ECONOMICS OF SCIENCE

Abstract

The importance of the economics of science is substantially due to the importance of science as a driver of technology and technology as a driver of productivity and growth. Believing that science matters, economists have attempted to understand the behavior of scientists and the operation of scientific institutions. One goal is to see how far science can be understood as a market, and how far the market for science and scientists can be understood as efficient. When inefficiency is found, a related goal is to propose changes in resource levels or incentives, to increase the speed of scientific advance.

Introduction

The economics of science aims to understand the impact of science on the advance of technology, to explain the behavior of scientists, and to understand the efficiency or inefficiency of scientific institutions.

The first economics of science may have been Adam Smith's idealistic, but sadly untrue, discussion in the *Theory of Moral Sentiments* (1976b 124) of Newton having been motivated purely by curiosity, rather than a desire to achieve fame and fortune. If Smith's account was the beginning of a positive economics of science, then Charles Babbage's argument (1830) for the inefficiency, and reform, of British scientific institutions, may count as one of the earliest instances of a mainly normative economics of science. Also usually mentioned as an early founder of the normative economics of science is C.S. Peirce who advocated of the application of economic tools of analysis to decide which scientific projects to adopt (see: Wible, 1998).

The "modern" economics of science grew out of three main issues. The first issue addressed how the advance of science contributed to the advance of technology, and hence productivity and growth. The second issue, which overlaps with concerns in history and philosophy of science, addressed how science advances. A third issue is the empirical data collection and econometric analysis of the supply, demand, compensation and productivity of scientists.

Diamond (1996) and Stephan (1996) both provide broad surveys of the economics of science, with Diamond perhaps devoting somewhat more space to interdisciplinary and policy issues. Special attention has been given to the contributions to the economics of science of three of the field's founders, Mansfield, Griliches and Stigler (Diamond, 2003, 2004, 2005). Several of the more important papers in the economics of science through 1998 are included in the two volume collection edited by Stephan and Audretsch (2000).

In what follows, we will first consider the literature that most makes the case for why the economics of science should be a priority for our attention: the literature on science as a contributor to economic productivity and growth. We will proceed to briefly summarize some economic discussion of some of the "deep" issues in the economics of science, that sometimes overlap with issues in philosophy of science, such as what are the objectives of scientists (truth, fame, fortune?), and what constraints are most relevant in their choices about which projects to pursue and which theories to adopt. Next we will look at some of the studies that have attempted to model and measure a variety of aspects of the market for science and scientists. Finally, we will give examples of some of the studies in normative economics of science that argue for changes in funding, or for institutional reform.

Impact of science on technology, productivity, and growth

The importance of technology as a driver of economic growth and well-being, has been appreciated since Adam Smith's *Wealth of Nations* (reprinted 1976a), and emphasized most notably by Schumpeter (1942). If technology is the main driver of economic growth, then the next question is what is the main driver of technology?

Rosenberg (1982) made the credible point that most economists, for most of the history of the profession, had viewed the process by which new technologies are developed and adopted as a "black box." In the years since, partly lead by Rosenberg himself, economists have increasingly attempted to say more about what goes on inside the box, especially concerning the role of science in advancing technology.

Several economic historians have examined the role of science in the advance of technology and economic growth over the broad sweep of history. Mokyr (1990, 2002), Rosenberg (Mowery and Rosenberg, 1989; Rosenberg and Birdzell, 1986), and Landes (1998), agree in the broad conclusion that the advance of science is a necessary, but not sufficient, condition for substantial and rapid advance in technology and economic growth.

Nelson (1959) cataloged many examples of how science had contributed to the advance of technology. More recently, in work with Mowery (1989 11-14) Rosenberg has claimed that the distinction between science and technology is often hard to make, providing several examples of how scientific advances have resulted from the pursuit of ‘practical’ results. Although most economists adopt the view of Nelson that mainly new science enables the advance of new technology, it is not hard to find examples where the advance of science was enabled by new instruments provided by advanced technology (Ackerman, 1985).

Mokyr (1990), in his broad economic history of the advance of technology over the ages, generally finds the advance to be slow and fitful until the industrial revolution. Up until the mid-1800s, the relationship between science and technology, was loose (Mansfield, 1968; Mokyr, 1990 167-170). Those who advanced science and technology shared an attitude of optimism about the prospects to understand and control nature (Landes, 1969). But beginning in the mid 19th century, and especially with the development of commercial labs toward the end of the 19th century, the relationship between science and technology became closer, with advances in science more often and more clearly being a necessary condition for technological advances.

Beginning with Nelson’s taxonomic paper (1959), evidence for this latter claim has been provided by economists in a variety of forms. Griliches’s main contribution, in a pair of papers (1957, 1959), was to measure the return to scientific research on hybrid corn and to measure and explain varying rates of adoption (see: Diamond, 2004). Surveys of research managers by Nelson (1986) and by Mansfield (1991, 1992) provided evidence that science is sometimes important for technical change, although the importance varies considerably by the industry and by the subfield of science.

The development by Romer (1986, 1990) and others of the “new growth theory” increased attention to science as a driver of technology, because such models include a more prominent role for knowledge (“recipes”) than earlier models. Such models implied the possibility of increasing returns to investments in knowledge, if various spillover effects were large enough. Partly stimulated by such models, and partly by independent research by economists such as Griliches (see: Diamond, 2004), considerable empirical work has been done measuring the spillover effects of scientific knowledge, e.g., Jaffe (1989), Adams (1990) and Jaffe, Trajtenberg, and Henderson (1993).

Deep understanding of science

Stigler and Becker (1977) have argued that everyone has the same utility function. This contrasts with Adam Smith’s suggestion in his *Theory of Moral Sentiments* (reprinted 1976b 124) that scientists, or at least the best scientists, were more purely motivated by curiosity. Gordon Tullock (1966 34-36), and Kenneth Arrow (2004), suggest that scientists have a range of motives, from those who fit the Smith ideal, to those motivated by fame and fortune (Levy, 1988). However, in their research on the behavior of scientists, economists usually follow Stigler and Becker, in studying scientists as though they mainly valued income, and prestige. Scientists valuing both income and prestige might explain Stern’s finding (2004) that industrial scientists are willing to give up some income in exchange for greater ability to publish their results.

The process of theory choice among scientists has been explained using economic tools (Diamond, 1988b; Hull, 1988; Goldman and Shaked, 1991). The explanations have been criticized by Hands (1997). Brock and Durlauf (1999) build on the work of Kitcher (1993) in their construction of a dynamic model of scientists' adoption of new theories. A key assumption of their set-up is that one source of a scientist's utility is the "conformity" of a scientist's views with the views of other scientists. The model allows the possibility that science is progressive, even if social considerations have some weight in scientists' utility functions, partially answering many of those in the social studies of science field who believe that the admission of social considerations undermines the special cognitive status of science.

Stigler authored several papers that presented evidence on hypotheses about the determinants of successful science. He provided evidence and arguments on questions such as: does a scientist's biography help us understand the scientist's contributions (1976), and how efficiently is error weeded out in science (1978)? Stigler generally framed his studies as seeking just to understand, not to reform, although the results sometimes stimulated thoughts of reform in others. A fuller account of Stigler's contributions to the economics of science appears in Diamond (2005).

The market for science and scientists

Michael Polanyi (1962) optimistically portrayed science as an efficient marketplace of ideas. Much research in the economics of science in the last few decades, shares Polanyi's research program of explaining the behavior of scientists and scientific institutions on the basis of rational optimization within an efficient marketplace.

Many of studies in the economics of science fall within the domain of labor economics, and assume that scientists are rational maximizers of income, and sometimes of prestige. Within labor economics, a significant theoretical and empirical literature has developed that examines the economics of higher education. Since many scientists, and especially most scientists who are credited with major scientific discoveries, have been associated with universities, this literature is relevant to the economics of science, even when the examples or data, are not explicitly from science.

Some of the earliest economics of science studies collected data to analyze the supply and demand of scientists. Early examples of this genre were Blank and Stigler (1957) and Arrow and Capron (1959). Richard Freeman's "cobweb" model (1975) of the labor market for physicists had an unstable equilibrium because students' occupational choice in the model is based on a systematically biased forecast of the future demand for physicists. Siow later (1987) showed that professional labor markets are better characterized by assuming the students' forecasts are based on rational expectations.

Human capital theory and measurement have been used to estimate earnings functions for scientists, including as independent variables, measures of productivity, such as articles produced, citations received, and teaching evaluations. Many of the studies also include one or more variables intended to measure hypotheses of discrimination, such as gender and race variables (e.g., McDowell, 1982). Yet other studies include variables intended to measure what has recently been called "social capital," and has previously been identified with Robert K Merton's 1968 "Mathew Effect" (the rich get richer), or with "old-boy" networks.

One goal of many of the earnings regression studies has been to learn how much of the variation in academic salaries can be explained on the basis of variation in measures of academic productivity. Lovell's 1973 paper was one of the first to include measures of research productivity in an academic earnings regression. Early studies tended to focus on number of articles published, as the measure of research productivity. A pair of papers by Stigler and Friedland (1975, 1979) helped establish the credibility of citations as a measure of the academic productivity. One of the first to include citations in an earnings function as a measure of productivity may have been Laband (1986). Subsequent studies using the citation measure include Hamermesh, Johnson and Burton (1982), Diamond (1986b), Sauer (1988), and Kenny and Studley (1993). A review of the literature on bibliometric measures of productivity can be found in Diamond (2000).

The simplest models of compensation assume that workers are paid the value of their current productive output (their "marginal revenue product"). To account for observed anomalies with this hypothesis, especially in professional labor markets, a literature has developed supposing that there are long-term implicit labor contracts. For example, universities may provide scientists with insurance for variability of research output, by paying the scientist more than the value of output in low-output years, and less than the value of output in high-output years. If scientists are uncertain at the beginning of their careers whether they will become high or low productivity scientists, they may also demand insurance against the possibility of their being low productivity scientists (Smith Freeman, 1977) This might explain the observed greater variability in measures of scientists' productivity than the variability in scientists' salaries.

An alternative explanation is to make use of a compensating differentials argument (Frank, 1984). The assumption is that scientists receive utility from being paid more than other scientists. So the top scientist would be paid less than the value of her productivity, because she is receiving a compensating differential in the form of being at the top of the pecking order.

Implicit contract models have also been developed to try to explain important scientific labor market practices, such as academic tenure. For example, Carmichael (1988) argues that tenure is an institutional device to reduce the costs to incumbent faculty of correctly identifying promising new faculty, while Waldman (1990) claims that faculty value tenure because it serves as a signal to outside institutions of the faculty member's quality, and hence increases outside higher salary offers. Siow (1998) claims that specializing is risky, since subfields of specialization may suddenly become obsolete. So without tenure as a form of insurance, faculty would under-specialize.

Implicit contract models are often clever, and sometimes plausible. But as alternative clever, plausible models multiply that explain the same stylized facts (e.g., the existence of academic tenure), the credibility of the exercise may suffer. It may also be worth mentioning that, ceteris paribus, economists will be more popular with their peers if they create models justifying tenure, and other academic institutions, than if they create models showing tenure is inefficient.

Other mainly empirical studies have examined the mobility of academic scientists between university positions (Rees, 1993), and the mobility of industrial scientists between technical and managerial jobs (Biddle and Roberts, 1994). Another extensive, mainly empirical, literature makes use of standard theory on the optimal allocation of time over the life-cycle, to motivate analysis of scientific productivity over the life-cycle. Life-cycle investment models (e.g., Diamond, 1984) often suggest that it makes sense to invest in human capital early in the life-cycle. These models often imply concave age-productivity profiles. Empirical evidence confirms this generalization (Diamond, 1986a; Stephan and Levin, 1992), but with very different peak productivity ages for different fields of science. Age-related differences in the rate of acceptance of new theories have also been examined (Hull, Tessner and Diamond, 1979; Diamond, 1980, 1988a, 1988b; Levin, Stephan, and Walker, 1995).

Efficiency and reform of scientific institutions

In the previous section, we discussed research that for the most part argues that the institutions for rewarding and allocating resources to scientists, can be explained as efficient aspects of a well-functioning market of ideas. Bartley (1990) has argued that while a “marketplace of ideas” is an appropriate goal and standard, it is an inaccurate description of the current institutions of science. In one of his headings (p. ix), he claims that our current institutions for academic science are "where consumers do not buy, producers do not sell, and owners do not control."

In an early study (1965), Stigler provided evidence that when economics transitioned from a science done by amateurs to one done by professionals, the discipline became much more theoretical and mathematical, and much less applied and policy oriented. Stigler's friend and colleague, Milton Friedman, argued (1981) that the funding of the National Science Foundation had had a similar effect, and argued further that this effect had slowed the advance of knowledge. The debate was renewed 13 years later (Friedman, 1994; Griliches, 1994). Edward Lazear (1997), has developed a model implying more modest advice for the NSF: the foundation should give fewer, but larger awards.

Other economists have studied the funding of science. Arrow (1962), Johnson (1972), and others have argued that science is a public good, that will be underprovided by the private sector. Dasgupta and David (1994) accept the public goods argument of the "old" economics of science of Arrow (1962), but want to add to it findings of some sociologists on the secrecy that sometimes result from the competition for priority, in order to develop a "new" economics of science. Their "new" economics of science argues for greater government funding of science, accompanied with increased incentives for scientists to share their findings sooner with other scientists, and with those seeking to apply the findings to new technologies.

Romer (2001) argues that if roughly half a million more scientists and engineers were supplied, and appropriately deployed, the U.S. economy could sustain a half a percent greater rate of growth in GDP. He suggests that major changes would be required in academic institutions, and government policies to achieve this goal, but he believes the resulting implications for the economy of success would be "staggering" (p. 227).

Kealey (1996) and Martino (1992) explicitly disputed, the traditional "public goods" argument for government support of science, on the grounds that private industry often has both the incentives and the ability to do substantial high quality scientific research. Hanson (1995) supports an alternative private form of science funding, when he argues that greater scientific innovation would occur if more of the funding for science came from a betting market, where those who predict accurately the outcome of scientific questions, receive more resources.

Although not opposing the science-as-public-good theory as strongly as Kealey and Martino, Rosenberg (1990) has emphasized the incentives that private firms have to invest in science. He examined firms that hire PhD scientists, and that allowed the scientists considerable leeway in the allocation of their time, and in the publication of their results. He argued that this was in the firm's interest because of the value of such scientists as a resource in keeping up with, and explaining scientific advances relevant to the firm's product development efforts. Besides Rosenberg's paper, there is a considerable literature measuring returns to firm investment in Research and Development. Some of these studies might be considered part of the economics of science to the extent that they study 'basic research,' a label that is sometimes used interchangeably with 'science.' Examples of this literature are surveyed in Audretsch et al (2002).

Several scholars have attempted to measure the extent to which public expenditures on science add to the total funding of science, or the extent to which they simply crowd-out private funding on science. Diamond (1999), for example, using highly aggregated time-series measures of government, and industry investment in science, found no evidence of crowding-out. An evaluative survey of this literature has been published by David, Hall and Tool (2000).

Some economists have explained the behavior of some scientists and the structure of some scientific institutions, as due to rent-seeking behavior. Rent-seeking (Tullock 1967) is a zero sum process in which an agent invests resources to obtain an uncompensated transfer from another agent. In one example, McKenzie (1979) suggested that there is a fixed fund for department salaries, and that department members can increase their share of the fund, either by being more productive themselves, or by sabotaging the productivity of others, perhaps, for example, by the calling of unnecessary meetings. Other rent-seeking accounts of academic institutions have been provided by Brennan and Tollison (1980), and Grubel and Boland (1986).

We mentioned in the previous section, economic models of academic tenure that attempt to explain the institution as an efficient response to features of the academic labor market. Others (e.g., Rogge and Goodrich, 1973) have followed Alchian (1959) in presenting a basically rent-seeking account of tenure as an inefficient institution that exists because it is in the interests of a sufficiently powerful special interest group.

In an account highly complementary to the rent-seeking hypothesis, Goolsbee (1998) has looked at data on federal funding of science and found that it largely results in windfalls for scientists. Goolsbee's results call into question the extent to which federal funding actually increases the amount of science produced.

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Some Possible Suggested Cross-References to Other Articles (these are from the edition currently being written):

Chemical Industry

Grilliches, Zvi

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