

Science and Its Economics – How Much Are They Worth?

(published in: *Journal of Economic Methodology* 11 (2004), 2, pp. 257-261)

Science Bought and Sold – Essays in the Economics of Science, ed. by P. Mirowski and E. Sent. Chicago: University of Chicago Press, 2002, 560 pages, ISBN 0226538575, Price \$33.00.

What can natural scientists learn from economists? According to the editors of this huge volume, economics of science is by no means a purely theoretical enterprise, but is to be of practical value: Natural scientists could profit from economists by understanding better the economic structures behind their increasingly precarious funding situation. This is at least one of the goals which Mirowski and Sent regard as a fundamental aim of their collection of articles that range from purely historical to contemporary essays and case studies.

So what is the proper method for economics of science? The editors continue to stress that there is as yet no monolithic or even coherent approach to an economic analysis of scientific activities. In their extensive introduction, Mirowski and Sent single out historically and systematically different approaches. Historically, the diverse attempts at an economics of science have been following closely the development of different regimes of science funding (at least in the US). After a first, protoindustrial phase during which US science was mainly learning from the German academic system but also setting up the first corporate labs, the second, Cold War phase was the real start not only for big government funding but for economics of science as well. The massive federal presence combined with a fundamental distinction between basic and applied science led to a rise in economic analysis of scientific activity as a sort of production controlling. The pioneers, like Derek de Solla Price, were in favour of academic freedom with little government control in the evaluation of scientific results. For them, the only major problem for science was its sheer size which might eventually lead to a state of diminishing returns (Gunther Stent, Nicholas Rescher).

After 1989, the Cold War situation has given way to a new one, which Mirowski and Sent call the “globalized privatization regime”. This involves fundamental theoretical shifts, like abandoning the distinction between basic and applied science, interpreting universities in different ways, and other aspects. Privatization has allowed for new career paths, for more “outsourcing” of scientific activity, and for a stronger sense of intellectual property rights. According to the editors, economics of science, in this line, has shifted away from

neoclassical economics towards greater recognition of cognitive processes and constraints. Likewise, as Mirowski and Sent illustrate vividly with examples, the social system of science has gained new problems, involving new conflicts on intellectual fraud or control of results by the private sector (e.g., in the medical companies).

After this thematic introduction, Mirowski and Sent try to sort out (at least) three historically and systematically different approaches. These are the subject of parts II, III, and IV of the book, which is organized in six parts.

The first part consists in a stock-taking of the current situation in science funding. While Sheila Slaughter and Gary Rhoades focus on tendencies toward commercialization and increased competitiveness in research and development, Paul Forman describes how normative criteria in science have shifted in post-modern times from purely ‘internal’ or self-referencing to ‘external’, social ones.

The second part provides examples of the historically first systematic approach to an economics of science. Within the context of the Cold War, the military interest in science was predominant. Science was seen as a tool for producing knowledge, its economic analysis was done by welfare economics and production theory. As in ‘ordinary’ markets, welfare economics spotted problems of market failure in science and proposed ways to correct them. Especially, the government was required to intervene within the area of basic research, as this could not be provided by private companies investing in research. Contributions by Richard Nelson and Kenneth Arrow are included here.

The third part (titled “Science conceived as a Problem of Information Processing”) focuses on economics of science based on implicit contracts theory and on the so-called cognitive turn in economics. This section comprises the classic two articles by Ch. S. Peirce, an article by James Wible on Peirce, and two recent works, one by Partha Dasgupta and Paul David, the other by Philip Kitcher. Especially the Kitcher article “The Organization of Cognitive Labour” is very important, as it is one of the first contributions by a philosopher of science taking up the economic toolbox for his own work. Kitcher tries to find an economic argument for defending scientific rationality. Economists could proceed from here in a practical direction. From an economic point of view, the organization of cognitive labour could be analyzed in more detail with respect to particular *institutions* shaping this organization and setting incentives.

The fourth part comprises articles that view science as the unintended outcome of a network of agents working under cognitive constraints. Four articles have been put in this section, by

Michel Callon on actor network theory, a more traditional piece on the “microeconomics of science” by John Ziman, a formal modeling work by William Brock and Steven Durlauf, as well as an article by Stephen Turner. The Brock/Durlauf article is particularly interesting. The main point of their formal analysis is that the importance of evidence changes with the social context, hardly a new point in general, but there have not been many economic models for this. As in Kitcher’s case, though, a practical and institutional perspective would be worth exploring. Without this, Brock and Durlauf’s models remain somewhat void.

The contributions in the fifth part are more case-study-oriented: They deal with the privatization of British universities (Shaun Hargreaves Heap), stress the importance of implicit contracts in collaborative research (Paula Stephan and Sharon Levin), condemn the automation of the university system (David Noble), and criticize the call for economic competitiveness in science as the continuation of Cold War science policy by other means (Steve Fuller).

The sixth part “The Future of Scientific ‘Credit’” contains a classical work by Michael Polanyi, “The Republic of Science”, an article on the instability of authorship in biomedicine by Mario Biagioli, and finally a text by Wade Hands on future possibilities for sociological and economic approaches to scientific knowledge. Hands argues that there will ultimately be problems of reflexivity for both approaches. Again, the practical and institutional perspective could be added.

The volume is definitely something that was missing from the current economics of science debate. It contains classical texts like Peirce’s which were not easily found before. It also contains many facts and results from the specialized literature which equally were not easily accessible before.

Nevertheless, there are some problems about this volume, general and particular ones. One general problem is that it tries to reach too many aims at once: identify and collect historically important texts, document and classify the approaches in the contemporary debate, provide an overview of the recent detailed work in the field. Maybe it would have been better to separate a historical reader from a systematic one. The historical volume could then have comprised even more historically important texts, e.g., by Solla Price, Tullock, Rescher, Stent (or what about W. Bartley, G. Stigler or G. Radnitzky?) or even the classical Vannevar Bush text which is cited here often. This would have clarified the structure of the volume, which has not really become transparent to me. It is neither purely historical nor purely problem-oriented. For example, the introduction could have made clearer how parts V and VI stand to the rest of

the book. The articles in part V are more than just case studies, and in what way do they relate to the different systematic approaches identified before? Or are they entirely separate from them? And the last part – probably supposed to be an outlook – fulfils this task only to some extent. Or is it thought of as a (very) critical and sometimes ironic reflection?

Moreover, the classification of articles sometimes is questionable. For example, couldn't Brock and Durlauf's contribution have been in the same section as Kitcher's? Shouldn't Stephan and Levin's article ("The Importance of Implicit Contracts in Collaborative Scientific Research") be in the implicit contracts section?

So much for the layout of the volume. Concerning the general view of economics of science presented by the editors, I have two criticisms: The extensive introduction seems a little one-sided at times. While it is ok to have a controversial opinion on the economics background of an economics of science, it is not unproblematic to make it seem like there is already a consensus on many controversial issues. For example, the editors try to make it clear – in very vivid style – that neoclassics is outdated and is already being replaced by cognitively or psychologically oriented approaches. My own view is that there are good reasons for keeping at least the homo oeconomicus core of neoclassics, but at least the outcome of this controversy – unlike Mirowski and Sent (e.g., 1, 53, 58f.) want us to believe – is far from decided.

One minor criticism I have to add: Why do the statistics on science collected in this volume nearly exclusively concern – as Mirowski and Sent openly admit – the US and Great Britain? Are there too few statistics available from other countries? In this line, cultural differences like the greater sociological orientation of economics of science in GB could have been elaborated on. Here, the interesting thing would be an analysis of how the different institutions shape the different scientific cultures.

Concerning the future of the economics of science: I agree with Mirowski in so far as you cannot employ the concept of a black-boxed marketplace of ideas (p. 59) any longer. Social structures, institutions should become chief objects of analysis by economists of science. The question is whether the core of economics has to be changed for this goal. The editors make much of the supposedly "unrealistic" character of homo oeconomicus. But should greater "realism" be the only (or even the most important) goal of economic models? The practical, institutional perspective calls for analytic tools that can be used to test the stability of institutions under conditions *if* actors behave in the homo oeconomicus way. And actors will behave in this way if they frequently run into problems of interaction (e.g. the prisoners' dilemma) which leave them worse off than their competitors. These situations are studied in

many branches of economics, and they could be used as a starting point for identifying institutional problems in science. This might be a fruitful perspective for economics of science, one which would certainly build on much work comprised in this volume.

Christoph Lütge

University of Munich

Department of Philosophy

Chair for Philosophy and Economics

Ludwigstrasse 31

80539 Munich

Germany

Christoph@Luetge.de