Constraints and Institutional Innovation in Science

Arthur M. Diamond, Jr.

Department of Economics

University of Nebraska at Omaha

Omaha, NE 68182-0048

Phone: (402) 554-3657

FAX: (402) 554-3747

Internet: adiamond@unomaha.edu

World Wide Web home page: http://cba.unomaha.edu/faculty/adiamond/web/diahompg.htm

Keywords for searching: scientific progress, institutional innovation, scientific constraints, scientific incentives, economics of science

Last revised: August 24, 1999

ABSTRACT

Innovation in the design of scientific institutions is meaningful if institutional incentives and constraints affect the behavior of scientists, and if progress in science is possible. The case is made that scientific progress can occur within differing mixes of scientific inputs. Institutional innovation would then aim at efficiently increasing the speed of scientific progress. The paper concludes with brief discussions of what we know about the efficiency of scientific institutions, and what forms of institutional innovations might be promising.

INTRODUCTION

Scientists often write as if their efforts were unaffected by incentives and constraints: pure research serving the progress of science. Sociologists of science recognize the importance of incentives and constraints, but often write as if their importance compromises the possibility of the progressiveness of science. Both positions undermine efforts to design innovative scientific institutions. For the scientists innovations are irrelevant because the incentives and constraints provided by institutions do not effect the science done. For sociologists of science, innovations are unanchored because, by rejecting the possibility of scientific progress, they allow themselves no metric for judging what innovations to advocate.

Here, I argue both that incentives and constraints matter in understanding the behavior of scientists, and that science can be progressive. Further, I argue that such a position provides a coherent context for innovation in the design of scientific institutions. The goal of the first two sections of the paper is methodological and programmatic: to answer a couple of objections to the applicability of economics to science. The following beliefs underlie my own work in the economics of science and provide a context for the argument in the paper: science is progressive and useful; economics can help us understand the behavior of scientists and scientific institutions; and the rate of scientific progress can be increased by innovation in the design of scientific institutions. In this paper, I will not defend the belief that science is useful, although I believe that it can be defended (e.g., Nelson, Rosenberg, Mansfield and Adams). Instead I will focus on answering some objections to the progressiveness of science and to the applicability of economics to understanding the behavior of scientists.

The first objection takes the form of a dilemma. On the one hand, if scientists are motivated by curiosity then progress is possible, but economics is not applicable. On the other hand, if they are motivated by fame and fortune, then economics is relevant, but science is not progressive. To steal my own punch line, I will argue that under either sort of motivation, progress is possible and economics is applicable.

The second objection that I will answer is that if science is subject to economic models of constrained optimization and path dependence, then the epistemological status of science is diminished.

In the third section of the paper, I sketch some of the areas in which economists have studied the effects of incentives on scientific institutions, looking in particular at tenure, rent-seeking, peer-review, and public vs. private funding of science. I argue that the research that most needs to be done in economics of science is the empirical research that would allow us to distinguish between various theoretical possibilities (e.g., in the tenure case). I suggest, in particular, that the attenuation of tenure in Britain presents us with a “natural experiment” that could be used to measure the effects of tenure on the quantity and quality of research.

In the fourth and final section of the paper, I briefly discuss experiments that might be conducted to see how scientific institutions might be made more efficient by taking greater account of incentives.

VALUES IN ECONOMICS AND SCIENCE

Scientists are sometimes insulted at the suggestion that incentives matter in explaining their behavior. Although Adam Smith is best known as an advocate of the self-interest motive in explaining human behavior, even he thought scientists were different (1976b, 124):

> Mathematicians, . . . , who may have the most perfect assurance, both of the truth and of the importance of their discoveries, are frequently very indifferent about the reception which they may meet with from the public. . . . The great work of Sir Isaac Newton, his Mathematical Principles of Natural Philosophy, I have been told, was for several years neglected by the public. The tranquillity of that great man, it is probable, never suffered, upon that account, the interruption of a single quarter of an hour. (124)

The evidence we now have that Smith's example of Newton is ill-chosen (see: Hall 1980, passim; Westfall 1980, 698-780), does not prove that scientists are never motivated by playful curiosity, or a desire to find important truth. Indeed, even for the case of Newton, the motives that moved him to achieve his greatest work may have changed by the time he carried on his priority disputes with Leibniz.[1] Many later scholars have followed Smith in emphasizing the non-pecuniary elements in scientists' motivation (Hagstrom 1965, 19; Ghiselin 1987, 271; Tullock 1966, 34-36). Levy explicitly, and many other economists implicitly, have argued (or assumed) that scientists value fame and fortune. Such a position acknowledges that both non-pecuniary and pecuniary motivations are important. Although some scientists (e.g., Watson) acknowledge the non-pecuniary motive of fame, most would prefer Smith's emphasis on the non-pecuniary motive of curiosity

Distinguished evolutionary biologist George Gaylord Simpson was perhaps blunter in a letter than he would have been in a more public utterance: "The highest possible scientific motive is simple curiosity and from there they run on down to ones as sordid as you like." (Simpson 1987, 47). Of the many motives that have been suggested for the pursuit of science by scientists, some have been viewed as admirable (such as the desire to advance knowledge or to make the world a better place); others as neutral (such as playful curiosity); and some as blameworthy (such as the desire for fame and fortune). To help clarify the situation, let’s consider a simplified situation where we group the “good motives” under the heading “curiosity model” and the “bad motives” under the heading “fortune model.” The basic utility maximization models for each case would be:

CURIOSITY MODEL:

U = f(contribution to advance)

subject to: contribution to advance = g(money)

FORTUNE MODEL:

U = j(money)

subject to: money = k(contribution to advance)

Both models could result in scientists making contributions to knowledge. Behavior could be identical in both models, since in the “curiosity” model scientists would value their contributions to the advance in science, and in the “fortune” model the constraint, combined with their desire for money, could induce them to act <u>as if</u> they valued their contributions to the advance of science.

Intermediate cases between the two extreme models are more likely to accurately reflect reality—most scientists probably value both money and their contribution to science. Moreover, the weight that they place on each probably differs for different scientists, and even differs for the same scientist at different times in the life-cycle.

If some scientists in fact value contributions to science, then society's costs of maintaining the constraint would be reduced if the "curious" scientists are promoted to the role of gatekeepers. Unfortunately, in such situations, all scientists will benefit from claiming to be pursuing curiosity rather than fortune. In that case, incentive structures might be constructed that would induce scientists to honestly self-select themselves into either the curious or the fortune seeking groups. An extreme example of such an incentive structure was suggested by Nobel-prize-winning economist Friedrich von Hayek who suggested that ". . . there is a strong case for institutions which fulfill the functions that the monasteries fulfilled in the past, where those who cared enough could, at the price of renouncing many of the comforts and pleasures of life, earn the opportunity of devoting all the formative period of their development to the pursuit of knowledge." (p. 527) In a less extreme fashion, perhaps the poverty of the graduate student period functions as a partial method of self-selection.

Sometimes these issues have been discussed in terms of the principal/agent problem. This problem, which has been widely discussed in the fields of labor economics and industrial organization, recognizes that the person paying for an activity (the principal) may have different goals than the person performing the activity (the agent). From the point of view of the principal, the problem is to devise institutions that provide incentives that induce the agent to act as if her goals were the same as the principal's.

In our case, the principal is the public, and the agents are the scientists (cf. Guston 1996). Assuming that the public values scientific advance, then there is no principal/agent problem under the curiosity model. If the fortune model is correct then there is a principal/agent problem and the public must invest resources to assure that the constraint, money=k(contribution to advance), remains in effect.

As Hull, and perhaps Kitcher, suggest, scientific institutions may function so that knowledge is produced, even if the scientists are seeking fame and fortune. Although not all institutions may have a beneficial invisible hand effect, a variety of them may. So we may see scientific advance under institutions as different as the great patron model of early 17th century Italy (Biagioli); the research institute model of Althoff in 19th century Germany (Diamond 1994); the financially independent gentleman scientist model of 19th century English geology (Rudwick) ; the competing university model of 20th century United States (Stigler 1988, pp. 85-86); and maybe even the Stalinist research institute/prison model (Solzhenitsyn).

How important, then, is the issue of whether scientists value ‘discovery’ or ‘fame and fortune’? This issue is very hard to settle and so it is fortunate that the outcome of the issue may matter less than many think. In particular, does the motivation of scientists matter for the progress of science? And secondarily, does the motivation matter for the applicability of economics to understanding the behavior of scientists?

On the first question, under appropriate institutions it may not matter whether the scientist is pursuing knowledge or fame and fortune, because, ceteris paribus, scientists with more time and money will be better able to produce knowledge.

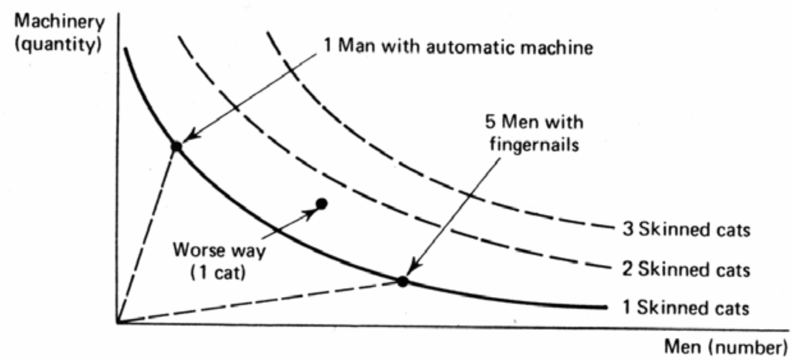
On the second question, even if all scientists were seeking knowledge rather than fortune, the economics of science would still be an important discipline, because constraints would still matter. For instance, life-cycle information constraints might lead scientists to differ on when to accept a new theory even if they all were motivated by curiosity (Diamond 1988b) And perhaps most obviously, decisions would still have to be made on how to allocate scarce scientific resources: which of the noble, curious astronomers should get access to the scarce Hubble telescope; and which of the noble, curious physicists should get access to the scarce particle accelerator? And at another level, how much training of how many astronomers and physicists should be funded (see, e.g., Stephan and Levin)? And how many Hubbles and accelerators should be built and maintained? And at yet another level maybe economics even has something to say about who should make these decisions: the scientists themselves, industry, the government, or private donors?

CONSTRAINTS IN ECONOMICS AND SCIENCE

I turn now to a second objection to the applicability of economics to science. The concern is whether the economic tools of constrained optimization and of path dependence are consistent with the belief in scientific progress.

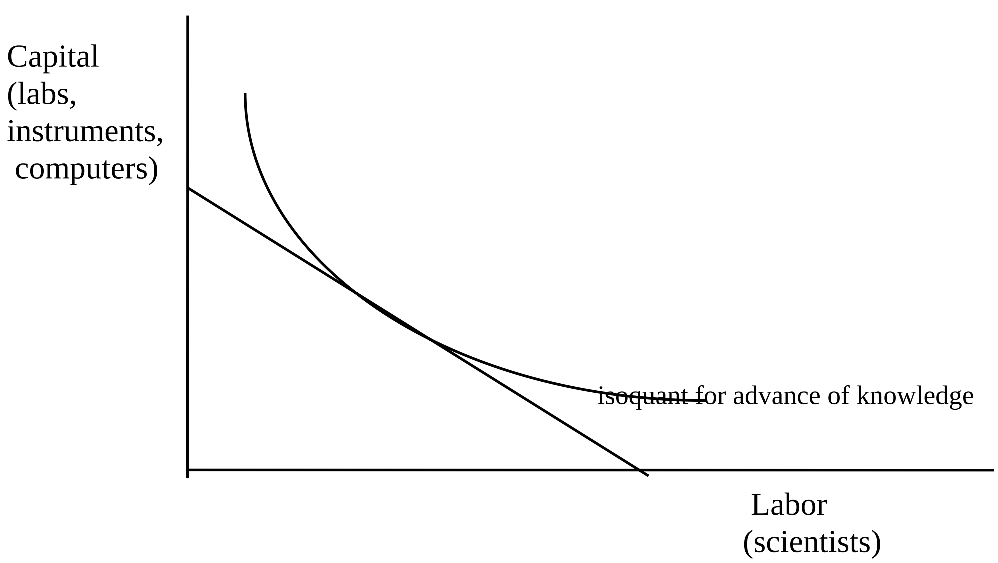
Economics often focuses on trade-offs. In an example sure to provoke animal rights groups, Deirdra McCloskey has shown how an economist would analyze the alternative ways to skin a cat (p. 164). In the illustration reproduced below, she emphasizes that you can use capital or labor intensive methods.

Figure 8.5
There Is More than One Way in Which to Skin a Cat



Isoquants display the technically efficient means of producing. Technical efficiency requires that the combination of inputs used to produce a given quantity of output is such that the quantity of one input cannot be reduced, holding the other inputs constant, without reducing the quantity of output.

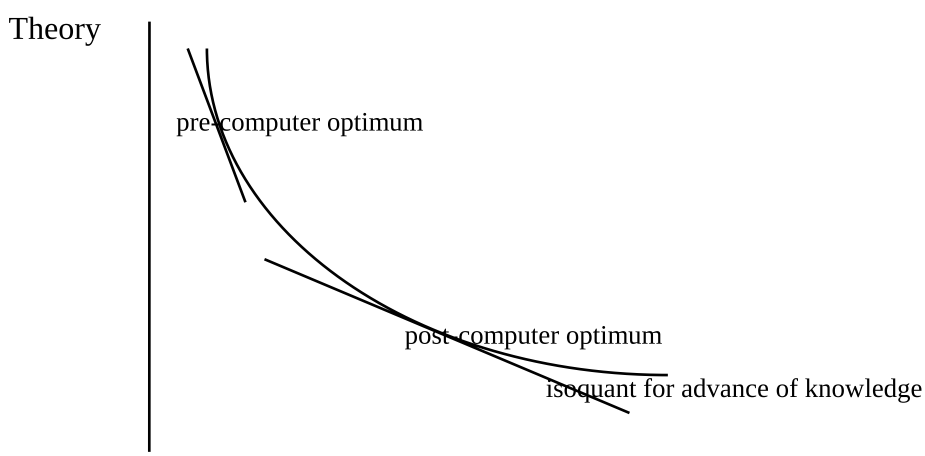
As in McCloskey's cat example, the most common analysis of production in economics focuses on labor and capital as the two main inputs. This analysis can also be directly applied to the case of production of scientific knowledge, as illustrated in the graph below.



You can advance science either by moving the isocost line out (increase spending), or by moving the output isoquants in (increase the efficiency of scientific institutions).

Jim Henderson (1997) has used this mode of analysis when he claims that some of the scientific instruments of 19th century British science functioned as substitutes for scientific human labor in the production of knowledge.[2] The point of alternative means to reach the same scientific end, can be illustrated in other ways as well. For example, the advance of science is often characterized as a combination of theoretical and empirical activities. A continuing, lively debate has revolved around the issue of the optimal combination of the two key inputs. Some have argued for example, that science would have advanced faster during the polywater episode in chemistry, if scientists had engaged in more empirical and less theoretical research (Diamond, 1988a).

In the science of economics, it has often been suggested that the optimal mix of empirical and theoretical work has shifted as the price and power of the personal computer has drastically lowered the price of doing empirical work. This suggestion is illustrated in the graph below:



Empirical

At first glance, this may seem similar to the Georgescu-Roegen position that Koppl and Butos (p. 27) have drawn our attention to, viz., that “lower costs of calculation may promote the redirection of scientific enterprise.” However the above graph suggests that the lower costs of calculation would effect the efficiency and speed of advance, but not the ultimate direction of research.

One way to interpret the view of Georgescu-Roegen is as an example of “path dependence.” Stated aphoristically, path dependence implies that history matters. This would seem obvious, to most. But economists are used to emphasizing stable equilibrium as a ubiquitous feature of society. In a situation of stable equilibrium, understanding past deviations from equilibrium are not important for understanding the current and future state, since deviations will still result in a return to the same stable equilibrium. An extreme version of path dependence is the currently stimulating and trendy “chaos theory.” Although the jury is still out on chaos theory, some frontier research by William Brock indicates that the theory may have limited applicability to economic processes.[3]

Even so, path dependence is an exciting idea that helps us explain some interesting puzzles. Most dramatically, Paul David has used it to explain why the less-efficient QWERTY typewriter keyboard has remained dominant over the demonstrably more efficient keyboard developed by Dvorak.[4] But I want to suggest that path dependence is more important for explaining the details than the big picture. It helps us understand whether the QWERTY or the

Dvorak keyboard wins, whether the Betamax or the VHS video tape player wins, and whether the MAC or the DOS operating system wins. But there may have been many paths that would have resulted in some computer keyboard, or some video tape system, or some PC operating system. The extent of path dependence may depend on the level of detail we are examining. According to Arthur Koestler's account (1959), Kepler came up with elliptical orbits because of his semi-mystical regard for perfect solids. But do any of us believe that elliptical orbits would not have been discovered by some other path, if the had not been discovered by Kepler's?

A possible example from the history of economic thought is that Walras, Jevons and Menger followed very different paths, at least in the sense of working from very different economic methodologies. Yet each arrived independently at the same destination: marginal utility theory.[5] Mark Blaug, one of our premier historians of economic thought, has made this point quite clearly in his main opus, Economic Theory in Retrospect. Referring both to the trio Walras, Jevons and Menger, as well as the earlier trio Dupuit, Gossen and Jennings, he says:

> . . . they struck on the law of diminishing marginal utility at about the same time but in response to totally different intellectual pressures and without the benefit of an inherited corpus of similar economic ideas. (p. 304)

A similar situation may also apply more generally: the details of current scientific theories may be path dependent, but the general progressiveness of science may be much less path dependent.

Some additional evidence on this issue might be obtained by examining other cases of independent multiple discovery. To the extent that similar discoveries occur in different institutional settings, to that same extent the importance of path dependence is diminished. The ultimate constraints are the phenomena to be explained. And as long as explanation is valued externally by the funders of science, or internally by a sufficient number of the gatekeepers of science (see Crane), then science may still progress (albeit at different speeds) under a variety of institutional and funding setups. In that case, one of the main goals of the economics of science is to increase the rate of scientific progress by reforming scientific institutions or devising new ones.

INCENTIVES AND INSTITUTIONS: WHAT WE KNOW AND WHAT WE NEED TO LEARN

The neo-Austrian school of economics has been criticized for spending far too much effort on program and method, and far too little effort on the delivery of substance. Although the risk of a similar situation in the economics of science is much less, its focus should still be on working toward the actual delivery of its promise. So for the remainder of the paper, I will sketch what has been done in the area of understanding incentives and institutions, looking in particular at tenure, rent-seeking, peer-review, and public vs. private funding. I will also suggest some important unanswered questions. (Other applications of the economics of science, such as the new growth theory, and the relevance of science to technology, are briefly discussed elsewhere in my review essay “The Economics of Science.”)

The key question is which institutions are most efficient at producing scientific advance? David Colander has pointed out that the largest percentage of funds in the United States spent for the support of scientific research do not come from government and private foundation grants, but rather from release time from teaching that universities grant to academics in order for them to perform research. So one of the most far-reaching issues effecting the efficiency of scientific institutions, is the question of who has access to this time to do research.

Several economists have analyzed the academic institution of tenure. Alchian views tenure as an inefficiency that occurs when managers of nonprofit organizations have insufficient incentive to reduce costs. Carmichael views tenure from a principal/agent viewpoint, arguing that administrators cannot independently judge who the "hot" job candidates are. Tenure induces the current faculty to accurately identify the "hot" prospects for the administration. A different principal/agent interpretation of tenure is elaborated by Siow. If universities only value teaching, and new hiring is done on the basis of research record, then tenure would be a means to provide faculty the job security to invest more in teaching and less in research.

Before the largely theoretical and speculative economics literature on tenure can have much policy bite, economists need to answer some tough empirical questions. Some of these would be: Does tenure increase the average quantity and quality of scientific research per scientist? How often does tenure induce scientists to pursue more risky research? Or does the process of achieving tenure favor the selection of those who are most risk-averse? (And how much risky research, as opposed to "normal" research, is optimal in science?) Does the institution of tenure reduce the mobility of scientists between schools, thus reducing scientific productivity by strengthening suboptimal matches between scientists and institutions? If universities mainly value faculty for teaching, then why don't there find ways to more accurately evaluate teaching ability, rather than using the imperfect proxy of research?

It might be possible to use natural experiments with alternative institutions to test some of these issues. For example the attenuation of tenure in Great Britain might provide an opportunity to contrast productivity before and after the attenuation (Dickson 1988). Of course other variables were changing, such as funding levels and department sizes, whose effect might be confounded with the effect of institutional changes in tenure. But those variables are themselves measurable, and so it should be possible to control for them.

Authors in the public choice literature in economics have looked at how current academic institutions provide incentives for rent-seeking behavior (Brennan and Tollison; McKenzie). Here “rent-seeking” means that an individual gains income or prestige by reducing the productivity of others rather than improving his own productivity. The proliferation of unnecessary committee work is one example. One theoretical (and untested) implication of this research is that larger departments will provide fewer incentives for rent-seeking. As with the tenure issue, Great Britain might provide a natural experiment for testing this implication, in this case Great Britain’s plan for consolidation of biology departments (Dickson 1989).

Applied game theory and experimental economics have increasingly been used to improve institutions, especially in the area of efficiently allocating scarce resources. Roth has suggested ways to improve the matching algorithm between medical residents and teaching hospitals. Boyes and Happel have shown how a bidding process can efficiently allocate office space of academics. Perhaps economists might also have a role in improving the efficiency of the matching of scientists to scientific institutions?

Hard to classify, are some of the specific suggestions for institutional innovation that have been made by various economists. Tullock (1966 142) believes that peer review would be greatly enhanced if foundations allocated funds to journal editors to improve their gatekeeping activities. Feigenbaum and Levy (and others) have suggested that likelihood of empirical work being replicated, will effect the care with which the original work is conducted. Anderson and Tollison find that time spent in reviewing the literature in science will be suboptimal, since citations are awarded to independent discovery, and not to finding truth in earlier literature.

Leo Szilard used to say that he would only submit a grant proposal for a project that he had already completed, because otherwise the peer reviewers would tell him that what he proposed was impossible (pp. 100-101). One of the most controversial discussions involving scientific incentives, is the issue of how to improve the efficiency of the current system of peer review for awarding grants. Some economists, most notably Gordon Tullock and Robin Hanson, and engineer Arthur Squires, have argued that the efficiency of allocating scientific money would be improved if the money was awarded as prizes for scientific work actually done, rather than as grants for work proposed to be done. One problem with the prize proposal is how a young researcher would receive funding for her early, pre-prize, research. Squires has suggested that institutions would be willing to support such research in return for the scientist's agreement to turn over some percent of the eventual prize money. Although the arguments for such institutional innovation have some appeal, we need to have more empirical evaluation of the effects of such a system.

Sometimes we look for general rules-of-thumb for efficiently funding science, e.g., we should fund basic sciences (physics) or we should fund applied sciences (biology); we should fund 'big' science or we should fund 'small' science. One problem is that there are no once-and-for-all answers to these questions. Another problem is that not everyone has equal knowledge of the best current answers to these questions and those who <u>do</u> know the answers are not always motivated to make allocation decisions based on them.

At the most crass, big science may be funded in preference to small science because it is more visible, and hence more prestige redounds to the person responsible (just as on college campuses it is sometimes easier to fund bricks and mortar even when what is most needed for progress is investment in brains and ideas). But often the influence of non-scientific considerations is more subtle. Many able scientists have worked for and with the NSF. But we would have to assume super-human nobility if we believed that Senator Proxmire's Golden Fleece awards never induced such scientists to fund politically safer research in preference to scientifically more promising research (Friedman; but see Newlon for contrary evidence)..

Are there solutions to these problems? One partial solution would be to increase the convergence between what is politically defensible and what is scientifically sound by increasing the public's understanding of science. The proportion of primary and secondary curriculum devoted to science could be increased and teaching methods in science could be improved. Also of great importance would be for scientists to view the clear communication of scientific results as essential to the advance of science, rather than irrelevant to it.

Another solution is to encourage multiple sources of science funding. Having multiple sources of funding serves two useful functions: 1.) it helps foster competition, and 2.) since even the best allocation will be made under uncertainty, a portfolio of different strategies will provide diversification against the chance that a single strategy turns out to be a dead-end.

It will be argued against this view that multiple funding sources might result in unnecessary and inefficient duplication of research. Such duplication may be an occasional price paid for progress. But it may be beneficial in other ways as well. Patinkin has suggested that having a critical mass of scientists working on the same research frontier may foster both the recognition and the communication of an advance when it occurs.

In developing alternative sources, it may be especially useful to encourage successful business entrepreneurs to fund science as one of their charitable activities. The skills and character traits that make one a successful entrepreneur in the business community have important similarities to those that make a successful scientist, e.g., energy, intelligence, persistence, and confidence. The entrepreneur's success at funding science would depend partly on her judgments about science, but would also depend partly on her ability to judge people. (In Squires' first-hand account of the Manhattan project, he reports that the project manager's success was not due to strategic planning or micro-management, but rather was due to his picking good scientists who were motivated, and allowing them ample room to exercise their judgment and use their specific knowledge.)

Even when private foundations are supported by small contributors rather than by successful retired entrepreneurs, they might prove to be more efficient than government agencies. Hayek, in his most famous article (1945), showed how the market takes advantage of each individual's local and specific knowledge to serve the public good. In the case of scientific foundations, a contributor could specialize in contributions to a foundation supporting a particular field of science. Such specialization would make it easier for the contributor to acquire the specific, local knowledge that is important in judging the prospects for scientific advance. Considerations from public choice would lead us to expect that a contributor has more incentive to invest in monitoring the spending of a foundation, than in monitoring the spending of tax-supported government institutions. (It is far easier for an individual contributor to withdraw a contribution, than for an individual taxpayer to reduce the spending on a given agency.) Case studies of institutions that have been dramatically successful (or unsuccessful) may be suggestive of hypotheses for more general assessment. (e.g., Swett). Eventually, what is needed in this area are empirical studies of the effects of type of institution on the efficiency of funding (Diamond, 1999a, 1999b).

CONCLUDING COMMENTS ON INSTITUTIONAL INNOVATION

We need to learn more about the operation of current institutions before we undertake major, large-scale innovation. But while we are waiting for those results to come in, it may be useful to undertake more modest experimental innovations. For instance, the Journal of Political Economy pays referees $100 if they return their report in one month, $50 if the return their report in two months, and nothing if they take longer than two months. Someone at JPE should study whether this has in fact resulted in quicker referee reports, and also whether the quality of the reports has suffered.[6]

David Levy (1997) has continued to draw our attention to the importance of replicability in science and has suggested innovations that might increase the degree of replicability. For example, other agencies and journals might follow the NSF’s lead in requiring researchers to make their data available to other researchers. And journals might require more robust estimation techniques, such as boot-strapping, before an empirical article is accepted for publication.

On the funding issue, we should experiment with a variety of competing patrons and forms of funding. Robin Hanson (1997), for instance, has continued his stimulating research on prizes as a form of funding for results. It would be useful to do more experimenting along the lines that Hanson suggests.

ENDNOTES

[*]The original version of the paper was prepared for an NSF-sponsored conference at Notre Dame on "The Need for a New Economics of Science." I am grateful to Phil Mirowski and Esther-Mirjam Sent for organizing the conference. I received useful comments from Steve Durlauf. Deirdre McCloskey graciously gave permission to reproduce her cats graph. A later version of the paper was presented to a meeting of the Society for Social Studies of Science.

[1] It has even been suggested that some of Newton's behavior later in life may have been due to his having ingested mercury from his alchemical experiments--one more example of constraints mattering?

[2] Henderson suggest that the introduction of new labor-saving instruments reduced the costs of the Key Observatory sufficiently to reverse Herschel's efforts to end British Association support for the Kew.

[3] The summary of Brock's (1991) FMA presentation from the program was: "This session reviews methods of testing for chaos and other "complex" nonlinearity in asset returns data such as stocks and exchange rates. Evidence of chaos is poor, but statistical techniques have turned out to be useful in detecting and suggesting other interesting nonlinearities and nonstationarities." In print (1991a) at about the same time, he wrote: "Chaotic dynamics are a very special type of nonlinearity. . . . Perhaps this is why there is considerable controversy about the relevance of the findings of chaotic dynamics for economic---or even for the natural sciences. . . . Define the dynamical process to by *chaotic* if it displays sensitive dependence on initial conditions. . . . The bulk of evidence suggests that low-dimensional (one to three) deterministic

chaos is not likely in aggregated economic data (. . .) although there is still some controversy on the matter." (pp 257-258) Steven Durlauf, who is associated with the Santa Fe Institute, has remarked in conversation (3/15/97) that chaos theory is no longer being actively developed and applied by economists at the Institute.

[4] For an alternative account from that presented by Paul David, see the Liebowitz and Margolis article.

[5] The episode is a standard "textbook" case of multiple discovery in economics. Cf., e.g.: Landreth and Colander 217-218.

[6] Steve Fuller experimented for awhile with rewarding referees in another fashion by assigning them various degrees of co-authorship for the articles they are reviewing. Steve abandoned the experiment because he believes few authors view the contributions of referees as sufficient to merit co-author status.

BIBLIOGRAPHY

Adams, James D. (1990) 'Fundamental stocks of knowledge and productivity growth,' Journal of Political Economy 98(4): 673-702.

Alchian , Armen A. (1958) 'Private property and the relative cost of tenure.' In Economic Forces at Work. Indianapolis: Liberty Press, 1977 (essay originally published in Philip D. Bradley, ed., The Public Stake in Union Power. Charlottesville, Vir.: The University Press of Virginia, 350-371).

Anderson, Gary M. and Tollison, Robert D. (1986) 'Dead men tell no tales,' The History of Economics Society Bulletin 8: 59-68.

Biagioli, Mario. (1993) Galileo, Courtier: The Practice of Science in the Culture of Absolutism. Chicago: University of Chicago Press.

Blaug, Mark. (1985) Economic Theory in Retrospect. 4th ed. Cambridge, UK: Cambridge University Press.

Boyes, William J. and Happel, Stephen K. (1989) 'Auctions as an allocation mechanism in academia: the case of faculty offices,' The Journal of Economic Perspectives 3(3): 37-40.

Brennan, H. Geoffrey and Tollison, Robert D. (1980) 'Rent seeking in academia.' In James M. Buchanan, Robert D. Tollison, and Gordon Tullock, eds., Toward a Theory of the Rent-Seeking Society. College Station, Texas: Texas A & M University Press, 345-356.

Brock, William A. (1991) 'Chaos and non-linear dynamics in the stock market.' Presented at the Chicago meetings of the Financial Management Association.

Carmichael, H. Lorne. (1988) 'Incentives in academics: why is there tenure?' Journal of Political Economy 96(3): 453-472.

Colander, David. (1989) 'Money and the spread of ideas.' In David Colander and A.W. Coats, eds. The Spread of Economic Ideas. New York: Cambridge University Press, 229-234.

Crane, Diana. (1972) Invisible Colleges: Diffusion of Knowledge in Scientific Communities. Chicago: University of Chicago Press.

David, Paul. (1985) 'Clio and the economics of QWERTY,' American Economic Review 75(2): 332-337.

____. (1986) 'Understanding the economics of QWERTY: the necessity of history.' In William N. Parker, ed., Economic History and the Modern Economist. New York: NY: Basil Blackwell, 30-49.

Diamond, Arthur M., Jr. (forthcoming 1999a) ‘Does federal spending ‘crowd-in’ private funding of science?’.Contemporary Economic Policy, 17(4), 423-431

____. (1994) ‘Economic explanations of the behaviour of universities and scholars,’ Journal of Economic Studies 20(4/5): 107-133.

____. (1996) ‘The economics of science,’ Knowledge and Policy 9(2 & 3): 6-49.

____. (1988a) ‘The polywater episode and the appraisal of theories.’ In A. Donovan, L. Laudan and R. Laudan, eds., Scrutinizing Science: Empirical Studies of Scientific Change. Dordrecht, Holland: Kluwer Academic Publishers, 181-198.

____. (draft 1999b) ‘The relative success of government and private foundations in funding important research.’

____. (1988b) ‘Science as a rational enterprise,’ Theory and Decision 24: 147-167.

Dickson, David. (1989) ‘British biologists learn small is not beautiful,’ Science 244: 766-767.

____. (1988) ‘Law weakens tenure, university autonomy,’ Science 246: 652-653.

Feigenbaum, Susan and Levy, David M. (1993) ‘The market for (ir)reproducible results,’ Social Epistemology 7(3): 215-232.

Friedman, Milton. (1981) ‘An open letter on grants,’ Newsweek: 99.

Ghiselin, Michael T. (1987) ‘The economics of scientific discovery.’ In Gerard Radnitzky and Peter Bernholz, eds. Economic Imperialism: The Economic Approach Applied Outside the Field of Economics. New York: Paragon House Publishers, 271-282.

Guston, David H. (1996) ‘Principal-agent theory and the structure of science policy,’ Science and Public Policy 23(4): 229-240.

Hagstrom, Warren O. (1965) The Scientific Community. New York: Basic Books, Inc.

Hall, A. Rupert. (1980) Philosophers at War: The Quarrel Between Newton and Leibniz. Cambridge: Cambridge University Press.

Hanson, Robin. (1995) ‘Could gambling save science? encouraging an honest consensus,’ Social Epistemology 9(1): 3-33.

____. (1997) ‘Patterns of patronage: why grants won over prizes in science’ (presented at Conference on the Need for a New Economics of Science, Notre Dame).

Hayek, Friedrich A. (1972a) The Constitution of Liberty. Chicago: Henry Regnery Co.

____. (1945) 'The use of knowledge in society,' American Economic Review 35(4): 519-530 [reprinted in: Individualism and Economic Order. Chicago: Henry Regnery Co., 1972b.]

Henderson, James. (1997) 'The British ass is a golden ass: funding victorian scientific research' (abstract of paper presented at Conference on the Need for a New Economics of Science, Notre Dame).

Hull, David L. (1988) Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science. Chicago: University of Chicago Press.

Kitcher, Philip. (1993) The Advancement of Science: Science without Legend, Objectivity without Illusions. Oxford: Oxford University Press.

Koestler, Arthur. (1959) The Sleepwalkers. New York: The Macmillan Co.

Kohler, Robert E. (1993) 'Drosophila: a life in the laboratory,' Journal of the History of Biology 26(2): 281-310.

Koppl, Roger and Butos, William. (1997) 'Science as a spontaneous order' (paper presented at Conference on the Need for a New Economics of Science, Notre Dame).

Landreth, Harry and Colander, David C. (1994) History of Economic Thought. 3rd ed. Boston: Houghton Mifflin Co.

Liebowitz, S.J. and Margolis, Stephen E. (1990) 'The fable of the keys,' Journal of Law and Economics 33(1): 1-25.

Levy, David M. (1988) 'The market for fame and fortune,' History of Political Economy 20(4): 615-625.

____. (1997) 'Optimal belief: new techniques for an old problem' (paper presented at Conference on the Need for a New Economics of Science, Notre Dame).

McCloskey, Deirdra (a.k.a. Donald N.) (1982) The Applied Theory of Price. New York: Macmillan Publishing Co., Inc.

McKenzie, Richard B. (1979) 'The economic basis of departmental discord in academe,' Social Science Quarterly 59(4): 653-664.

Mansfield, Edwin. (1971) Technological Change. New York: W.W. Norton & Co., Inc.

Merton, Robert K. (1973) 'Singletons and multiples in science.' In The Sociology of Science. Chicago: The University of Chicago Press, 343-370.

Nelson, Richard R. (1959) 'The simple economics of basic scientific research.' Journal of Political Economy 67: 297-306.

Newlon, Daniel H. (1989) 'The role of the NSF in the spread of economic ideas.' In David Colander and A.W. Coats, eds. The Spread of Economic Ideas. New York: Cambridge University Press, 195-228.

Patinkin, Don. (1983) 'Multiple discoveries and the central message,' American Journal of Sociology 89(2): 306-323.

Rosenberg, Nathan. (1994) Exploring the Black Box. Cambridge: Cambridge University Press.

Rosenberg, Nathan. (1982) Inside the Black Box: Technology and Economics. Cambridge: Cambridge University Press.

Roth, Alvin E. (1984) 'The evolution of the labor market for medical interns and residents: a case study in game theory,' Journal of Political Economy 92(6): 991-1016.

Rudwick, Martin J.S. (1985) The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists. Chicago: The University of Chicago Press.

Simpson, George Gaylord. (1987) Simple Curiosity. (ed. by Léo F. Laporte) Berkeley: University of California Press.

Smith, Adam. (1976b) The Theory of Moral Sentiments. Oxford: Clarendon Press [first edition was published in 1759].

Solzhenitsyn, Alexander. (1969) The First Circle. New York: Bantam Books, Inc.

Squires, Arthur M. (1986) The Tender Ship. Boston: Birkhauser.

Stephan, Paula E. and Levin, Sharon G. (1992) Striking the Mother Lode in Science: The Importance of Age, Place, and Time. New York: Oxford University Press.

Stigler, George J. (1982a) 'Does economics have a useful past?' In The Economist as Preacher and Other Essays. Chicago: The University of Chicago Press, 107-118 [first appeared in History of Political Economy 1 (Fall 1969)].

____. (1988) Memoirs of an Unregulated Economist. New York: Basic Books, Inc.

____. (1965) 'The nature and role of originality in scientific progress.' In Essays in the History of Economics. Chicago: The University of Chicago Press, 1-15.

Swett, Timothy W. (1995) 'The advanced research projects agency: a discussion of evaluation techniques and overview of the agency's enchanted past and uncertain future.' (M.A. thesis, University of Nebraska at Omaha).

Szilard, Leo. (1961) The Voice of the Dolphin and Other Stories. New York: Simon and Schuster.

Tullock, Gordon. (1966) The Organization of Inquiry. Durham, N.C.: Duke University Press.

Watson, James D. (1968) The Double Helix. New York: Signet Books.

Westfall, Richard S. (1980) Never at Rest: A Biography of Isaac Newton. Cambridge: Cambridge University Press.