journal was co-authored by Merton and his son Robert C. Merton, a distinguished finance professor at Harvard. The article, which has not yet appeared, is entitled "Unanticipated Consequences of the Reward System in Science: A Model of the Sequencing of Problem-Choices." Merton graciously sent me a draft which contains early versions of the first two sections.⁵ In the present context, what is significant about the article is that Merton found it fruitful to apply a thoroughly economic model, in form and assumptions, to problems that are traditionally discussed in the "sociology of science."⁶

In a letter recruiting members for a new Rational Choice section of the American Sociological Association (ASA), Coleman suggested that "... sociology is at one time the social science discipline containing most resistance to rational choice and the discipline which can give it the broadest reach." He urged nonmembers of the ASA to join in order to gain "... a voice in a discipline that is on the verge of change." It may also be significant that Coleman's sociology department at Chicago has gone so far as to offer departmental membership to Nobel prize-winning economist Gary Becker.

Many other sociologists of science either have made use of the concepts and models of economics, or at least have seemed open to what economists have to offer. Many of those who have seemed most interested in the possible contributions of economists have been those, such as Lowell Hargens and Harriet Zuckerman, who may be considered to be members of the "Merton/Price" school of sociology of science (after the path-breaking scholars Robert K. Merton and Derek de Solla Price).⁷ Merton/Price sociologists are more likely to share with most economists the view that science is a "different," and somehow better form of knowledge, that can be understood, at least in part, by systematic (often statistical) empirical investigations.

The openness of Merton/Price sociologists to economics has occasionally been reciprocated. Stigler (1982a; 1982c; 1983, 535), Patinkin (1983), and, more recently, Dasgupta and David (1994) have found in the work of the Merton/Price school, hypotheses and empirical evidence that might be fruitfully used in the economic study of science.

But openness to economic issues in the study of science is not limited to those in the Merton/Price school. Many of those in the currently ascendent "constructivist" school of sociology of science have also made use of economic concepts or theories. These would include Latour and Woolgar (1986), Knorr-Cetina (1991), Pickering (1984), and Fuller (1988, 1991). Some of the relevant passages in the first three references are noted and discussed in Hands (1993).

Although the constructivist school is ascendent, many of those who

accept the school's position, still find a need for what Cozzens and Gieryn (1990) call a "post-relativist" (12) attitude that explores theoretical and empirical "roads not taken" (13). They do not explicitly mention economic analysis and concepts as one of the potentially fruitful roads. But consideration of the economic road certainly seems consistent with their call for open-minded exploration of new and alternative approaches. Although none of the contributors to the collection is an economist, several of the essays treat issues where economic analysis might be fruitfully applied (e.g., the essays by Restivo; Chubin; Cozzens; Turner; and Shrum and Morris).

Stephen Turner, who has recently co-authored a major appraisal of the history and prospects of the sociology discipline (1990) downplays the degree of competitiveness of sociology with the other social sciences, including economics. He suggests (1994:57) that relations between the disciplines have often been "friendly" and that there has been fruitful "two-way traffic," citing, as examples, the two economics Nobel Laureates (Simon and Becker), who were members of the American Sociological Association.

Sociologists of science have not been alone in their openness to economic analysis. Simonton (1988a and 1988b) has taken economics very seriously in his careful and stimulating psychological analysis of creativity in science. Economist Rubenson and psychologist Runco (1992) have collaborated to convince an audience of psychologists that economics has insights into the behavior of scientists.

C.S. Peirce is generally considered to have been the first philosopher of science to explore "the economy of research" (1979). More recently, Thomas Nickles has argued that "we need economics . . ." because the " . . . economy of research is an indispensable component of methodology" (1985:182). Other philosophers of science who have found economic concepts useful to their analysis include Toulmin (1972:267–268), Rescher (1976; 1978), Radnitzky (1987), Goldman (1983), Kitcher (1990), Fuller (1988; 1991), and Goldman and Shaked (1992). The concepts and arguments found in Hull's sociobiological account of science (1978 and 1988) are, in many ways, homologous to concepts and arguments within economics. Economists have also applied economic tools of analysis to illuminate philosophical issues (e.g., Diamond, 1988b and 1988c).

One of the most important early contributions to the economics of science was made by Michael Polanyi, a polymath who has been labeled a chemist, philosopher, and sociologist. In his "The Republic of Science" (1962), Polanyi argues for important similarities between the efficient market for goods and the efficient community of scientists. When functioning properly, both are governed by an "invisible hand" that coordinates individual actions to produce a socially optimal result. In each case, central planning fails because it restricts the ability (and incentives) of the individual to act on the basis of her unique and specialized knowledge. (For the case of the market, this argument has been made most

strongly [and famously] by Hayek in his "Use of Knowledge in Society" [1945].)

Sociologists, historians, psychologists, anthropologists and constructivists have gathered an impressive, and frequently empirical, array of systematic and anecdotal knowledge of how science changes and how scientists work. Often their priority is to record accurately (and fully) the rich detail of real-world science. These studies are sometimes incomplete, however, in their lack of a general theoretical framework. The strength of economics among the social sciences is precisely the presence of a unifying, general framework in the form of the maximization-under-constraints model. The theoretical generalizations of economists are aimed at explaining "a lot with a little." On the other hand, sometimes it seems as though the aberrant fact is of interest to the economist only to the extent that she can find a way of making it fit the theory. Economists sometimes fall victim to what Schumpeter called "the Ricardian Vice," which consists of applying "... an excellent theory that can never be refuted and lacks nothing save sense" (Schumpeter 1954:473).

The complementarity of the work of economists with that of other social scientists (and humanists) in science studies has potential because each side has something important to contribute. The noneconomists are used to being open to all of the kaleidoscopic detail of the real world, while the economists have models and a habit of mind that help them to pull the detail together into a useful "big picture."

2. Implications for Improving the Reward Structure of Science

Philosopher of science Gonzalo Munevar presented a paper a few years ago at an interdisciplinary conference on science policy. At one session a distinguished official from the National Science Foundation rose and said that the discussions were ignoring the fundamental problem that he faced—how to efficiently allocate scarce resources to research projects.

The economics of science brings the powerful tools of economics to bear on the question of how most efficiently to allocate resources so as to provide the most scientific advance per dollar spent. Integrating and deepening our understanding of the reward structure of science is crucial for economic development in the nation and in the world. Citizens of the former Eastern block and of traditional third world countries need to know the impact of science on economic growth in order to travel the fast track toward Western economic productivity. The efficiency of universities at extending and communicating knowledge is an especially important topic at a time when the economic competitiveness of the U.S. is being widely questioned. Citizens in the United States need to know the impact of science on economic growth as part of their effort to understand if the alleged decline in the rate of growth is real and, if so, how to reverse it.

Nelson and Wright (1992) document decline, while Griliches (1994)

argues that some of the alleged decline may be due to measurement problems in the high-growth sectors. Bezis, Krugman and Tsiddon present (1993) a theory that eventual decline is inevitable. They distinguish normal, incremental technological advance from the rarer, major technological advance. Since new technology is less profitable at first, advanced countries will have a higher opportunity cost of adopting it. By the time the technology becomes more profitable, other, previously less advanced nations, will have a head-start in its adoption and development. Presumably, major technological advance is often a result of science, although the authors are silent on this issue. Although the scenario painted by Bezis and co-authors does not result in continued United States technological dominance, it does result in continued technological advance in the world—an advance that increases world-wide standards of living. So, even under their scenario, applying economic reasoning to more efficiently advance science remains an important objective.

One general type of contribution that economists have already made to science policy discussions has been to argue for the appropriate use of monetary and psychic incentives. For example, to solve the problem of how a scientist appropriates the benefits of new ideas, Hanson (1995) has cleverly and persuasively argued for “betting” on ideas as a partial answer (an idea briefly and vaguely suggested and rejected by Arrow [1962:612]). In another example, McCloskey, has argued (1985) that journal editors should place more emphasis on clarity of writing when they evaluate articles for possible acceptance. Simon (1991) suggests that a “Pareto optimal” reform of academic rules would permit senior research faculty to buy off teaching time in exchange for a reduction of salary.

Mason and his co-authors (1992) identify considerable discontent among economists about the length of time it takes journals to evaluate and publish research. Eighty-four percent of the respondents in their survey favored a system where reviewers were paid for promptness of response, although 21.1 percent feared that the quality of the reviews would suffer in such a system. The *Journal of Political Economy* has had such a policy for several years, but I know of no reports or evidence on whether it has speeded up, or affected the quality of, the review process. *Social Epistemology* offers reviewers the nonpecuniary reward of seeing their names in print with the article reviewed if their contribution is deemed sufficiently profound.

Earlier, we noted that Gordon Tullock claimed that, as editor, he had lowered standards (lowered the price) to publish on subjects that he wanted to encourage. In the absence of pecuniary rewards for editorship, such power may be one of the compensating differentials of the job. Elsewhere (1966:142) Tullock claims that editorship is one of the most important forms of scientific administration. Since it is also one of the lowest paid, many able scientists choose other, less productive, forms of scientific administration when their own research productivity declines. Tullock argues that foundations could increase their effectiveness at ad-

vancing science if they diverted some of their funds into increasing the compensation of editors of scientific journals.

Tullock's advice appears in his *Organization of Inquiry* (1966), which also contains a rich array of speculations on the function and reform of scientific institutions. He argues, for instance, that increased emphasis on prizes would enhance the progress of science. This idea has been persuasively elaborated by Squires (1986) and others. Elsewhere (1973), Tullock suggests that older scientists are less productive because the increase in lifetime income due to an additional article is lower the older the scientist is and the more articles the scientist has already published. His suggested solution is that "universities should discriminate against assistant professors" by providing senior professors with more research resources.

Other economists advocating institutional reform include Anderson and Tollison (1986), who argue that the level of investment in the history of economic thought is suboptimal because knowledge re-discovered in the "classics" results in fewer citations (and hence less other professional advancement) than does knowledge discovered *de novo* through original research. Boyes and Happel (1989) have argued that the most efficient way to allocate faculty offices is by an auction, with the highest bidder taking the best office.

Several of the science policy issues that would seem most likely to yield to the tools of economic analysis have yet to receive much attention from economists. One such issue is the debate on whether it is more efficient to invest money for science in big science projects or in small science projects. The issue has constantly been in the science press in the last few years, especially in discussions of the Hubble telescope, the genome mapping project, and the supercollider.

Another example of a policy issue that economic analysis might provide light on, is the debate on the optimal size for an academic department. This debate has been especially active in Great Britain, where there has been a movement to consolidate departments on the grounds that, in the past, significant economies of scale have gone unexploited (see, e.g., Dickson 1989). Support for the policy might also be obtained from McKenzie's argument (1979) that the incentives to engage in rent-seeking behavior diminish with department size. Economists have considerable experience in estimating economies of scale (and hence the optimal size) for firms (e.g., Stigler 1968). A potentially fruitful direction for future research would be to apply these techniques to the question about the optimal size of departments.

Although no empirical research on returns to scale has yet been done at the department level, de Groot and co-authors (1991) have estimated multiple output variable cost functions for a sample of 147 United States research universities. They found economies of scale for teaching and for research, and also found economies of scope for the joint production of graduate students and undergraduate students.

Other potentially fruitful research questions in the economics of sci-

ence policy include: the role of government in financing science; whether science resources should be concentrated on elite scientists; and whether scientific decisions should be made by committees of peers.

Acknowledgments

Some passages in this article are derived from a paper entitled "Economic Explanations of the Behavior of Universities and Scholars" that was presented to a conference on the Althoff System in 1989 and published, in revised form, in the *Journal of Economic Studies*. An earlier version of some of the section on technology and economic growth, first appeared in "The Impact of Pure Science on Technology and Economic Growth," that was presented at the meetings of the Western Economic Association in 1991. Earlier versions of this article were presented at the joint meetings of the Society for Social Studies of Science and the European Association for the Study of Science and Technology in Göteborg, Sweden, on August 13, 1992, and to the American Economic Association meetings in Anaheim, California, on January 6, 1993. The essay and comments served as the basis for an NSF-supported conference on the Economics of Science that was held in Washington, D.C., on January 5, 1995. The essay has been supported by a grant from the University Committee on Research of the University of Nebraska at Omaha and by a grant from the National Science Foundation. I am especially grateful to Rachelle Hollander and Daniel Newlon of the NSF for their encouragement and support. John Mulvey provided research assistance.

Notes

1. The "opportunity cost" is the value of the most highly valued alternative that is foregone in order to participate in the chosen activity.
2. Whether job security increases productivity is an open question. Although often presenting a positive view of tenure to the outside world, among themselves academics are often more cynical: "The juvenile sea squirt wanders through the sea searching for a suitable rock or hunk of coral to cling to and make its home for life. For this task it has a rudimentary nervous system. When it finds its spot and takes root, it doesn't need its brain anymore so it eats it. It's rather like tenure." *Young Scientist's Network Digest* (on Internet as quoted in: John C. Dvorak, "Inside Track," *PC Magazine* 13, no. 4 [February 22, 1994], pg. 95).
3. Merton calls the norm "communism" and says of it: "Secrecy is the antithesis of this norm; full and open communication its enactment" (p. 274).
4. Levy and Terleckyj (1995) have surveyed (and added to) the literature on the effects of federal R&D on output. One puzzling result is that the impact of federal R&D expenditures seems to be zero (see the results in Levy and Terleckyj and the results they survey). Levy's *Economic Letters* (1990) solution may be too neat. From a public choice point of view it assumes an optimistic result—that public goods will in fact be provided to the point where they are optimal (i.e., where the value of marginal product is zero). An alternative explanation is that there is a crowding out effect, and that private funders are better able to allocate the R&D funds. Hence, when (relatively inefficient) federal R&D increases, then (relatively efficient) private R&D decreases.
5. Although three of the intended five sections are missing, the typescript is substantial, running to 39 pages. According to Merton, the draft dates from about 1982.
6. Merton's recent appreciation of economic issues does not represent a radical departure from his earlier attitudes. Although his doctoral dissertation (1938; see also 1939) is mainly remembered for its treatment of the effect of Puritanism on the rise of English science, he points out (1970:xii) that a higher percent of the pages of the

dissertation were devoted to "economic and military influences" on science, than to the Puritanism hypothesis. Looking back on his dissertation thirty years later, in 1969, he found himself "... more partial to the section dealing with economic and military influences ..." (1969:xiii) than to the section on Puritanism.

7. I am not aware that the openness to economics of Hargens has been stated in print. Some of Hargens's recent work has consisted of systematic empirical studies of the journal refereeing process (1988; 1990a; 1990b). Zuckerman is perhaps best known for her definitive study of recipients of the Nobel Prize (1977). Price's bold and stimulating work (1963; 1975) is less well known to the broader scholarly community than is Merton's.

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