Scientific Competition

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What is scientific competition? When this question is posed by an economist, many people think they already know what the answer must be: science is a market of ideas, and scientific competition is like market competition. Surprisingly, the economics of science[1] gives quite a different answer.

Of course, a certain part of science, called commercial or proprietary science, is a market of ideas. In proprietary science, the results of research are protected by intellectual property rights, mostly patents or trade secrets; they can be bought and sold, and their market value derives from the market value of the goods they help to produce. Moreover, the expected market value of an idea provides the incentives for investments in research.

Competition in proprietary science is not *like* market competition; it *is* market competition. In contrast, scientific competition means competition within academic or open science and its institutions: learned societies, scientific journals, the peer review system, Nobel prizes, and modern research-oriented universities.

In open science, ideas are not protected by intellectual property rights. Contributions to open science are published, and the ideas they contain can be used free of charge by anybody who wishes to do so. Although these ideas are nobody's property in a legal sense, their use is regulated by moral rights or norms. Researchers morally "own" results if they were the first to publish them (the so-called priority rule, see Merton 1973); they have a moral right, then, to be cited by those using their results. The extent to which a researcher's ideas are used by others determines the researcher's status in the scientific community (Merton 1973, Hull 1988). Status is not only a reward on its own (Marmot 2004), but also the key to other, material rewards in open science. Just like patents in proprietary science, then, the norms of open science generate incentives to invest in new ideas.

Is open science a market of ideas? There are certainly many similarities. In open science as in markets, we observe production, division of labor, specialization, investments, exchange, risk-taking, competition but also cooperation, and so forth.[2] However, these are aspects of almost all human endeavors. It is more informative to look for differences. The most important difference is that both institutions use different mechanisms of collective

[1] For surveys, see Diamond (1996, forthcoming), Stephan (1996, forthcoming).

[2] On differences and similarities between competition in science and on markets, see Walstad (2002).

Many collective decisions are made through voluntary contributions, from the cleanliness of public spaces, which is largely determined by voluntary individual effort, to the financial volume of private disaster relief. Usually, voluntary contributions determine only the supply of some good. The special twist of scientific competition is that the voluntary contributions mechanism regulates both, supply of and demand for research.

Looking at the supply side, we find that researchers in open science are not paid for each contribution. They receive a lump-sum salary that covers research and, possibly, other activities, notably teaching, but in the short run neither this salary nor other possible rewards vary with the number and quality of their contributions. Since, in most cases, nobody demands a specific contribution, individual contributions are voluntary, unsolicited, and unpaid.

The motives behind volunteering are well-known.[3] We can distinguish between consumption and investment motives. Consumption motives are enjoyment of one's work, reciprocity or altruism (which are similar to enjoyment), and the striving for recognition and status, especially among insiders. In the case of science, curiosity is often mentioned, which is an aspect of enjoyment. Enjoyment of work usually requires the freedom to choose one's tasks and the absence of control, which are characteristics of open science. Investment aspects are networking, building human capital, and signaling one's ability. In the case of science, signaling one's ability goes hand in hand with acquiring status among insiders; it does not matter whether one emphasizes the investment or the consumption aspect.

Looking at the demand side, we see that the scientific community decides, in a decentralized way, about a contribution's success. Science is cumulative: one researcher's output is the next researcher's input. A successful contribution is one that is used by other researchers as input for their own research. The more it is used, the higher the success. Citation statistics and impact factors are relevant because they measure the use of ideas.[4]

Researchers in open science compete in providing inputs for their peers. If they want to be successful, they must anticipate what kind of input other researchers would like to use; their success depends on the decisions of their peers. This mechanism should not be confused with peer review. Peer review is used to select among research proposals that compete for funding, or among papers that compete for publication in prestigious journals. It is a secondary

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[3] See the overview in Hackl et al. (2005), partially published in Hackl et al. (2007).

[4] Though only very approximately: important ideas are used without citation when they have become textbook knowledge; on the other hand, many citations do not indicate use of ideas but only demarcate the contribution of a paper.

Why scientific competition? Traditionally, economists have taken it for granted that the price mechanism is the only efficient mechanism of collective decision making. From this point of view, scientific competition should be replaced by the price mechanism. However, with the rise of the new institutional economics (see Furubotn and Richter 2005) and its integration in the economic mainstream, the traditional view has lost its plausibility. Economists have learned that markets are not always better than hierarchies, and that majority voting may be ex ante efficient. Similarly, the economics of science started with an argument against the price mechanism.

In their pioneering contributions, Nelson (1959) and Arrow (1962) analyzed the shortcomings of the price mechanism in scientific research: The exclusion of potential users of an idea is inefficient because additional users create no additional costs. Even with patent protection, the returns on investment in research can be appropriated only to some extent. The outcomes of research are highly unpredictable; thus, researchers will need insurance, but insurance dilutes the researchers' incentives. Consequently, investment in research and utilization of its results will typically be too low. Moreover, results will sometimes be kept secret, which impedes further research. These problems will be more pronounced for basic than for applied research.

With respect to basic research, Nelson and Arrow considered open, or not-for-profit, science as a solution, without, however, analyzing it in detail. This was done by Dasgupta and David (1994). At the heart of their argument for open science is a massive delegation problem. In basic research, employers of researchers lack the knowledge to judge the quality of research results and, consequently, the achievements of researchers. They cannot effectively monitor the efforts of researchers, and they cannot judge the results of these efforts. Hence, they cannot hire researchers on the basis of incentive contracts that condition payment on the quality of results. Scientific competition solves this delegation problem. It provides incentives to researchers and generates evaluations of researchers (i.e., scientific reputations) and of research results (i.e., extent of use by the scientific community) that can be observed and used by employers. Indeed, these achievements of scientific competition may explain the existence of open science (David 1998, 2004).

Why care about scientific competition? European science policy seems currently to be fixated on the idea that promoting competition between universities is the key to improvements in the European system of scientific research (see, e.g., EU Commission 2003, 2005).

Historically, however, university competition has been neither sufficient nor necessary for the flourishing of scientific research. The successes of the 19th century Prussian university system were, to a large degree, due to central used it to hire young scientific high-potentials and to reward renowned researchers. Thus, the ministry circumvented university competition and, instead, made use of and promoted scientific competition. This central-planning regime was preceded by a very competitive decentralized system where universities competed for student fees. Every employee, from the professor to the caretaker, got their share: a textbook case of incentive pay. However, in this system, the scientific standards of university education were very low, and universities played no role in research.[5]

The point of these historical facts is, of course, not that central planning works better than competition, but that scientific competition is more important than university competition.

Scientific competition provides common pool resources for universities:[6] incentives for researchers to do research and to conform to scientific standards; evaluations of research results, which are used by universities for the development of academic curricula; and evaluations of researchers, which are used by universities for hiring and promotion decisions. These resources are only available, however, if universities allow their academic staff to participate in scientific competition.

Competition between users of a common pool resource easily leads to over-exploitation. Consider, for instance, the following plausible scenario. Universities compete for the services of renowned researchers, who get contracts that allow them to do their own research. Less renowned researchers have less bargaining power, and administrators put them to other uses: teaching, administration, and research that is profitable to the university but of no scientific interest. This is rational from the administration's point of view. However, scientific competition requires that researchers decide collectively about reputations, by accepting or rejecting new ideas as inputs for their own research. If universities want to employ researchers who have earned a reputation in this process, they must collectively bear the costs of letting other, less renowned researchers participate. Yet, each university is better off if it makes use of scientific competition without bearing its share of the costs. In this scenario, university competition will destroy scientific competition.

This is not the place to evaluate current policies. Our concern here is with the scientific basis of these policies, which fails to take scientific competition

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[5] See Clark (2006) and, specifically on the "System Althoff", Vereeck (2001). See Burchardt (1988, 185) for an example for the distribution of fees from the university of Berlin, and this university's statutes, *Statuten der Friedrich-Wilhelms-Universität in Berlin v. 31.10.1816,* which were typical for the time. I am am obliged to Lydia Buck for bringing these historical facts and the relevant literature to my attention.

[6] On common pool resources and their governance, see Ostrom (1990).

successful policies can be developed on such a basis.

The Contributions to this Volume

The papers in this volume deal with core aspects of the theory and policy of scientific competition. They have all been presented and extensively discussed at a conference in Saarbrücken in October 2005. They appear here in revised form, together with the revised versions of the comments that were also presented at the conference.

The economics of science has always been an interdisciplinary undertaking. Economists have learned much from sociology (see esp. Merton 1973). Problems of intellectual property rights are discussed by lawyers and economists. There are also strong connections between the philosophy of science, which has taken an institutionalist turn with the work of Karl Popper, and the economics of science (H. Albert 2006). The present volume continues the interdisciplinary tradition and contains contributions from economics, law, philosophy of science, political science, and sociology.

The first four papers are concerned with supply-side considerations: the supply of researchers and their productivity. Paula Stephan starts from the observation that employment conditions in science have changed. Today, the prerequisites for productive research – access to equipment and colleagues, a certain degree of autonomy, job or funding security – are often missing. An increasing percentage of young researchers get stuck in laboratory jobs where they are not doing their own research. These employment conditions will reduce the future supply of young researchers since the current generation's experiences influence the next generation's expectations. The current system of research may not be sustainable, then, since it requires a large supply of young researchers motivated by the expectation of getting one of the research positions that are becoming increasingly scarce.

Günther Schulze also looks at the supply of researchers, but from a very different perspective. He analyzes the supply of university professors through the states in a federal system. The number of professors is an important part of educational services; indeed, Schulze treats this number as a proxy for educational services. He shows that states have an incentive to attract high school graduates from other states by providing capacity in tertiary education, thereby free riding on educational services provided in the primary and secondary education by other states. Optimal tertiary education is less than proportional to the size of the jurisdiction. For Germany he shows current trends in provision of professors and the production of new professors, The next two papers are concerned with the measurement of productivity in science. Gustavo Crespi and Aldo Geuna consider the determinants of science research output (as measured by publications and citations) in the UK. They use an original dataset including information for the 52 "old" UK universities (which account for about 90% of research expenditure) across thirty scientific fields for a period of 18 years, from 1984/85 to 2001/02. On this basis, they investigate the relations between the investment in higher education and the research outputs, rejecting the model of a global science production function for the UK in favor of four significantly different production functions for the medical sciences, the social sciences, the natural sciences and engineering.

While Geuna and Crespi look at the macroeconomics of scientific productivity, Michael Rauber and Heinrich Ursprung focus on the microeconomic aspects. They argue that a bibliometric evaluation of researchers should take life cycle effects and vintage effects into account, and demonstrate the crucial importance of these effects in a bibliometric study of the research behavior of German academic economists. On the basis of this study, they develop a simple ranking formula that could be used for performance-related remuneration and track-record based allocation of research grants. They also investigate the persistence of individual productivity, which is relevant for tenure decisions, and develop a faculty ranking which is insensitive to the faculty age structures.

These supply-side considerations are followed by five papers that are concerned with specific institutional aspects of open science. Martin Kolmar compares open and proprietary science from a theoretical perspective. For the purposes of his paper, proprietary science is identified with research leading to patents. Open science is modeled as a contest for a prize (research grants, tenure, etc.), with the research output becoming a public good. Kolmar considers a case where the research results may be used to reduce production costs in an oligopolistic downstream market. Thus, the focus is on applied science, which is quite often viewed as the natural domain of proprietary science. Nevertheless, the patent system turns out to be inefficient, because the patent holder has an incentive to restrict the number of licenses too much and because incentives for research are too weak. Open science, on the other hand, may be efficient, and even when not, it may be second-best optimal.

Christine Godt is also concerned with problems of the patent system. She questions, from a lawyer's perspective, the view that the possibility of patenting actually provides incentives for a better technology transfer from research institutions to industry. The problem is that the accumulation of royalties through several stages of a typical innovation process – a phenomenon called "royalty stacking" – eats up the profit margins on the downstream tractual mechanism is primarily due to the transition from sale contracts to lease contracts in the downstream market. In combination, these two mechanisms can impede the technology transfer when the royalty share becomes too large.

Nicolas Carayol analyzes the theoretical basis of the so-called Matthew effect in science. This effect was proposed by Merton as an explanation of the typical career patterns in science. It assumes that early successes in science lead to a more successful career because successful young researchers get better jobs with better research opportunities. Thus, an outstanding career in science may be the result not of exceptional ability, but of accidental early success. Carayol explains the Matthew effect in a dynamic model of university competition. The basis of the effect is an externality between researchers: successful old researchers confer an advantage to their younger colleagues. This implies that young researchers who get jobs at high-reputation universities will go on to be more successful than their peers at low-reputation universities, which perpetuates the reputation differences between universities.

Carayol's model hints at a further important aspect of academic life. Externalities between researchers can be interpreted as access to research networks. The great practical importance of these networks becomes much clearer in Dorothea Jansen's paper, which reviews the results of a large sociological research project under her direction. The project focuses on networks in astrophysics, nanotechnology and microeconomics, collecting data on existing networks and analyzing correlations between network properties like size and density on the one hand and success in research on the other hand. The European and German science policies actively promote such networks. Among others, the empirical results show the first consequences of these policies.

Christian Seidl, Ulrich Schmidt and Peter Grösche present the results of an empirical investigation of the referee processes of economic journals. Peer review, and especially the referee process of scientific journals, is a central institution of modern open science. Seidl, Schmidt and Grösche argue that publications in refereed journals today serve mainly as quality signals, influencing personal advancement, research opportunities, salaries, grant-funding, promotion, and tenure. For this reason, they consider the validity, impartiality, and fairness of the referee process as very important. The literature, however, casts doubts on the idea that journal referee processes satisfy these requirements. Their own investigation shows that authors in economics value competence and carefulness of the reports more than positive decisions by editors. Competence and carefulness, however, are often missing. Moreover, reports in economics often fail to help authors improve their manuscripts.

philosophy of science with game theory, he conceives of science as a game of persuasion in which competition for status forces scientists to accept methodological rules and to acknowledge the contributions of their competitors. On the basis of a specific model, he argues that mutual control in a scientific community ensures that the norms of science are followed frequently, if not perfectly.

Christian List discusses collective decision making in science from a very different, non-competitive perspective, namely, social-choice theory. Drawing on models of judgment aggregation, he addresses the question of how a group of individuals, acting as a multi-agent cognitive system, can "track the truth" in the outputs it produces. He argues that a group's performance depends on its "aggregation procedure" – its mechanism for aggregating the group members' inputs into collective outputs; for instance, voting on the truth of propositions – and investigates the ways in which aggregation procedures matter. These considerations are highly relevant in connection with scientific committees that try, against the background of scientific competition with its differences of opinion, to formulate a scientific consensus, as, for instance, in the case of climate change.

These eleven papers, with accompanying comments, highlight the diverse problems and questions turning up when we try to understand scientific competition. They also illustrate the breadth of contemporary economics of science, its many ties with neighboring fields, and its potential to improve science policies.

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