Chapter 5

THE ECONOMICS OF SCIENCE

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Contents

Abstract 218
Keywords 218
1. Introduction 219
2. The public nature of knowledge and the reward structure of science 219
    2.1. The importance of priority 220
    2.2. Financial remuneration and the satisfaction derived from solving the puzzle 222
3. How knowledge is produced 225
    3.1. Time and cognitive inputs 225
    3.2. Research resources 228
    3.3. Serendipity 231
4. Choice of scientific contests and character of research 232
    4.1. Choice of contests 232
    4.2. The character of research 234
    4.3. Production of dual knowledge 234
5. Outcomes 235
    5.1. Is science a young person's game? 237
        5.1.1. Age and publishing 239
        5.1.2. Age and exceptional contributions 239
        5.1.3. Age and patenting 240
    5.2. The presence of cohort effects 240
    5.3. Gender 241
    5.4. Inequality 242
6. Efficiency considerations and funding regimes 243
    6.1. Efficient nature of the reward system 243
    6.2. Funding regimes 244

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    6.3. Are there too many contestants in certain contests? 248
    6.4. The incentive to share knowledge in a timely fashion 249
7. Scientists in industry 251
8. Scientific labor markets 253
    8.1. A description of scientific labor markets 254
        8.1.1. Where they train 254
        8.1.2. Where they work 255
        8.1.3. International mobility patterns 255
    8.2. Studies of supply and demand for new entrants to science 256
    8.3. Forecasting scientific labor markets 258
9. Science, productivity, and the new growth economics 259
10. Conclusion 264
Acknowledgments 266
References 266

Abstract

This chapter examines the contributions that economists have made to the study of science and the types of contributions the profession is positioned to make in the future. Special emphasis is placed on the public nature of knowledge and characteristics of the reward structure that encourage the production and sharing of knowledge. The role that cognitive and noncognitive resources play in discovery is discussed as well as the costs of resources used in research. Different models for the funding of research are presented. The chapter also discusses scientific labor markets and the extreme difficulty encountered in forecasting the demand for and supply of scientists. The chapter closes with a discussion of the relationship of scientific research to economic growth and suggestions for future research.

Keywords

economics of science, knowledge production, patenting, priority, publishing, research

JEL classification: O31, O34, O43, Z13

1. Introduction

Science commands the attention of economists for at least three reasons. First, science is a source of growth. The lags between research and growth may be long, but the economic impact of science is indisputable. The evidence is quite tangible. Advances in information technology, for example, have contributed significantly to growth in the service sector in recent years. Medical research has done a considerable amount to extend work and life expectancy, first with the introduction of antibiotics and more recently with the introduction of new classes of drugs and medical devices.

Second, scientific research has properties of a public good. It is not depleted when shared and once it is made public others cannot easily be excluded from it. As economists, we have special concerns regarding the failure of economies to produce public goods efficiently. A major reason for studying science is that a reward system has evolved in science that goes a long way toward solving the appropriability problem associated with the production of a public good.

Third, the public nature of research and the spillovers inherent in such a system are fundamental to the concept of endogenous growth theory developed by Paul Romer and others that is now a cornerstone of growth theory in economics.

This chapter attempts to bring together lines of inquiry concerning science and to incorporate into the discussion salient facts about science and scientists that have been observed by colleagues working in other disciplines. We begin by discussing the public nature of knowledge and characteristics of the reward structure. Special attention is given to the recognition that priority of discovery is a form of property rights. We then explore how science is produced, emphasizing not only labor inputs but also the important role that materials and equipment play in scientific discovery and the ways in which discovery is affected by advances in technology. This is followed by a discussion of scientific contests and the character of research. We next discuss outcomes. Included is a discussion of the relationship of gender to productivity and the inequality observed among both publishing and patenting outcomes.

The second half of this essay begins with a discussion of efficiency considerations and funding regimes. Included is a discussion of efficiency considerations related to the reward system in science and whether there are too many contestants in certain scientific contests. This leads to a discussion of how the incentives to disclose information in a timely fashion relate to the type of property right sought. We see that it is not uncommon for scientists in industry to publish, nor for scientists working in the nonprofit sector to "privatize" information. We continue by discussing scientists working in industry, and more generally discuss the market for scientists and engineers. We close with a discussion of empirical studies relating scientific research to economic growth and endogenous growth theory.

2. The public nature of knowledge and the reward structure of science

In his 1962 article concerning the economics of information, Kenneth Arrow discussed properties of knowledge that make it a public good. Others (e.g., Dasgupta and David, 1987, 1994; Johnson, 1972; Nelson, 1959) have also commented on the public nature of knowledge: it is not depleted when shared, and once it is made public others cannot easily be excluded from its use.[1] Moreover, the incremental cost of an additional user is virtually zero[2] and, unlike the case with other public goods, not only is the stock of knowledge not diminished by extensive use, it is often enlarged. This means that the transmission of knowledge is a positive sum game (Foray, 2004, p. 93).[3]

Economists were not the first to note the public nature of knowledge. More than 190 years ago, Jefferson (1967 edition, p. 433, section 4045) wrote:

> *"If nature has made any one thing less susceptible than all others of exclusive property, it is the action of the thinking power called an idea, which an individual may exclusively possess as long as he keeps it to himself; but the moment it is divulged, it forces itself into the possession of every one, and the receiver cannot dispossess himself of it. Its peculiar character, too, is that no one possesses the less, because every other possesses the whole of it. He who receives an idea from me, receives instruction himself without lessening mine; as he who lights his taper at mine, receives light without darkening mine."*[4]

A cornerstone of economic theory is that competitive markets provide poor incentives for the production of a public good. The nonexcludable nature of public goods invites free-riders and consequently makes it difficult for providers to capture the economic returns. Thus, incentives for provision are not present. Moreover, the nonrivalrous nature of public goods means that if and when public goods are produced, the market will fail to provide them efficiently where marginal cost equals marginal revenue since the marginal cost of an additional user is zero. Such observations regarding the provision of public goods, however, relate to incentives that are market based. An important contribution of the sociologists of science and the economists who have extended their work is the demonstration that a nonmarket reward system has evolved in science that provides incentives for scientists to produce and share their knowledge, thus behaving in socially desirable ways. In the sections that follow, we analyze the components of that reward system as well as the behavior it encourages.

2.1. The importance of priority

As economists, we owe a substantial debt to Robert Merton for establishing the importance of priority in scientific discovery. In a series of articles and essays begun in the late 1950s, Merton (1957, 1961, 1968, 1969) argues convincingly that the goal of scientists is to establish *priority of discovery* by being first to

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[1] Research findings only become a public good when they are codified in a manner that others can understand. The distinction, therefore, is often drawn between knowledge, which is the product of research, and information, which is the codification of knowledge (Dasgupta and David, 1994, p. 493).

[2] In reality, the marginal cost of use is greater than zero because users must incur the opportunity cost of time as well as the direct cost of access to journals or attendance at meetings. Information, of course, is only of use to those who possess the requisite intellectual framework and know the "code." Michel Callon (1994) argues that the public nature of science is greatly overstated. Tacit knowledge (discussion to follow), which by definition cannot be codified, is more costly to learn than knowledge that is codified.

[3] It is the user value of knowledge that does not diminish with use. The market value of knowledge can fall with dissemination.

[4] Jefferson also noted that ideas are "like fire expansible over all space, without lessening their density in any point." (quoted in David, 1993, p. 226). David stresses the infinite expansibility of knowledge rather than the nonrival characteristics of knowledge.

communicate an advance in knowledge and that the rewards to priority are *the recognition awarded by the scientific community for being first*. Merton further argues that the interest in priority and the intellectual property rights awarded to the scientist who is first are not a new phenomenon but have been an overriding characteristic of science for at least three centuries.

The recognition awarded priority has varied forms, depending upon the importance the scientific community attaches to the discovery. Heading the list is eponymy, the practice of attaching the name of the scientist to the discovery. Haley's comet, Planck's constant, Hodgkin's disease, the Copernican system are all examples.[5] Recognition also comes in the form of prizes. Of these, the Nobel is the best known, carrying the most prestige and the largest purse (approximately $1.4 million in 2009), but hundreds of others exist, a handful of which have purses in excess of $500,000, such as the Lemelson-MIT Prize with a $500,000 (US) purse, the Shaw Prize ($1 million US) and the Spinoza Prize (1.5 million euros).[6] The number of prizes awarded has grown in recent years. Zuckerman (1992) estimates that approximately 3000 prizes in the sciences were available in North America alone in the early 1990s, five times the number awarded 20 years earlier. Although no systematic study of prizes has been done since, anecdotal evidence suggests that the number continues to grow. *Science*, the highly cited journal of the American Association for the Advancement of Science, regularly features recent recipients of prizes, many of which are awarded by companies and recently established foundations and often have purses in excess of $250,000.[7]

Publication is a lesser form of recognition, but a necessary step in establishing priority. While eponymy or the receipt of a prestigious prize is perceived by most to be beyond their reach, the reward of publication is within the reach of most scientists. A common way to measure the importance of a scientist's contribution is to count the number of citations to an article or the number of citations to the entire body of work of an investigator. While this used to be a laborious process, changes in technology, as well as the incentives to create new products, such as Google Scholar, have meant that researchers, and those who evaluate them, can quickly (and sometimes incorrectly) count citations to their work as well as where they stand relative to their peers. Thompson Scientific, for example, markets a product that ranks scientists, within a field, in terms of citations.[8]

It is important to stress that priority is established by being first. The behavior such an incentive structure elicits is one of the themes of this chapter. One consequence is the perceived need to publish quickly. It is not unknown for scientists to write and submit an article in the same day. Neither is it

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[5] The Higgs particle is much in the news these days with the construction of the new accelerator at CERN (the LHC) and its associated four colliders. Named for the Scottish physicist Peter Higgs, who first postulated its existence, its existence has been sought at every collider since then.

[6] The Fields Medal is the closest equivalent to the Nobel Prize in math. Awarded every 4 years, to up to four mathematicians under the age of 40, it carries a nominal purse of around $13,000. It garnered considerable attention in 2007 when one of the four recipients of the Medal, Grigory Perelman, honored for his proof of the Poincaré conjecture, refused the prize. In 2002, the Norwegian government established the Abel Prize in mathematics; the 2006 award carried a purse of $920,000, making it the largest prize in mathematics.

[7] By way of example, Johnson&Johnson established the Dr Paul Janssen Award for Biomedical Research in 2005 with a purse of $100,000; the Heinz Foundation awards Heinz Prizes ($250,000); the Peter Gruber Foundation began to award several prizes beginning in 2000, including one in genetics for $250,000; GE partnered with *Science* to create the Prize for Young Life Scientists in 1995 ($25,000); General Motors awards the General Motors Cancer Research Prize ($250,000).

[8] Such lists are not without errors. The presence of common names, especially among the Asian community, means that attribution can be incorrect and thus such rankings must be cautiously used and carefully monitored.

unknown to negotiate with the editor of a prestigious journal the timing of a publication or the addition of a "note added" so that work completed between the time of submission and publication can be reported, thus making the claim to priority all the more convincing (Stephan and Levin, 1992). The time between receipt of a manuscript and publication is considerably shorter in science than in the social sciences. At the extreme is the practice of the journal *Science* to ask that referee reports be returned within 7 days of receipt and to then publish quickly following the editorial decision to accept. Ellison (2002) documents discipline differences and how these have changed over time. The move to electronic publication is quickening the process and may narrow the difference between science and the social sciences.

Another consequence of a priority-based reward system is the energy that scientists devote to establishing priority over rival claims. Moreover, such practices are not new. Merton (1969, p. 8) describes the extreme measures Newton took to establish that he, not Leibniz, was the inventor of the calculus.[9]

Science is sometimes described as a "winner-take-all" contest," meaning that there are no rewards for being second or third. One characteristic of science that contributes to such a reward structure is the difficulty that occurs in monitoring scientific effort (Dasgupta, 1989; Dasgupta and David, 1987). This class of problem is not unique to science. Lazear and Rosen (1981) have investigated incentive-compatible compensation schemes where monitoring is costly. Another factor that contributes to such a reward structure is the low social value of the contributions made by the runner-up. "There is no value added when the same discovery is made a second, third, or fourth time." (Dasgupta and Maskin, 1987, p. 583).

But it is somewhat extreme to view science as a winner-take-all contest. Even those who describe scientific contests in such ways note that it is a somewhat inaccurate description, given that replication and verification have social value and are common in science. It is also inaccurate to the extent that it suggests that only a handful of contests exist. True, some contests are world class, such as identification of the Higgs particle or the development of high-temperature superconductors. But there are many other contests that have multiple components, and the number of such contests appears to be on the increase. By way of example, while for many years it was thought that there would be "one" cure for cancer, it is now realized that cancer takes multiple forms and that multiple approaches are needed to find a cure. There will not be but one winner; there will be multiple winners.

A more realistic metaphor is to see science as following a tournament arrangement, much like tournaments in golf or tennis, where the losers, too, get some rewards. This keeps individuals in the game, raises their skills, and enhances their chances of winning a future tournament. A similar type of competition exists in science. Dr X is passed over for the Lasker Prize, but her work is sufficiently distinguished that she is invited to give an important lecture, consistently receives support for her research and is awarded an honorary degree from her undergraduate institution.

2.2. Financial remuneration and the satisfaction derived from solving the puzzle

Financial remuneration is another component of the reward structure of science. While scientists place great importance on priority and are highly motivated by an interest in puzzle-solving, money clearly plays a role in the reward structure. Rosovsky (1990) recounts how, upon becoming dean of the Faculty

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[9] A tension that exists between experimentalists and theorists in physics is the "awkward matter of credit." "Who should get the glory when a discovery is made: the theorist who proposed the idea, or the experimentalist who found the evidence for it?" (Kolbert, 2007, p. 75).

of Arts and Sciences at Harvard, he asked one of Harvard's most eminent scientists the source of his scientific inspiration. The reply (which "came without the slightest hesitation") was "money and flattery." (p. 242).

The tournament nature of the race places much of the risk on the shoulders of the scientists.[10] It is, therefore, not surprising that compensation in science is generally composed of two parts: one portion is paid regardless of the individual's success in races; the other is priority-based and reflects the value of the winner's contribution to science. While this clearly oversimplifies the compensation structure, counts of publications and citations play a significant role in academic promotions and raises, at least in the United States, although empirical work regarding the relationship is considerably dated (Diamond, 1986b; Tuckman and Leahey, 1975).[11] Salaries and resources are based on productivity in other countries, as well. Chinese researchers who place in the top half of their colleagues in terms of bibliometric measures can earn three to four times the salaries of coworkers (Hicks, 2007). The funding for academic departments in the United Kingdom is based in part on published output, as is that in Australia (Hicks, 2007). Unfortunately, we know little about the reward structure for scientists in industry or in government labs, particularly as the reward structure relates to priority.[12]

The flat profile of earnings in science (at least for those employed in academe) is frequently noted. Ehrenberg (1992), for example, calculates that the average full professor in the physical and life sciences earns only about 70% more than the average new assistant professor. In countries where faculties are civil servants, the profiles are also rather flat. The shape of the profile arguably relates to monitoring problems and the need to compensate scientists for the risky nature of their work. On the other hand, if earnings are expanded to include other forms of compensation, the profiles are not as flat as is assumed. The additional monetary awards that await the successful scientist take the form of prize money, speaking and consulting fees, and royalties. A fruitful area for further research would be to investigate the shape of the earnings profile when the definition of income is broadened to include other forms of compensation briefly elaborated below.

Royalties from patents are one form of additional compensation available to certain university faculty. Thursby and Thursby (2007) find that 10.3% of faculty at the highly selective US universities they study disclosed an invention to the technology transfer office in 1999. While many disclosures are not patented and most patents produce a small royalty stream at best, some produce substantial sums and in rare cases extraordinary sums. For example, Emory University in July 2005 sold its royalty interests in emtricitabine, also known as Emtriva®, and used in the treatment of HIV, to Giliad Sciences, Inc. and Royalty Pharma. The university received $525 million (US). The three Emory University scientists involved received approximately 40% of the sale price, reflecting the university policy that was in place at the time (http://sec.edgar-online.com/2005/08/04/0001193125-05-157811/Section7.asp).

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[10] Arrow (1962) noted that it is fortuitous that teaching and research activities are two sides of the same profession since the arrangement provides for researchers to be remunerated not on the basis of research (which would lead to a highly irregular pattern) but on that of teaching.

[11] The relationship between productivity and salary can be enhanced by the awarding of an endowed chair which pays a supplement over and above the scientist's salary. In some US universities the relationship between compensation and productivity is further enhanced through the university's practice of sharing indirect costs with faculty as a way to increase incentives for faculty to submit grant proposals.

[12] There is some evidence that increasing amounts of risk are being shifted to the scientist. For example, in the US university scientists, even those who are tenured, increasingly are expected to raise a portion of their salary from grants and contracts.

Royalty payments received by universities have dramatically increased in recent years, suggesting that faculty royalty payments have increased as well. Within the United States, for example, the amount of annual net royalty payments received by the university went from $195.0 to $866.8 million (US) during the period 1993–2003 (National Science Board, 2006, Table 5-28, vol. 2). University policies vary in terms of how royalties are shared with faculty inventors, but in all cases the inventor receives a portion of the stream of revenues. Lach and Schankerman (2008) have investigated how the structure of the sharing formula relates to invention disclosure and provide empirical support for the view that invention activity, as measured by invention disclosures, is positively related to the share of license income accruing to faculty.[13]

Faculty may also earn income and wealth through their role in start-up companies. In the most extreme case, the faculty member reaps rewards when the company goes public. Sometimes these are of staggering proportions, at least on paper. A case in point is Eric Brewer, a computer scientist at UC Berkeley, who was listed on *Fortune* magazine's list of the 40 richest Americans under 40 in October 1999 with a net worth of $800 million (US), a result of the role he played in founding a company that went public in 1998 (Wilson, 2000). Edwards et al. (2006) document that, in the event a biotechnology firm makes an initial public offering, the median value of equities held by an academic with formal ties to the company, based on the IPO's closing price, ranged from $3.4 million to $8.7 billion, depending upon the period analyzed. The incidence of being on a scientific advisory board (SAB) is nontrivial. Ding et al. (2006b) identify 785 academic scientists who are members of one or more SABs of companies that made an initial public offering in biotechnology in the United States.

The other reward often attributed to science is the satisfaction derived from solving the puzzle. Hagstrom (1965, p. 16), an early sociologist of science, noted this when he said "Research is in many ways a kind of game, a puzzle-solving operation in which the solution of the puzzle is its own reward." The philosopher of science Hull (1988, p. 305) describes scientists as being innately curious and suggests that science is "play behavior carried to adulthood." Feynman (1999), explaining why he did not have anything to do with the Nobel Prize (which he won in 1965), said: "I don't see that it makes any point that someone in the Swedish Academy decides that this work is noble enough to receive a prize—I've already got the prize. The prize is the pleasure of finding the thing out, the kick in the discovery . . ." This suggests that time spent in discovery is an argument in the utility function of scientists. Pollak and Wachter (1975) demonstrate that maximization problems of this type are generally intractable, because implicit prices depend upon the preferences of the producer. While this provides a rationale for excluding the process of discovery from models of scientific behavior, the failure of economists to acknowledge the puzzle as a motivating force makes economic models of scientific behavior lack credibility. Recent work by Sauermann and Cohen (2007) seeks to address this in part for scientists and engineers working in industry.

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[13] Not all inventions made by faculty are patented by the university. Thursby et al. (2009) find that 29% of patents by US faculty are assigned to firms. Likewise, the practice of "professor privilege" that exists in several European countries means that inventions made by professors need not be assigned to the university. Crespi et al. (2009) find that the large majority of university-invented patents in their sample are not owned by universities. Instead, most are assigned to firms. We know virtually nothing about the royalties from patents assigned outside the university.

3. How knowledge is produced

> *"Any new idea—a new conceptualization of an existing problem, a new methodology, or the investigation of a new area—cannot be fully mastered, developed into the stage of a tentatively acceptable hypothesis, and possibly exposed to some empirical tests without a large expenditure of time, intelligence, and research resources."*

So Stigler (1983, p. 536) described the "production function" for knowledge in his 1982 Nobel lecture. Here we explore these components in more detail.

3.1. Time and cognitive inputs

Although it is popular to characterize scientists as having instant insight, studies suggest that science takes time. Investigators often portray productive scientists—and eminent scientists especially—as strongly motivated, with the "'stamina' or the capacity to work hard and persist in the pursuit of long-range goals." (Fox, 1983).[14]

Several dimensions of cognitive resources are associated with discovery. One aspect is ability. It is generally believed that a high level of intelligence is required to do science, and several studies have documented that, as a group, scientists have above average IQs.[15] There is also a general consensus that certain people are particularly good at doing science and that a handful are superb.[16] Another dimension of cognitive inputs is the knowledge base the scientist(s) working on a project possesses. This knowledge is used not only to solve a problem but to choose the problem and the sequence in which the problem is addressed.

The importance knowledge plays in discovery leads to several observations. First, it intensifies the race, because the public nature of knowledge means that multiple investigators have access to the knowledge needed to solve a problem. Second, knowledge can either be embodied in the scientist(s) working on the research or disembodied, but available in the literature (or from others). Different types of research rely more heavily on one than the other. The nuclear physicist Leo Szilard, who left physics to work in biology, once told the biologist Sydney Brenner that he could never have a comfortable bath after he left physics. "When he was a physicist he could lie in the bath and think for hours, but in biology he was always having to get up to look up another fact" (Wolpert and Richards, 1988, p. 107).

Third, the knowledge base of a scientist can become obsolete if the scientist fails to keep up with changes occurring in the discipline. On the other hand, the presence of fads in science (such as in particle physics) means that the latest educated are not always the best educated (Stephan and Levin, 1992). Vintage may matter in science but not always in the way that Mincer's "secular progression of knowledge" would lead us to believe (Mincer, 1974, p. 21).

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[14] Hermanowicz (2006) reports that slightly over one-half of the physicists in his sample chose persistence from the list of 25 adjectives in response to the question "What do you think are the most important qualities needed to be successful at the type of work you do?" No other quality came close to persistence. Smartness was second, mentioned by 25%.

[15] Harmon (1961, p. 169) reports that PhD physicists have an average IQ in the neighborhood of 140. Catherine Cox, using biographical techniques to estimate the intelligence of eminent scientists, reports IQ guestimates of 205 for Leibnitz, 185 for Galieleo, and 175 for Kepler. Roe (1953, p. 155) summarizes Cox's findings.

[16] Feist (2006) examines the psychological forces at play in the development of an individual's interest, talent and creativity in science.

Fourth, there is anecdotal evidence that "too much" knowledge can be a bad thing in discovery in the sense that it "encumbers" the researcher. There is the suggestion, for example, that exceptional research may at times be done by the young because the young "know" less than their elders and hence are less encumbered in their choice of problems and the way they approach a question.[17]

Finally, the cognitive resources brought to bear on a problem can be enhanced by assembling a research team or, at a minimum, engaging in a collaborative arrangement with investigators in other labs and countries. Research is rarely done in isolation, especially research of an experimental rather than theoretical bent (Fox, 1991). Scientists work in labs. How these labs are staffed varies across countries. For example, in Europe research labs are often staffed by permanent staff scientists, although increasingly these positions are held by temporary employees (Stephan, 2008). In the United States, while positions such as staff scientists and research associates exist, the majority of scientists working in the lab are doctoral students and postdocs. Stephan et al.'s study (2007b) of 415 labs affiliated with a nanotechnology center finds that the average lab has 12 technical staff, excluding the principal investigator (PI). Fifty percent of these are graduate students; 16% are postdocs, and 10% are undergrads.[18] Such patterns mean that labs in the United States are disproportionately staffed by young, temporary workers. The reliance on such a system, with its underlying pyramid scheme, at a time when there has been minimal expansion in faculty positions, has resulted in an increasing supply of scientists trained in the United States (as well as those trained abroad, who come to the United States to take a postdoctoral position) who are less and less likely to find permanent PI positions in the university.[19]

One way of seeing how team size and collaboration have changed is to examine trends in coauthorship patterns. Adams et al. (2005), for example, find that the mean number of authors per paper increased from 2.8 to 4.2 for an 18-year interval, ending in 1999.[20] The rate of growth was greatest during the period 1991–1996 when use of email and the Internet was rapidly accelerating. The growth has been due both to a rise in lab size and to an increase in the number of institutions—especially foreign institution—collaborating on a research project. During the period 1988–2003, for example, the number of addresses on an article with at least one US address grew by 37% while the number of foreign addresses more than tripled (National Science Board, 2006, Table 5-18).[21]

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[17] There is a literature suggesting that individuals coming from the margin—"outsiders" if you will—make greater contributions to science than those firmly entrenched in the system (Gieryn and Hirsch, 1983). Stephan and Levin (1992) argue that this is one reason why exceptional contributions are more likely to be made by younger persons. In studying Nobel laureates they conclude that although it does not take extraordinary youth to do prize-winning work, the odds decrease markedly by midcareer.

[18] Approximately a third of the PIs were affiliated with departments of engineering, a third with departments of chemistry and the remainder with departments of physics.

[19] Hollingsworth (2006) argues that the organizational structure of the institution in which the research is being performed also contributes to productivity. He sees extreme decentralization, permitting exceptionally productive scientists a high degree of autonomy and flexibility, to be a key characteristic of organizations where major discoveries occur.

[20] The study is restricted to articles in science and engineering having one or more authors from a top-110 US university.

[21] During the same period, the number of names increased by approximately 50%, suggesting that lab size was growing slightly faster than institutional collaboration growth (National Science Board, 2006, Table 5-18).

Several factors contribute to the increased role that collaboration plays in research.[22] First, the importance of interdisciplinary research and the fact that major breakthroughs often occur in emerging disciplines, encourages collaboration. Systems biology, which involves the intersection of biology, engineering, and physical sciences, is a case in point.[23] By definition, no one has all the requisite skills required to work in the area; researchers must rely on working with others. Second, and related, researchers arguably are acquiring narrower expertise over time in order to compensate for the educational demands associated with the increase in knowledge (Jones, 2005b). Narrower expertise, in turn, leads to an increased reliance on teamwork for discovery. Third, the rapid spread of connectivity, which began in the early 1980s with the adoption of bitnet by a number of universities and accelerated in the early 1990s with the diffusion of the Internet, has decreased the costs of collaboration across institutions (Agrawal and Goldfarb, 2008; Levin et al., 2006). Another factor that fosters collaboration is the vast amount of data that is becoming available, such as that from the Human Genome Project (and the associated GenBank database). Although that is probably the best known, many other large databases have recently come online, such as PubChem, which as of this writing contained 17,655,303 recorded substances, and the Worldwide Protein Data Bank (wwPDB), a worldwide depository of information regarding protein structures.[24] The practice of sharing research materials also leads to increases in the number of authors appearing on an article.

Increased complexity of equipment also fosters collaboration. At the very extreme are the teams assembled to work at colliders. CERN's four colliders have combined team size of just under 6000: 2520 for the Compact Muon Detector (CMS), 1800 for the Atlas, 1000 for ALICE, and 663 for LHCb (Overbye, 2007). Barnett et al. (1988) suggest two other factors that lead persons to seek coauthors. One is the desire to minimize risk by diversifying one's research portfolio through collaboration; the other is the increased opportunity cost of time. An additional factor is quality. The literature on scientific productivity suggests that scientists who collaborate produce "better" science than do individual investigators (Andrews, 1979; Lawani, 1986; Wuchty et al., 2007). Some of the factors encouraging collaboration are new (such as connectivity) but growth in the number of authors on a paper is not. Wuchty et al. (2007) find that team size has grown in all but one of the 171 S&E fields studied during the past 45 years.

Other chapters in this volume will address the role of networks in research. Here we note that governments on both sides of the Atlantic have bought heavily into the importance of funding collaborative research across institutions. The National Institutes of Health (NIH), in an effort to encourage collaborative research, funds P01 grants which support broadly based multidisciplinary research with multiple investigators. On the other side of the Atlantic, the European Union (EU) is committed to funding networks of excellence. While such grants clearly create incentives for individuals to work together, research has yet to show their effectiveness relative to other forms of funding. One possible reason for creating these networks is to improve incentives for labs to share data and material across labs.

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[22] Changing patterns in collaboration present certain challenges for organizations. For example, as the number of coauthors grows, it becomes increasingly difficult to evaluate curriculum vitas at tenure and promotion time. Historically, for example, individuals were penalized if they only published with their mentor after completing a postdoctoral appointment. In recent years, however, programs such as the Medical College at the University of Pennsylvania have relaxed this rule and now consider such individuals for promotion.

[23] Systems biology studies the relationship between the design of biological systems and the tasks they perform.

[24] The Large Hadron Collider (LHC) at CERN will create vast amounts of data. According to Kolbert (2007, p. 74), "If all the LHC data were burned onto disks, the stack would rise at the rate of a mile a month."

3.2. Research resources

The production of knowledge also requires resources. In the social sciences this generally translates into a personal computer, access to a database and one or two graduate research assistants. For the experimental physical sciences the resource requirements are considerably more extensive, involving access to substantial equipment. Sometimes this equipment is in the lab, but particle physics experiments require time on an accelerator; astronomers require time on a telescope. Research in nanotechnology requires "clean" labs and specialized equipment such as a scanning tunneling microscope. Super computers increasingly play a role in research, both at the theoretical and at the experimental level. Moreover, as large databases become increasingly available, the use of super computers is accelerating.

Research in the biomedical sciences also increasingly requires access to sophisticated equipment. The DNA gene sequencer and synthesizer and the protein synthesizer and sequencer comprise the technological foundation for contemporary molecular biology. The revolution in proteomics and systems biology relies on analytical tools such as mass spectrometry (Chait, 2006). Robotics technology is becoming increasingly important in sequencing proteins. Research in the biomedical sciences is not only *in vitro*. *In vivo* studies have become progressively more important, especially those involving mice, which are estimated to account for more than 90% of all mammals used in research (Malakoff, 2000).

The increasing sophistication of research tools in the biomedical sciences has dramatically changed the output of a lab. While in 1990 the best equipped lab could sequence 1000 base pairs a day, by January 2000 the 20 labs involved in mapping the human genome were collectively sequencing 1000 base pairs a second, 24/7. The cost per finished base pair fell from $10.00 in 1990 to under $0.05 in 2003 (Collins et al., 2003) and was roughly $0.01 in 2007 (www.biodesign.asu./edu/news/232/) (see Figure 1). Measured in terms of base pairs sequenced per person per day, for a researcher operating multiple machines, productivity increased more than 20,000-fold from the early 1990s to 2007, doubling approximately every 12 months (http://www.bio-era.net/news/add_news_18.html).[25] More recently, next-generation sequencing machines, which first came on the market in 2007 and read millions of sequences at once, have made the earlier technology for sequencing obsolete.

The increasing sophistication of research tools means that the "capital–labor ratio" for research, at least in the biomedical sciences, is changing. In 2008, for example, the Venter Institute eliminated 29 sequencing-center jobs, announcing that the staff reduction "is a direct result of a technology shift and is not a reflection of the tough economic times that we are all facing in the United States today." (http://www.jcvi.org/cms/press/press-releases/full-text/article/j-crai...quencing-staff-positions/?tx_ttnews%5baclkPid%5D=67&Hash=db443577b0).

The substitution of capital for labor in research is an underresearched area which has clear implications for the demand for scientists. The dramatic changes in technology that have occurred have also substantially changed the nature of dissertation research. For example in chemistry, nuclear magnetic

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[25] The decline in the cost of sequencing has led to the hope that personal genomes can be sequenced for $1000 or less (www.biodesign.asu.edu/news/232/). In March 2007, the Archon X Prize for Genomics was established with the goal of awarding $10 million to the first group that can "build a device and use it to sequence 100 human genomes within 10 days or less, with an accuracy of no more than one error in every 100,000 bases sequenced, with sequences accurately covering at least 98% of the genome, and at a recurring cost of no more than $10,000 per genome." (http://thepersonalgenome.com/category/sequency-cost/).

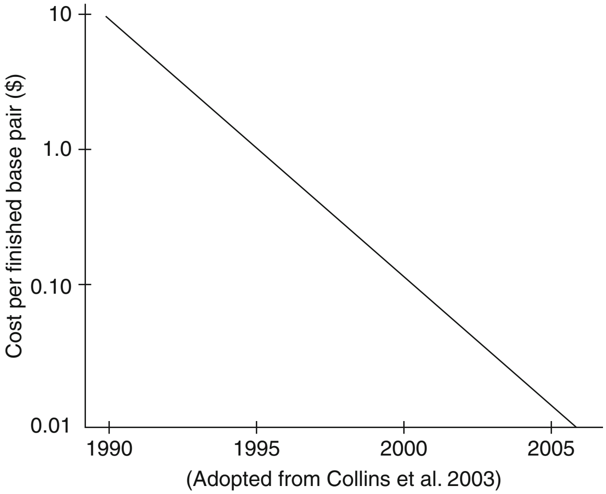

Figure 1. Cost per finished base pair ($).

resonance combined with X-ray crystallography and advanced computing power allows protein structures to be elucidated much more rapidly. As a result, while a PhD thesis used to be focused on defining the structure of a single protein domain, now a thesis in a similar field might examine and compare dozens of structures.

The importance of equipment is one reason to stress the nonlinearity of scientific discovery. Scientific research can lead to technological advance, but technology very much affects advances in science. The history of science is the history of how important resources and equipment are to discovery—a theme in the research of Rosenberg and Mokyr, among others. In some instances, and perhaps what is most efficient, the scientist is both the researcher and the inventor of new technology (Franzoni, 2009). The biologist Leroy Hood, author of more than 500 papers and winner of the 1987 Lasker Award for Basic Medical Research, exemplifies the researcher–inventor. In recognition of his inventions, which include the automated DNA sequencer and an automated tool for synthesizing DNA, he received the 2002 Kyoto Prize for Advanced Technology. In 2003 he was the recepient of the Lemelson-MIT Prize for inventing "four instruments that have unlocked much of the mystery of human biology, including the automated DNA sequencer."[26] (http://web.mit.edu/invent/n-pressreleases/n-press-03LMP.html).

Equipment for research is costly.[27] At the extreme are costs associated with building and running an accelerator. The 27-km-long LHC which is scheduled to come online early in 2008 at CERN will cost $8 billion; the Spallation Neutron Source (SNS) at Oak Ridge National Laboratory in the United States cost

[26] Hood's interest in tools and cutting edge research was instilled in him by his mentor William Dreyer, who reportedly told the then Cal Tech doctoral student "If you want to practice biology, do it on the leading edge and if you want to be on the leading edge, invent new tools for deciphering biological information." (http://web.mit.edu/invent/a-winners/a-hood.html).

[27] US academic institutions spent $1.8 billion (US) in 2003 for research equipment, approximately 2.5 times the amount spent 20 years before in real dollars (National Science Board, 2006, Appendix Table 5-13).

$1.41 billion (*Science*, vol. 312, 5 May 2006; p. 675). A microscope used for nanotechnology research can cost $750,000 (http://www.unm.edu/∼market/cgi-bin/archives/000132.html). A sequencer, such as Applied Biosystems' 3730 model costs approximately $300,000. Next-generation sequencers cost between $400,000 and $500,000.

Mice are not free. An inbred off-the-shelf mouse costs between $17 and $60; mutant strains begin around $40 and can go to $500 plus. Prices are for mice supplied from live breeding colonies. Many strains, however, are only available from cyropreserved material. Such mice cost considerably more: in 2009 the cost to recover any strain from cryopreservation (either from cryopreserved sperm or embryos) was $1900. For this, investigators receive at least two breeding pairs of animals in order to establish their own breeding colony.[28] Custom made mice can cost much more. Johns Hopkins University, for example, estimates that it costs $3500 to engineer a mouse to order.

With the large number of mice in use (over 13,000 are already published), the cost of mouse upkeep becomes a significant factor in doing research. US universities, for example, charged from $0.05 to $0.10 per day per mouse (mouse per diem) in 2000 (Malakoff, 2000). This can rapidly add up. Irving Weissman of Stanford University reports that before Stanford changed its cage rates he was paying between $800,000 and $1 million a year to keep the 10,000 to 15,000 mice in his lab.[29] Costs for keeping immune deficient mice are far greater (on the order of $0.65 per day), given their susceptibility to disease.[30]

The importance of equipment and research materials in scientific research means that exchange, which has a long tradition in science (Hagstrom, 1965), plays a considerable role in fostering research and in creating incentives for scientists to behave in certain ways. For example, scientists routinely share information and access to research materials and expertise in exchange for citations and coauthorship.[31] But, as research materials have become increasingly important, exchange has arguably taken on more importance. Walsh et al. (2005, 2007) examine the practice of sharing materials (such as cell lines, reagents, and antigens) among academic biomedical researchers and find that 75% of the academic respondents in their sample made at least one request for material in a 2-year period, with an average of 7 requests for materials to other academics and two requests for materials from an industrial lab (Walsh et al., 2005).[32]

Murray examines how the advent of patenting life-forms has influenced patterns of exchange among mouse researchers during the past 100 years. She argues that although mouse geneticists resisted the imposition of patents, in recent years they accommodated them, incorporating them into their exchange relationship: "Having patents became a signal to other scientists that you were a valuable exchange

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[28] The NIH and a number of other foundations have provided long-term support for the Jackson Laboratories which serves as a critical institution for the preservation and upkeep of thousands of mice, making them available to researchers and providing important economies of scale. More than 67% of the strains from the Jackson lab are only available from cryopreserved material.

[29] Given such costs, it is no surprise that "mouse packages" play a role in recruitment. The McLaughlin Research Institute in Great Falls, Montana, for example, successfully recruited a researcher when they offered him a mouse package with a per diem of $0.036 (Vogel, 2000). (Cage costs converted to mice costs at the rate of 5 mice per cage.)

[30] Researchers not only buy mice and equipment to take care of the mice; they also buy equipment to observe and record mouse activity. For example, the titanium dorsal skinfold chamber (which is designed to fit under the back of a mouse) allows the researcher to "nondestructively record and visualize microvascular functions" according to an ad placed in *Science* (June 9, 2006, p. 1439).

[31] LaTour (1987) provides a detailed account of how academics use exchange to nurture their expertise.

[32] This is not to say that scientists always "share" or exchange data and resources. See discussion of "having one's cake and eating it too" in section 6.4.

partner and therefore worthy of coauthorship. Scientific collaboration was never entered into indiscriminately but under the commercial regime, a patent became a way of signaling your value to other scientists and co-opting them in your bid for prestige and reputation." (Murray, 2006, p. 34).

The overwhelming importance of equipment to the research process and the associated costs of equipment mean that in most fields access to resources is a necessary condition for doing research. It is not enough to decide to do research, as a standard human capital model might assume. One must also have access to research inputs. At US universities, equipment, and funding for graduate and postdoc stipends, are generally provided by the dean at the time of hire in the form of start-up packages.[33] Thereafter, equipment, some buyoff for faculty time,[34] and the stipends that graduate students and postdocs receive, become the responsibility of the scientist. Scientists whose work requires access to "big" machines off campus must also submit grants to procure time (e.g., beam time) at the research facility. This means that for a variety of fields funding becomes a necessary condition for doing "independent" research that is initiated and conceived by the scientist. Scientists working in these fields in the United States take on many of the characteristics of entrepreneurs. As graduate students and postdocs they must work hard to establish their "credit-worthiness" through the research they do in other people's labs. If successful in the endeavor, and if a position exists (see discussion of cohort effects), they will subsequently be provided with a lab at a research university. They then have several years to leverage this capital into funding. If they succeed, they face the onerous job of continually seeking support for their lab; if they fail, the probability is low that they will be offered a start-up package by another university. The emphasis on the individual scientist to generate resources is not as strong in many other countries, where researchers are hired into government-funded and government-run laboratories such as CNRS in France. Nevertheless, fits and starts in funding for such programs translate into the possibility that certain cohorts of scientists enter the labor market when conditions are favorable for research while other cohorts do not.

3.3. Serendipity

Serendipity also plays a role in scientific discovery; it is not that uncommon for researchers to find different, sometimes greater, riches than the ones they are seeking. Although serendipity is sometimes referred to as the "happy accident," this is a bit of a misnomer. True, Pasteur "discovered" bacteria while trying to solve problems that were confronting the French wine industry. But his discovery, although unexpected, was hardly "an accident." Distinguishing between the unexpected and the "accidental" is especially difficult when research involves exploration of the unknown. The analogy to discovery makes the point: Columbus did not find what he was looking for—but the discovery of the new world was hardly an accident.[35]

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[33] Ehrenberg et al. (2003) survey US universities regarding start-up packages. They find that the average package for an assistant professor in chemistry is $489,000; in biology it is $403,071. At the high-end it is $580,000 in chemistry; $437,000 in biology. For senior faculty they report start-up packages of $983,929 in chemistry (high-end is $1,172,222); and of $957,143 in biology (high-end is $1,575,000).

[34] Universities increasingly expect faculty to write off part of their academic-year salary on grants. This is an absolute necessity for faculty on soft money positions, but also is becoming increasingly common for tenure and tenure-track faculty.

[35] I thank Nathan Rosenberg for this analogy.

Thus, it is perhaps more appropriate to think of serendipity as the act of finding answers to questions not yet posed. Important medical advances, for example, have come from fundamental, nonmission directed, research. A scientist studying marine snails found a powerful new drug for chronic pain. A widely used cancer medication came out of studies of how electricity affects microbes.[36] The discovery of AGM-1470—a drug being tested for an entirely different approach to the treatment of cancer, is described as having started with a "laboratory accident." The narrative: the dish in which Don Ingber was culturing capillary endothelial cells became contaminated with fungus. Ingber noticed that the fungus induced cell rounding, which his previous work had shown to be associated with inhibition of capillary growth.[37] The hope: that the drug will block the growth of blood vessels, which tumors need in order to survive. It may have been an accident, but, to quote Pasteur, "Where observation is involved, chance favors only the prepared mind."

Scientists not only benefit from serendipitous occurrences; they also note them at times, as does Robert Richardson, Nobel laureate in Physics in 1996, in his short bio (National Academies, 2005, p. 148):

> *"He (Richardson) obtained his PhD degree from Duke in 1966. His thesis advisor was Professor Horst Meyer. In the Fall of 1966 he began work at Cornell University in the laboratory of David Lee. Their Research goal was to observe the nuclear magnetic phase transition in solid ^{3}He that could be predicted from Richardson's thesis work with Horst Meyer at Duke. In collaboration with Douglas Osheroff, a student who joined the group in 1967, they worked on cooling techniques and NMR instrumentation for studying low temperature helium liquids and solids.* In the fall of 1971, they made the accidental discovery that liquid ^{3}He undergoes a pairing transition similar to that of superconductors. The three were awarded the Nobel Prize for that work in 1996."

4. Choice of scientific contests and character of research

4.1. Choice of contests

The importance attached to priority of discovery dictates that scientists choose the contests they enter with care. The probability of being scooped is a constant threat. This is particularly true in the case of "normal" science, where the accumulated knowledge and focus necessary for the next scientific breakthrough is "in the air."[38] Young scientists, in particular, must choose their contests with care if they are to successfully signal their ability and "resource worthiness" to receive funding. Young biomedical researchers in the United States must choose a research trajectory that is sufficiently independent from that of their mentors to appeal to funders, yet sufficiently close to signal the effectiveness of their training.

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[36] National Institutes of General Medical Sciences: 2008–2012 Strategic Plan (http://publications.nigms.nih.gov/strategicplan/chapter2.htm).

[37] www.aids.org/atn/a-135-04.html

[38] Note the distinction between social and individual risk. Because accumulated knowledge is an important input in the process of discovery, normal science is not especially risky from the social point of view (Arrow, 1962; Dasgupta and David, 1987, p. 526). From the individual investigator's point of view, however, risks can be substantial: being in the air is entirely different from being in scientist X's air.

Scientists can minimize the threat of being scooped by seeking ways to monopolize a line of research. During the seventeenth and eighteenth centuries discoveries in process were sometimes reported in the form of anagrams for the "double purpose of establishing priority of conception and yet not putting rivals on to one's original ideas, until they had been further worked out" (Merton, 1957, p. 654). It was also not uncommon to deposit a sealed and dated manuscript with a learned society to protect both priority and idea. More recently, the ownership of apparatus or strains has proved to be a convenient way to monopolize a line of research. Another strategy for minimizing the threat of being scooped is to develop a particularly novel technique for research and to then collaborate with others in applying the approach or technique to a range of questions. Scientists can also minimize the threat of being scooped by choosing to work on problems that fall outside the mainstream of "normal science" or by working "in the backwaters" of research (Stephan and Levin, 1992). The downside of such a strategy is that, while the low number of competitors increases the probability of being first, the contest that is won may be of little interest to the larger scientific community and hence receive minimal recognition.

Researchers must choose not only a line of research. They must also choose a research strategy, because more than one method can be used to address the same question (Dasgupta and David, 1994). In the life sciences, this involves not only choosing one's research topic but also the approach for one's research. Here, too, uncertainty enters the equation.[39] The use of novel methods, for example, can prove rewarding, but the risk of coming up empty-handed can be quite large when an unorthodox approach is employed or when a difficult problem is approached in a way that is not divisible into intermediate outputs.[40] The uncertainty associated with the *process* of discovery can be substantial. The outcome may not have been envisioned, neither may the outcome relate to the original objective of the researcher. As noted above, in the process of trying to solve some very practical problems concerning fermentation and putrefaction in the French wine industry, Pasteur established the modern science of bacteriology (Rosenberg, 1990).

Research often provides answers to unposed questions.[41] Consequently, the risk associated with such research can be lessened by shifting goals during the course of research. Nelson (1959) argues that this strategy is more appropriate for scientists working in a nonprofit-based environment than for scientists working in the profit sector because the former can more easily capture the rewards regardless of where the research leads. On the other hand, companies having a broad technological base can benefit from research that is not directed to a specific goal. At the time General Electric developed synthetic diamonds, for example, it was the most diversified company in the United States.

A number of institutional arrangements have evolved in science to help minimize risk or provide some insurance against risk. Some of these, such as the ability to monopolize a line of research, have already been noted.[42] Others include the adoption of a research portfolio that contains projects with

[39] Susan Linquist, an HHMI Investigator at MIT who studies protein function, reports the risky choice she made early in her career to change her research focus from fruit flies to yeast (Dreifus, 2007).

[40] A consequence is that rival teams often select highly correlated research strategies. From a social point of view, highly correlated research strategies produce inefficiencies by failing to provide the kind of portfolio diversification that society would choose if it were allocating resources in a way to maximize the probability of success (Dasgupta and David, 1994). The gains to society from sponsoring multiple lines of independent research are examined by Scherer (1966).

[41] The unpredictable nature of scientific discovery is explored by Polanyi (1962).

[42] The ability to monopolize a line of research is being weakened by the increasingly rapid disclosure requirements being placed on researchers by databases as well as rules placed on researchers, such as the Bermuda Rule for gene sequence disclosure.

varying degrees of uncertainty, the formation of research teams and networks and the practice of “gift giving” whereby scientists, by acknowledging intellectual debts to their colleagues (via citations), pay “protection money” to insure that those colleagues “won’t deny their grants, spread slander, or—worst of all—ignore their work altogether.” (Fuller, 1994, p. 13).

4.2. The character of research

It was common practice for many years to classify research as either basic or applied and many government statistical agencies continue to classify research accordingly. Such a classification, while useful for governmental statistical agencies, oversimplifies the research process and reasons for doing research. Stokes (1997) notes that much of today’s research is both “use inspired” and inspired by a quest for fundamental understanding. In honor of Louis Pasteur, Stokes classifies such research as falling into “Pasteur’s Quadrant.” Stokes argues that increasingly scientists work in Pasteur’s Quadrant, in part because of the scientific opportunities that have become available in recent years in such areas as molecular biology and, to extend his argument, nanotechnology. Stokes contrasts this to research that falls in “Bohr’s Quadrant”—research that is motivated exclusively for fundamental understanding—and research in “Edison’s Quadrant”—research inspired exclusively by use.

It is also an oversimplification to assume that research occurring in the public sector is distinct from that occurring elsewhere. The research boundaries between public sector and other sectors are porous, and are becoming increasingly so. Gibbons et al. (1994) see this as one characteristic of what they call Mode 2, a new mode of knowledge production, which they argue is distinct from what they call Mode 1, where research is done within the university, within disciplinary boundaries, and is homogeneous and hierarchical. By contrast, “The new mode operates within a context of application in that problems are not set within a disciplinary framework. It is transdisciplinary rather than mono- or multidisciplinary. It is carried out in nonhierarchical, heterogeneously organized forms which are essentially transient. It is not being institutionalized primarily within university structures.” (p. vii).

While there is considerable debate over some of these claims (e.g., the “newness” of Mode 2; Pavitt, 2000), it is clear that university researchers work with researchers outside their own disciplines. It is also clear that university researchers are heavily influenced by the research and technological opportunities that occur outside the academy and that they frequently work with scientists and engineers located outside the university. Moreover, this cross-sectoral work often enhances the research activity of academic scientists and engineers. Zucker et al. (1998a,b), for example, find that the productivity of academic scientists is enhanced when they work with scientists in biotechnology companies; Mansfield (1995) found that academic researchers with ties to firms report that their academic research problems frequently or predominately are developed out of their industrial consulting and that the consulting also influences the nature of work they propose for government-funded research.

4.3. Production of dual knowledge

One choice that scientists working in the public sector increasingly must make is whether to disclose their findings exclusively through publication, to seek intellectual property protection, or to both patent and publish. While the presence of time in the production function for knowledge suggests that patenting and publishing may be substitute activities, there are good reasons to argue that complementarity is more likely and that patents can be a logical outcome of research activity that is designed first and foremost with an eye to publication. The reasons for complementarity are threefold. First, the results of research, especially research in Pasteur's Quadrant, can often be both patented and published, having a dual nature. Second, the increased opportunities that academic researchers have to work with industry may enhance productivity and encourage patenting. Third, the reward structure in academe encourages patenting as one outcome of research.

A handful of studies in recent years have examined the relationship of publishing to patenting (Agrawal and Henderson, 2002; Calderini et al., 2007; Carayol, 2007; Wuchty et al., 2007). While various methodological issues arise, such as endogeneity, most find evidence that publishing and patenting are complementary rather than substitute activities. Researchers have also examined the relationship between patenting and publishing. Azoulay et al. (2009), for example, examine the impact of patenting on the publication activity of university researchers working in areas related to biotechnology and find that patenting has a positive effect on publication. Markiewicz nd Di Minin (2004) also find patents to have a positive and significant effect on publication production of university researchers in their sample of US scientists, as do Breschi et al. (2009) in a study of Italian scientists.[43]

5. Outcomes

Articles are a major output of scientific research. Over time, the number of articles written has increased substantially, as well as the distribution of those writing the articles. This is best seen in Figure 2, which shows worldwide article production in science and engineering (measured by fractional counts) for the 16-year period 1988–2003. The numbers are impressive—the most recent data enumerate more than 650,000 articles (fractional counts). The figure also shows how the dominance of the United States has waned in recent years, as counts from the EU-15 and the East-Asia 4 have dramatically increased.

Academics contribute disproportionately to research that is codified through journal publication. This is seen in Table 1, which gives article output (fractional counts) by sector for selected years for the United States. During the period, the academic share rose from about 72% to 74%. That of industry and the Federal government declined, while that of private nonprofits and Federally Funded Research and Development Centers (FFRDCs) remained approximately the same. We will return to this table later in the chapter when we discuss scientists working in industry.

Patents provide another indicator of research output. As in the case of articles, over time the number of patents has increased substantially, as has the number granted to an academic institution. For example, the number of patents granted in the United States almost doubled between 1990 and 2003, going from 90,000 to 169,000 (National Science Board, 2006, Table 6-12).[44] Some of the dramatic increase undoubtedly relates to problems with the patent system (Jaffe and Lerner, 2004). During the same period of time, the number of patents granted to US universities increased by more than 2.5 times, going from about 1300 in 1993 to 3450 in 2003 (National Science Board, 2006, Table 5-28). Similar

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[43] Their research suggests that the positive effect is not due to patenting *per se* but to advantages derived by having strong links with industry.

[44] The number of US patents granted to a foreign inventor more than doubled during this same period.

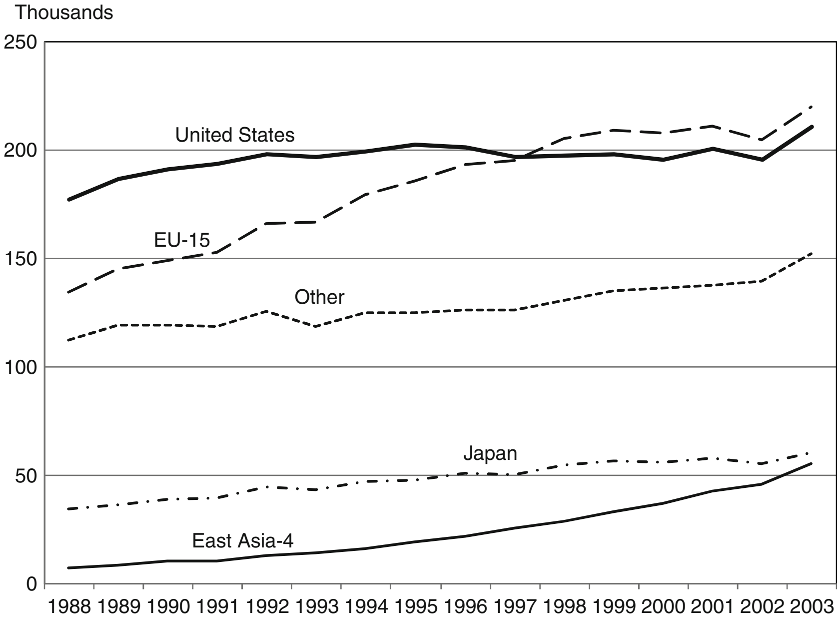

Figure 2. S&E article output (fractional counts) of major S&E publishing centers: 1988–2003. *Notes*: Article counts are on a fractional basis, that is, for articles with collaborating institutions from multiple publishing centers, each publishing center receives fractional credit on the basis of the proportion of its participating institutions. East Asia-4 includes China, Singapore, South Korea, and Taiwan. China includes Hong Kong. Source: National Science Foundation (2007, Figure 6).

Table 1
S&E article output (fractional counts) of US institutions: 1988–2003 (1000s)

Year	FFRDC	Federal government	Other	Private-for-profit	Private nonprofit	Academe	Total
1988	4.9	14.4	3.5	15.1	12.4	127.3	177.6
1991	5.1	15.2	3.8	16.9	13.5	139.3	193.8
1993	4.7	15.3	4.1	16.4	14.6	142.3	197.4
1995	5.4	15.5	3.7	16.4	15.4	146.5	202.9
1997	5.2	14.3	3.9	14.6	15.0	144.6	197.6
1999	5.2	13.9	4.1	14.5	15.4	145.5	198.6
2001	5.2	14.0	3.7	14.2	16.0	147.8	200.9
2003	5.7	14.1	4.0	14.5	16.3	156.6	211.2

Source: National Science Board (2006, Table 5-19 and table underlying Figure 5.51).

trends exist in Europe although academic patents are more difficult to trace because of the practice in many countries of "professor privilege."

The productivity of scientists and engineers, especially those working in academe, has been studied by a number of researchers. While most of this work focuses on the publication of articles, in recent years researchers have also examined the patenting output of faculty. Most of the early work was conducted by sociologists; in more recent years sociologists have been joined by economists and researchers in public policy in their efforts to understand factors related to productivity, especially at the individual level. Issues of interest include: (1) whether science is a young person's game; (2) the extent to which cohort effects are present in science; and (3) the degree to which output is related to gender and underlying reasons for such a differential, if it is found to exist. In addition, there has been considerable interest in the distribution of output across scientists and factors leading to the extreme inequality that is observed.

Data for productivity studies is drawn from a variety of sources. Early studies, for example, generally used survey data collected specifically for the study. Some researchers have matched public data with outcome data (Levin and Stephan, 1991; other researchers have collected data from national funding organizations or institutes (Gonzalez-Brambilia and Veloso, 2007; Turner and Mairesse, 2003),while others rely on data that is available from CVs (Cañibano and Bozeman, 2009). Here we examine several of these studies, organizing the discussion by the type of research question addressed. Two types of outcomes are examined, where relevant: (1) publication measures and (2) patent measures.

5.1. Is science a young person's game?

Einstein once said that "a person who has not made his great contribution to science before the age of thirty will never do so." (Brodetsky, 1942, p. 299). There is a great deal of anecdotal evidence (Stephan and Levin, 1992) that he was right, that science is the domain of the young. However, investigating the veracity of the statement statistically is fraught with problems: measurement issues abound, as do the confounding of aging effects with cohort effects, as well as the availability of appropriate databases. We examine these issues, prefacing them with a discussion of theoretical reasons that one might expect age to be related to productivity.

For economists, the theoretical reason to expect a relationship between age and productivity rests on human capital theory.[45] General models of human capital predict that, due to the finiteness of life, investment behavior declines (eventually) over time. Several authors have adapted the human capital framework to develop life-cycle models of scientists or academics. Like their first cousins, these models are driven by the finiteness of life and investigate the implications this has for the allocation of time to research over the life cycle. The models differ in the assumptions they make concerning the objective function of the scientists but reach somewhat similar conclusions. In its simplest form the objective is the maximization of income, itself a function of prestige capital (Diamond, 1984). In a more complex form, the objective is the maximization of a utility function that includes income as well as research output (Levin and Stephan, 1991).[46] The latter is included given the strong anecdotal evidence that puzzle solving is part of the reward to doing science.[47] The implications of these models are that the stock of prestige capital peaks during the career and then declines and that the publishing profile

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[45] Sociologists, psychologists, and neurologists have other reasons regarding why there may be a relationship between age and productivity. See Stephan and Levin (1993) for a summary.

[46] The objective function can also include fame as an end in itself, not only as a means for generating income.

[47] This way of dealing with the puzzle issue is not completely satisfactory because it assumes that it is the *product* of discovery that enters the utility function, not the input of time in discovery. Yet, it is the *process* of discovery that is often reported as giving enjoyment to scientists.

declines over the life cycle. The addition of puzzle solving to the objective function produces the result that research activity is greater at any time, the greater is the satisfaction derived from puzzle solving; it also produces the strong suggestion that the research profile is flatter, the larger is the satisfaction derived from puzzle solving.[48]

Several classes of problems present themselves in studying research productivity in a life-cycle context. These include measurement, the confounding of aging effects with cohort effects, and the availability of an appropriate database.

Publication counts are generally used as a proxy for research activity. This is justified on the grounds of the high acceptance rates—often in excess of 70% (Hargens, 1988) that exist among scientific journals and that publication is a necessary condition for communicating research findings and establishing priority. The question of attribution regarding coauthored articles is sometimes addressed by prorating article counts among coauthors. Article quality is often proxied by weighting article counts by some type of citation measure.

Because scientists of different ages come from different cohorts, aging effects are confounded with cohort effects in cross-sectoral studies. One type of cohort effect is associated with change in the knowledge base of the scientific field. If, for example, there is a secular progression of knowledge (to paraphrase Mincer, 1974, p. 21), the latest educated should be the best educated and hence the most productive, other things being equal. Another factor that affects research productivity and varies by cohort is access to resources that affect research. Variation occurs primarily through fluctuations in the job market that lead certain cohorts to have relatively easy access to jobs rich in resources while others, who graduate during periods when job openings in the research sector are scarce, have considerably less access to the resources that contribute to productivity.

The presence of cohort effects dictates a research design that uses a pooled-cross-section time series database.[49] Such databases are not only costly to create: issues of confidentiality can limit access to the ones that do exist. Diamond (1986a) uses a database he assembled for mathematicians at Berkeley; Levin and Stephan develop a database by matching records from the National Science Foundation's (NSF) biennial 1973–1979 Survey of Doctorate Recipients (SDR) with publishing information from the Science Citation Index. Weiss and Lillard (1982) use a sample of Israeli scientists; Turner and Mairesse examine the productivity of solid-state physicists working at CNRS in France; Bombaradaro and Veloso examine the productivity of scientists supported by the National System of Researchers (SNI) of Mexico.

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[48] Thursby et al. (2005) expand the life cycle model to examine the effects of licensing on academic research. Their model builds on that of Levin and Stephan (1991), but divides research into a basic and applied component. The latter component has the potential of producing income through licenses. Work in progress by Doh-Shin Jeon (correspondence) suggests that life-cycle effects can be mitigated by the presence of teams in science, especially for stars. The idea is that the inclusion of a star scientist as a member of the team provides certification value. As the number of individuals working in an area increases, more scientists propose ideas and the star can select among the best. This not only increases the productivity of the star but also increases the probability that the star will remain active over a longer period of the life cycle.

[49] See Hall et al. (2007) for a discussion of the problems arising in identifying age, cohort, and period effects in studying scientific research productivity.

5.1.1. Age and publishing

Levin and Stephan analyze six areas of science. They find that, with the exception of particle physicists employed in PhD-granting departments, life-cycle effects are present in a fully specified model that controls for fixed effects such as motivation and ability.[50] For the fields of solid-state physics, atomic and molecular physics, and geophysics, the evidence suggests that publishing activity initially increases but declines somewhere in mid-career. For particle physicists at FFRDCs, as well as for geologists, the profile decreases throughout the career. The absence of life-cycle effects for particle physicists at PhD-granting institutions is not totally unexpected. Theorists working on unification are often depicted as involved in a "religious quest," handed them by Albert Einstein, or, as is commonly stated in the literature, the "search for the Holy Grail."

Diamond finds that the publishing activity of Berkeley mathematicians declines slightly with age. Weiss and Lillard use a pooled model to estimate the growth rate of publications for 1000 Israeli scientists. They find that the average annual number of publications tends to increase in the early phase of the academic career and then decline. They also find that, along with the mean, the variance of publications increases markedly over the first 10–12 years of the academic career.

Turner and Mairesse find virtually no "aging" effects for their sample of condensed matter physicists working at CNRS during the period 1986–1997. Several reasons may explain the difference between their findings and those of Levin and Stephan. These include their controlling for career stage, which is highly correlated with age, their use of a highly selective sample (by definition all of the sample are "research" scientists) while Levin and Stephan focus their research on university faculty, many of whom may not be doing research. Because their research spans a later period, they may also be picking up the fact that as publications have become more important to careers and, as more coauthors are involved, the incentives and ability to stay productive over longer periods of time have changed. They are also able to control for variables related to the lab in which the researcher works, something that Levin and Stephan could not do. Gonzalez-Brambilia and Veloso examine publication activity of Mexican researchers supported by the Mexican SNI. Their sample is restricted to individuals who have at least one publication during the period of observation. They find that conditional upon being supported by SNI and having published at least one article there is a fairly consistent level of publishing output over time within broad disciplines.

5.1.2. Age and exceptional contributions

Research on Nobel laureates suggests that the relationship between age and exceptional contribution is more pronounced than the relationship between age and productivity for what could be thought of as "journeymen" scientists. Stephan and Levin (1993), for example, find for Nobel laureates during the period 1901–1992 that although it does not require extraordinary youth to do prize-winning work, the odds decrease markedly in mid-life. The relationship is field dependent as well as dependent upon the definition used to measure the age at which the award-winning work was done. But regardless of field, the odds of commencing research for which a Nobel prize is awarded decline dramatically after

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[50] Vintage variables (discussion to follow) cannot be included in a fixed-effects model because the vintage variable is invariant over time for an individual. Equations were also estimated that included vintage variables but excluded the fixed-effects.

age 40 and very, very few laureates undertake prize-winning work after the age of 55. To wit, during the period studied less than 2% of the laureates commenced their work after age 55. Jones (2005a) finds that the age at which peak output occurs for Nobel laureates has increased over the past century. When Stephan and Levin extend their analysis to include laureates from 1993 to 2006, they find that the median age at the time laureates began their research increased by 1 year, going from 31 to 32.

5.1.3. *Age and patenting*

The increase in patenting among faculty has been accompanied by a spate of studies that examine determinants of faculty patenting behavior. The focus of some of these studies (noted in Section 4.3 of this chapter) is the relationship between patenting and publishing, with a special focus on whether the two are substitutes or complements. The focus of others is broader, examining specific determinants of patenting activity. The work of Azoulay et al. (2006) is a case in point and examines the patenting behavior of a panel of 3884 academic life scientists. Each scientist is observed from the year that he or she earned a PhD until 1999, beginning in 1967. Those who exit academia are dropped from the sample at the time they leave. The authors find "pronounced life-cycle effects on the propensity to patent, with mid-career academics being much more likely to patent than younger and older faculty members" (Azoulay et al., 2005, p. 1).[51] Thursby and Thursby (2007) examine the disclosure activity of a panel of scientists and engineers working at six universities over a 17-year period. They find that patenting declines with age; they also find that other things being equal, newer cohorts are less likely to patent than are earlier cohorts.

5.2. *The presence of cohort effects*

There are various reasons to expect scientific output to be related not only to age but also to the cohort to which the scientist belongs. Levin and Stephan, for example, focus on the relationship between productivity and the "vintage" of the scientist, arguing that certain vintages may be more productive than others. They investigate the hypothesis by identifying changes in each of the six subfields that they study that had the potential of making scientists in the subfield obsolete. Changes were identified through the use of case studies conducted through personal interviews, a small mail survey, and various publications. They then estimate a model that controls for age, time period effects,[52] and vintage. The most striking finding of this aspect of their research is that at conventional levels of significance, in no field are the latest vintages more productive than the earliest, benchmark, vintage. Stated differently, there is no evidence that the latest vintage, with supposedly the most up-to-date knowledge, engages in more research than does the earliest vintage. Furthermore, in several subfields, depending on the output measure used, there is some indication that the latest vintages are less productive than are the earliest vintages.

There is, of course, reason to believe that other types of cohort effects may exist, relating to such factors as variability in job market conditions over time or changes in the "culture" of research. For

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[51] The authors also find that patenting is often accompanied by a "flurry" of publication activity in the preceding year. They also create a variable which measures the latent patentability of the scientist's research through the use of keywords and find a positive relationship between this measure and the propensity to patent.

[52] The inclusion of time period effects is desirable given that such things as resources for research vary over time.

example, in recent years it has been particularly difficult for young life scientists in the United States to obtain tenure-track appointments, just as it has been exceedingly difficult for young researchers in Italy, France and Germany to find permanent research positions. This is in contrast to earlier times, when research budgets were growing and universities (in the case of the United States) had healthy budgets to create new positions (Stephan, 2008). Oyer (2006) has examined how variability in initial labor market outcomes affects research over the long term for a sample of economists. Oyer's research shows that "initial career placement matters a great deal in determining the careers of economists." Consistent with the cohort hypothesis, the effect persists holding innate ability constant; that is, initial placement matters independent of ability.

5.3. Gender

The presence of a gender differential in publishing outcomes is well established. Fox (2005), for example, finds that women published or had accepted for publication 8.9 papers in the 3-year period beginning in the early 1990s, compared to 11.4 for men. The difference owes to disparities at both extremes of the productivity distribution. Women are almost twice as likely as men to publish zero or one paper during the period (18.8% compared to 10.5%); men are almost twice as likely as women to publish 20 or more papers during the period (15.8% for men compared to 8.4% for women).[53] Gender differentials have also declined over time. Xie and Shauman (1998) find the female-to-male ratio to have been about 0.60 in the late 1960s, and to have increased to 0.82 by 1993.

The question as to why research output is related to gender has long interested those studying scientific productivity. In economic terms, the question is often examined in terms of supply versus demand characteristics. Stated in these terms, the question is whether women publish less than men because of specific attributes, such as family characteristics, amount of time spent doing research, etc., or whether women publish less than men because they have fewer opportunities to be productive, due to hiring and funding decisions as well as possible network outcomes. This dichotomy is misleading, of course, to the degree that interactions exist between the two. Differential placement opportunities, for example, may lead women to allocate their time to activities that are rewarded (such as teaching) but diminish publishing activity. One of the most in-depth studies to be done on the subject in recent years is that by the sociologists (Xie and Shauman, 1998, 2003, p. 23). After carefully analyzing four datasets that span a 24-year period, they conclude that "women scientists publish fewer papers than men because women are less likely than men to have personal characteristics, structural positions, and facilitating resources that are conducive to publication." In other words, both demand and supply play a role.

The increase in patenting among academic scientists raises the question of whether differential patenting patterns exist by gender. The question is of interest not only because patenting is another indicator of output but also because of the role that royalty payments from patents can play in remuneration as well as the role that patents arguably play in exchange and hence, indirectly, in fostering productivity (see above).

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[53] Kelchtermans and Veugelers (2007) find that, relative to men, women faculty at Katholieke Universeteit Leuven are more likely to consistently not publish and slightly less likely to be in the top performance category.

The most thorough study of patenting to date has been Ding et al.'s study (2006a) of life scientists who received their PhDs between 1967 and 1995. Among the 4227 in the sample who had at least a 5-year history of post-PhD publishing at an academic institution, women were found to be far less likely to have at least one patent then men: 5.65% of the women in the sample; 13.0% of the men. The hazard models that they estimate indicate that gender differences cannot be entirely explained away by such things as contact with industry, number of coauthors, past publications, institutional support for patents (as measured by the number of patents the institution has received), or subfield. Although controlling for these measured characteristics reduces gender disparity, the coefficient on women in their proportional hazard model remains positive and significant, indicating that other things being equal women patent at 0.40 times the rate of men. In light of the earlier discussion, it is interesting to note that they also find indication of strong cohort effects. The cumulative hazard for patenting for those who received their PhDs in the earliest period studied (1967–1975) was 4.4 times higher for men than that for women; the differential declined to 2.1 for those who received their PhDs in the middle period (1976–1985) and further declined to 1.8 for those who received their PhDs in the latest period studied (1986–1985). These findings are consistent with the views older women expressed in interviews conducted by the authors that they felt excluded from industry relationships early in their careers and were never able to develop an understanding of how commercial science works.[54]

5.4. Inequality

A defining characteristic of contests that have winner-take-all characteristics such as those that exist in science is extreme inequality in the allocation of rewards. Science, too, has extreme inequality with regard to scientific productivity and the awarding of priority. One measure of this is the highly skewed nature of *publications*, first observed by Lotka (1926) in a study of nineteenth century physics journals. The distribution that Lotka found showed that approximately 6% of publishing scientists produce half of all papers. Lotka's "law" has since been found to fit data from several different disciplines and varying periods of time (Price and Solla, 1986).[55]

Patents are even more highly skewed than are publications. Stephan et al. (2007b), for example, find for a sample of 10,962 US academics studied over the 5-year period 1990–1995, that 90.1% reported zero patents; 8.7% reported 1–5 patents; 0.4% reported 6–10, and 0.1% reported greater than 10. By comparison, only 14.4% reported zero publications, 40.8% reported 1–5 publications, 20.9% reported 6–10, and 23.9% reported over 10 articles.[56]

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[54] There is also considerable evidence that a gender gap exists in entrepreneurial activity among university scientists (Stephan and El-Ganair, 2007).

[55] Lotka's law states that if k is the number of scientists who publish one paper, then the number publishing n papers is k/n^2. In many disciplines this works out to some 5% or 6% of the scientists who *publish at all* producing about half of all papers in their discipline. Although Lotka's Law has held up well over time and across disciplines, David (1994) shows that other statistical distributions also provide good fits to observed publication counts.

[56] Inequality appears in other dimensions of science, as well. Terviö (2006) measures departmental influence using a method similar to that used by Google to rank web pages and finds for the fields studied that the distribution of influence is significantly more skewed than the distribution of academic placements.

Inequality in scientific productivity could be explained by differences among scientists in their ability and motivation to do creative research (to have the "right stuff"). But scientific productivity is not only characterized by extreme inequality at a point in time; it is also characterized by increasing inequality over the careers of a cohort of scientists, suggesting that at least some of the processes at work are state dependent. Weiss and Lillard (1982), for example, find that not only the mean but also the variance of publication counts increased during the first 10–12 years of the career of a group of Israeli scientists.

Merton christened his explanation for inequality in science the Matthew Effect, defining it to be the accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark (Merton, 1968, p. 58).

He argues that the effect results from the vast volume of scientific material published each year, which encourages scientists to screen their reading material on the basis of the author's reputation. Other sociologists (e.g., Allison and Stewart, 1974; Cole and Cole, 1973) have argued that additional processes of "cumulative advantage" are at work in science, such as the ability to leverage past success into research funding as well as the "taste" for recognition that success engenders. A funding system such as NIH's that awards grants, at least in part, on past success clearly contributes to cumulative advantage. While the interaction of the "right stuff" and the processes of cumulative advantage are not fully understood, a strong case can be made that a variety of factors are at work in helping able and motivated scientists leverage their early successes and that some form of feedback mechanism is at work (David, 1994). This observation is consistent with other work in winner-take-all contests. Frank and Cook (1992, p. 31) observe that "in all their manifestations, winner-take-all effects translate small differences in the underlying distribution of human capital into much larger differences in the distribution of economic reward."

6. Efficiency considerations and funding regimes

6.1. Efficient nature of the reward system

The socially desirable properties attached to a reward system that is priority-based are substantial. Priority solves the monitoring problem. "Since effort cannot in general be monitored, reward cannot be based upon it. So a scientist is rewarded not for effort, but for achievement." (Dasgupta and David, 1987, p. 530). Priority also means that shirking is rarely an issue in science. The knowledge that multiple discoveries are commonplace makes scientists exert considerable effort.[57] A reward structure based on priority requires that scientists share information in a timely fashion if they are to establish priority. Such a process in turn permits peer evaluation, which discourages plagiarism and fraud and builds consensus in science (Dasgupta and David, 1987; Ziman, 1968). The process also provides scientists the reassurance that they have the capacity for original thought (Merton, 1957) and encourages scientists to acknowledge the roots of their own ideas, thereby reinforcing the social process. Reputation also serves as a signal of "trustworthiness" to scientists wishing to use the results of another in their own research without incurring

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[57] The prevalence of multiples in science is discussed below. Fox (1983) and Hull (1988) discuss the effort and work patterns of successful scientists.

the cost of reproducing and checking the results. It also serves as a signal of trustworthiness to foundations. As such, reputation provides an answer to the agency problem (Turner, 1994) posed by Coase (1937).[58]

From an economist's point of view, an exceedingly appealing attribute of a reward system that is rooted in priority is that it offers nonmarket-based incentives for the production of the public good "knowledge." (Stephan, 2004). Merton noted the functionality of the reward system in the inaugural lecture of the George Sarton Leerstoel that he delivered October 28, 1986 at the University of Ghent. In the lecture, published 2 years later in *Isis*, Merton spoke of the public nature of science, writing that "...a fund of knowledge is not diminished through exceedingly intensive use by members of the scientific collectivity—indeed, it is presumably augmented..." (Merton, 1988, p. 620). Merton not only recognized this but stood the public–private distinction on its head, proposing that the reward structure in science of priority functioned to make a public good private. "I propose the seeming paradox that in science, private property is established by having its substance freely given to others who might want to make use of it." He continues (1988, p. 620) by saying that "only when scientists have published their work and made it generally accessible, preferably in the public print of articles, monographs, and books that enter the archives, does it become legitimately established as more or less securely theirs" or, as he says elsewhere, "one's private property is established by giving its substance away" (1988, p. 620).

Dasgupta and David (1987, p. 531) express the private–public paradox exceedingly well: "Priority creates a privately owned asset—a form of intellectual property—from the very act of relinquishing exclusive possession of the new knowledge." Arrow (1987, p. 687), commenting on their work, articulates the cleverness of such a system:

> *"The incentive compatibility literature needs to learn the lesson of the priority system; rewards to overcome shirking and free-rider problems need not be monetary in nature; society is more ingenious than the market."*

6.2. *Funding regimes*

The conventional wisdom holds that because of problems related to appropriability, public goods are underproduced if left to the private sector. Although priority goes a long way toward solving the appropriability problem in science, this ingenious form of compensation does not insure that efficient outcomes will be forthcoming. In addition to problems caused by uncertainty and indivisibilities

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[58] This is not to say that the reward structure is without problems. Fraud and misconduct occur with some frequency in science (Kohn, 1986). In recent years there have been several high-profile cases involving misconduct and fraud, including the fabrication of data by Woo Suk Hwang regarding the creation of embryonic stem cells (various online sources) and the University of Wisconsin researcher, Elizabeth Goodwin, who, according to a University of Wisconsin investigation, falsified data in grant applications (Couzin, 2006). In China an "unprecedented number of researchers stand accused of cheating—from fudging resumes to fabricating data—to gain fame or plumb positions" (Xin, 2006, p. 1464). According to Lu Youngxiang, president of the Chinese Academy of Science (CAS), "Too many incentives have blurred the reasons for doing science in some people's minds." (p. 1464). Feigenbaum and Levy (1993) discuss the market for (ir)reproducible results; Fox and Braxton (1994) discuss other issues related to fraud. There is also the considerable issue that the reward structure in science appears to have favored white and Asian men over women and members of underrepresented groups.

(Arrow, 1962), there is the problem that scientific research requires access to substantial resources. Unless priority can be translated into resources, it cannot come close to generating a socially optimal amount of research. Research must be subsidized, by either the government or philanthropic institutions.[59] The government's rationale for supporting scientific research also rests on the importance of research and development to defense, the desire to win what Johnson (1972) calls the "Scientific Olympics"; and the importance of science to economic growth.

Many countries fund scientists indirectly by supporting the research institutes where they work, such as the CNRS in France, the CNR in Italy, and the Institute of Molecular and Cell Biology in Singapore. In some instances this means that scientists are directly employed by the government; in other instances the funds pass from the government to the institute where the hiring arrangements are made. The practice of funding the institute rather than the scientist is less common in the United States, especially in academe, but the practice exists outside academe. The National Institute of Standards and Technology (NIST), for example, which is federally funded, operates on such a model and has been the research home of several Nobel laureates. FFRDC's, of which SLAC (formerly called the Stanford Linear Accelerator) is an example, work on such a model as well and NIH has several large intramural research programs. Notwithstanding the above, competitive processes also exist outside the United States, especially in Europe, for funding researchers. In the United Kingdom, for example, researchers are supported by a grants program administered by various councils such as EPSRC—Engineering and Physical Research Council; in Belgium, the Flemish Science Foundation (FWO) provides a peer-review system for supporting research. The European Union has long supported research through the "Framework Program," which is now in its seventh form (Seventh Framework Program (EP7)). A particular focus in recent years has been the fostering of networks across countries and universities.[60] In an effort to encourage peer-reviewed-investigator-initiated research, the European Research Council (ERC) was established in 2006 with a focus on "cutting-edge" basic research (Vogel, 2006, p. 1371).

As noted earlier, in the United States, scientists working in academe and at certain research institutes are responsible for raising their own funds through the submission of proposals to funding agencies. The largest agencies funding academic researchers are NIH, NSF, Department of Defense (DOD), and Department of Energy (DOE), in that order. While each agency uses a somewhat different approach in evaluating projects, NIH and NSF rely on peer review.[61] The NIH review process puts considerable weight on the presence of preliminary data as well as past accomplishments, in terms of publications as well as of "lineage" as measured by where the scientist trained and did postdoc work. NSF puts less emphasis on reputation, but does require a two-page bio.

Reputation also plays a role in the funding available to academic departments and research institutes. In the United Kingdom, for example, departments receive funding based in part on the quality of their research and the number of students, through the Research Assessment Exercise (RAE) which occurs

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[59] Callon (1994) proposes that public support of science is needed to ensure that multiple lines of inquiry remain open.

[60] The FP7, as proposed, has four parts: (1) funds to foster cooperation for applied research projects that require participation from many labs or companies across the continent; (2) funds to support portable Marie Curie grants for young researchers; (3) funds to support new research infrastructure; and (4) funds for the newly created European Research Council (Enserink, 2006).

[61] Not all funds dispersed by government agencies are awarded by a process of evaluation. A common practice in the United States (De Figueiredo and Silverman, 2007) is for universities to receive funds "earmarked" by Congress at the time of the appropriation.

every 5 years. A related system exists in the Netherlands.[62] In Germany, the Wissenschaftsrat evaluates the institutes that are to be placed on the blue list (Blaue Liste). One consequence of such a system is the recruitment of stars to bolster rankings ("just-in-time hiring") and thus funding.

Certain private foundations also support research at universities. The largest in the United States is the Howard Hughes Medical Institute, which funds research in the life sciences. A number of smaller foundations as well as disease-specific foundations (such as the American Cancer Society) also support research. Funding of research by philanthropic organizations also occurs outside the United States. In France, for example, the Association Against Myopathies (AFM) funds a considerable amount of research in the biomedical sciences; the Wellcome Trust in the United Kingdom is the world's largest medical research charity, funding research in human and animal health.

Industry also provides support for academic research; the importance of this source grew during the 1980s and 1990s in the United States as well as in other OECD countries (National Science Board, 2006, Figure 4-44). Moreover, in certain countries the amount of support is substantial. In Germany, for example, industry currently supplies 12% of funding for academic research and in Canada industry supplies close to 8%. By these standards, the percent of academic research supported by industry in the United States (which peaked at around 6% in 1999) is modest.

Governments (and to a much lesser extent nonprofit foundations) also support research by encouraging the study of science and engineering through the provision of fellowships. Such funds can be targeted directly to students (as in the case of NIH training grants, NSF dissertation awards, and Marie Curie awards) or indirectly, through the support of faculty research which includes funds for graduate students and postdoctoral students. The amount of funds provided can vary considerably over time and in response to perceived needs, as occurred when the United States responded to the launching of Sputnik by creating the National Defense Education Act to encourage the study of science in the late 1950s.[63]

Differences in funding regimes raise the question of whether knowledge advances more rapidly under the peer-review grants system or under the "institute" approach. The issue, to the best of our knowledge, has been ignored by the economics profession. It is therefore hoped that the *ad hoc* discussion that follows will stimulate research on this important topic.

The institute approach has its benefits: it insures that scientists can follow a research agenda (with an uncertain outcome) over a substantial period of time, and it exempts scientists from devoting long hours to seeking resources. These benefits are not trivial.

The costs of the institute approach are also substantial. Foremost is the question of the research agenda. In many institutes the agenda is set by the director, and younger scientists are constrained from following leads they consider promising. The guarantee of resources also encourages shirking; consequently, alternative methods of monitoring must be found. The institute approach also enhances stratification in science and hence the possible waste of human resources. Most appointments are made early in the career. If the scientist does not succeed in getting a tenured appointment, the scientist

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[62] In late 2006, the Reading University became the 21st university in the United Kingdom since 1997 to announce the closure of its physics department. The reason: not enough new students—or enough research income (Another Physics Department Down, 2006).

[63] The focus of government funding, as well as the amount, is also quite variable. For example, in recent years in the United States, while funds for the life sciences have grown significantly, funds for the physical sciences, earth sciences and mathematics have languished, as have funds more recently for engineering research.

will have minimal access to resources in that country for the rest of his or her career. One effect of such a system is to encourage migration of those who do not obtain such an appointment.

The grants system also has its benefits. It encourages scientists to remain productive throughout the life cycle, because scientists who wish to have a lab must remain active. To the extent that success in the grants system is not completely determined by past success, the system provides some opportunity for last year's losers to become this year's winners. Peer review arguably promotes quality and the sharing of information. The system also encourages entrepreneurship among scientists. Getting money from a venture capitalist is not that much different from getting money from a funding agency. Both require making a "pitch."

Just as some of the benefits of the grants system are costs of the institute system, so, too, some of the benefits of the institute approach are costs of the grants system. Grant applications and administration divert scientists from spending time doing research.[64] A 2006 survey of US scientists found that scientists spend 42% of their research time filling out forms and in meetings; tasks split almost evenly between pregrant (22%) and postgrant work (20%). The tasks cited as the most burdensome were filling out grant progress reports, hiring personnel and managing laboratory finances (Kean, 2006).[65] The grants system also encourages scientists to choose sure(r) bet short-term projects that in the longer run may have lower social value. The system also implicitly encourages scientists to misrepresent their work or the effort required to generate certain outcomes. It is typical, for example, for scientists to apply for work that is nearing completion (yet not acknowledge the degree to which it has been performed) and to use some of the proceeds of funding to support research that may lead to future funding or research of a riskier nature that may not be fundable.

The process used to evaluate proposals is not without its problems. For example, considerable concern exists regarding the peer-review system used to evaluate proposals. At NIH, for example, the increased number of proposals that accompanied the doubling of the budget led to an increasing percentage of proposals being triaged and thus not reviewed. Agencies report problems getting individuals, especially experienced individuals, to be reviewers, and the charge has been made that the quality of reviews is declining. A related issue is the extent to which scientists engage in "gift-giving" by awarding favorable reviews to acquaintances and coauthors.

While the grants system, in theory, should be more open than the institute system, there is evidence that early career scientists are having difficulty at NIH. The average age at which one receives first independent funding increased from 37.3 to 42.0 between 1985 and 2005; the percent of R01 grants awarded to individuals 40 or younger fell from 25.2% in 1995 to 15.0 in 2005, while the percent awarded to individuals 51 or older increased from 29.1 to 45.8.[66]

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[64] The Framework programs in the European Union award contracts, rather than grants. The ERC will award research grants instead, which are viewed by ERC leadership as being potentially less burdensome (Vogel, 2006).

[65] The survey was completed by 6083 university scientists. The study was sponsored by the Federal Demonstration Partnership, a coalition of university and federal officials interested in streamlining government research regulations.

[66] The R01 is the basic independent research grant awarded by NIH; more than 50% of the Institute's resources are used to support R01 research. The data come from the Office of Extramural Research (OER), NIH.

6.3. Are there too many contestants in certain contests?

Governments also encourage the production of knowledge by granting property rights to the discoverer. With rare exception, patents have been the primary form of intellectual property rights that economists have examined, arguing that patents provide for appropriability while placing knowledge in the public domain.[67] Moreover, it has been shown (Dasgupta and Stiglitz, 1980) that under a wide array of circumstances social inefficiency results from patent races among rival groups. This inefficiency manifests itself in "excessive duplication of research effort (or) . . . too fast a pace of advance of the frontiers of knowledge" (Dasgupta and David, 1987, p. 532).

The recognition that priority is a form of property rights leads to the question of whether there are "too many" contestants in certain scientific contests. Would the social good be served by having fewer? In a speech delivered at the conference commemorating the 400th anniversary of the birth of Francis Bacon, Merton detailed the prevalence of what he called "multiples" in scientific discovery. And Merton was not the first to note their presence. In what Merton calls a "play within a play," he gives 20 "lists" of multiples that were compiled between 1828 and 1922. Moreover, Merton is quick to point out that the absence of a multiple does not mean that a multiple was not in the making at the time the discovery was made public. This is a classic case of censored data where scooped scientists abandon their research after a winner is recognized. Indeed, Merton argues that "far from being odd or curious or remarkable, the pattern of independent multiple discoveries in science is in principle the dominant pattern rather than a subsidiary one." (Merton, 1961, p. 356).[68]

The presence of multiple discoveries is due in part to the free access scientists have to knowledge and in part to the fact that uncertainty associated with who will make a discovery leads scientists to choose research portfolios that are correlated (Dasgupta and Maskin, 1987).[69] The knowledge that multiples exist keeps scientists from shirking and moves the enterprise of science at a rapid pace. Such observations invite the question of whether science moves at too rapid a pace and whether certain contests attract too many entrants. Dasgupta and David (1987, p. 540) argue that the priority system can create excesses, just as the patent system does, provided the "reward to the discoverer . . . is tempting enough." They make no effort to define the boundary of temptation, but one wonders if the general knowledge that certain contests deserve the Nobel Prize does not attract an excessive number of scientists.[70]

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[67] While neither goal is perfectly achieved by the patent process, the goal of disclosure arguably suffers the most. "The imperfections we have examined in the patent as a device for rewarding disclosures of knowledge are not at all surprising; a stone flung at two birds really ought not be expected to make a clean strike on either" (Dasgupta and David, 1987, p. 534).

[68] Stigler (1980) argues that multiples are less common than Merton assumes and that incomplete knowledge of who "is working on a problem and what his achievement will be" is the only reason why full multiples should occur.

[69] Despite the popularity of patent race models, multiples are arguably more common in science than technology. The reason is that science is concerned with laws and facts, while technology is looking for practical ways to solve problems. Hence, while there is often only one answer to a scientific question, there usually are a variety of distinct ways of solving the practical problem.

[70] On the other hand, the common lament of interest groups that there are not enough entrants in certain races of apparent Nobel proportions (e.g., a cure for breast cancer) leads one to be cautious in making broad generalizations. It is, of course, possible that such groups are expressing the concern that victory is undervalued by the community. It is also possible that a cure is not "in the air" and applying more resources to the contest would be inefficient.

A related question is whether scientists at universities direct too much time to research as opposed to teaching. The fact that only a handful of scientists contribute the lion's share of output suggests that substantial inefficiencies arise when yeomen scientists devote long hours to research. Efficiency concerns also arise with regard to the large number of individuals working in postdoctoral positions. The work of Lazear and Rosen (1981) suggests that an efficient allocation of resources can result in a tournament model, such as that which exists in science. But while rock stars, opera singers and soccer players do not have tenure, professors do. This means that creative young scientists, despite their demonstrated ability, may find it difficult to secure a lab of their own, especially if the number of tenure-track positions does not grow.[71]

6.4. *The incentive to share knowledge in a timely fashion*

Despite the similarities between priority rights and proprietary rights such as patents, they differ markedly in the incentives they provide to disclose research findings in a timely fashion. On the one hand, the quest for priority requires scientists to share discoveries quickly because it is only by sharing that priority rights can be established. The quest for proprietary rights, on the other hand, discourages the rapid sharing of information, because the very purpose of proprietary rights is to provide a means for capturing the economic rents attached to a new product or technology. And, while some forms of proprietary rights require the sharing of knowledge in recognition of its public nature (e.g., the patent process), incentives to divulge the knowledge *quickly* are not present.[72]

The distinction is so crucial that Dasgupta and David (1987, p. 528) argue that the two types of property rights, and the implications they hold for appropriability and disclosure, differentiate science from technology: "If one joins the science club, one's discoveries and inventions must be completely disclosed, whereas in the technology club such findings must not be fully revealed to the rest of the membership."

This distinction between science and technology often leads to the (erroneous) conclusion that science is done by scientists at universities and public labs and results in published knowledge, while the focus of scientists working in industry is the development of proprietary technology (Nelson and Winter, 1982).[73] While location does correlate with the incentive to share knowledge in a timely fashion, the relationship is far from perfect. Some firms make the results of their research public; some academics engage in tactics that lead to the "privatization" of knowledge. In many instances

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[71] Other efficiency concerns exist. One is the degree to which the process of cumulative advantage excludes talented individuals from making contributions. Another is the question of whether there is a critical point at which additional resources allocated to a scientist for research lead to marginal results. The question is of policy importance given that during the doubling of the NIH budget some extremely successful scientists went from having two R01 research grants to having three or more. Dasgupta and David (1994, pp. 506–507) discuss additional efficiency concerns.

[72] A patent application in the United States must be filed within 1 year of publication. Many other countries require that the patent application be filed prior to publication.

[73] Philippe et al. (2008) specify a model that does not rely on the public nature of research as the rationale for academic research but rather on control-rights consideration. To be more specific, they argue that "the fundamental tradeoff between academia and the private sector is one of creative control versus focus." (p. 617). In academe, scientists are free to pursue their own interests; thus academia fosters early-stage research. By way of contrast, the private sector directs scientists toward higher payoff research that is of a later stage.

agents can eat their cake and have it too, selectively publishing research findings while monopolizing other elements with the hope of realizing future returns. Eisenberg (1987) argues that such behavior is more common among academics than might initially be presumed because they can publish results and at the same time keep certain aspects of their research private by withholding data, failing to make strains available upon request, or restricting the exchange of research animals, such as mice.[74] If such were the case in 1987, one might hypothesize it to be even more so today, as academic scientists increasingly engage in patenting. But intellectual property does not appear to play a major role in restricting access to knowledge and materials used in subsequent research. Walsh et al. (2007) find that access is largely unaffected by patents, primarily because of issues related to enforceability. But access to the research materials of others, such as cell lines, reagents, and antigens, is restricted: 19% of the material requests made by their sample were denied. Competition among researchers played a major role in refusal, as did the cost of providing the material. Whether the material in question was a drug or whether the potential supplier had a history of commercial activity were also relevant factors in refusal.[75] This is not to say that instances do not exist where patents play a major restrictive role. A recent example concerns human embryonic stem cells. The University of Wisconsin, where they were discovered, has used their control, both through patents and material rights to the cell lines, to impose limits and conditions on other academics (Murray, 2007).

The ability to eat one’s cake and have it too is not only facilitated by the fact that publication is not synonymous with replicability. It is also facilitated by the fact that certain kinds of knowledge, especially knowledge that relates to techniques, can often only be transferred at considerable cost, in part because their tacit nature makes it difficult, if not impossible to communicate in a written form (or codify). Tacit knowledge is thus “sticky” (Von Hippel, 1994) and requires face-to-face contact for transmission. It is one reason, as we will see, for arguing that knowledge may be geographically bounded and hence for expecting spillovers to have a geographic dimension.[76] The private aspect of technology is a major reason patents are not a necessary condition for successful research and development and underlies the willingness of industry to share knowledge through publications.

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[74] Eisenberg (1987) suggests that the patent process may be more congruent with the scientific norms of disclosure and replication than the publishing process in certain areas of the life sciences. This is because patents in the biological sciences require that the material in question be placed on deposit. This is not a requirement for publication; neither are materials themselves part of the published text.

[75] There is the closely related anticommons issue of how multiple property right claims, sometimes in the hundreds, dampen research by requiring researchers to bargain across multiple players to gain access to foundational, upstream discoveries (Heller and Eisenberg, 1998). Walsh et al. (2007) ask academic respondents reasons that may have dissuaded them from moving ahead with a project. Lack of funding (62%) or being too busy (60%) were the most commonly reported reasons. Scientific competition (29%) was also an important reason given for not pursuing a project. Technology control rights related to terms demanded for access to inputs (10%) and patents (3%) were significantly less likely to be mentioned.

[76] Some aspects of technical knowledge have a strong tacit component, meaning that they cannot be completely codified and made explicit in the form of blueprints or instructions, but instead must be learned through practice. Nelson and Winter (1982) discuss tacit knowledge, particularly as it relates to skill, as does Foray (2004). Dasgupta and David (1994) use the term tacit somewhat differently to connote knowledge that, for whatever reason, is not codified and argue that the boundary between what is codified and what is tacit is not simply a question of epistemology. Rather, as suggested above, the boundary is “a matter, also of economics, for it is determined endogenously by the costs and benefits of secrecy in relation to those of codification.” (p. 502).

There are other reasons why firms engage in disclosure. Foremost among these is recruitment of talent. Scientists and engineers working in industry value the ability to publish and are willing to pay for the privilege. Stern (2004) finds for a sample of postdoctoral biologists that firms which allow their employees to participate in the norms of science by publishing pay on average 25% less than firms which do not.[77] It is not only an interest in priority; the ability to publish allows scientists to maintain the option of working outside the for-profit section. The reputation of the lab, which is directly related to publication activity, also affects the ability of the company to hire scientists and engineers (Scherer, 1967); it may also affect its ability to attract government contracts (Lichtenberg, 1988). Hicks (1995) explores a number of other factors leading companies to opt for disclosure through publication. A critical element is the company's ability to screen the material that is published, thereby insuring that its proprietary interests are maintained. In the process, however, the firm must be mindful that delays can lower morale among research scientists. Hounshell and Smith (1988, p. 369) describe the loss of morale that occurred at Du Pont when research managers implemented what turned out to be a de facto moratorium on publishing.

From Table 1 we see that industry authors approximately 16,000 articles a year (measured by fractional counts) or about 7.5% of all articles with a US author. The number of articles peaked in the early 1990s and then declined for the next 10 years; output of the for-profit sector shows a modest increase in 2003. Declines were most notable in the fields of chemistry, physics, and engineering and technology (National Science Board, 2006). Coauthorship patterns, which are not shown in the table, are also of interest. During the same period, the coauthorship share (measured on a whole-count basis) of the private-for-profit sector with academe increased from 31.1% in 1988 to 47.3% in 2003 (National Science Board, 2006, Table 5-22).

7. Scientists in industry

Approximately two million researchers were employed in business enterprises in EU-15 countries, Japan and the United States as of 1999. Slightly more than 50% of these were working in the United States, where the percent of researchers working in the private sector is approximately 83%, compared to 51% in EU-15 and 67% in Japan (European Commission, 2003, Table 4.1.1 and Figure 4.1.4). Although research in industry is not restricted to those with a PhD, many PhDs do work in industry, especially in the United States where fully one-third of individuals with an S&E PhD work in the private sector. Moreover, the percent has grown in recent years, rising from around 23% in 1973 to 36% in 2003 (see Table 2).

Although the general assumption is that scientists and engineers are hired by industry to work in R&D, Stephan (2002) shows this to be an oversimplification, documenting that many scientists and engineers are employed outside the traditional R&D activities of firms. Some of this undoubtedly reflects promotion to managerial levels. But it also reflects the fact that innovation occurs in non-R&D sectors of firms, such as in sales, acquisitions and communications. Thus, studying scientists and engineers working in industry can provide another view (and measure) of innovative activity, different from such measures as R&D-expenditure data or patent-count data. Studying the employment pattern of scientists and engineers in the private sector also sheds light on sources of growth in the

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[77] The finding depends on the inclusion of researcher fixed effects, leading Stern to conclude that the finding is conditional on scientific ability.

Table 2
Sector of employment of doctoral scientists in the United States, 1973–2003 (%)

Year	Total	Business/industry	Universities and 4-year colleges	Federal government	Other
1973	130,355	30,887 (23.7)	77,289 (59.3)	12,522 (9.6)	9657 (7.4)
1979	175,588	45,518 (25.9)	100,073 (57.0)	15,634 (8.9)	14,363 (8.2)
1985	218,328	64,962 (29.8)	119,365 (54.7)	16,860 (7.7)	17,141 (7.9)
1991	233,303	82,166 (35.2)	114,417 (49.0)	17,616 (7.5)	19,104 (8.2)
1997	279,430	97,300 (34.8)	133,530 (47.8)	23,670 (8.5)	24,930 (8.9)
2003	321,950	114,580 (35.6)	150,550 (46.8)	25,550 (7.9)	31,270 (9.7)

Fields included in the definition of science are: physical, mathematical, computer, environmental, and life. Self-employed are included in business and industry. “Other” includes state and local government, private not-for-profit, “other” educational institutions and “other.”

Note: The dramatic changes in 1991 may in part reflect a change in survey methodology.

Source: Stephan (1996, Table 2), National Science Foundation (1999, Table 17), and National Science Foundation (2006, Table 13).

nonmanufacturing sector of the economy. For example, in recent years, PhD scientists and engineers have been increasingly employed in the service sector of the economy and have arguably contributed to the growth that the sector has experienced in recent years.

Firms engage in research for a variety of reasons. In some instances the research is a by-product of the development of a new product or process (Rosenberg, 1990). In other instances, the production of generic knowledge is, itself, the goal and is motivated by the belief that a particular new product or process innovation will result from that knowledge. Research activities (and the related publications) can also be a signal that the firm is worthy of receiving third-party funds, either in the form of research grants, such as Small Business Innovation Research (SBIR) awards, or venture capital. Allowing scientists and engineers to engage in basic research is also a recruitment mechanism, as noted above. Basic research is needed if the company is to stay abreast of developments in relevant scientific fields and more readily absorb the findings of other scientists (Cohen and Levinthal, 1989). Sometimes firms are motivated by the expectation that fundamental research will provide a scientific foundation for the company’s technology. Firms have even been known to engage in basic research because of a concern that the fundamental knowledge required for the industry to advance is lacking and unlikely to be forthcoming from the academic sector. When Charles Stine made his presentation to the Executive Committee of Du Pont in 1926, for example, he argued that fundamental research was necessary because “applied research is facing a shortage of its principal raw materials” (Hounshell and Smith, 1988, p. 366).

This means that the research of some scientists and engineers working in firms is virtually indistinguishable from that of their academic counterparts.[78] This used to be especially the case for scientists employed at major industrial labs such as Bell Labs, IBM, and Du Pont. It remains the case for scientists and engineers working in certain sectors, such as pharmaceuticals, medical devices, and IT, but the

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[78] A number of scientists and engineers from industry have received the top honors that their field can bestow. Bell Labs, Du Pont, IBM, Smith Kline and French, Sony, and General Electric have each been the research home to scientists who have subsequently won the Nobel Prize. In 1994, 3.8% of the then 2088 members of the US National Academy of Sciences came from industry.

demise of Bell Labs and the change in mission of IBM and Du Pont has been one of several factors contributing to the decline in "basic" research performed in industry in the United States.[79]

The reasons for industry to publish research findings, as well as the economic incentives for adopting a basic research agenda, have been noted above. This should not, however, be taken as an indication that economists (or others, for that matter) have adequately studied scientists in industry doing "science" and the role that pecuniary and nonpecuniary incentives play in innovation. Sauermann and Cohen (2007) are beginning to address this void by analyzing, at the individual level, the impact that preferences for benefits and job characteristics have on innovative effort and performance. But many questions remain unanswered and—perhaps even more fundamental—unposed.[80] For example, why do companies adopt compensation strategies that impair the productivity of scientists by frequently tying salary increases to the assumption of managerial responsibilities? Does the strategy adopted by IBM and Du Pont of creating well-paid research-fellow positions help alleviate the problem? What role do publications play in facilitating movement between the industrial and nonprofit sectors? How is basic research in industry monitored? The unpredictable nature of research, as well as the belief that creativity requires freedom of choice, suggests that success is hampered if company scientists are managed too closely. Yet firms can ill afford to fund research that has little promise of (eventually) relating to the company's objectives.[81]

8. Scientific labor markets

Science emerged from World War II with enhanced respect. Its successes were credited with shortening the war and reducing fatalities of Allied troops. There was also a growing appreciation of the important role science could play in stimulating economic growth and employment in peacetime. In a report prepared at the invitation of the White House, Bush (1945) argued that science provided an endless frontier and should be more heavily supported by the government. One response to Bush's report was the formation of the US National Science Foundation in 1950.[82]

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[79] In the mid-1950s, approximately one-third of basic research performed in the United States was done by industry; in 2004, the last year for which data are available, the proportion had declined to approximately 16% (National Science Board, 2006, Table 4-8, vol. 2). Other factors contributing to the decline, in addition to the closure or refocusing of certain large industrial labs, include an increased propensity to "outsource" research to the university sector, as well as possible changes in definition and classification. At the same time that industry's share of basic research declined, their share of applied research rose from 56.3% to 61.8% (National Science Board, 2006, Table 4-12, vol. 2); the combined share of basic and applied research went from 50.1% to 40.3%.

[80] Our knowledge of scientists working in industry comes largely from a number of excellent case studies. These include Gambardella's (1995) study of the pharmaceutical industry, Hounshell and Smith's (1988) study of Du Pont, Willard Mueller's discussion of Du Pont (1962); Nelson's (1962) study of the development of the transistor, and Sobel's (1986) study of RCA. For a discussion of specific industries, see Mowery and Rosenberg (1998).

[81] Scherer (interview) reports that Bell Labs solved this problem by giving "the glassy-eyed stare" to scientists who were seen as straying too far from the Lab's purpose. Recipients knew that they had the choice of either modifying their research or being ostracized.

[82] The Bush report personifies the linear model to the extent that it argues that innovations flow out of basic research. But ironically the Bush report contributed to the nonlinearities of the innovation system by growing the scientific labor force available to work in industry. The effect was indirect but profound. Research grants awarded to PIs created a demand for doctoral students and postdocs. And the newly trained increasingly headed to industry as the academic sector proved less and less able to absorb the increased supply of newly trained PhDs.

The groundswell of support for science, heightened in the West in the 1950s by the threat of Soviet scientific and technological superiority, underscored the need to understand the workings of scientific labor markets. Stellar talent was drawn to this question. First, Blank and Stigler (1957) published a book on the demand and supply of scientific personnel; then Arrow and Capron (1959) wrote an article concerning dynamic shortages in scientific labor markets. Both studies set the stage for work to come.

8.1. A description of scientific labor markets

8.1.1. Where they train

The United States and Europe (defined here to be France, Germany, and the United Kingdom) were for many years the primary producers of PhDs in the natural sciences and engineering, as is seen in Figure 3. (The jump in the European data in 1989 is due to the inclusion of French data which prior to that date were unavailable in series form.) This pattern changed, however, in the 1990s, when the number of PhDs awarded in Asia began to rise rapidly and now surpasses the number produced in the United States. Part of the increase in the number of PhD's awarded in Asia reflects an increased proclivity for

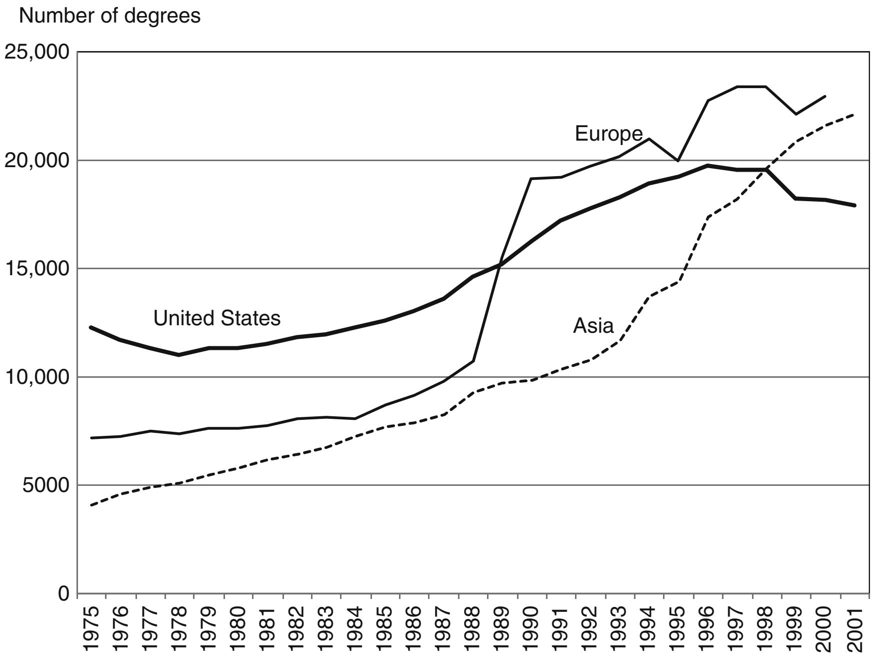

Figure 3. NS&E doctoral degrees in the United States, Europe, and Asia: 1975–2001. *Notes*: NS&E includes natural (physical, biological, earth, atmospheric, and ocean sciences), agricultural, and computer sciences; mathematics; and engineering. Europe includes only France, Germany, and the United Kingdom. Asia includes only China, India, Japan, South Korea, and Taiwan. The jump in the European data in 1989 is due to the inclusion of French data, which were unavailable in this data series before 1989. French data are estimated for 2000. Source: National Science Board (2004, Figure 2-38).

Asian students to train at home, rather than in the United States. For example, the number of PhD degrees awarded in South Korea steadily increased in the 1990s while the number of South Koreans receiving PhDs in the United States peaked in the early 1990s and declined steadily for 7 or 8 years thereafter. In recent years, however, it has risen again (Sunwoong, 2007), in part because the job prospects for Koreans receiving degrees in Korea declined after the Asian meltdown in the late 1990s.

8.1.2. Where they work

It is far harder to describe employment patterns of PhD scientists and engineers. Data simply are not readily available outside the United States and Canada to study sector of employment, although efforts are currently underway in the EU and other OECD countries to produce reliable counts of PhD employment by sector. We must thus regrettably, and for the time being, limit our discussion of sector of employment to the United States, where, since 1973, data has been routinely collected on the career outcomes of PhD scientists and engineers trained in the United States and working in the United States.

As can be seen in Table 2, the majority of doctoral scientists and engineers in the United States are employed in institutions of higher education and in business and industry. A distinct minority work at FFRDCs, the government and nonprofit institutions. Over time, the sectoral composition has shifted substantially as business and industry have employed proportionately more scientists and academe proportionately fewer. While in 1973 almost 60% of all scientists worked at universities and 4-year colleges, this had fallen to 47% by 2003.[83] At the same time, the percent working in business and industry (which includes self-employed) increased by 150%, growing from 23.7% to 35.6%.

8.1.3. International mobility patterns

Science, perhaps more than any other enterprise, is international in scope. We see this in terms of location of training, location of work and, as we have noted earlier, in coauthorship patterns. In terms of training, a very large percent of degrees, especially in Europe and the United States, are awarded to foreign students. While the percent has fluctuated over time in response to such things as changes in available funding and visa policies, overall the percent of PhDs awarded to international students in the United States has grown considerably during the past 30 years. By 2006, 36.0% of PhDs awarded in science and 58.6% of those awarded in engineering went to candidates on a temporary visa while 6.0% of science PhDs and 4.5% of engineering PhDs were awarded to noncitizens on permanent visas (National Science Foundation, 2006, Table 3).[84] A somewhat similar situation exists in Europe, especially in the United Kingdom, where in 2003 over 50% of engineering PhDs and approximately 45% of math and computer science degrees were awarded to foreign students (National Science Board, 2004, Figure 2-40). The percent of foreign PhDs awarded in France is somewhat lower, but close to 30%

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[83] There has also been a structural shift away from tenure-track positions to nontenure-track positions, including staff scientists. In 1993, 78% of academic appointments were either in tenured positions or in tenure-track positions; in 2003, 62% were tenured or in tenure-track positions (National Science Foundation, 1996, Table 17; 2006, Table 21).

[84] Note that since some individuals do not respond to the question regarding citizenship, it does not follow that the remainder were all awarded to US citizens.

of PhDs in math/computer science went to foreign students in 2001; 22% of engineering PhDs were awarded to foreign students.

Science is also international in terms of work location. In 1990 (the latest date for which data is available at the time of this writing), 24.7% of all doctoral-trained scientists working in the United States were born outside the United States (Levin and Stephan, 1999). While many of these came to the United States to receive their training, a not insignificant number came to the United States after receiving either a baccalaureate degree abroad (16%) or a PhD abroad (10.7%). When newer data becomes available, we would expect to see an increase, reflecting the increased proportion of PhDs awarded to foreign-born scientists and engineers, but also the increasing number of postdoctoral fellows who come to the United States after receiving a PhD abroad, as well as the inflow of doctoral-trained foreign talent, especially from Russia, during the 1990s.

Star scientists are highly mobile, as Zucker and Darby (2007) show in recent work which tracks the most cited scientists and engineers during the period 1981–2004. They measure "home country" by address on first publication and mobility in terms of subsequent addresses appearing on publications. They find that 8.6% of the stars make a "one-way trip" from their home country to a different country; another 8.4% make a "round trip."[85]

One factor that contributes to international mobility is the wide differences that exist in funding for research and development across countries. Even among OECD countries there is considerable variability in the amount of funding for R&D, as is blatantly obvious from Figure 4, which shows R&D intensity as share of gross domestic product for eight OECD countries for the period 1981–2003. Italy (more recently, Russia) is at the bottom, and Japan and the United States are at the top. The figure also shows the wide fluctuations that occur in R&D expenditures within any country; these directly affect the market for scientists and engineers and contribute to cohort effects. The aggregate nature of the data conceals the mix of a country's R&D expenditures. In recent years this was exemplified in the United States when the NIH's budget doubled over a 5-year period, going from approximately $14 billion to $28 billion (current US), while other R&D budgets grew marginally, at best.

8.2. *Studies of supply and demand for new entrants to science*

A number of studies have examined the market for new entrants to science. Leslie and Oaxaca (1993) do an excellent job of surveying this literature and summarizing the major findings, as does Ehrenberg (1991, 1992).[86] Variables that are usually found to affect the supply of enrollees (or the number of graduates) in field j are salary in field j, salary in an alternative occupation such as law or business, and (for men) the draft deferment policy.[87] These variables almost always have the expected sign and are highly significant. The magnitude of the implied elasticities, however, varies considerably across studies, even when field is held constant (Ehrenberg, 1992). Another market variable often included in predicting supply is some measure of concurrent, past, or future supply. Other things being equal,

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[85] The analysis is for scientists and engineers working in a top-25 science and technology country (Zucker and Darby, 2007).

[86] Most studies focus on long-run adjustments. A few, however, examine the short-run responsiveness of the market by also focusing on the movements of trained personnel between fields and sectors (Blank and Stigler, 1957).

[87] Groen and Rizzo (2007) conclude that a major reason the propensity of US men to enroll in graduate school declined in the early 1970s was the end of Vietnam War draft deferments for graduate students.

Figure 4. R&D share of gross domestic product, by selected countries: 1981–2003. Source: National Science Board (2006, Figure 4-30).

enrollments are positively associated with present cohort size. Various lag structures are used in estimating these models and it is common to assume some form of adaptive (or rational) expectations. Supply variables generally ignored by these studies (primarily because of a reliance on aggregated data) include type of support available while in school, debt level upon graduation from college, and average time to degree.

Demand equations prove more difficult to specify, partly because we know so little about the behavior of universities and governments. There is, however, convincing evidence that demand relates to R&D expenditures and that these expenditures in turn affect supply decisions. Freeman (1975) finds degrees at the B.S., M.S., and PhD level in physics for the period 1950–1972 to be significantly related to R&D expenditures. Salaries also play a role. The propensity of recent graduate students to work in industry in the United States, for example, is in part a reflection of the higher relative salaries that are available in industry (Ehrenberg, 1991). It also undoubtedly reflects the softness of the academic labor market, given that most graduate students and postdoctoral students express a strong preference for a job at a research university (Davis, 2005; Fox and Stephan, 2001).

Several factors explain the softness of the academic market in recent years in the United States. First, cutbacks in public funds and lowered endowment payouts, especially in the early 2000s, clearly affect hiring. Second, salaries of tenure-track faculty are higher than those of nontenure-track faculty and this leads to a substitution away from tenure-track positions (Ehrenberg and Zhang, 2005). Third, funding for nontenure-track positions, such as staff scientist, is available in research grants. The high cost of start-up packages also plays a role in explaining these trends. Given the costs of such packages (which range from $300,000 to well over $1 million), when universities do hire in the tenured ranks, they are tempted to recruit senior faculty away from another university, rather than hire an as yet untested junior faculty member. The financial risk is considerably lower. While the start-up packages are generally higher at the senior ranks (Ehrenberg et al., 2003), the university gets an immediate transfer of grant money, because the senior faculty generally bring existing research grants with them when they come.

It is not only in the United States that the market for scientists and engineers in the academic sector has been soft in recent years. The job prospects of young PhDs in Italy in the university sector, for example, have also been bleak. The situation was aggravated in 2003 when a "no new permanent position" policy was put into effect. This has resulted in a situation in which the share of temporary researchers at universities had reached 50% in some instances, with young people being heavily concentrated in temporary positions (Avveduto, 2005). Reflecting these problems, the average age of faculty in research positions in 2003 in Italian universities was 45; for those in associate professor positions, 51; and for those in full professor positions, 58 (Stephan, 2008).

The academic labor market also appears soft in Germany. Schulze (2008) documents that the number of professors at German universities peaked in 1993 at about 23,000 and has been, with few annual exceptions, steadily declining ever since. In 2004, the last year for which he reports data, the number stood at just slightly over 21,000. The decline is not due to a decline in the number of students. During the same period the number of high school graduates has increased significantly and the ratio of professors per 100 high school graduates "has deteriorated significantly from 11.26 in 1996 to 9.43 in 2004" (p. 23). The decline has come at the same time that the number of Habilitationen, a requirement for obtaining an appointment as a professor at most institutions and in most fields, has grown dramatically.[88] Using a back-of-the-envelope type of calculation, Schulze estimates that the ratio of new applications to job openings rose from roughly 3/2 to 5/2 during the 14-year period that he analyzes.

A similar situation exists in South Korea, where universities, particularly private universities, under pressure to reduce expenditures on teaching personnel, are relying increasingly on part-time instructors. Sunwoong (2007) estimates that the number of full-time instructors in 4-year colleges and universities in 2006 was approximately 43,000 while the number of part-time instructors in 2003 was more than 50,000. Because of the slow turnover and the sluggish expansion of new positions, the problem for newly trained PhDs is likely to exacerbate.

8.3. Forecasting scientific labor markets

Although models of scientific labor markets have been somewhat successful in providing insight into factors affecting demand and supply, reliable forecasts of scientific labor markets do not exist, partly because of the unavailability of reliable predictions of exogenous variables. While this problem is endemic to forecasting in general, the ups and downs of funding for research (see Figure 4), as well as changes in policies, make forecasts of scientific labor markets particularly unreliable.

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[88] The typical academic career path in Germany involves preparing the Habilitation. After completion, and pending availability of a position, one is hired into a C3 position, which must be at an institution other than where the Habilitation was prepared.

Forecast error is common with regard to scientific labor market outcomes (Leslie and Oaxaca, 1993). In 1989, the NSF predicted an impending shortage of S&E doctorates in the United States (National Science Foundation, 1989). Others also predicted an impending shortage in the late 1980s (Atkinson, 1990; Bowen and Sosa, 1989). The underlying rationale was based on two assumptions: (1) an aging faculty, hired when higher education was expanding in the United States in the late 1950s and 1960s, would retire and be replaced; and (2) increases in the student body, as Baby Boomers' children headed to college, would increase demand for faculty. By the mid-1990s, if not before, it was clear that these forecasts had widely missed the mark, as was indicated by the dramatic increase in the proportion of new PhDs in nonpermanent jobs, the lengthening of time in postdoctoral positions and a decrease in the proportion of recent PhDs holding tenure-track positions. The reason for the forecast error related to a failure of the forecasters to predict changes in demand. These changes were brought about by the elimination of mandatory retirement, by an economic recession, and by political pressure to downsize the federal budget and the demise of the Cold War, which led to cuts or plateaus in federal funding.

In response to forecast error, a National Research Council Committee was created to examine issues involved in forecasting demand and supply. The committee was chaired by Daniel McFadden. The report, issued in 2000 (National Research Council, 2000), should be mandatory reading for anyone tempted to enter this arena. The committee concluded that forecast error could occur from: (a) misspecification of models, including variables, lag structure and error structure; (b) flawed data, or data aggregated at an inappropriate level; (c) unanticipated events. Even if model specification and lag structure are improved upon, unanticipated events continue to plague the reliability of forecasts. Both the fall of the Wall and the events of 9/11 had profound effects on scientific labor markets and would have been difficult to incorporate into any forecasting model.

Despite the report, and the well-known proclivity of forecasts to miss the mark, it is common for policy groups on both sides of the Atlantic to declare an impending shortage of scientists and engineers. A 2003 report issued by the National Science Board concluded that "Analyses of current trends . . . indicate serious problems lie ahead that may threaten our long-term prosperity and national security" (p. 7). A 2004 report from the European Union concluded that "Increased investment in research will raise the demand for researchers: about 1.2 million additional research personnel, including 700,000 additional researchers, are deemed necessary to attain the objectives, on top of the expected replacement of the aging workforce in research" European Commission, 2004, (p. 11).

9. Science, productivity, and the new growth economics

The foremost reason economists have for studying science is the link between science and economic growth. That such a relationship exists has long been part of the conventional wisdom, articulated first by Smith ([1776] 1982, p. 113). That the relationship is nonlinear has more recently entered into the conventional wisdom, as the role that technology plays in shaping scientific advances has been investigated and articulated by Rosenberg and Mokyr, as well as others. The nonlinearity relates not only to the role played by equipment in scientific discovery but also to the role that technological breakthroughs have played in fostering scientific insights as well as to their role in encouraging scientists in the public sector to develop new programs and research agendas. Solid-state physics is but one of many cases in point.

It is one thing to argue that science affects economic growth or to establish that a relationship exists between R&D activity and profitability. It is another to establish the extent to which scientific knowledge spills over within and between sectors of the economy and the lags that are involved in the spillover process. To date, four distinct lines of inquiry have been followed to examine these relationships. One inquires into the relationship between published knowledge and growth. Another surveys firms, with the goal of understanding the role that public knowledge plays in innovation. A third examines how the innovative activity of firms relates to research activities of universities (and other firms) by using measures of innovation as well as paper trails provided in patents and initial public offerings. A fourth looks at the degree to which firm performance is mediated by links with public research.

Adams (1990) uses the published-knowledge line of inquiry to examine the relationship between research and growth in 18 manufacturing industries between the years 1953 and 1980. The study is ambitious; for example, Adams measures the stock of knowledge available in a field at a particular date by counting publications in the field over a long period of time, usually beginning before 1930. He creates industry "knowledge stocks" by weighting these counts by the number of scientists employed by field in each of the industries being studied. He then relates productivity growth in 18 industries over a 28-year period to stocks of "own knowledge" and stocks of knowledge that have flowed from other industries. Adams finds both knowledge stocks to be major contributors to growth of productivity. He also finds that the lags are long: in the case of own knowledge, on the order of 20 years; in the case of knowledge coming from other industries, on the order of 30 years.

A necessary step in the growth story that Adams documents is that public science "leak out" to firms. Recent work by Adams et al. (2006) estimates a measure of the lag involved in this phase by analyzing citation patterns from industry-authored papers to university-authored papers. They report an average modal lag across the six disciplines studied of 3.02 years. The lag is longest in computer science (4.12) and shortest in physics (2.06).[89]

A different way to study the relationship between public science and innovation is to survey firms with an eye to ascertaining the role that university research plays in product development. Mansfield (1991) uses such a technique. He surveys 76 firms in seven manufacturing industries to ascertain the proportion of the firm's new products and processes commercialized in the period 1975–1985 that could not have been developed (without substantial delay) in the absence of academic research carried out within 15 years of when the innovation was first introduced. He finds that 11% of the new products and 9% of the new processes introduced in these industries could not have been developed (without substantial delay) in the absence of recent academic research. Using sales data for these products and processes, he estimates a mean time lag of about 7 years. He also uses these data to estimate "social" rates of return of the magnitude of 28%.

The interaction between firms and faculty is reciprocal: Relationships with firms also enhance the productivity of faculty. Mansfield (1995) finds that academic researchers with ties to firms report that their academic research problems frequently or predominately are developed out of their industrial consulting, and that this consulting also influences the nature of work they propose for government-funded research. Agrawal and Henderson (2002), in their study of MIT patenting, find similar

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[89] Tacit knowledge is most easily transmitted by face-to-face interaction. Stephan (2007) traces the placement of newly minted PhDs in industry as another means of the transmission of knowledge from the public sector to the private sector.

sentiments. An engineer whom they interview reports that "it is useful to talk to industry people with real problems because they often reveal interesting research questions . . ." (p. 58). Zucker et al. (1998a,b) find that the productivity of academic scientists is enhanced when they work with scientists in biotechnology companies.

Cohen et al. (2002) use a related approach, drawing on data from the 1994 Carnegie Mellon Survey (CMS) of industrial R&D, to determine the extent to which public knowledge is utilized by firms in their R&D activities and the means by which knowledge flows from the public sector to the private sector. They find that public research plays a major role in R&D in a few industries, particularly pharmaceuticals, and is generally more important in manufacturing than in other sectors. People and publications play a major role in transmission: firms rated publications, attendance at conferences and informal interaction as the most important channels for accessing public research. The licensing of university patents plays a substantially smaller role. Whether the licensing result would persist if the data were to be collected today remains to be seen.[90] They also find that "public research is used at least as frequently to address existing problems and needs as to suggest new research efforts." (Cohen et al., 2002, p. 2).[91]

Knowledge spillovers can also be studied by examining the relationship between some measure of innovative activity of firms and the research expenditures of universities. This production-function approach finds its roots in the work of Griliches (1979), who posited what has become known as the knowledge-production function. This line of inquiry ignores the lag structure, but focuses instead on the extent to which such spillovers exist and are geographically bounded. The rationale for expecting them to be bounded is that transmission of tacit knowledge is greatly facilitated through face-to-face communication. The approach is not restricted to examining the relationship between innovation and university research, but often includes a measure of private R&D expenditure in the geographic area to determine the extent to which spillovers occur within the private sector as well. Sometimes the measure of innovative activity is patents (Autant-Bernard, 2001; Jaffe, 1989); sometimes it is counts of innovations (Acs et al., 1992). Sometimes (Black, 2004) it is counts of SBIR grants. In any case, measured at the geographic level, innovative activity is found to relate to R&D expenditures of universities and firms in the same geographic area. There is some indication that these spillovers, particularly those coming from universities, are more important for small firms than for large firms (Acs et al., 1994).[92]

Patents provide a means of establishing a paper trail of knowledge spillovers, given the requirement that previous art be cited. Although it is the patent examiner who has the final say on which citations to include, the applicant is legally required to disclose any knowledge of prior art. Jaffe et al. (1993) use citations to other patents to analyze knowledge spillovers. They find that citing patents are in closer geographic proximity to the cited patent than they are to the sample of "control" patents that have the same temporal and technological distribution but are not linked through citation. The effect is most notable at the SMSA level but also holds, to a lesser extent, at the state and country level.

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[90] Jinyoung et al. (2005) find an increasing incidence of firm patents that list one or more inventors who had previously appeared as an inventor on a patent assigned to a university.

[91] Adams (2006) surveys 220 R&D labs. He finds that state universities in the South and Midwest are more often cited as a source of knowledge by mature industries, while younger industries are more likely to look to private US universities and universities in coastal regions.

[92] Adams (2002), for a sample of 220 R&D labs, finds academic spillovers to be more localized than industrial spillovers.

Patent citations to university articles also provide a paper trail of knowledge spillovers. Here, too, there is evidence that spillovers have a geographic dimension (Hicks et al., 2001).

Information on inventors can also be used to establish a paper trail by examining the mobility of inventors over time as measured by inventor addresses recorded on the patent. Using such a paper trail for Italian patents, Breschi and Lissoni (2003) conclude that mobility of researchers between firms is the mechanism by which knowledge spills over. And, because mobility is often within the same geographic area, knowledge spillovers have a geographic dimension. Indeed, their research indicates that localization effects (as measured by citations) tend to vanish in the absence of a network relationship between inventors. Their work is consistent with that of Almeida and Kogut (1999) which analyzes the interfirm mobility of patent holders in semiconductors and finds that labor markets have strong spatial characteristics, especially in Silicon Valley, where intraregional mobility is high and interregional moves are much smaller. Zucker and Darby's work (2007) also affirms the important role that people play in the spillover process. They show that where star scientists are active plays a key role, over and above the location of universities, in determining where biotech firms develop.

Start-up firms provide another indication of knowledge spillovers. Stanford University estimates that (http://www.stanford.edu/group/wellspring/index.html) over 2400 full-time companies have been founded by members of the Stanford community during the past several decades. The BankBoston's study (1997) is widely cited to show the important role that MIT has played in creating new companies in the Boston area.

Founders and members of SABs provide still another paper trail for studying knowledge spillovers. Audretsch and Stephan (1996) examine the location of university-based scientists having such a formal relationship with a biotech firm. They find that proximity matters, but that it does not matter that much. The majority of scientists (70%) do not live in close geographic proximity to the company. They conclude that when spillovers are mediated through people, they need not be geographically bounded if firms require expertise that may not exist in the local area. This is consistent with work by Mansfield (1995) that suggests that industry, when looking for academic consultants, is likely to use local talent for applied research, but focuses on getting the "best" regardless of distance when basic research is involved.[93]

A fourth vane of studies examines the relationship between a measure of firm performance and the firm's links with open science. Zucker et al. (1998a,b) find that the more articles a biotechnology firm has coauthored with a star, the better the firm performed, whether measured by products in development, products on the market, or employment. Cockburn and Henderson (1998) find that pharmaceutical firms that coauthor with publicly funded researchers have a higher performance as measured by research productivity.

Characteristics of a firm's patent portfolio, as measured by citation patterns, also relate to the valuation of the firm. Deng et al. (1999) build a model of stock performance based on closeness to science (as measured by cited articles in the firm's patents) and the influence of the patent (as measured by cites to the patent). Using such a methodology, CHI Research, Inc. identified undervalued firms and

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[93] Not addressed here, but of clear policy importance, is the degree to which university scientists are able to appropriate rents for their knowledge (Zucker et al., 1998a,b, p. 302). A related question can be asked with regard to knowledge that is transferred between firms. If it is people that provide the means by which knowledge is transferred between firms, then the resulting externalities may be fully captured or, at best, only pecuniary externalities may arise (Breschi and Lissoni, 2003).

compared them to overvalued firms. Doing retrospective analysis of the 20 most undervalued and 20 most overvalued firms, and updating the list annually, CHI found that the performance index of undervalued firms grew from 100 to approximately 2500 during the period 1990–2001, while the overvalued portfolio grew from 100 to approximately 250. In a somewhat related study, Hall et al. (2001) demonstrate that the market-to-book value of a firm is related to the number of times a firm's patent has been cited in other patent applications.

Despite the crudeness of the measures and the problems inherent in the various approaches, these studies go a long way toward demonstrating that the spillovers between scientific research and innovation are substantial, as are the lags. We cannot, however, leave the growth story here. Knowledge spillovers not only are a source of growth; they are endogenous. The story goes something like this: In an effort to seek rents, firms engage in R&D. Public aspects of this R&D then spill over to other firms, thereby creating increasing returns to scale and to long-term growth (Romer, 1994). The work of Schmookler (1966) and Scherer (1982), which demonstrates the responsiveness of R&D to demand factors, is consistent with this concept of endogenous growth. So is the work of Jaffe (1989), Acs et al. (1992), and Autant-Bernard, 2001, among others, whose work suggests that firms appropriate the R&D of other firms. Empirical work summarized above also implies that scientific research conducted in the academic sector of the economy spills over to firms.

Does this mean that research in the academic sector is an important component of the new growth economics? The answer depends upon the extent to which scientific research in the public sector is endogenous.[94] If it is not, spillovers from the public sector to firms are important, but not as a component of the new growth economics. Five aspects of science that we have developed in this chapter lead us to argue that an endogenous element of academic research exists. First, profit-seeking companies support academic research. Second, the problems that academic scientists address often come from ideas developed through consulting relationships with industry. Third, markets direct, if not completely drive, technology and technology affects science (Price, 1986; Rosenberg, 1982).[95] Fourth, government supports much of public-sector research, and the level of support available clearly relates to the overall well-being of the economy. Finally, there is evidence that relative salaries and vacancy rates affect the quantity and quality of those choosing careers in a field. "Hot fields" like biotechnology and computer science have attracted a disproportionate number of people in recent years when the rewards (at least to a few) have been extraordinary. The impact on academic research has been substantial.[96]

One could even argue (and many have) that public researchers (and the institutions where they work) have become too responsive to economic incentives for the good of science, or for the long-term good of the economy. Hundreds of patents can create thickets; competition can lead scientists to deny others access to research material; industrial sponsorship of public research can encourage secrecy and delay of publication.

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[94] It goes without saying that the science performed in companies is endogenous and spills over to other companies. A portion of this chapter has been devoted to demonstrating that profit-seeking companies hire scientists, direct them to do basic research, and often allow (encourage) them to share their research findings with others.

[95] The counter thesis of "technology push" is also important. That is, in many cases the invention of a new technology leads to new demands.

[96] This is not to argue that outcome X is endogenous, but merely that the growth of public knowledge has an endogenous component. At any point in time constraints clearly exist to discovery, either through the technology that is available to address the problem or because of lack of fundamental knowledge in an area necessary to the inquiry. Many of these constraints must be viewed as being exogenously determined, at least over a specific period of time (Rosenberg, 1974).

One could also argue that public institutions have been overly successful in selling the contribution they make to local economic development. It is one thing to find that knowledge spills over; it is another to create new universities and research programs with the goal of generating significant local economic development. Yet governments are doing precisely that. The California system opened its new campus in Merced in the fall of 2005. At least part of the impetus for its construction was the California Legislature's belief that the investment could bring economic development to the San Joaquin Valley. The News from Texas in August 2006 was that the state had decided to invest $2.5 billion for science teaching and research in the University of Texas system. A primary focus of the initiative is to build up the research capacity at campuses in San Antonio, El Paso, and Arlington in an attempt to turn these cities into the next Austin, if not the next Silicon Valley. Texas and California are not unique. Across the world, governments are working to turn universities and public research institutes into engines of economic development. Such investments will undoubtedly contribute to economic growth in the long run; but the extent to which it is a rational policy for fostering local economic development is not clear.

10. Conclusion

This chapter suggests several areas of inquiry in which economists have added significantly to an understanding of science and the role that science plays in the economy. Some of our discussion draws heavily on the work of sociologists and demonstrates the continued need to approach the study of science from an interdisciplinary perspective.

First, we have begun to quantify the relationship between science and economic growth, both in terms of payoff and lag structure. We have also achieved a better understanding of how science relates to growth, as a result of two threads of research coming together. One demonstrates that firms benefit from knowledge spillovers. The other suggests that knowledge spillovers are the source of growth and that these spillovers are endogenous. Although the authors of the new growth economics focus on the role that the R&D activities of firms play in this spillover process (both as creator of spillovers and recipient of spillovers), a case can be made that research in the nonprofit sector also has endogenous elements that are set in motion by profit-seeking behavior.

Second, the priority-based reward system that has evolved in science provides incentives for scientists to behave in socially beneficial ways. In particular, the reward of priority encourages the production and sharing of knowledge and thus goes a long way toward solving the appropriability dilemma inherent in the creation of the public good knowledge.

Third, science is not only about fame; it is also about fortune. Many of the financial rewards in science are a consequence of priority: salary, for example, is positively related to both article and citation counts. Because the financial rewards often come in the form of consulting and royalty income, we will never know the full extent of the relationship until we have reliable data on nonsalary dimensions of the income of scientists. There is also evidence that reputation matters to industry. We know, for example, that some firms encourage scientists to publish. We also know that startup companies benefit from affiliations with highly cited scientists.

Fourth, economics has been brought to bear on understanding the way in which scientific labor markets function. This in turn provides insights into how various government policies, intentionally or unintentionally, affect the market for scientists and engineers. Our ability to understand and model labor markets, however, is seriously hampered in some countries by the unavailability of data.

Fifth, our understanding of the many exogenous factors affecting demand and supply has led to the conclusion that we cannot forecast market conditions for scientists and engineers with much accuracy. It has also led to expressions of caution (and skepticism) concerning forecasts (usually of shortages) that policy groups are wont to make.

Sixth, numerous studies done in the late 1990s and early 2000s have contributed considerably to our understanding of the productivity of scientists and engineers. Moreover, we have extended studies of productivity to include patents as well as publications and considerable work has been done regarding the relationship of patents to publishing and vice versa.

But much remains to be understood and modeled. Foremost is a study of labs. Economists almost always approach productivity issues by studying individual scientists rather than the labs in which the scientists work. While individuals matter, science is increasingly about teams and collaboration. Yet we continue to focus on the individual. Our bias is caused by at least three factors: (1) ease of data collection, (2) an econometric tool kit that invites analyzing individual behavior, and (3) a funding system, at least in the United States, that continues to place great emphasis on the individual scientist despite the importance of labs.

Once we shift to a study of labs, numerous questions invite exploration. For example, we need to learn more about the production function of the lab, the degree of substitutability between capital and labor and whether the capital–labor ratio has changed over time as equipment has become more sophisticated. We need to know more about how lab size is determined. To what extent do economic factors come into play? Is size determined by the tradition of giving a researcher two rooms, with eight at a bench per room? Is there an efficient lab size? Is it efficient to increase lab size, as happened with the NIH doubling?

There are other ways economists can contribute to a better understanding of the workings of science. Seven are mentioned here. First, we need a better understanding of how outcomes relate to changes in funding. By way of example, to what extent has the practice of funding departments and programs on the basis of publications and citations led to "just-in-time" hiring? To what extent has the practice changed the submission and publishing patterns of scientists, especially outside the United States, where the changes have been the most notable? How has this, in turn, affected the refereeing process? Related is the question of the degree to which networks, in which funding agencies have placed great stock, contribute to productivity.

Second, economists can contribute to a discussion of other efficiency questions: Are there too many entrants in certain scientific contests or, more generally, too many scientists? A related question concerns whether science is organized in the most efficient way, particularly in the nonprofit sector. Is the demand for graduate students as research assistants and subsequently as postdocs so strong that it masks market signals concerning the long-run availability of research positions and encourages inefficient investments in human capital? Could other kinds of personnel (e.g., permanent research scientists) substitute for graduate students and postdocs in the lab?

Third, economists have a comparative advantage in understanding and analyzing the role that risk and uncertainty play in science. We can, for example, explain why risk aversion on the part of funding agencies dissuades scientists who are by disposition willing to take risk from engaging in this kind of research. We have the tool kit required to understand choices as outcomes of games and the possibility of using experimental economics to better understand how outcomes depend on rewards and funding.

Fourth, economists can contribute to an understanding of science by extending to the study of science approaches that have proved fruitful to the study of firms. Work in industrial organization that examines the entrance and survival of new firms could provide a framework for studying careers. Another possibility is to view the production of scientists through the lens of an evolutionary model (Nelson and Winter, 1982). Diversity and selection—the heart of evolutionary economics—are clearly present in the way in which scientists are trained, promoted, and rewarded.

Fifth, economists can contribute to a better understanding of how the reward structure of science leads some scientists to behave in socially undesirable ways. Issues include the fragmentation of knowledge that a focus on article counts encourages and the temptation to engage in fraudulent behavior.

Sixth, as a discipline we need to pay considerably more attention to understanding the way scientific effort is organized, monitored, and rewarded in industry. We also need to learn more about how scientists contribute to productivity outside of the traditional industrial R&D labs. We could learn much, for example, by studying scientists and engineers working in the service sector.

Seventh, the question of how opportunities for entrepreneurial behavior affect the practice of science bears continued exploration. So, too, does the question of whether policy makers have oversubscribed to the idea that knowledge spillovers lead to local and regional economic development.

In short, economists have accomplished a reasonable amount in our study of science; but other issues await investigation. It is hoped that this chapter will encourage that process.

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