How Institutional Incentives and Constraints Affect the Progress of Science[1]

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ABSTRACT   *Scholars studying science policy have long wondered how the progress of science is affected by scientists' motives, and by the incentives and constraints that scientific institutions create. This paper aims to answer two objections to the soundness and applicability of the 'economics of science' that arise from such issues. I argue that the progress of science can occur even if scientists exhibit a wide range of motives; but that the pace of scientific progress will depend, in part, on the incentives and constraints provided by scientific institutions. I also discuss the implications of path dependence for the epistemological status of science.*

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 affect 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 paper is methodological and programmatic: to answer a couple of objections to the applicability of economics to science.

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 the economic process of path dependence, then the epistemological status of science is diminished.

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:

> Mathematicians … who may have the most perfect assurance, both of the truth and of the importance of their discoveries, are frequently very indifferent about the reception which they may meet with from the public. … The great work of Sir Isaac Newton, his *Mathematical Principles of Natural Philosophy*, I have been told, was for several years neglected by the public. The tranquility of that great man, it is probable, never suffered, upon that account, the interruption of a single quarter of an hour.[2]

The evidence we now have that Smith's example of Newton is ill-chosen,[3] 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.[4]

Many later scholars have followed Smith in emphasizing the non-pecuniary elements in scientists' motivation.[5] Levy explicitly, and many other economists implicitly, have argued (or assumed) that scientists value fame and fortune.[6] Such a position acknowledges that both non-pecuniary and pecuniary motivations are important. Although some scientists[7] 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'.[8] 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 us 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)

where *f, g, j* and *k* represent generalized (but positive) functional relationships.

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 as if 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.

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.[9]

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.[10] 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,[11] 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 seventeenth century Italy;[12] the research institute model of Althoff in nineteenth century Germany;[13] the financially independent

gentleman scientist model of nineteenth century English geology;[14] the competing university model of the twentieth century United States;[15] and maybe even the Stalinist research institute/prison model.[16]

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.[17] 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?[18] 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 plausible, and frequently observed, economic process known as path dependence is consistent with a belief in scientific progress.

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 is 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 that has received considerable attention is called 'chaos theory'.[19]

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.[20]

If, like many nonequilibrium situations in economics, scientific results exhibit path dependence, then those results will be different depending on which of many possible paths actually occurs. In what sense, then, can scientific knowledge be considered to have progressed?

As a preliminary to answering this question, I suggest that it may be fruitful to consider how the economist analyzes a key concept: input trade-offs in production. 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.[21] In the

illustration reproduced below (Figure 1), she emphasizes that you can use capital or labor intensive methods.

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 in Figure 2 below.
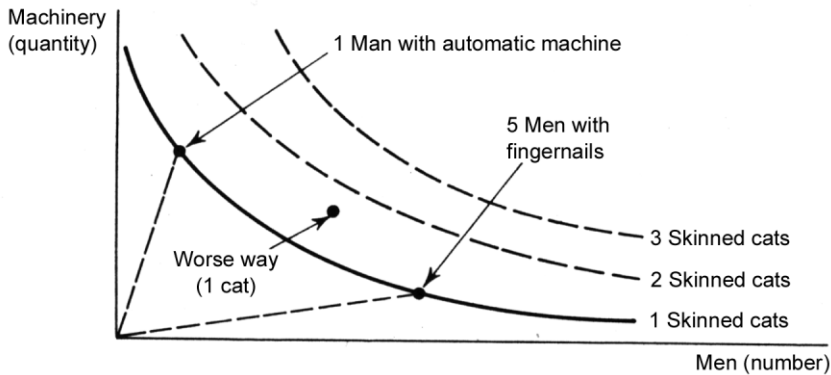
Isoquants display the technically efficient means of producing. Technical efficiency requires that the combination of inputs used to produce a given quantity of output is such that the quantity of one input cannot be reduced, holding the other inputs constant, without reducing the quantity of output.

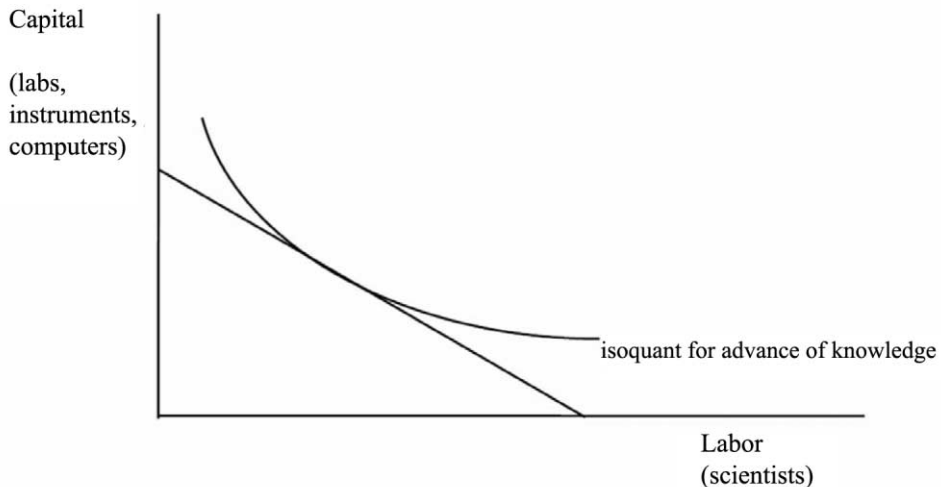**Figure 1.**   McCloskey graph on technologies for cat skinning (reproduced with permission).

Figure 2.   Different combinations of labor and capital can advance science.

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 has used this mode of analysis when he claims that some of the scientific instruments of nineteenth century British science functioned as substitutes for scientific human labor in the production of knowledge.[22] 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.[23]

In the science of economics, it has sometimes 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 shown in Figure 3 below.

At first glance, this may seem similar to the Georgescu-Roegen position that Butos and Koppl have drawn our attention to, viz. that 'lower costs of calculation may promote the redirection of scientific enterprise'.[24] However the above graph suggests that the lower costs of calculation would affect the efficiency and speed of advance, but not the ultimate advance of knowledge.

One way to interpret the view of Georgescu-Roegen is as an example of 'path dependence'. 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,[25] Kepler came up with elliptical orbits because of his semi-mystical regard for perfect solids. But do any of us believe that
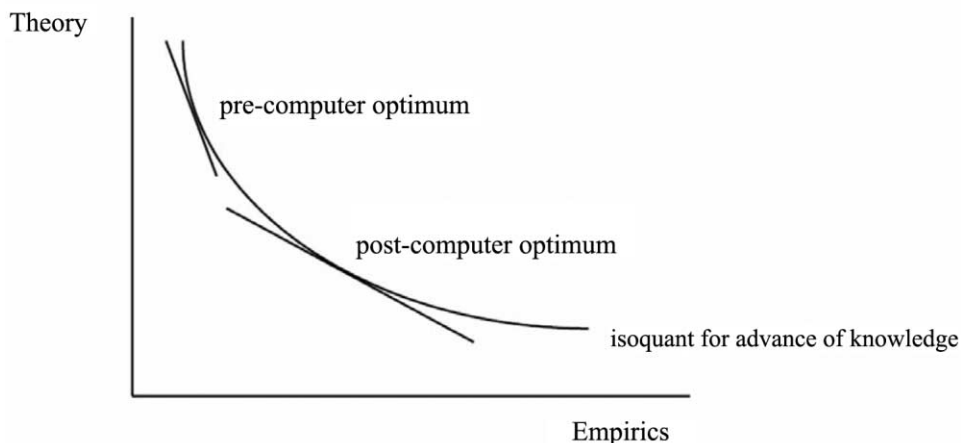

Figure 3.   Different combinations of empirics and theory can advance science.

elliptical orbits would not have been discovered by some other path, if they 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.[26] Mark Blaug, one of our premier historians of economic thought, has made this point quite clearly in his main opus, *Economic Theory in Retrospect*.[27] 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.[28]

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,[29] 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.

Conclusion

My goal has been to answer some plausible challenges that might be made to the applicability of the 'economics of science' research program. I have argued that as long as scientific resources are scarce, the economics of science will have an important role to play in science policy. In particular, I have argued that this role is secure whether scientists are motivated primarily by curiosity, or by fame-and-fortune. In either case issues of efficient allocation of scarce resources arise. A notion of scientific progress is also possible in either case, so long as scientific institutions can be found, or devised, that provide scientists with the incentives to at least act as if they were motivated by curiosity. The costs of operating such institutions, or of over-coming imperfections in such institutions, will be lower if at least the 'gatekeepers' in science are genuinely motivated by curiosity.

The hard and important task is to proceed, through experiment based on theory and evidence, to develop better institutions that hasten the advance of scientific knowledge.

Notes and References

 1. The original version of this paper was prepared for an NSF-sponsored conference on 'The Need for a New Economics of Science'. 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.

2. Adam Smith, *The Theory of Moral Sentiments*, Clarendon Press, Oxford, 1976, p. 124; first edition was published in 1759.

3. See: A. Rupert Hall, *Philosophers at War: The Quarrel Between Newton and Leibniz*, Cambridge University Press, Cambridge, 1980, *passim*; Richard S. Westfall, *Never at Rest: A Biography of Isaac Newton*, Cambridge University Press, Cambridge, 1980, pp. 698–780.

4. 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?

5. Warren O. Hagstrom, *The Scientific Community*, Basic Books, Inc., New York, 1965; Michael T. Ghiselin, 'The economics of scientific discovery', in Gerard Radnitzky and Peter Bernholz (eds), *Economic Imperialism: The Economic Approach Applied Outside the Field of Economics*, Paragon House Publishers, New York, 1987, pp. 271–82; Gordon Tullock, *The Organization of Inquiry*, Duke University Press, Durham, NC, 1966, pp. 34–6.

6. David M. Levy, 'The market for fame and fortune', *History of Political Economy*, 20, 4, Winter 1988, pp. 615–25.

7. For example, James D. Watson, *The Double Helix*, Signet Books, New York, 1968.

8. George Gaylord Simpson, *Simple Curiosity*, edited by Léo F. Laporte, University of California Press, Berkeley, 1987, p. 47.

9. Friedrich A. Hayek, 'The use of knowledge in society', *American Economic Review*, 35, 4, September 1945, pp. 519–30 at p. 527. Reprinted in *Individualism and Economic Order*, Henry Regnery Co., Chicago, 1972.

10. Cf. David H. Guston, 'Principal–agent theory and the structure of science policy', *Science and Public Policy*, 23, 4, August 1996, pp. 229–40.

11. David L. Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science, Science & Its Conceptual Foundations*, University of Chicago Press, Chicago, 1988.

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

13. Arthur M. Diamond, Jr, 'Economic explanations of the behaviour of universities and scholars', *Journal of Economic Studies*, 20, 4/5, 1994, pp. 107–33.

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

15. George J. Stigler, *Memoirs of an Unregulated Economist*, Basic Books, Inc., New York, 1988, pp. 85–6.

16. Alexander Solzhenitsyn, *The First Circle*, Bantam Books, Inc., New York, 1969.

17. Arthur M. Diamond, Jr, 'Science as a rational enterprise', *Theory and Decision*, 24, 1988b, pp. 147–67.

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

19. 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. The summary of Brock's (1991a) 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 (1991b) 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 be chaotic if it displays sensitive dependence on initial conditions … William A. Brock, 'Causality, chaos, explanation and prediction in economics and finance', in J. L. Casti and A. Karlqvist (eds), *Beyond Belief: Randomness, Prediction and Explanation in Science,* CRC Press, Boca Raton, Fla, 1991b, pp. 230–79. 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. 257–8). Steven Durlauf,

who is associated with the Santa Fe Institute, has remarked in conversation (15 March 1997) that chaos theory is no longer being actively developed and applied by economists at the Institute. (The FMA Brock paper was: William A. Brock, 'Chaos and non-linear dynamics in the stock market', presented at the Chicago meetings of the Financial Management Association, 12 October 1991a.)

20. Paul David, 'Clio and the economics of QWERTY', *American Economic Review*, 75, 2, May 1985, pp. 332–7. For an alternative account from that presented by Paul David, see: S. J. Liebowitz and Stephen E. Margolis, 'The fable of the keys', *Journal of Law and Economics*, 33, 1, April 1990, pp. 1–25.

21. Deirdra McCloskey, *The Applied Theory of Price*, Macmillan Publishing Co., Inc., New York, 1982, p. 164.

22. Henderson suggests 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. James Henderson, 'The British ass is a golden ass: funding Victorian scientific research', abstract of paper presented at the Conference on the Need for a New Economics of Science, 1997.

23. Arthur M. Diamond, Jr, 'The polywater episode and the appraisal of theories', in A. Donovan, L. Laudan and R. Laudan (eds), *Scrutinizing Science: Empirical Studies of Scientific Change*, Kluwer Academic Publishers, Dordrecht, Holland, 1988a, pp. 181–98.

24. William N. Butos and Roger Koppl, 'Science as a spontaneous order: an essay in the economics of science', in Hans S. Jensen, Lykke M. Richter and Morten T. Vendelø (eds), *The Evolution of Scientific Knowledge*, Edward Elgar, Cheltenham, UK, 2003, p. 27.

25. Arthur Koestler, *The Sleepwalkers*, The Macmillan Co., New York, 1959.

26. The episode is a standard 'textbook' case of multiple discovery in economics. Cf., e.g. Harry Landreth and David C. Colander, *History of Economic Thought*, 3rd edition, Houghton Mifflin Co., Boston, 1994, pp. 217–8.

27. Mark Blaug, *Economic Theory in Retrospect*, 4th edition, Cambridge University Press, Cambridge, UK, 1985.

28. *Ibid*, p. 304.

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