Peddling Science: An Essay Review of *Science Bought and Sold: Essays in the Economics of Science**

Jesús P. Zamora Bonilla†‡

Science Bought and Sold collects a large portion of the most relevant works on the 'economics of scientific knowledge production,' as well as other more recent and unpublished papers on the topic, and the long introductory essay by the editors is an illuminating guide to the field. In this critical notice, I argue that economic theorising about scientific research is providing a peaceful meeting point for many of the combatants in the 'science wars,' one from which both epistemic and political questions about science can be more rationally set forth.

Philip Mirowski and Esther-Mirjam Sent (eds.), ***Science Bought and Sold: Essays in the Economics of Science***. Chicago: The University of Chicago Press (2002), ix + 573 pp., $80.00 (cloth), $33.00 (paper).

That some *social* aspects of scientific research are extremely relevant for an adequate understanding of epistemological topics is a thesis that will hardly surprise philosophers of science; even a majority of them will agree on it, at least under some common interpretations. Until very recently, however, the *economic* facts and ideas about the working of science have been largely ignored within the philosophical realm, as simply something paradigmatic of what had to be taken as an *external* (i.e., non-epistemic) factor to the process of scientific research. From the point of view of epistemologists and methodologists, and probably for many other people

*Received May 2003; revised June 2003.

†To contact the author write to Faculted de Filosofía, UNED, 28040 Madrid, Spain; e-mail: jpzb@fsof.uned.es.

‡The author thanks the support of Spanish government MCYT's research projects BFF2002-04454-C10-01 ('The culture of techno-science') and BFF2002-03656 ('Cognitive roots in the assessment of new information technologies').

Philosophy of Science, 70 (October 2003) pp. 833–839. 0031-8248/2003/7004-0010$10.00

engaged or interested in science, the influence of money and other economic resources amounted simply to the principle that, the more resources are devoted to a line of research, the more likely it is that right results are discovered sooner (at least if there is some little bit of truth to it), though, on the other hand, when too much money is put in the hands of a scientist, the more likely it is that he or she becomes distracted from pure scientific motivations. Underlying this point of view was the idea that the 'economic' aspects of science were only those related to the material, and more particularly, the financial means devoted to it. Nevertheless, during the last ten or fifteen years a growing cluster of people have become interested not only in analysing the flow of economic *resources* within the domain of science, but in applying, in a more or less systematic fashion, economic *thinking* to the study of a wide range of aspects of the process of production, circulation and exploitation of scientific knowledge.

The book edited by Philip Mirowski and Esther-Mirjam Sent, which collects nineteen papers plus a long and enlightening introduction, is probably the best place to get a genuine idea of how wide and how powerful the applicability of economic methods to the study of science can be. The book is also honest enough in not trying to artificially construct, through the pieces collected in it, something like a 'canonical theory' of the economics of scientific knowledge, a theory which is far from existing. Rather, the book rightly presents the field as a dynamic landscape resulting from the intersection and overlapping of several heterogeneous (and, at more than one place, mutually contradictory) traditions of thought, like neoclassical economics, postmodern history of science, actor-network theory, the economics of information, naturalistic epistemology, economic methodology, or social studies of science. In a sense, the papers collected here serve to exhibit the new economics of science as a frame where a productive dialog between all these traditions is emerging, quite differently than it was, for example, in the case of the 'science wars.' If just because of this, philosophers of science should be interested in approaching this evolving field, though, as I shall soon explain, there are other reasons as well that make the economics of science extremely relevant for an appropriate understanding of scientific knowledge, reasons that are well exemplified in the papers collected in *Science Bought and Sold*.

These papers can be classified chronologically into three different categories. First, we have the 'classics,' some articles which are very often quoted but which were more or less difficult for may readers to access; these are Charles Sanders Peirce's "Note on the Theory of the Economy of Research" (1879; chapter 5), Richard Nelson's "The Simple Economics of Basic Scientific Research" (1959; chap. 3), Kenneth Arrow's "Economic Welfare and the Allocation of Resources for Invention" (1962;

chap. 4), and Michael Polanyi "The Republic of Science" (1969; chap. 17). Second, the editors have included four papers from the beginning of the nineties, which are milestones in the recent development of the economic study of science: Philip Kitcher's "The Organisation of Cognitive Labour"(excerpted from the last chapter of his 1993 book *The Advancement of Science;* chap. 8), where game theoretic modelling was first applied to a reconstruction of the 'entrepreneurial' behaviour of scientists during research processes; Wade Hands' "The Sociology of Scientific Knowledge: Some Thoughts on the Possibilities" (1994; chap. 19), where the idea of an 'economics of scientific knowledge' (parallel to the 'sociology of scientific knowledge') was firstly proposed; Partha Dasgupta and Paul David's seminal article "Toward a New Economics of Science" (1994; chap. 7), which introduced the use of the economics of information in the discussions about science policy; and James Wible's paper discussing Peirce's economy of research (1994; chap. 6).

Lastly, there is a set of papers either published after 1995 or previously unpublished, and most of them proceeding from the conference the editors organised in 1997 at the University of Notre Dame, on "The Need for a New Economics of Science." The topics covered in this last set of papers are multifarious, but they basically respond to two main worries: the new economic conditions of science in the globalization era, and the conception of scientific research as a process carried out by 'rational' agents. Some of these papers deal more directly with the first topic: Sheila Slaughter and Gary Rhoades' "The Emergence of a Competitiveness R&D Policy Coalition and the Commercialization of Academic Science and Technology" (chap. 1), Shaun Hargreaves-Heap's "Making British Universities Accountable: In the Public Interest?" (chap. 13), Paula Stephan and Sharon Levin's "The Importance of Implicit Contracts in Collaborative Research" (chap. 14), David Noble's "Digital Diploma Mills: The Automation of Higher Education" (chap. 15), and Steve Fuller's "The Road Not Taken: Revisiting the Original New Deal" (from his book *The Governance of Science* chap. 16). On the other hand, the conception of researchers as economic agents is discussed in John Ziman's "The Microeconomics of Academic Science" (chap. 10), William Brock and Steven Durlauf's "A Formal Model of Theory Choice in Science" (chap. 11), and Stephen Turner's "Scientists as Agents" (chap. 12). The three remaining papers clearly show the intimate connection between both problems; they are Paul Forman's "Recent Science: Late-Modern and Postmodern" (chap. 2), Michel Callon's "From Science as an Economic Activity to Socioeconomics of Scientific Research" (chap. 9), and Mario Biagioli's "The Instability of Authorship" (chap. 18).

As it is clear from the classification just offered, the 'new economics of science' is a field created by the crossing of essentially two different but

related families of concerns. In the first place, there is a generally uncomfortable feeling within academia about the trend toward the privatisation of scientific research and teaching, and toward the commoditization of scientific knowledge. Not all members of the academy (nor obviously other members of society) are so unhappy about this situation, however, for the trend exists because there is a combination of economic and political interests (e.g., researchers who become billionaires thanks to the patenting of their discoveries, politicians who can save substantial parts of the public budgets thanks to the enhanced self-funding capability of the universities) pushing the research system in that direction. But the works selected by Mirowski and Sent focus almost exclusively on the (truly worrying) negative aspects of this tendency, and perhaps the inclusion of some paper more favourable to the privatisation of science would have given the book a more neutral stance about this problem. After all, one may argue that much complaining about the 'lost good old days' when scientists were more generously funded, had more liberty for choosing their research projects, and were less controlled in their teaching activities (if those days have existed at all), just expresses the scientists' desire for economic and social privileges. What really deserves to be discussed is whether the new 'science regime,' in which knowledge production tends to be financed by private firms through the selling of final goods and services, *is in general better or worse for the average consumer-taxpayer-citizen* than the past regime, under which basic science was fundamentally financed as a public good by means of tax revenues. I have to confess that no clear and convincing answer to this question emerges from the reading of *Science Bought and Sold* (nor, by the way, from other works in the new economics of science!), though the papers by Forman, Callon, Hargreaves-Heap, Nobel and Fuller, all provide some interesting data and suggestions (for a radical defence of the privatisation of basic research, though probably not very well grounded from an economic point of view, see Kealy 1996).

In the second place, the tendency towards the 'naturalisation' of epistemology has led, among other things, to the search for 'scientific' methods to scrutinise the behaviour of scientists. An obvious choice is the powerful toolbox of economics (particularly, microeconomic theory), which can either be seen as a kind of analysis competing with more traditional sociological approaches, or as a perspective which is complementary of them. In favour of their complementarity we can say, first, that modern microeconomic analysis does not reduce to the assumption of utility maximisation by omniscient individuals, but has developed a much richer and more realistic set of conceptual and mathematical approaches. (Some of them are well exemplified in the papers by Kitcher, Dasgupta and David; and Brock and Durlauf; the papers by Callon, Turner, and Stephan and

Levin also refer to some of those formal approaches, though without putting them explicitly to use.) And second, several social conceptions of science (in particular, those in so-called 'laboratory studies' and 'actor-network theory') have very often employed the notion of scientists as more or less rational agents engaging in a social network of interdependencies, something which is absolutely apt for being studied with the help of those mathematical techniques (in this sense, it is certainly a pity that the seminal paper by Bourdieu (1975), relating the circulation of scientific credit to the capitalist system, has not been included in the book). In general, microeconomic reconstructions of research processes can lead to an analysis of the social-constructedness of science which is conceptually more transparent, more suitable for rational criticism, and more useful for drawing practical remedies for the detected problems, than the extravagant language employed by some postmodern deconstructionists in their analyses of science.

In spite of this, being myself a practitioner of 'microeconomic epistemology,' my own impression is that the efforts made by people like Kitcher, Brock, and Durlauf, have not received until now the attention they deserve among philosophers and sociologists of science, and I hope that the inclusion of their works in a book like *Science Bought and Sold* may help to remedy this situation. The main reasons why these efforts have not stimulated the production of more research of this kind may be the following three, which are applicable as well to other cases where 'non-economic' phenomena have been approached with the help of economic conceptual tools. First, microeconomic models are usually so difficult to understand for people without appropriate training in the relevant mathematics, that a majority of philosophers or sociologists prefer simply to ignore this kind of works, and may feel that this expansionism of economics is an unacceptable intrusion in 'their' fields. Second, many authors complain that the instruments of microeconomic analysis are not well tuned enough to capture the complexity of many social situations, nor the expressive (not merely instrumental) nature of much social behaviour. Third, outside of the economic realm, where we are lucky enough of having the 'measuring rod of money,' other aspects of social behaviour do not apparently have a system of quantities to which we could apply the standard mathematics of economic analysis, and so, everything quantitative we could assert about those social phenomena would be extremely ambiguous and uncertain. Nevertheless, there are responses to all these arguments: first, a mutual effort should be made by philosophers and sociologists, on the one hand, to improve their understanding of mathematical tools, and by economists, on the other hand, to make the tools more accessible to the non-expert; second, the economic modelling of social phenomena does not attempt to offer an exhaustive explanation,

but only a new useful perspective from which to understand those phenomena, a perspective which, as I just said, is better seen as complementary of others; and third, it is simply not true that economic models, particularly game-theoretic ones, presuppose that the phenomena to be explained need be essentially quantitative. Surely, not all these conditions, and particularly the first one, are met in every paper collected in *Science Bought and Sold,* for it is true that economists, like many other scholars, seem to experience some kind of delight in writing in a way which is profoundly unintelligible for 'outsiders,' but we can hope that many works written *after* the publication of this book will be influenced by its spirit of transdisciplinary dialog.

Be all that as it may, some might still express doubts about the stability of a research field which is created by the more or less accidental encounter of researchers with so different interests: people preoccupied in one sense or another about the future of publicly funded research, people engaged in an arms race on both sides of the science wars, and people interested in fostering a demand for the economic modelling techniques they are supplying. Certainly, the evolution of a field like this can hardly be predicted; it can either be the case that in twenty years time all this will be seen as just another ephemeral intellectual fashion that entertained the minds of a fistful of people during a little time, or, instead, that the economics of scientific knowledge becomes established as a powerful discipline, as well as a strong bridge connecting the intellectual interests of other disciplines now much separated, as rationalistic epistemology, economic theory, and social studies of science. If only for having the chance of witnessing 'from the inside' a so marvellous case study in the sociology of academic disciplines, I think both philosophers and sociologists of science should have an interest in taking part in the intellectual dialog which is taking place within this under-construction field. *Science Bought and Sold* can be seen both as a determined attempt to push the evolution of the economics of scientific knowledge towards the second possibility mentioned above, and also (given the well known philosophical positions of their editors) as a reflexive exercise in 'case-studying' the very same discipline they are helping to constitute.

A testimony of this is the skepticism about the objectivity of economic theorising that one finds at many places of the "Introduction," as, for example, when the editors assert that "the papers on science as a production process are Cold War artifacts through and through" (43), or that "this 'implicit contracts' version of an economics of science has all the drawbacks of inertia often attributed to old generals" (53). In general, Mirowski and Sent try to persuade the reader that the conceptions developed for the economic study of science in a certain period of history are ultimately (though not only) a result of the way science is economically and polit-

ically organised in that period, and to a great extent an ideological justification of this organisation. If this were true, we would not have any hope of developing a convincing economic explanation of the institutional evolution of science (i. e., of the evolution of its norms), or at least of having some epistemic progress in our understanding of this problem, which is an essential goal of many chapters in the book. As a way of closing this review, I do not resist the temptation of using here one piece of my own work as a counterexample to at least one of the arguments the editors employ in their attempt to relativize the (certainly modest) objectivity claims of some contributions to the economics of science. In their comments on the Brock-Durlauf model (on consensus formation within a scientific community), the editors assert:

> "(it) is a slightly modified version of a model found in statistical mechanics, that of Ising spins and phase transitions using mean field techniques . . . As in so many other cases in the history of neoclassical economics, the issue will not be so much to challenge the exact isomorphism between the physical model and the social phenomenon . . . , but rather to ask, Why is this metaphor deemed compelling or net? . . . Isn't the role of the economist rather an attenuated one of merely cheering on the project of metaphorical transfer but ultimately desisting from proposing anything that is really more intrinsically 'economic' in scope? . . . It would seem that in the case of Brock and Durlauf, that is essentially what has happened." (56–57)

The fact is that too much is derived by Mirowski and Sent from their premise about the origin of the mathematical techniques employed by Brock and Durlauf. This can be asserted because there is another economic model (Zamora Bonilla 1999) which reaches essentially the same conclusions as theirs, but which uses a completely different formal approach, one which, I can assure, owes nothing to physical modelling techniques, of which I am thoroughly ignorant! Obviously, this does not certify that these economic models are 'right,' nor makes the study of the origin of their intellectual roots irrelevant or uninteresting, but the story suggests that the more important criticisms of an economic model should always be those referring to what it explicitly asserts.

REFERENCES

Bourdieu, Pierre (1975), "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason", *Social Science Information* 14: 19–47.

Kealy, Terence (1996), *The Economic Laws of Scientific Research.* London: Macmillan.

Zamora Bonilla, Jesús P. (1999), "The Elementary Economics of Scientific Consensus", *Theoria* 14: 461–88.